George Stigler

Enigmatic Price Theorist of the Twentieth Century

Edited by Craig Freedman

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Cover credit: Stigler papers, Regenstein Library Archives, University of Chicago. London 1947, on their way to the first Mont Pelerin Society meeting, from left to right: Aaron Director, George Stigler and Milton Friedman

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The Journey—A Reminiscence

Me Came to Chicago,
way back,'53
Worked for Tolley and Gale,
Gregg, Gary and Al
Then for Stigler, in years, 33
They said I was destined to fail
Stigler was, as you know, hard as nails
But under that shell, oh,
Beat a heart of red Jello
And I coasted through on his coat-tails![1]

—Claire Friedland

[1]This previously unpublished poem is by George Stigler's long time research assistant and indispensable co-worker, Claire Friedland. As the poem points out, she spent 33 years working alongside Stigler from the early days of his return to Chicago until his death (1958–1991). She writes, "I was an RA in my student days for in order: George Tolley, D. Gale Johnson, H. Gregg Lewis, Gary Becker and Arnold [Al] Harberger" (Correspondence with Claire Friedland, October 2017). Claire Friedland has always proved to be a treasure trove of information, insight and inspiration in my own search to understand the ever elusive George Stigler. The poem points out a surprising Japanese contradiction in Stigler's nature, namely the difference between his *tatemae* (the outside appearance he displayed to the world) and his *honne* (the inside truth which generated an intense loyalty from his circle of friends). My thanks and appreciation to Claire Friedland for granting the rights to publish her thoughts.

To my daughters Emily and Nicola who neither know, or care to know, who George Stigler might be.

Heart on a Sleeve

> A student once asked me what was "beneath George Stigler's hard, sarcastic exterior." How could I resist answering "A hard, sarcastic interior"? In reality, it was a question I couldn't answer at the time; it had been only 10 or 15 years that I'd been doing research for, and with, George. Today I think I'd respond "Sarcastic? Well, yes. But hard? No, I don't think so." Although I had often said that George was irrationally rational, in certain areas he was irrationally generous. (Friedland 1993: 780)

Growing up as a single child in suburban Seattle, shy and certainly not gregarious, George Stigler learned to keep his feelings close to his vest. Instead, he seemingly uses his caustic wit to keep friends, colleagues, students, and opponents at arm's length, if not further. The heart that beat under all that protective camouflage was hard to detect and certainly even the best researcher can only provide glimpses that attempt to explain such a complex figure. A safer strategy, forming the bulk of this volume, is to focus on his academic output which is certainly more open to analysis. Still, George Stigler, as a man, remains an enticing mystery. In some sense he is something of a contradiction, a shadowy romantic outlaw. (Though in fiction, as opposed to everyday life, the figure is almost a cliché.)

One aspect of Stigler, the one more open to inspection, is his career as an academic gunslinger. Not quite a gun for hire as much as a gun in the service to a cause, the ideological right-handed quick-draw of the marketplace. To opponents, perhaps it would not be too far off the mark to regard him as a figure drawn directly from the classic western *Shane.* Specifically the Jack Palance character, attired completely in black and giving the evil eye to all the quivering and fearful sodbusters. As the living manifestation of that cinematic creation, George Stigler appeared to be almost programmed to mercilessly eliminate all opposition by whatever available means might prove to be effective. Eradication, rather than engagement, was both his strategy and objective.

But there is also a hidden Stigler, shown by an almost unacknowledged generosity that imbues him with a deeply romantic heart. Throughout his career he was also something of a Don Quixote figure in the best sense of that description. Someone who felt compelled to defend the honour of the marketplace, seeing it as almost his duty and obligation to embellish its institutional beauty and to ignore its blemishes. Done not because of any intrinsic dishonesty, but to serve a higher goal. Stigler is even akin to Fitzgerald's Jay Gatsby, creating himself seemingly out of thin air from his beginnings as a gauche provincial boy. In neoclassical price theory he discovered his Daisy Buchanan, the ideal for which he strove. He successfully created the world he wanted so badly, seeming to capture for himself what had slipped so carelessly through Jay Gatsby's fingers. Something of that life as an academic and romantic adventurer can be glimpsed in the cover photo in this volume. In it George Stigler and his two co-conspirators (Milton Friedman and Aaron Director) are caught at the start of an ascent that would change the shape and face of economics. This picture, taken in 1947 right before the first Mont Perlerin meeting, shows these three still young men at the base camp of their careers, in some sense parallel to Edmund Hillary before his achievement in scaling Mount Everest. There is a wide-eyed determination in their vision as though ready for whatever was to come, even though at that moment they had little conception of where their attempt to refigure economics would eventually take them. The journey George Stigler would ultimately complete would transform him into an appropriate subject for detailed and almost forensic analyses. Future investigations must further delve into essential aspects of his research, papers and speeches. This volume serves as something of an introduction to unraveling the enigma of George Stigler.

Sydney, Australia February 2020 Craig Freedman

By Way of a Contradiction

> But in any case I really feel that he is one of the most interesting and in some sense, most enigmatic characters in our profession in this last century. (Conversation with Arnold Harberger, August 1997)

Since any potential reader is faced in this case with a rather voluminous edition, my perceived responsibility, at least as far as I can ascertain, is to write something of a non-preface-preface. Or as that original thinker Owl (resident of the 100 Acre Wood) sharply perceived, a response that is more of a contradiction than either an introduction or definite statement. Here I am relieved of a great deal of responsibility by knowing that only the obsessive or the hopelessly compulsive bother to pay any serious attention to a preface no matter how it might be labeled. Accordingly, I feel as though I have been granted free range to doodle along a bit without needing to meet any subscribed objectives or conventions.

In which case, let me confess that neither here, nor in what passes as an introduction, will you find anything like a discussion of the individual contributions to this volume. My residual faith is that those opening the book can read and judge for themselves. If not, if in fact anything like a breezy tour through the book is required, perhaps potential readers would find more rewarding alternatives elsewhere.

The book, after an introduction that attempts to set the stage, commences with a series of conversations conducted back in 1997 with a number of individuals who knew George Stigler to varying degrees. While interesting in themselves, they not only provide contemporary judgments of Stigler's extensive contributions to the discipline but also sketch a portrait of the man himself. Doing so is perhaps of particular importance when it comes to understanding Stigler. He was a skilled writer who took pains with the language he employed. He also had a caustic wit which would inevitably bubble up in all his writings. Therefore, even what he would classify as his scientific tracts need to be read with care and placed in context with all his other writings and thoughts. Mistaking him for some generic writer can assist in confusing a more prosaic reader from gaining any deeper understanding of his work.

Lastly, inclusion into this volume is not any reflection of my own views. These simply are of small importance. Contributions were chosen for their interest and insight. The chapters should make you think and reconsider your own positions. Moreover, George Stigler during his lifetime greatly helped to shape modern economics. The issues he grappled with are still very much alive today. The hope then is that this volume will assist those economists who take their work seriously, without at the same time overinflating their own efforts.

There are a few required acknowledgments, a list that will be deliberately incomplete. Like film credits, such acknowledgments have grown into something resembling a field of kudzu over the last couple of decades. Everyone is included in the fear of omitting and offending someone, even though they might only be peripherally attached to a given project. But I do want to acknowledge those who were gracious enough to talk to me about George Stigler either formally or informally. Such include, Milton and Rose Friedman, Aaron Director, Armen Alchian, Harold Demsetz, Claire Friedland, Stephen Stigler, Sam Peltzman, Ronald Coase, Lester Telser, Gary Becker, James Kindahl, Paul Samuelson, Robert Solow and Paul Sweezy, many of whom, unfortunately, are no longer with us. Mistakes, errors and points of contention

assuredly exist in this volume as they exist inevitably in every publication. Fortunately there is never a lack of readers and critics, who are more than eager to point out any shortcomings.

The book comes complete with a nifty cover and can work effectively as a doorstop or a paperweight. Reason enough to purchase a copy even though a small loan might be necessary to finance the cost.

Sydney, Australia Craig Freedman
February 2020

The Lonely Heart: An Attempt in Search of a Preface

I have now spent some decades trying to puzzle out the ever deepening enigma that was George Stigler. Because of the nature of the man, increasingly the truth remains that I thought I understood more about him two decades ago than I do today. This sad fact has become increasingly true, especially on my more befuddled mornings. He remains the perfect embodiment of the dichotomy on which the Japanese have bestowed the labels *honne* and *tatemae*. The problem remains that the *tatemae*, the outside show that is on display, is often only a very treacherous reflection of the *honne*, the unvoiced or expressed core essence of a person. More than most, George Stigler remains a puzzle, whose composed and revealed bits refuse to form a coherent whole. The problem then remains that since a more interpretative judgement is required when weighing up and gluing down all the available existing evidence, the final result inevitably becomes more arguable. The unfortunate allure when faced with such a high degree of complexity, is to try to force all the ragged jigsaw pieces into forming a simple, conjoining picture. Part of the problem in doing so is that Stigler, like many men of his generation, was not given to deep introspection.[1] He could not reveal that which remained hidden to him as well. Like the strong, silent Hollywood types of the thirties, he was what he did. Or in the words of a role model of that era, 'A man's gotta do what a man's gotta do'. We then only have his work and actions to guide us.[2] His deepest thoughts remain buried.

Even his very writing style can pose a serious distraction to delving into his fundamental intentions and meanings.[3] His writing is often tongue in cheek, larded with sardonic observations and judgments. These pronouncements lack any notion of reliable signalling, so that even the most acute reader may fail to catch their drift. Thus reading his work in a fundamentalist or literal manner is likely to lead one astray. To comprehend what the underlying narrative is in much of his work, one needs in some degree to understand the man, his goals and strategies.

What the curious reader will then find in this volume is a brave attempt to parallel Stigler's methods. The contributions are varied and may at times be contradictory, somehow reflecting the discordant pieces composing Stigler's thoughts, despite his persistent quest for consistency and a palpable need to develop an all-embracing comprehensive theory. Many of the articles that have found their way into this volume will appear to apply different ideological lenses. That result is almost inevitable if an editor is searching for thought provoking pieces rather than a priori restatements of set ideas. In keeping with such purposes, the introductory chapter breaks sharply from standard practice by not referring to the volume's individual contributions, let alone previewing them, like the endless trailers one is force-fed in darkened cinemas. The editor trusts in the judgement of the volume's readers and would rather let them form their own evaluations. Instead, the introduction hopes to provide a starting point in this lengthy investigation into one of the post-war era's best and sharpest minds (at least those that are economically bent). Again, that overview is simply the way in which the editor understands this Nobel Prize academic after many decades of trying to locate the *honne* hidden beneath those protective layers of *tatemae.*

Sydney, Australia
February 2020

Craig Freedman

Notes

1. Perhaps Japanese style *honne* is difficult to discover in the case of Stigler simply because it is covered by so many protective layers that even he was loathe to disturb. In his own way he shares one Lear-like feature, without hardly qualifying as either a tragic or foolish figure. As Regan evaluates her father, it is more the inability or unwillingness to engage in self-examination, rather than the infirmity of old age, that leads Lear to make deplorable decisions. 'Tis the infirmity of his age; yet he hath ever but slenderly known himself' (Act 1, Scene 1: line 294—*King Lear*).
2. In a fundamental sense he acted in an economically efficient, albeit, expedient fashion. He never wasted words arguing unless he determined the objective was worth the opportunity cost of the effort. In most cases he would attack opponents only when he deemed the issue to be of sufficient import. Having stirred the pot, he would move on, never to return. Otherwise he would not deign to take note of opposing views.

    > He was a very strange person to sum up because his methods were not those of anyone else. I think he was quite unique. If you put a point to him, he liked to answer it with a joke. Then if you pressed him, he produced some fact or other. You pressed again, he'd give you his answer. But he was sort of an economist even in argument. He used the easiest way. (Conversation with Ronald Coase, October 1997)

3. Certainly trying to appreciate the different levels of his arguments is not for the very literal minded. He chooses his words and descriptions carefully. When he mockingly speaks of 'Lord Keynes', the title is not given as a sign of respect for class differences or honorifics. Looking at his words without taking into account the ever present bite is analogous to being an art critic while lacking any appreciation for, or knowledge of, chiaroscuro in painting. His reasoning is often more montage than straight line plodding that perhaps forms the norm for many other economists.

    > I think he was one of the most difficult people to explain because I mean, there is no one like him. I've described how in an argument he jumps around. He puts in a bit of theory, a bit of statistics, a reference to the earlier economists. It's like no one else's form of argument that you can recall. (Conversation with Ronald Coase, October 1997)

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Acknowledgements

The Inevitable Set of Acknowledgements—Leaving No Turn Un-stoned

> We have not read these authors: we should consider their arguments preposterous if they were to fall into our hands. Nevertheless we should not, I fancy, think as we do if Hobbes, Locke, Hume, Rousseau, Paley, Adam Smith, Bentham and Miss Martineau had not thought and written as they did. (John Maynard Keynes quoted in Routh 1975: 1)

Years ago I was accused by a reviewer of churlishly not providing a sufficient number of expected acknowledgments to a set of collected articles. Of course, this was someone who zealously tracked down each and every typo appearing in that work. (There were no lack of them.) However, the reason I sometimes seem parsimonious in my list of acknowledgements, like some modern-day Scrooge only reluctantly paying out words of praise, is that I prefer to limit such figurative tips of the hat to those who actually provided significant assistance. This rule of thumb does recognizably go against the more recent tradition built upon a morbid fear of offending. People are named for just being

in the general vicinity of the writer during the composition and prior research that generates any given piece of work. Recognition is spread so widely and thinly that the few people who provided essential assistance get lost in the legion of acknowledged names. This defensive stance of covering all bases has exploded the closing cinematic credits from a few concise minutes at the end of a film to something closer to a ten to fifteen-minute marathon that induces only masochists to sit through. So I will dutifully restrict my thanks to those that did provide that level of essential support.

Given this raised barrier, I am obliged to thank all the valiant contributors to this volume who took the time and put in the effort to insure a successful result. They are all listed on the contents page and their names need not be repeated. I should also like to thank sincerely and deeply all those who were willing to sit down and speak with me some two decades ago. Some of those conversations are included in this volume, but a complete list is more than appropriate. I list in chronological order, namely from first spoken to till last:

- Milton Friedman
- Rose Friedman
- Aaron Director
- Harold Demsetz
- Arnold Harberger
- Armen Alchian
- Claire Friedland
- Sam Peltzman
- Stephen Stigler
- Sherwin Rosen
- Ronald Coase
- Gary Becker
- Lester Telser
- Paul Samuelson
- Robert Solow
- James Kindahl
- Paul Sweezy
- Mark Blaug

Unfortunately, many on this list are no longer with us. Quite naturally I need to acknowledge George Joseph Stigler. Without him, this volume would have made no sense whatsoever. Lastly, I have to deeply thank the ghost of John D. Rockefeller without whose generosity the University of Chicago would have failed to exist, making a Chicago School of economics a mere figment of someone's disturbed imagination.

Reference

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Contents

Notes on Contributors

M. Ali Khan (Abram Hutzler Professor of Political Economy) obtained his B.Sc. (Econ) from the London School of Economics, and M. Phil. and Ph.D. from Yale in 1969, 1972 and 1973, respectively. He is interested in how economics stands in relation to mathematics and language as well as to issues of ethics and epistemology, and how these registers call the robustness of disciplinary boundaries of social sciences into question. This has led him to the "economics of the eighteenth century", and more generally to the language of commerce in a variety of texts that includes the Quran.

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Marina Bianchi is Professor of Economics at the University of Cassino, Italy, Department of Economics and Law. Her main area of interest is the analysis of consumption choice, preference formation and the role of novelty in consumption. She has tried to provide an alternative approach to the traditional economic framework that can explain apparent paradoxes of behaviour ranging from addiction to collecting, from fashion to the social dimension of consumption. More recently, her interests have focused on the role of curiosity and exploratory behaviour in shaping aesthetic preferences, and how storytelling helps providing the mental maps that form choices.

Peter J. Boettke is a University Professor of Economics and Philosophy at George Mason University and the Director of the F.A. Hayek Program for Advanced Study in Philosophy, Politics, and Economics at the Mercatus Center at George Mason University. Before joining the faculty at George Mason University in 1998, Boettke taught at New York University. In addition, Boettke was a National Fellow at the Hoover Institution for War, Revolution and Peace at Stanford University during the 1992–1993 academic years and the F.A. Hayek Fellow in 2004 and 2006 at the London School of Economics as well as a Visiting Professor or scholar at the Russian Academy of Sciences in Moscow, the Max Planck Institute for Research into Economic Systems in Jena, Germany, the Stockholm School of Economics, Central European University in Prague, and the Charles University in Prague. In March 2011, Boettke was a Fulbright Fellow at the University of Economics in Prague, Czech Republic. He has served as the President of the Southern Economic Association from 2015 to 2017, the Mont Pelerin Society from 2016 to 2018, the Association of Private Enterprise Education from 2013 to 2014, and the Society for the Development of Austrian Economics from 1999 to 2001. In 2013, Boettke became a Founding Honorary President and Honorary President for the World Interdisciplinary Network for Institutional Research. He currently serves as the Co-Editor-in-Chief of *The Review of Austrian Economics* and the Associate Editor of the *Journal of Economic Behavior & Organization*.

Gordon L. Brady is currently Senior Research Fellow, Center for Free Enterprise at Florida Southern College. His work builds upon micro and macroeconomic theory with emphasis on Public Choice and the economic analysis of law and regulatory institutions. In addition to his academic work, he has extensive experience in government as a senior economic advisor with emphasis on research and publications in fiscal austerity policy, environmental economics, regulation, and management of common property resources. One of his major interests is the history of the Virginia school of political economy and its roots in the Chicago school of economics.

Rosolino A. Candela is an Associate Director of Academic and Student Programs and a Senior Fellow with the F.A. Hayek Program for Advanced Study in Philosophy, Politics, and Economics at the Mercatus Center at George Mason University. Prior to George Mason University, Candela taught in the Department of Economics at Brown University, where he was also a Postdoctoral Research Associate in the Political Theory Project. He was also a Visiting Professor of Economics at Universidad Francisco Marroquin, and a Visiting Fellow in the Department of Political and Social Sciences at the European University Institute. He currently serves as the Book Review Editor of *The Review of Austrian Economics*.

Professor Francesco Forte is a very prominent Italian economist, statesman, politician, public servant and businessman. He was born to a family of public servants and has continued this tradition throughout his multifaceted career. His grandmother was a baroness and his father was the public prosecutor in Busto Arsizio (Varese), Italy. Although his current position is Professor Emeritus of Economics at Sapienza, the University of Roma, he is anything but retired. He is senior editorial writer on economic matters and policy issues for *Il Foglio* and *Il Giornale.*

Craig Freedman Having failed to pursue a successful career as a CIA operative, Craig Freedman turned to a comparable dubious profession, that of attempting to masquerade as an academic economist. Whether that decision ultimately unlocked a treasure chest of valuable experience remains debatable. However, he has more recently managed to slip away from many of the constraints defining a career in the economics profession, enabling him to pursue a continued interest in, and a virtual immersion into, the mysteries of advanced tatting.

J. Daniel Hammond is Scholar in Residence at the Eudaimonia Institute, Wake Forest University, and Hultquist Family Professor Emeritus of Economics. He received a B.A. from Wake Forest and Ph.D. from the University of Virginia. His research has focused on the history of American economics in the twentieth century, especially at the University of Chicago. He served as President of the History of Economics Society in 2001–2002. Among his publications are *Theory and Measurement: Causality Issues in Milton Friedman's Monetary Economics* (Cambridge University Press, 1996), *Making Chicago Price Theory: Friedman-Stigler Correspondence, 1945–1957* (with Claire H. Hammond, Routledge, 2006), and "Paul Samuelson on Public Goods: The Road to Nihilism" (*History of Political Economy*, annual supplement, 2015).

Neil Hart is affiliated with the School of Economics and Industrial Relations Research Centre at the University of New South Wales, Australia, and the Institute of Business Research, University of Economics, Ho Chi Minh City, Vietnam. Journal and book publications have covered research areas including the history of economic thought, macroeconomic theory and policy, and heterodox approaches to economics.

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David M. Levy is Professor of economics at George Mason University. He is a distinguished fellow of the History of Economics Society. His Ph.D. dissertation at the University of Chicago was directed by George Stigler.

David Mitch is Professor and Chair, Department of Economics, University of Maryland, Baltimore County where he has taught since 1983. Since 1997, he has regularly taught a course on Asian Economic

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Edward Nik-Khah is Professor of Economics at Roanoke College. He is the author of *The Knowledge We Have Lost in Information* (Oxford University Press, 2017), written with Philip Mirowski and *Markets (In Search of Media)* (University of Minnesota/Meson Press, 2019), written with Armin Beverungen, Philip Mirowski, and Jens Schröter; he is co-editor of *The Contributions of Business to Economics* (*History of Political Economy*, Symposium Issue, Duke University Press, 2017), with Robert Van Horn. His previously completed research has addressed such topics as the history of the Chicago School of Economics, economics imperialism, and market design.

Sandra J. Peart is President of the International Adam Smith Society and a past president of the History of Economics Society, where she began the Young Scholars Program. She is the author or editor of nine books, including *Hayek on Mill: The Mill-Taylor* Correspondence and Related Writings and Escape from *Democracy: The Rule of Experts and the Public in Economic Policy*, with David Levy. Dr. Peart obtained her doctorate in economics from the University of Toronto. She began her career as an assistant professor of economics at the College of William and Mary and then joined the faculty at Baldwin-Wallace University. In 2007, she became the fourth dean of the Jepson School of Leadership Studies at the University of Richmond.

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Robert Van Horn is Associate Professor of Economics at University of Rhode Island. His research has primarily focused on the history of Chicago economics and the contributions of business to economics. He is co-editor (with Edward Nik-Khah) of *The Contributions of Business to Economics* (*History of Political Economy,* Symposium Issue, Duke University Press, 2017) and co-editor (with Philip Mirowski and Thomas Stapleford) of *Building Chicago Economics* (Cambridge University Press, 2011). *Cambridge Journal of Economics*, *Economy and Society*, *Journal of Economic Methodology*, and *Journal of the History of the Behavioral Sciences*, among others, have published his work. For information: http://www.uri.edu/faculty/vanhorn/index.htm.

Richard E. Wagner is Holbert L. Harris Professor of Economics at George Mason University, USA. He is Senior Fellow of the Academic Advisory Board at the Independent Institute, the James Madison Institute for Public Policy Studies and the Virginia Institute for Public Policy. He was co-editor of the *Scholarly Journal Constitutional Political Economy* from 1989 to 1997, and he is currently a member of the editorial boards of Constitutional Political Economy, the *Journal of Public Finance and Public Choice*, and the *Review of Austrian Economics*. He was one of Buchanan's dissertation students, a co-author with Buchanan, and a faculty colleague for some 25 years.

Jeffrey T. Young is Emeritus Professor of Economics at St. Lawrence University in Canton, New York, USA. His has published extensively on Adam Smith including *Economics As a Moral Science: The Political Economy of Adam Smith* (Elgar, 1997). He also edited *The Elgar Companion to Adam Smith*. He currently resides in Springvale, Maine, where he continues to work on the moral philosophical underpinnings of Smith's economics among other research projects.

The Protestant Father as Economist

Craig Freedman

> He [George Stigler] very much maintained an image of this strong, Protestant father. That was his heavy vibe. His colleagues would tell you this. You know, he could get his point across with two words, and the way he was turning his head. (Conversation with Sam Peltzman, October 1997)

Piercing the lingering mystery surrounding the Japanese has seemingly persisted as something of a self-defeating project. Confusion might begin to dissipate if more attention were to be paid to the distinction separating what can be classified as *tatemae* in nature from what remains allotted to the *honne* category in Japan. The cited semantic contrast persists to differentiate that which is superficially perceived, namely the outside face displayed publicly, from what is hidden within, the closely guarded secrets of the true self.[1] Such a comparable disparity has naggingly complicated the task of understanding George Stigler and continues to do so. Attempting to elucidate his long years of remarkable academic output quickly becomes transformed into something that more closely resembles an elusive challenge. The task stubbornly refuses

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to be reduced to the more mundane and simple chore of performing straightforward economic analysis. As such, the effort required remains dauntingly and persistently difficult, even to this day.[2]

Stigler of course was part of a generation (born 1911) in which men did not comfortably wear their feelings on their sleeves or open themselves up for public examination.[3] They opted to imitate instead strong, silent types. These masculine epitomes were best typified by the Hollywood stars dominating the era when Stigler was still a young man.[4] Specifically, ingrained aspects of his generation most closely mirror the Western celluloid heroes of that age. Cowboys who often seemed more comfortable around their horses than with the women folk living in those frontier outposts; men like John Wayne, Gary Cooper or even Jimmy Stewart.[5] Stigler himself, even after years of international recognition and accumulated acclaim somehow remained the gawky, young provincial from suburban Seattle. He was someone who had travelled widely, but still persisted in reflecting an archetype of insular America.[6] Perhaps, as those who knew him best understood, the start of an explanation lay in the fact that he was essentially a shy and sensitive man who covered up his vulnerability by deploying a sharp and sardonic wit.[7] The confusion may in fact have arisen from a set of protective, psychological defensive ramparts carefully constructed over the course of a number of decades.

> But his *public* persona was this very strong Protestant father figure. And you just don't cross that father figure. Behind it there was something else. You could see a side of him at times which was very shy. This was kind of a shell that he used to defend himself. You could see that aspect of him. But the public persona was unequivocal. And it was this public persona which was very much responsible in some sense for his image. He was a real leader … You just dreaded the warning look … that dirty look from George. That is the way he led. (Conversation with Sam Peltzman, October 1997)

Accordingly, to make any inroads into logically encompassing the man behind the screen, or effectively grasping the subtext of his research, an initial recognition of what is required becomes almost mandatory.

Consequently, the investigative starting point becomes an innate realization of the unalterable complexity of the man and thus of his work.[8] In some sense, to fully comprehend his economic insights, even if limited to what Stigler might classify as his strictly scientific output, it becomes necessary to try to understand the man himself.[9]

> I [Arnold Harberger] did not have a helluva lot of day-to-day type contact with him. His workshop used to meet over at the Law School for example, the other side of Midway. He was a professor in the Business School as well as in the Department and so on. But in any case I really feel that he is one of the most interesting and in some sense, most enigmatic characters in our profession in this last century. (Conversation with Arnold Harberger, October 1997)

George Stigler would gain a good degree of hard-earned renown in his life. However, he did fail to become the larger than life public figure that infused the avatar of someone like Milton Friedman. Stigler proved either incapable, or more likely unwilling, to carve for himself a parallel pose.[10] Still, George Stigler was a Nobel Prize winning economist who could command considerable fees as an expert witness or when appearing as a keynote speaker, no matter what the locality or occasion might be. Nonetheless, despite these hard-won accolades, George Stigler to some extent would remain the ultimate outsider, a persistently private individual, no matter to what heights his success might propel him. He would embody a perennial provincial presence, the single child of immigrant parents from the suburbs of Seattle (before Seattle had the slightest shred of cachet). George Stigler would manage to blossom almost naturally at the University of Chicago, gaining intellectual prowess in a department that had a history of defying conventions and of posing as proud mavericks.[11] Nevertheless, despite a carefully cultivated gruff exterior, he may have been hiding, or cautiously protecting, a shy and not entirely self-confident individual, as some of his closest friends and colleagues have posthumously noted.[12]

> That's interesting because in some ways I always felt that he was sort of unsure of himself, too, in his manner. I always thought that for a person

> of his stature, he showed less confidence than you would have expected. That was what I felt. Others probably responded quite differently.
>
> *Funny you mention that, because at least Claire Friedland always suspected that there was a bit of shyness, or a bit of sensitivity, or a need to sort of keep people at a distance. And that's maybe why he was always making jokes.*
>
> Yes, I think that was maybe right. The feeling I always had was that, I don't know, there was a sort of insecurity about the man. At least, I felt that. For which there was no justification at all. (Conversation with Ronald Coase, October 1997)

Inevitably, speculation when it comes to analysing personalities often proves to provide an entertaining, though often misleading, diversion. In the case of George Stigler's political leanings or even ideology, he claimed that they remained largely unformed until he arrived in Chicago.[13] Stigler at least, unlike a number of academics who later became arch-conservatives, never claimed any initially pronounced socialist leanings,[14] even during the bleak days of the depression.[15] However, Chicago proved to offer a defining moment for the young graduate student. His always razor-sharp mind quickly succumbed to the influence exerted by such Classical Liberal economists as Simons, Viner and of course Knight, who wove something of a magical web around the Chicago graduate students of that time.[16] Even more importantly, he discovered compatriots in the form of Milton Friedman and Allen Wallis. It would be Wallis, acting as the go-between, who would succeed in luring Stigler back to the University of Chicago in 1958, aided by the well-endowed Walgreen Chair.[17] The appointment significantly extended not only to the economics department but to the business school as well.[18] Bringing Stigler back to Chicago can, at least in retrospect, be viewed as a decisive and perhaps crucial moment in the construction and ultimate buttressing of the embryonic Chicago School of Economics.

Being so profoundly under the influence of Frank Knight meant that he initially imbibed very deeply from the Classical Liberal approach favoured by his academic role model. Knight, especially as he got older, persistently stressed the limitations of economics, at least in its pose as a scientific theory. He was critical and iconoclastic, but his scepticism was not simply one sided. Knight didn't reserve his scorn merely for his ideological opponents. "The big job of economics is to divest people of prejudices – to have them see the questions as they are" (Knight quoted in Patinkin 1973: 791). While he installed perfect competition as a central core of economic science, the reach of such science was to remain extremely limited. Thus Stigler's dissertation work on theories of production and distribution[19] was heavily influenced by Knight's thinking as was some of his early published work.[20]

> On the other hand, if you read his first book, *History of Production.* He has a lot of highly critical things to say about Marshall there. But then he says highly critical things throughout that book. I once spoke to him about that book and said something like that to him and he indicated he hadn't read it for a long time, didn't propose to read it in future. That was something he had done in the past. I think he questioned whether parts of it were right. (Conversation with Ronald Coase, October 1997)

Stigler would quite naturally experience an initial difficulty in breaking away from the heavy hand of Frank Knight, though even his earliest pieces display a stance indicating that he isn't simply a rigid follower of a given doctrine. However, in one of his earliest published pieces, Stigler (1937) exhibits the clear influence of his teacher, displaying not only an expected antagonism to constructs of imperfect competition, but posting arguments that seek to persuade in a fashion conducive to Knight's thinking.

> If indivisible resources are typical or important, then long-run decreasing average costs are typical or important, and competition is impossible. The presumption to be drawn from theoretical analysis, however, is that indivisibilities are exceptional and unimportant. Professor Knight has stated the case well: "No fallacy is more pernicious with reference to intelligent economic policy than the popular illusion that large-scale business is in general more economical than small-scale." A priori the case for indivisible factors is very weak, and urgently needed empirical studies will probably make it much weaker. (Stigler 1937: 714)

But in a discernible fashion, Stigler shifted away from the spirit of Knight and his complex methodological version of Classical Liberalism. Instead, he displayed flashes of apostasy. There were distinct points, even in his early work, that were defined by a momentary loosening of faith. He seemed to be questioning his adherence to the tenets of the traditional, limited reach of economic theory (at least according to Knight's chosen gospel). Stigler's wholehearted embrace of the economic rational man, for instance, might have proven more defendable and even strategic, had it been strictly utilized as a convenient heuristic, as he sometimes does in his early, more Knightian period. Though even given a particularly generous interpretation of instrumental deployment, a desirable level of conceptual precision appears to be absent from Stigler's formulations during this time.[21]

> It is elementary to all scientists that certain methodological assumptions, which everyone admits are contrary to fact, are indispensable to theoretical reasoning. No one begrudges the physicist the right to ignore friction, and the mathematician is permitted perfect circles no one will ever see. The case for the economic man is just as strong, and had he not been imported into economics, today there would be no science worthy of the name. (Stigler 1937: 713)

What we can observe here in embryo is the emergence of what would become the Friedman/Stigler version of positivism.[22] The detectable thrust of this new direction involves not only an implicit championing of empiricism. Knight of course had no truck with anything but economics as a deductive science.[23] But this same style of methodological positivism would increasingly be employed as a mechanism extending the reach of economics far beyond the very limited boundaries favoured by Knight.

> And, he had already revealed the kind of attitude, which I subsequently realized influenced me enormously. It was a methodological position, which, only when Milton Friedman published his famous (methodology of positive economics) article in 1954 did I then realise he was using the type of argument that sounded exactly like the kind of things Stigler

> would drop in his articles. It was a kind of (what shall I call it) a poor man's Popperism. I mean it is basically Karl Popper's falsification with a tremendous emphasis on prediction, etc. And I later realised, discovered this because I asked him, that he and Milton Friedman talked about all these things. (Conversation with Mark Blaug, April 1998)

The same push and pull (embracing the more familiar routines versus championing new horizons) is also reflected in Stigler's (1943) criticism of the New Welfare Economics. This particular theoretical structure had blossomed in the late 1930s, winning numerous adherents among the younger cohort of economists. On balance, the criticism displayed in that particularly pointed piece of dismissal could still have flowed, for the main part, directly from the pen of Frank Knight. The reader can easily imagine the sound of Knight's own squeaky voice emanating from the article.[24] Stigler, at this stage at least, appeared somewhat reluctant to simply abandon the comfort offered by mimicking Knight's preferred line of thought.

> The familiar admonition not to argue over differences in tastes leads not only to dull conversations but also to bad sociology. It is one thing to recognize that we cannot *prove*, by the usual tests of adequacy of proof, the superiority of honesty over deceit or the desirability of a more equal income distribution. [Yes, Stigler here is clearly establishing the desirability of having a more equal income distribution.] But it is quite another thing to conclude that therefore ends of good policy are beyond the realm of scientific discussion. (Stigler 1943: 357)[25]

Certainly, Stigler failed to be swept up by the interventionist tide of Roosevelt's "New Deal" or the swell created by the incoming wave of Keynesian policy. Immersion in the ideas minted by Knight, Viner and Simons would have been proof against any such temptations.[26] Nor did his Washington war-time service in the Office of Price Administration cause his faith in markets to waver.[27] Undoubtedly his subsequent Columbia University stint at the Statistical Research Group served only to buttress his underlying ideology rather than shift it in any noticeable way. Without delving into the realm of psychological profiling, clearly being reunited with his old Chicago running mates, Allen Wallis, who ran the unit, as well as Milton Friedman, would have served only to strengthen any former proclivities.

> Now, what you have to understand with somebody like Allen Wallis, and so to a degree those people who were in his circle, is that Allen Wallis had the sharpest priors - I'm using the language of Bayesian probability - of anybody I ever knew. Almost no new data could change his view for this reason. On the other hand, if he thought of somebody as a dangerous, or an incompetent thinker, but Jimmy Savage assured him that the man was very smart and had good judgement that carried more weight with Allen Wallis than a two-year study of the person's vitae and an audit of his writings. There's an in-group of the good guys and the much larger out-group. This showed itself in things that aren't even political. Just as an amusement I used to do a little Diogenes-like anthropological study of statistician friends of mine on what their attitude was on cigarette smoking. This was in the years when it had been nominated as an important cause of mortality, excess mortality. Let's say for my money that already the evidence was overwhelming. But this was denied. And when I went to Allen Wallis he said, 'There's nothing to it. Next thing you know they'll be saying coffee causes cancer.' Or something like that. And this was right on my prediction. Before I went to talk to him, I predicted what his response would be. And I remember saying to him, 'Now, what about Milton?' And he said 'Well, Milton agrees with me. However, he has quit smoking'. (Conversation with Paul Samuelson, October 1997)

Fortunately, Stigler found his own personal Damascene road when invited to join Knight, Director and Friedman on what would turn out to be a virtual pilgrimage to the heights of Mont Pelerin, located snugly in the Swiss Alps. The Mont Pelerin Society, including its aims, machinations and much more have been painstakingly related elsewhere.[28] But simply put, Hayek (1944) managed to employ the fame (or notoriety) of his pointedly dystopic work to fund this gathering. Assembled at that Swiss resort were those on the right who fancied themselves, rightly or wrongly, to be dedicated disciples upholding Classical Liberal Principles.[29] More pragmatically, they were gravitationally united, at least at this embryonic stage, only by what they definitively chose to oppose. Namely, they sensed a clear and present danger in having the prevailing intellectual debate hijacked by a distinctly collectivist/left-leaning mode of thought. Their concerted purpose, accordingly, lay in an implacable desire to shift the terms of debate onto more congenial and hence defendable grounds.

For Stigler, such a heady mix of Classical Liberal thought (imbibed originally at Chicago) strained through the perceived urgency of the then prevailing political situation, ended up exerting a reformative influence on Stigler's still developing economic and political thought. The event consequently helped to shape the contours of his subsequent career. Ironically or otherwise, the sort of Classical Liberalism rampant at that first meeting would be casually jettisoned by Stigler in the years to follow. Instead, he would help to create a more modern, reformulated brand of liberalism, one geared to battle opposing forces. Perhaps ultimately, the most significant consequence flowing from that initial meeting, at least for Stigler, was that it marked his first significant and sustained interchange with Aaron Director. Though an instructor at Chicago during Stigler's graduate years, the Mont Pelerin expedition would draw these two like-minded and sharp witted academics together. Subsequently, Director would come to exert a great deal of influence on the construction and focus of George Stigler's developing thought. Only Gary Becker would possibly be able to vie with Director in this respect.[30]

> Although I continue to the Day 721,371 to believe in and hopefully to practice a thorough going scepticism of mind, and commend this attitude to all others, I also have come to respect wisdom. If a rigorous mathematical analysis is reinforced by an exemplary econometric study in asserting some relationship R, and Aaron Director on reflection denies the relationship, I would consider it rash beyond forgiveness to venture 5¢ on the existence of relationship R. (Stigler 1976: 4)

Clearly this 1947 meeting sharpened his vision, not only politically but economically as well. "I confess that none of the later meetings I attended equalled for me the interest of the first session" (Stigler 1988: 145).[31] His work at this point noticeably shifted. Research had until that moment consisted largely of a variety of tentative probes, defining a set of potential directions. His published output paralleled the work of many other young academics in looking for a way to precisely define his budding career and his future objectives.[32] The Mont Pelerin meeting managed to crystallize his purpose as an academic. What perhaps became clear, and in essence urgent, was the responsibility of any self-identified Classical Liberal to battle against collectivist forces seeking to limit individual choice and freedom.[33] At stake was no simple theoretical dispute, but from his newly discovered Mont Pelerin perspective, a virtual struggle for liberty.[34]

This sense of urgency would serve as an underlying rationale for abandoning Classical Liberal methodology[35] in favour of adopting a more scientific approach to economics. The result was a noticeably narrowed path, one in harmony with the modernism that had come to dominate this post war period. In embryo, the five lectures which he would deliver to the assembled economists of the LSE in 1948 (Stigler 1949)[36] would encapsulate, to a somewhat remarkable extent, the path his future research would henceforth blaze.[37] The lectures succeeded in simultaneously pointing the way forward, as well as reflecting on roads previously travelled. Thus within these lectures are elements of Knight's Classical Liberalism mixed in, magpie style, with myriad indications of a distinct split from the now limiting perspective of his teacher. In a sense, a careful reader can see Stigler's growing belief in a scientific modernism based on empirical evidence emerging from the scepticism that Classical Liberalism provides. A reconstituted Liberalism would necessarily underwrite a more grandiose view of the theoretical reach of economics.[38]

> On the one side, a concrete problem almost peremptorily directs economic discussion. No intelligent person can fail to modify and adapt his general position to suit its peculiarities. There are obviously important pieces of evidence, and he must take account of them. They are usually sufficiently numerous and complex and obstinate to render unnecessary and undesirable the invention of further difficulties. There are indisputable developments, and his theory must give an account of them. The theorists' equal, and proper, striving for generality is disciplined by the facts. These,

> I repeat, are the effects of analysis of concrete problems. The mere currency and interest of a question, broadly posed, are much more likely to generate an especially low level of economic analysis. (Stigler 1949a: 35)

Post-war economics was notable, as previously indicated, for its rejection of the methodology of Classical Liberalism. Stigler, in his ideological battle with those advocating a more active economic role for government, would combat the modernism represented by the Arrow-Samuelson approach with an alternative that bound itself to a rather fuzzy attachment to logical positivism.[39] This is clearly formulated in Stigler's (1949c) scathing attack on Chamberlin's monopolistic competition in which he definitively rules out of court arguments based on the validity of their assumptions. "Does a theory incorporating this viewpoint contain more accurate or more comprehensive implications than the neoclassical theory?" (Stigler 1949c: 33).[40] Instead, he favours a testing of the implied conclusions derived from each and every theory. In this aspect, Stigler (and his Chicago colleagues) would attempt to recapture the scientific mantle from those opponents stubbornly enthroned at MIT, Harvard, or Berkeley. This alternative focus would evolve and become Stigler's call for a quantitative crusade, as reflected in his 1964 American Economic Association Presidential address. Reading it today, delivers a still pungent whiff of promises entangling "a brave new world". Economics would finally achieve validation, buoyed by a deliberately scientific foundation.[41] Many of those listening to this vision for the first time would have undoubtedly been either energized or perhaps downright startled at the evangelistic tone coating Stigler's words.

> It was just that he was so enthusiastic about quantitative measures. He thought that he was going to change the world.
>
> *You think he ever modified his beliefs on that?*
>
> I don't think so. I was sitting with Aaron Director at the time when he gave his Presidential address and we did look at one another at the time to try to see what each one thought about all of this. (Conversation with Ronald Coase, October 1997)

Unravelling the Riddle: Stigler as the Perennial Hedgehog Disguised as a Fox[42]

> He was a guy with a lot of ideas and yet impatient. George had an impatient personality. Even in playing tennis, if he didn't get the point in two shots you could tell he really tried for a hard shot, he didn't want to just sit and hit it back and forth. He was a good tennis player, particularly when he was younger, but he had a real impatience. And that was true intellectually too. He wouldn't sit with one problem for five years as some people might do. And so in his papers, he has an idea and he tests it. So there are a lot of good ideas in his work, but he doesn't sit and stay with a problem for a long time. I think that's true. In that I think that he's substantially different from Milton, you know his work on money or his work on consumption functions were longer ventures, he took longer periods of time. He had more patience. George was not the most patient researcher. (Conversation with Gary Becker, October 1997)

As previously indicated, when trying to understand George Stigler's distinguished career, one point of difficulty lies in attempting to differentiate the *honne* aspects from the more apparent *tatemae* particulars composing what can only be described as a puzzling and complex personality. Often the seeming, what most easily appears, would serve to baffle understanding. Investigators are more likely to be misled by perceptible persiflage than to grasp the rarely glimpsed undercurrents mirroring the fundamental aspects of Stigler's endeavours. Unfortunately, investigations tend to limit themselves to the more obvious and thus more superficial values. Such efforts yield ambiguous conclusions. Consequently, a quick hop through Stigler's voluminous output might seem to portray someone who almost compulsively skipped from one key area of research to the next, making his mark quickly, but then moving on to the next glittering challenge. This defining characteristic would then seem to locate him permanently among the tribe (or perhaps among the skulk) of foxes, those who are said to desire to know and subsequently hoard many little things. Yet his career consistently baulks at arriving at such a destination. Instead, he persistently soldered an unyielding, if largely unstated, ideological imperative to a much more discernible theoretical objective. Economics in this configured vision was fated to become a unified science, one based entirely on the certainty provided by the price mechanism. The methodology constructed to underwrite this proposition would apply to all human decision making. Individual rational choice would deliberately emerge as the focal point on which to erect a more universal scaffolding for economic analysis.

Consequently, a system of individual choice would provide an irrefutable scientific approach effectively defending liberty. Deft modelling would allow free markets to become equated with free choice. This persistent, if unremarked, goal imperceptibly transformed into a desire to expand and promote the core theory of perfect competition. In this way, an abstract and generalized model could be allowed to evolve into a definitive framework reflecting actual market mechanisms. The result would yield a carefully devised formulation conveniently applicable to a much wider range of experience and observations. At this classic fork in the post war road, Stigler would deliberately choose to part company with a methodology recognizably employed by Simons, Knight and Viner. Undeniably, individual liberty[43] always formed an essential element defining Classical Liberalism. Ever since the time of John Stuart Mill, if not before, concerns over liberty had acted as a necessary and essential focal point for most nineteenth century economists. However, the original basis upon which earlier economists supported such a focus was neither derived from economic theory nor neatly grafted on from any theoretical analysis of the market mechanism.[44] In contrast, though Stigler's focus paralleled similar concerns, his near obsession demanded a more rigorous foundation underwriting what he assumed was a self-evident truth.

> A second trait was a strong concern for the preservation and enlargement of individual liberty. An interesting thing about this belief is that it was not vividly and continuously reinforced by the skilful demonstration of the efficiency of competitive markets in performing a thousand tasks. There were elements of such an argument in Knight's famous essay on social cost, but the support was philosophical rather than functional. (Stigler 1976: 5)

To evolve into something analogous to a unified field theory of choice, this perspective on decision making would need to be clearly applicable across all fields of social science. For George Stigler, such an imperative reduced to a necessity for modelling all individual choices as being performed only by rational agents. Such a theoretical construction would imply a level of formulation that was almost defiantly abstract. Only then could the required extent of generalization and applicability become reasonably achievable. Consequently, the only viable and sustainable nugget of logic, given such constraints, must inevitably lead to the conclusion that, in an almost Gertrude Stein bit of mental phrasing, "a market is a market is a market". The common mechanism characterising markets ineluctably became the prime focus of any investigation. Whether the specific subject examined happened to encompass stock markets, labour markets or political markets made no matter whatsoever.[45] The same contractual relationships and roles played by prices, whether explicit or implicit, must dominate any contrary distinctions or individual specifics. These could only be acknowledged as resembling, at best, a minor variation on a much more dominant and consistent theme. Under this dispensation, economists were excused from being cognizant of any of the specifics involved in their areas of designated study. Consequently, the airline industry, couched in purely economic terms, could continue to remain merely "marginal cost with wings" in the formulation of a rather dismissive quote attributed to the economist Alfred Kahn.[46]

> *You once said that abstraction and generality are virtually synonyms. Is this your point here: that an abstract theory is a general theory, that it has the potential to tell us more about the world?*
> Yes, you can subject it to a wider range of challenging applications. For example, consider the standard assumption of competition that the rate of return tends to equality in all areas in which returns are allowed to flow. We can use that in a million applications. (Stigler 1988b)

A key to establishing a one size fits all bit of stylized theory, cleverly tying markets to individual liberty, was the almost casual reification of the rational decision maker. This slice of instrumental theory provided convenience and ideological consistency in developing an all-encompassing economic framework. Given the focus on individual choice, such a central agent allowed for the ultimate extension of economic methodology to all of social sciences.[47] One adroit simplification allowed for homogeneous or representative agents to sidestep the complications otherwise offered by the essential complexity of human nature. Stigler, however, cultivating an almost moral imperative to propel his arguments, needed these agents (of his constructed economic adventures) to be endowed with more than some instrumental idea of utility. Such a dispensation could be granted merely by fabricating a convenient assumption. (Namely, by examining whether the theories and conclusions drawn from such an assumption proved fruitful, meaning capable of providing productive insights.) For Stigler, just as perfect competition became a mirror of the workaday marketplace, so did the rational decision maker. Exercising this strategic prerogative, one-dimensional individuals, consistently spurred by narrow self-interest, transformed easily, in Pinocchio-like fashion, into real human beings. These neatly conjured rational agents supported a delineation necessary to infuse a concrete reality into the topology of perfectly competitive markets. In turn, such markets lent credence to the reality of decision makers relentlessly in sync with the embedded incentives provided by the price mechanism. In this fashion, economic agents trapped within Stigler's universe were effectively shorn of any trace of either ideology or psychology.[48]

> But as I said, he really believed in the rational mind. You'd show him some example of an irrational behaviour … there's a lot of this sort of work going on now it just so happens … and he would show you that it can't be true.[49]
> *Almost by definition …*
> Almost. Almost. It's getting more and more, more and more part of him as he got older actually, this whole view. He insists it's rational. He would tell you, 'There is some rational explanation for it. It's just that you haven't looked completely into it and found it'. (Conversation with Sam Peltzman, October 1997)

The ultimate connection between markets and liberty was as elegant as it was simple. The basis depended on a Hobbesian style solution[50] that cleverly resolved the dilemma facing any given society. Private economic power by definition inhibited individual liberty and choice. To eliminate the difficulties and complexities generated by such elements of power, individuals surrendered all such economic dominance, transferring it to the confines determined by the marketplace. In essence, this allowed individuals to respond to market incentives, relinquishing any potential ability to dictate market results. By surrendering all pretence to economic power, individuals became free to choose and thus personal liberty was insured. Perfect competition by defining a state of market dominance became the link between price adjusted markets and individual freedom. Consequently, this configuration developed into an established condition for George Stigler, not only as a theoretical framework but as a working reality as well.

> Yeah. Well, because I had the same view as George, on that issue. That I don't think you can talk with restaurant managers, in fact, about such things. You know they are not trained, they know in a certain deep sense, but they are not trained to articulate why things are happening. But any restaurant owner does in fact recognise it's good if you can get customers to come in and you can lose your audience pretty easily. You know there's unstable demand. Even when you're in the door, you can go out. That, they're all aware of, and so, in that sense they would say, 'sure this is going on.' In terms of pricing and so on, that would be a hard thing for them to articulate. I think that would be a hard thing to get by asking them. So I would have the same view, that yes, I use surveys in labour economics a lot. Surveys may give you suggestions about behaviour but you can't really take that as the same type of evidence. That's my view in that little note. It actually was written, published, just about the time George was close to his death. I think I got it published in 1991 in the *JPE* in the later issues of that year. And he died December 1st 1991. So it was around that time. I'm trying to think if I knew what his reaction to that note was, but I can't really remember. (Conversation with Gary Becker, November 1997)[51]

Given his clear and resolute objectives, Stigler's career followed a logical path. The LSE lectures indicated his first major strategic move, a sort of prefiguration of the course to which his future research would adhere. If perfect competition was to establish itself as the essential and all-encompassing theory of economics, pretenders to this throne would need to be conclusively removed from contention. Consequently, Stigler took careful and steadied aim at any alternative that could be categorized as a theoretical aberration to standard price theory. Essentially, he targeted opposing frameworks that at least potentially could undermine, or even chip away at, the flexible nature and applicability of the perfect competition paradigm. He defended ruthlessly at times his preferred formulation, which served to elevate a model of individual choice delivered solely by rational decision makers.

> Oh, George was a puzzle solver. George was definitely that. As far as George was concerned, I would think that the system building had already been completed by Adam Smith and there was not a hell of a lot of room for him or for anybody else to do that. He was interested, I would say primarily, in a particular sort of puzzle and it's a typical Chicago puzzle. And I don't mean that in any bad way, it's the sort of puzzle that the Chicago School's presuppositions require. Show me an apparent anomaly, something that does not seem to be explicable using the Smithian apparatus and the Marshallian apparatus and I will show you that it can be explained that way.[52] That was exactly the sort of thing that George went looking for. And that's not a bad thing. I'd have to say that it can actually be very good. (Conversation with Robert Solow, October 1997)

The goal then was not simply to cut away at the undergrowth of opposing theories for the sole purpose of clarifying the wished for, scientific nature of economics. The definitive intention was to obliterate the heretofore serious issues raised by theories of imperfect competition, as well as by those emphasizing the prevalence of market failures. In doing so, Stigler firmly believed he was rooting out bad practices and unfounded formulations leading the profession astray. But his focus was not merely negative in practice. The core objective driving Stigler's research was the necessity of substituting an alternative explanation that would not only compete with, but would effectively triumph over all other contenders by being empirically founded. In this sense, Stigler could be understood to limit his carefully fashioned "demolition derbies" to those opposing theoretical positions that had gained a measure of popularity. His critical pieces then did not merely represent some sort of indiscriminate scorched earth device applied willy-nilly.[53] Moreover, he was intent not simply to dismiss the issues these opposing models sought to resolve, but laboured to come up with solutions amenable to the fundamental workings of the price mechanism. For instance, in his pioneering work on Oligopoly Theory combined with the insights contributed by his Economics of Information, Stigler (1961, 1964) presented a solidly constructed alternative to more non-orthodox pricing regimes that he viewed as wrong, unproductive and possibly destructive to economics as a science. For Stigler, such malodorous theories included the likes of Chamberlin's monopolistic competition, which he had sought to eviscerate in his LSE lectures.

> Stigler's article [1964] was a landmark in the theory of industrial organization and in the practice of antitrust. For industrial organization economists it focused attention on the sorry state of oligopoly theory and, using information theory, proposed a theory that could explain the deviations of oligopoly pricing from competitive pricing. For antitrust practitioners the article came to have an important impact on the application of antitrust law, especially in the merger area. Indeed, it is not an overstatement to say that Stigler's theory of oligopoly remains a central pillar in merger policy in most, if not all, antitrust regimes around the world. (Carlton et al. 2010)

Clearly, his theoretical work implied a consistent and almost inflexible approach to economic policy. Although this link may only be implied in his scientific articles and constructed models, the underlying implications are not mere academic matters. The infamous Coase Theorem, as constructed, seemingly resolved the Pigovian loophole of externalities, by forcefully yanking the price mechanism out from that heretofore shadowy region. Price theory was allowed to bask once more in the sunshine of unalterable truth and overt reality. Though clearly transaction costs are never actually zero, if fairly negligible, all markets would continue to operate in nearly textbook fashion.

> Well that's a very strong externality. Because if you don't get vaccinated you endanger the others. I think that this is one of the reasons that

> Coase's work was so important to George. Coase's work indicated that an awful lot of things which *were* externalities or had the *potential* to be externalities might be handled in a non-governmental way. Might. That's it. And I know there is a problem with Coase. I've read a lot about the literature based on Coase for Stigler's "Law or Economics" article because that deals specifically with Coase. And it was very interesting. Many people interpreted Coase as saying it only works with *zero* transaction costs, and that wasn't how *I* interpreted Coase. Not that I'm anybody, but I thought that Coase was saying that to the *extent* that transaction costs are important, markets will be *less* effective, but not that you had to have zero. A lot depends upon how you interpret that one item. If you have to have 'zero', we don't have any 'zeros'. Now the Coase approach worked to some degree. You see that once again we have some empirical issues to test and measure. How much are the transaction costs that we do have? What mechanisms are at work to get rid of them? The way George saw the economy was as one in which the market *constantly* was adapting to all the non-market deficiencies that so much of the profession were concerned with. He knew that they were out there, that externalities were out there. However, he said, "Look, the market's rushing in every moment to take care of them. Here's Coase opening this big door for the market to rush in." And *that* was what George was focused on, starting perhaps in 1960 or '61, whenever it was that Coase gave his famous talk. George described in his memoirs that wonderful talk in which he said that everybody in Chicago who was there was wrong and Coase was actually right. George was focused on the way the market marches into *eliminate* the externalities, to work *around* them to make them a market problem instead of a non-market problem. I think I've quoted him in my memoir as saying something like, 'externalities are what the market has not *yet* eliminated.' That's not an exact quote but in my memoirs I do have the exact quote. You see he saw the market as *the* force. He was looking at the other side of the market, at how the market may provide an appropriate solution. He said he saw this arbitrage going on all around him. Whenever there was a situation that somebody could take advantage of to make money he would. That was what solved a lot of these problems. (Conversation with Claire Friedland, October 21, 1997)

However, unlike his close friend Milton Friedman, Stigler felt that the logic of his own research demanded that he avoid at all costs explicit policy recommendations.[54] Given his own view of market potency, by its very nature, policy promotion must logically lack any chance of efficacy. Even when transplanted to the political marketplace, policy symptomatically could only reflect self-interested, individual decisions. Economics could afford to conjure up a post-rationalization, or provide symptomatic cover, for every such decision, but the discipline would perennially fail to promulgate any actual result.[55]

> Well, he very much believed that the role of economists in formulating or moving policy was overstated. More than I do. It's not something I agree with him. He would always take this very strong position. We were part of what Marx would call the superstructure. Bought by one side or another and we really didn't have an independent role to play in the evaluation of policy. (Conversation with Sam Peltzman, October 1997)

Given his intellectual agenda, the market for regulation, policy, and other government action operated within the same supply and demand parameters as did the commercial marketplace. Outcomes represented those who wished to expend resources on given initiatives in order to manufacture and capture government created rents. Any such intervention would by definition inevitably change income distribution and ultimately residual wealth. Consequently, strong incentives were unalterably in place to influence legislation and its implementation. In Stigler's (1971) world, action for the sake of achieving some vaguely defined public good, or driven by moral intentions, was inconsequential.[56] Following out the sense of this logic, the thrust of American deregulation that gained increased popularity in the late 1970s, and built up steam in the 1980s, was not a triumph of some rational encroachment enshrining Chicago-style economic wisdom. Instead, the movement represented the changing face of the marketplace. For example, rents accruing to a regulated airline industry would predictably shrink over the decades, until the search for greater profits led the self-same companies to eschew government oversight and open up competition within domestic routes.

> Again, the deregulation of the airlines became possible when major carriers (notably United Airlines) became convinced that they would do better in a more competitive industry. It can be argued that the relaxation of America antitrust laws under William Baxter is an abandonment of depression-spawned small-business doctrines in favour of a new era of merchandising institutions and practices. For every deregulation of recent years it is easy to find a new and stronger regulation. The wave of protectionism that most western nations have been experiencing provides many examples, such as the United States bargains with Asiatic countries on export quotas on automobiles (perhaps soon to be replaced by a "domestic contents" law), textiles, sugar, and so on, and with England over steel. The Sierra Club proved to be stronger than James Watt. The Congress apparently will not tolerate the idea of having individuals pay the full cost of their telephones, or a market price for natural gas. (Stigler 1983: 2)

What at first glance might seem no more than a mix of fatalism endowed with a good dose of cynicism, was instead Stigler being true to his own derived logic and methodology. Unlike his colleague and good friend Milton Friedman, who remained ready to proselytize at the drop of a hat, George Stigler claimed that not only was such partisanship futile, but simply not consistent with the role allotted to an honest academic. In this highly refined version of research as an almost religious calling, designated saints, such as the blessed Frank Knight, were able to resist the craven call of tainted money.[57] Economic scientists and true academics steered clear of such money encroachments. Somehow by recognizing the irresistible pull of self-interest, practising economists could potentially inoculate themselves against such fatal temptations. Though perhaps it might be more the case that Stigler recognized his lack of the sort of public charisma that helped to create a bankable persona.

> He played tennis every day, or every other day with Milton Friedman. I remember an amusing conversation I had with him at one time. Milton Friedman was making his *Free to Choose* television program. George Stigler had advised him not to do it.'Don't do it. It's only cheap publicity and really a television program can't express ideas properly.'

> And he said it again, when it was very successful. I had the feeling that George Stigler was slightly jealous. (Conversation with Mark Blaug, April 1998)

However, Stigler failed to conclusively resolve his posed policy conundrum. Death, as always, managed to reserve the final word. In fact, toward the end of his career, he succeeded in ultimately painting himself into a proverbial corner. He quite honestly attempted to take his method and approach to its logical and consistent conclusion. If any market (implicit or explicit) could be analysed in the same fashion no matter what its characteristics, then the political markets, to which he turned his final attention, must operate as did any other consumer-driven operation.[58] The inherent logical difficulty conjured in this case meant that allotting the final say to consumer sovereignty in the realm of politics inevitably meant accepting policies that Stigler would admit were economically unjustified. But given his framework, if such political initiatives had remained in place for decades, then logically such efforts must embody that variant of income redistribution most reflecting citizens' desires.[59]

Thus in a posthumous article (1992), Stigler (speaking from beyond the grave) considers the sugar subsidies long endured by the United States, which would seem to favour only a small group of cane growers in Florida. However, maintaining the logic that aligns with his consistent market approach, if the American public had disapproved of this blatant form of income redistribution, they could have certainly signalled their displeasure over the relevant decades through their voting patterns. Given Stigler's (1958) belief in his own version of a "survival principle" (his test of time), he is almost forced to conclude that such subsidies are warranted since they have lasted undisturbed for more than half a century. They must unarguably reflect the preferences of the country's citizens against whose tastes no court of appeals exists.[60] That conclusion leaves Stigler with nowhere to really turn. Within his operative structure, there was no denying preferences since consumers are assumed to be the best fount of knowledge pertaining to their own well-being. To intrude upon these wishes would be to violate the carefully constructed nexus between individual choice and perfect liberty, which best flourished within competitive markets.[61] Paternalism, given Stigler's perspective, failed to have any claim to legitimacy. This dominating need to have a universal theoretical apparatus defined by consistent logic thus caused him to break with other erstwhile Chicago colleagues and other like-minded economists.[62]

> But what exactly would he allow as a proper realm for government? Then he got into this – I remembered his name for it after talking to you – 'paradox of legitimacy' he called it, or sometimes he called it the '*problem* of legitimacy'. At the time of his death, this was one of the problems he was working on. It was *very* much of a concern to him. You have to remember the kind of public persona he had. If something bothered him a lot *I* saw the side of him that said, "I don't know what to do about this problem!" But the rest of the public saw that other side, "Here's what I've *done* about this problem and isn't it convincing." But he was very much concerned about how you could call something inefficient in the political arena. We do have a democracy, more or less. If we have a representative government, then how can something that has been allowed to happen be inefficient? After all, that government has allowed some programs to go on and on, year after year. The sugar program was sixty years old, the anti-trust law was over a hundred years old by the time George died. If it reflects the public demand for it, how can we call it inefficient? I think *that* is part of the answer to your question 'what do we let the government do?' It's not an answer. It is something that he was worried about. Something he was thinking about. How to reconcile consumer sovereignty, or voter sovereignty, with his previous notions of inefficient government? Can we say this is illegitimate if the public wants it? Is that consistent with our extreme position on consumer sovereignty which is that no matter what horrible things the public wants, as free market economists we can never question it. That's certainly one of the basic principles of neo-classical economics. Consumer economics sovereignty is both the end of the story and the beginning. And we don't argue with the consumer, no matter how self-destructive these demands are or how inappropriate. Anyway, if you want consumers to be free to choose in the market place, how can we argue with them in the political arena where, in a sense, they are acting as consumers too? (Conversation with Claire Friedland, October 1997)

Here a certain honesty shines forth. He was too much of an academic and economist to quickly glide over a core contradiction at the heart of his approach. Perhaps the dilemma was unresolvable even for Stigler's nimble mind. In a sense, it brought him back to the very start of his journey in a rather Hegelian (not Marxian) fashion, playing out his own dialectic journey. From his 1948 LSE lectures onward he clearly saw individual liberty as an undeniable human objective. Such an irrefutable axiom defined for Stigler the meaning of old fashioned liberalism. But he clearly disliked, perhaps even abhorred, intentional income redistribution by government agencies. He considered such measures as inevitably distorting incentives that would otherwise serve to promote accountability and personal responsibility. Consequently, the logical brick wall he resolutely faced in his last years was in squaring the two within the realm of political markets. In some sense, he ended his life tangled in a familiar state of complexity.

> Well, he very much believed that the role of economists in formulating or moving policy was overstated. More than I do. It's not something I agree with him. He would always take this very strong position. We were part of what Marx would call the superstructure. Bought by one side or another and we really didn't have an independent role to play in the evaluation of policy. Yet he had this belief that the world should be a certain way. It's clear. You know, he was a believer in markets. He didn't like the sugar subsidy for sure. I don't know how you really square it ultimately with his position that this is the optimal way to redistribute. I guess he didn't like redistribution. He feels this down in his gut. That's all he can tell you. He couldn't say, I don't like the sugar subsidy because although I really like the redistribution that occurs, this is an inefficient way of doing that redistribution. He was beyond that. That is what we teach in Econ I. He is beyond, clearly beyond that. He wasn't going to take that position. So the only thing that's left is, on some other principle I don't like the redistribution, personally. Right? But there was a conflict there. There was a clear conflict there that a lot of us economically doing regulation kind of feel. (Conversation with Sam Peltzman, October 1997)

Barbarians at the Gate: Protecting the Neoclassical Damsel in Distress

> Evidence of Stigler's attachment to neoclassical price theory is also given by that part of his work mainly critical of the work of others. Price rigidity, administered price inflation, the theory of monopolistic competition, and X-efficiency were prominent targets, and each of them denied the efficacy of the neoclassical analytical framework. (Demsetz 1993: 800)

To opponents, especially those facing him for the first time but knowing him only by reputation, it would not be too far off the mark to regard him as something of an old-style gun slinger, Jack Palance attired completely in black and giving the evil eye to each and every snivelling sodbuster.[63] These farmers would know, unless they were unaccountably foolhardy, that they did not stand the slightest chance of standing toe to toe with Jack Wilson's [Palance] blazing guns. Likewise, George Stigler appeared programmed to mercilessly eliminate all opposition by whatever available means proved most effective.[64] Eradication, rather than engagement, was both his strategy and objective. Unfortunately, the process, though unintentional, could appear ruthless. Stigler seemed to lack the filter most others cultivate over time. If he thought of a witty remark, and he seemed incapable of not doing so, he just let it rip, no matter who was on the receiving end of his guided missile. Though perhaps not intended to be personal, only those encased in the toughest of hardened shells could fail to be affronted.[65]

> As for George's caustic wit, he never let go one of his barbs for the sake of mere one-upmanship. They were always aimed at the target's ideas, not the target himself: even when a workshop speaker asked whether he should deliver his paper standing or seated and George responded, "With a paper like this, under the table would not be inappropriate". (Friedland 1993: 781)

The difficulty when carefully analysing Stigler's scathing attacks is to comprehend how he could seemingly misread articles, even fairly non-complex ones, to such a degree of syncopation. Looking at his famous scorching of Sweezy's kinked demand curve (1947b), a reasonable studied response is to wonder exactly what article he could possibly have read as a foundation for his subsequent criticism. He seemingly ignored, or remained oblivious to, some of Sweezy's explicit assumptions and caveats defining the article. What Stigler in fact delivers here, and in a number of other cases, is a classic straw man argument.[66] The one overwhelming characteristic of such constructed straw men is that they are intentionally highly flammable.[67] Not surprisingly then, Stigler isn't reluctant in dousing his flimsy artefacts with accelerant before striking a decisive match. The aim is always intently focused on opposing theories, those disputing the efficiency of the Chicago version of price theory. Consequently, no effort would be spent, no valuable resources allowed to be frittered away, on anything but those theories that seemed to be gaining some significant traction, either within the profession itself or among the general public.

> I gradually realised, I don't know when I realised, that he was one of the most fascinating examples, which is why I'm interested in your essays, of how economists act even though they say that economics ought to be value-free (and it is sometimes value free). I find it extremely difficult to resist reading a lot of economic theory, politically. Now, you can see the way George Stigler had a wonderful nose for attacks on neo-classical economics, let's call it, which were dangerously capable of undermining the very foundations of orthodox economics. It's no accident that he went for Gardiner Means' *Administrative Prices* Harvey Leibenstein's *X-Efficiency*, Galbraith's *Affluent Society* and his other things, the *Kinked Oligopoly Curve* of Sweezy. These are all ideas that are very, very dangerous. They are subversive even of orthodoxy. (Conversation with Mark Blaug, April 1998)[68]

What makes Stigler's behaviour complex, is that there is no evidence whatsoever of any intention to deceive in any of his critical forays. What appears in print is his honest reading of the relevant literature. But his clear and overriding purpose is to seek out any perceived weakness in a theory, essentially those which he sees as posing a serious danger to standard price theory.[69] Consequently, the most reasonable explanation of the disparity between his analysis and what appeared in a given work was his compelling need to find within a chosen article exactly what he needed to find. In other words, he approached each article with a given set of expectations. These prior conceptions parameterize what he subsequently read, and his understanding of the text in question. In employing this particular methodology, Stigler is hardly exceptional. There exists research to confirm what perhaps a number of people may strongly suspect given only an anecdotal foundation. Namely, that many people simply cannot read in the sense of trying to discover in a somewhat unbiased fashion the meaning and intention of an author.[70] Reading with the intent to destroy is hardly conducive to gaining any broad comprehensive understanding of a given article or book. "The illusion of knowing is the belief that comprehension has been attained when, in fact, comprehension has failed" (Glenberg et al. 1982: 597).

> The interesting thing is he was a great enthusiast for quantitative methods. So, it doesn't seem altogether consistent. But he certainly was. On the other hand, he knew what the answer was going to be. He just regarded it, as I say, as a way of persuading other people. (Conversation with Ronald Coase, October 1997)

George Stigler was far removed from a simple soul tumbling out from the pages of a Dostoyevsky novel. Whether in quiet moments he more closely resembled the tortured characters peopling those novels seems impossible to discern. Certainly, it has repeatedly proven difficult, if not impossible, to uncover the *honne* of such an individual. However, by drilling down through the multi layers of *tatemae*, it may be possible to excavate and bring to the surface at least a few insights. These gleanings can prove useful, forming a scaffolding that provides a context for his actual writings. Whatever the ultimate academic impact of growing up as a single child might be (harboured to a degree from the gales of the great depression), is likely to remain ambiguous at best. But clearly his formative graduate experience at Chicago cannot be dismissed. Faculty such as Knight, Viner and Simons all had their impact, as did such fellow students as Milton Friedman and Allen Wallis. The first Mont Pelerin meeting seemed to have cemented an influential and life-long bond with Aaron Director. Along with Gary Becker, Director would have a decided impact in contouring Stigler's future views.

No doubt his fierceness and unwavering determination, as well as his research focus, was shaped by such experiences. Certainly, his no holds barred approach to opponents, aided by a focus on winning at all costs, was a lasting inheritance nurtured at Chicago. In some ways, more than even Milton Friedman, who became the public face of the Chicago School, George Stigler grew to embody the sense of scrappiness that defined a tradition having its roots in the inception of the department.

> *Milton Friedman*: I think you are getting something that is (a) the atmosphere at Chicago, and (b) intensified by Knight. That an academic is concerned not with being diplomatic, not with trying to avoid hurting people's feelings, but an academic is concerned with saying what's right. Telling the truth, or trying to get at it. And if you disagree with somebody you don't say 'well, now there may be something in what you say'
>
> *Rose Friedman*: You may be right
>
> *Milton Friedman*: You say that's a bunch of nonsense.
>
> *Aaron Director*: Exactly. That's not surprising. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

Notes

1. The simplest approach insists on imposing familiar standards when trying to understand unfamiliar territory. It's not unlike an American refusing to realize that he or she needs to convert to the metric system outside the cosy confines of the United States. Thus Japan's history and past necessities have established institutions and incentives that have a logic of their own, one not necessarily in sync with that of a number of Western countries. Thus understanding individuals, even economists, is often less than straight forward. One can be easily misled if seduced by the obvious path.
2. My first investigations of the work of George Stigler commenced in 1993. Over the years, his economic analysis, intentions, and mode of thought has, if anything, become even more difficult to unravel.

3. Some people are said to wear their hearts on their sleeves. George Stigler clearly was never "some people". More accurately, he dominated his academic realm by adopting a Gary Cooper-like attitude within his profession. He steadfastly remained someone who refused to willingly waste words or open himself up to public view.

   > He was a very strange person to sum up because his methods were not those of anyone else. I think he was quite unique. If you put a point to him, he liked to answer it with a joke. Then if you pressed him, he produced some fact or other. You pressed again, he'd give you his answer. But he was sort of an economist even in argument. He used the easiest way. (Conversation with Ronald Coase, October 1997)

   His actions were meant to stand alone as the measure of his worth. Yet attempting to glimpse the inner Stigler, through his work or even private correspondence, persistently reveals an enigma. He remains a riddle as unfathomable as Churchill's description of the old Soviet Union ("I cannot forecast to you the action of Russia. It is a riddle, wrapped in a mystery, inside an enigma").
4. Understanding that particular state of mind might be difficult for those born of fathers from a later generation. Very few of Stigler's colleagues, and even fewer of his cohort, are left. That generation has an alien tinge to it, making the leap toward comprehension difficult and perhaps somewhat impossible.

   > In his generation, that's what you did. You didn't beat your breast and go on TV. That was not the manly way of doing business. My father was like that. I think a lot of old timers where like that. [laughs] Different cultures. So I think that just would be the *custom* of his generation. I kind of like it better in some ways. (Conversation with Sherwin Rosen, October 1997)

5. It is often entertaining, though perhaps not enlightening, to think of individuals in terms of familiar (or even unfamiliar) literary figures. In some sense, Stigler fits the mould of mysterious Jay Gatsby from Fitzgerald's *The Great Gatsby*, though Stigler achieved success as an academic rather than as a prohibition gangster during the jazz age (nor would anyone have dared refer to him as "Stigler, old sport"). Both in their own individual ways worshipped the idea behind the American dream. In this conception, nothing was impossible or left to mere chance. Individuals could be their own self-creation, raising themselves up by their own bootstraps to become a "somebody". They need not be consigned to live a life as some poor, gauche boy from the provinces. Economics, and particularly markets ruled by incontrovertible price theory, became the equivalent of Gatsby's Daisy Buchanan. Stigler

shared a similar sense of romanticism, which can ultimately undercut even the best work done with the most worthwhile of purposes. "Much of his work centered around saving the damsel in distress, neoclassicism, from her attackers: hence his work on the economics of information and his enthusiasm for the Coase theorem" (Friedland 1993: 780).

6. The examples he resorted to in his teaching and research reflected a world largely enclosed by US borders. That markets would operate differently in other countries, or that students wouldn't be familiar with American history or terminology, seemed alien to his conception.

> Another story about George, I've always found it to be a problem, which is how incredibly American he was. I used to be shepherding these Latinos through and here they would come to some question in his Price Theory examination. 'Explain something, something about the Dred Scott Decision'. (Conversation with Arnold Harberger, October 1997)

7. Friends and colleagues of George Stigler, people with diverse personalities and views, ranging from Claire Friedland to Ronald Coase, thought of him as fundamentally shy and even perhaps sensitive.

> This behaviour reflected a feature of George's complex personality that he did his best to conceal: his personal sensitivity. His smart cracks were in part a way of covering that sensitivity as was his half-embarrassed laugh. Others frequently were offended by his cracks especially during early years, but George never made them with the intention or expectation of offending. He was in fact as sensitive to others as to himself. (Friedman 1993: 770)

Certainly three of his closest friends and associates saw him in exactly that fashion, despite his endless stream of wise cracks.

> *I know that Claire Friedland wrote that she suspected that it hid a shyness, his always responding with a wisecrack.*
> *Rose Friedman*: That's right. She's absolutely right there.
> *Milton Friedman*: Absolutely.
> *Rose Friedman*: He was sensitive and shy.
> *Milton Friedman*: No question.
> *Rose Friedman*: George was a very sensitive person. Though most people didn't appreciate that aspect. They thought of him as being …
> *Milton Friedman*: Tough.
> *Rose Friedman*: Right. And he really wasn't.
> *I know that within the profession itself, there's very divided opinion.*
> *Rose Friedman*: He antagonized a lot of people.
> *Milton Friedman*: Those who studied or worked with him have one view and everybody else has another. There's nobody in the world that could make smart cracks faster than he could.
> *Rose Friedman*: He had a tremendous wit. And it was usually at other people's expense.
> *It seems that, sometimes, he didn't know when to pull back.*
> *Rose Friedman*: Yes, basically he really didn't mean to offend people.

It seems like he just couldn't help himself.
Rose Friedman: That's right. (Conversation with Rose Friedman, Milton Friedman and Aaron Director, August 1997)

8. There is of course a temptation to portray ourselves (the contributors to this volume) as so many Totos barking at the man behind the curtain. Thankfully, we have largely eschewed such a self-aggrandizing position.
9. The degree to which it is possible to isolate purely scientific contributions from more relaxed offerings is inevitably arguable. The more formal aspect must reflect the alternative in a number of discernible ways. Though his work is mostly directed, in one way or the other, to his professional colleagues, he did not refrain from publishing items in the *National Review, Forbes* and *The Wall Street Journal* as well as other venues (See for instance Stigler 1977, 1987, 1989). He also gave talks to non-professional groups. Talks, which were later published, though not perhaps in professional journals (These often appeared in the Selected Paper Series of the Graduate School of Business (University of Chicago). For instance, a well-known debate with Paul Samuelson held at Swarthmore College (1963) appeared in that series.)

   If you look at his work, there is really, I would think, two classes of works, two classes of things that he does. One is he does things that you and I would call scientific work. That scientific work may inform people who set policy but that is not the open objective of that work. And the other is he argues with his professional colleagues. But there is very little that I can remember, there is nothing that I can remember right now, where he is arguing with politicians about what to do. So he doesn't really do that in his work. He mainly talks to the profession in his books of essays and things like that, and one of the things he tries to do in there is he tries to persuade them to do scientific work. (Conversation with Harold Demsetz, October 1997)

10. George Stigler was held back from achieving a position similar to his close friend Milton Friedman by his cultivated sardonic personality, one that was edged with a deeply acerbic wit. He seemed incapable of filtering out comments, "his zingers", no matter who the target might happen to have been. This proclivity made him something of an equal opportunity insulter.

    He could make mincemeat out of anyone, because he was so truthfully clever and objective. And the amazing thing is he was never, ever like that on an individual basis. I can see that distorted very much what people were saying about George. I think, that was a flaw in his character. Okay, if he had wanted in one or two sentences to make me look like a jackass, but he would occasionally do that with younger people, or inferior, I don't mean that literally I mean in rank, people with lower status than him. I thought that was terrible. He would just pounce on them. And I would tell him so. He would agree it was terrible. But he couldn't help himself, he couldn't resist. (Conversation with Robert Solow, October 1997)

11. A number of Chicago's most eminent economists were essentially poor boys from the provinces or those who definitely fell outside the boundaries defined by more glamorous metropolitan areas. Thorstein Veblen, an early protégé' of the first department chair, James Laughlin, certainly fits this profile, perhaps all too well. Frank Knight came out of a large Tennessee family, members of a small, ultra, if not deadly, religious sect. All of these individuals tended to have pointy elbows and often abrasive temperaments, perhaps forever driven by a need to prove themselves. Someone like Milton Friedman not only had a tenacious personality, a product of scrapping his way out of constrained family conditions, but also had to face a pernicious anti-Semitism, particularly rife at many of the more established, Eastern Universities. Friedman, perhaps universalized his specific success in clawing his way to the top by making each individual solely responsible for his or her life's outcome. (If I could do it, why can't you?)

    Now Milton had certain troubles, because of two things. Anti-Semitism, but also people were afraid of him. His corrosiveness and so forth. Gottfried Haberler wanted Milton Friedman to be appointed to Harvard and somebody like Ed Chamberlin, who was a very conservative person, was the department member most violently opposed. Because the Chicago School hated both the theories of Imperfect and of Monopolistic Competition. (Conversation with Paul Samuelson, October 1997)

12. His reputation was not entirely justified. ("A student once asked me what was 'beneath George Stigler's hard, sarcastic exterior'. How could I resist answering 'A hard, sarcastic interior'?" [Friedland 1993: 780]). Many considered him a loyal friend and a valuable colleague. He was a loving husband and father, certainly not treating his children like rational utility maximizers. In private moments he was actually capable of regretting (perhaps) some of his more barbed quips, though never breaking himself of the insidious habit of making them.

    Yes. I mean, it wasn't just that he criticized Gardiner Means, or that he criticized John Kenneth Galbraith, but he was sarcastic. He was funny, but he was sarcastic. He devastated them. They didn't take it too well. George was just telling them what he thought. He didn't want to hurt anybody's feelings. He wanted to show that there was something wrong with their thinking. But he didn't soften his remarks in order not to hurt their feelings or in order to allow them to save face. He really went after them and did his best to demolish their arguments and, that was George. And you don't make a lot of friends that way. (Conversation with Claire Friedland, October 1997)

13. After completing a MBA at Northwestern University, where he first cultivated a lifelong interest in the distribution of income (wrapped within

an ostensible study of the history of the discipline), he spent a year at the University Washington. During that time he applied to do graduate work at Chicago and Harvard. Had Harvard not proven to be so off-putting for a somewhat gauche young man from the provinces, remaining administratively remote and seemingly disinterested, George Stigler might have ended up trained by Harvard. Perhaps he would have quite naturally gravitated toward, and come under the influence of, Schumpeter or Haberle (as did another erstwhile Chicagoan, Paul Samuelson). What difference such an alternative path might have made is an interesting, but ultimately unanswerable question.

> It's hard to know. If he had gone to Harvard I'm sure it would have made a great difference. It was a different environment and an emphasis on different virtues and values. (Conversation with Milton Friedman, August 1997)

14. To be more precise, even Stigler could not resist genuflecting to the almost magical and irresistible power exerted by a Chicago based conversion to market reality. However, he fails to sketch as dramatic a political shift as Knight's other prize student, Frank Buchanan. The basis for claiming any prior left leaning political views, no matter how vaguely formulated, is never convincingly validated. Conservatives do have a certain fondness for the "once was blind, but now can see" sort of dramatic transformation. Though Stigler, as ever, delights in singing slightly off-key from the standard conservative evangelical songbook. He is deliberately ambiguous about what his political allegiance might have been prior to his Chicago education.

> Before I went to the University of Chicago I suppose I had vaguely liberal political inclinations, but no strong convictions. (Stigler 1988: 138)

15. At least according to his academic autobiography, Stigler (1988) never faced any particularly straightened circumstances, since his father found a means to prosper entrepreneurially after losing his job. Certainly, unlike other teenagers of that era (and even those much younger), Stigler never seemed to have held down a job during those years. Unlike Galbraith, part of a large family struggling on an Ontarian farm during that bleak era, Stigler as a single child never recounts any similar struggles that might parallel those of Galbraith or others during those depression years.
16. Stigler would prove to be one of the few students to complete a Ph.D. under the always irascible Frank Knight. "Only in his eighth decade did Viner permit himself to say privately, 'I always felt we should have treated Frank as if he were on the verge of a nervous breakdown in the

1930s. His financial problems and concerns about the disintegrating world economy and society were an important part of the picture'" (Samuelson 2011a: 591).

> At that stage he [George Stigler] was, as many people were at the University of Chicago, quite besotted with Frank Knight … Frank Knight's influence on the student body was profound and not, I say in retrospect, a hundred per cent positively constructive. (Conversation with Paul Samuelson, October 1997)

17. Wilson Allen Wallis served as dean of the University of Chicago, Graduate School of Business from 1956–1962.
18. Without becoming overly whimsical, Stigler never in spirit really left Chicago. He had simply made some geographical detours between the time he went off to Iowa State in 1936 and when he returned some twenty-two years later. He had been less than pleased at being balked in 1946 when a position at Chicago was seemingly his for the taking. Something went wrong at his meeting with the President of the University. Though Stigler reports that he was thought to be too empirical, the question of what the sharp tongued Stigler might have said during that interview necessarily remains a mystery.

> In the spring of 1946 I received the offer of a professorship from the University of Chicago, and of course was delighted at the prospect. The offer was contingent upon approval by the central administration after a personal interview. I went to Chicago, met with the President, Ernest Colwell, because Chancellor Robert Hutchins was ill that day, and I was vetoed! I was too empirical, Colwell said, and no doubt that day I was. So the professorship was offered to Milton Friedman, and President Colwell and I had launched the new Chicago School. We both deserve credit for that appointment, although for a long time I was not inclined to share it with Colwell. (Stigler 1988: 40)

Notice the typical use Stigler makes of sardonic language here. Reading beneath the lines it is clear that he was disappointed, if not entirely outraged, at not acquiring the promised position. However, he does not come directly out and announce his annoyance. Only a careful reader knowing Stigler's intellectual feints, his weakness for tongue in cheek writing and at least a bit about his history might notice the underlying sentiment. The reality is that while at Columbia, Chicago did make several attempts to woo Stigler, but every such attempt was suitably repelled.

> I was also struck by the lame excuses he gave for not accepting successive offers from Chicago that Ted Schultz pressed on him. … Though he always treated his initial rejection as a joke, as in his *Memoirs*, he had clearly been deeply hurt. I have no doubt that that was the real reason that he turned down the offers from Chicago. (Friedman 1993: 770)

He was however never one to openly complain or allow his feeling to show, choosing to John Wayne it whenever possible. So although he might have felt, at times, jealous of the attention accorded to his friend, Milton Friedman, it would betray his strict code of conduct to ever openly suggest any such emotions. Indications would be buried deeply, perhaps only hinted at by a quip or sublimated in some manner or other. As pointed out, the tension between the *honne* and *tatemae* of his life continued throughout.

> George Stigler always slightly resented the fact that the entire world learned all this stuff from Milton Friedman, when in fact, if you look at the order of precedence, George Stigler was slightly ahead in this sort of attitude to the testing of hypotheses. (Conversation with Mark Blaug, April 1998)

19. The reference here is to George Stigler's first published book *Production and Distribution Theories* (1941). The publication is based on the dissertation he completed in 1937 under the guidance of Frank Knight. Stigler was one of the very few graduate students to finish a dissertation with Knight as an advisor. The work itself can be superficially categorized, or dismissed, as an extended bit of history of economic thought. Stigler focuses on theoretical approaches to questions of production and distribution as posited by economists ranging from William Stanley Jevons to John Bates Clark. The work was heavily influenced by Knight, which is perhaps the source of Stigler's later discomfit with his effort.
20. Very few students at Chicago ventured to begin a dissertation with Frank Knight as supervisor, and even fewer completed. Students who did, had to have remarkable qualities to retain any form of equanimity in dealing with Knight. Besides George Stigler, the select group included Frank Buchanan, who was awarded a Nobel Prize as well as Homer Jones and Henry Simons. These were among the few who survived this ordeal, at least as orchestrated by Frank Knight. Though just as only the very best students would seem to gain an appreciation of Knight as a teacher (see Patinkin [1973] for some useful insights here), only the exceptional would manage to complete a dissertation under his baleful guidance.

> In fact, Knight's major influence at that time resulted in the view that Knight had done everything and there was nothing left to do. So, he was the cause that a pretty important generation of Chicago economists never got their Ph.D. degrees (Conversation with Paul Samuelson, October 1997).

21. Unfortunately for Stigler's argument, the physicist can look to the heavens to observe frictionless movement and the mathematician works in a field where the real objects are mental ones, namely mathematical objects. There may be observed physical objects that resemble circles, but the material world remains firmly outside the interest of the mathematician. Stigler may want to imply that discovered discrepancies between observations and the outcomes afforded by an idealized decision maker have at its root an informational causation. From Stigler's perspective, any apparent divergence could be rectified given additional data properly supplemented by more insightful analysis. However, that particular understanding would seem to eliminate ex cathedra any alternative psychological constructs. These might provide equally useful heuristics for economic modelling. Recent developments in behavioural economics challenge the more rigid stance defended by Stigler.
22. The reference here is to Milton Friedman's famous article which is still a controversial mainstay of economic methodology (Friedman 1953). This effort was intended as a first and last word on the subject and served for some decades as the unofficial position of the Chicago School. Fifty years after, at the annual January *American Economic Association Meetings*, Professor Friedman saw no need to alter or to adapt his original thoughts. A shorthand version of this stance can be seen in one of George Stigler's *Five Lectures* (1949) presented in 1948 to the London School of Economics "Monopolistic competition in retrospect". Previous correspondence between the two makes the interdependency of their ideas even clearer. See Hammond and Hammond (eds.) (2006) for the actual letters.

    > George and I carried on an intensive correspondence while he was exiled to Brown and Columbia. On perusing the surviving records recently, I was struck by his contribution to my methodology article, in the course of exchanges between us about an article that he was writing on imperfect competition. (Friedman 1993: 770)

23. Knight recognized no valid place occupied by data analysis in constructing a limited realm in which economic science was operative. "When I began study at the University of Chicago, Frank Knight and Aaron Director planted in me the false notion that somehow deduction was more important than induction" (Samuelson 2011: 891).

    > Knight would not look at any data. In fact, Frank could hardly be convinced by any data. Like inequality. Knight always thought inequality was growing in the United States while all the evidence up until 1970 said it was falling. And Stigler and Friedman and others would point this out to Knight, and George told me this,

> Knight would say 'yes, yes' but next time he'd say the same thing. So, I guess, he differed with Knight in this regard, but that was not unusual. He began to differ with Knight in a lot of respects. (Conversation with Gary Becker, October 1997)

24. We can easily anticipate Knight sharing his pupil's scorn for the arrogance of these young economists. Their temerity allowed them to blithely propose an imagined capacity for regulating policy by means of their theoretical constructs. For Knight, this would demonstrate the absurdity of trying to overreach the bounds of the discipline's capabilities. But by the time that Stigler joined with his colleague Gary Becker in writing "De Gustibus Non Est Disputandum" (1977), imagination would have to stretch to its farthest reaches to associate Stigler with his earlier, Knightian inspired pronouncements.
25. This assertion is noticeably diametrically opposed to the position defended in his seminal piece with Gary Becker (1977). But by the seventies, apparently deeply influenced by the persuasive logic of Becker, a given and fixed set of preferences formed an intrinsic component of extending economic rationality to any endeavour involving human decision making. (Some experimental economists would later quite cheerfully encompass non-human decision making under this same rational framework.)
26. Retrospectively at least, Stigler removes any doubt of his rejection of the Keynesian movement in his 1988 autobiography.

    > There was nothing in these views to repel a student; or to make Keynes attractive. On the contrary, so far as policy was concerned, Keynes had nothing to offer those of us who had sat at the feet of Simons, Mints, Knight, and Viner. (Stigler 1988: 153)

27. The effect that this stint had on his thinking was quite opposite from that of someone like John Kenneth Galbraith. The ability of economists (and others) to glean exactly what they need out of experience (serving to reinforce prior inclinations) is far too common.

    > Of course we were fighting a hopeless battle, and once price control powers were given to the OPA, we were overwhelmed by the price controllers, who soon procured John Kenneth Galbraith to be their administrator of the price system. (Stigler 1988: 60)

28. For one of the best and most balanced looks at the formation of the society, its intricacies, and the various opposing factions that it attempted to encompass, see Burgin (2012).
29. It was hardly accidental that one of the suggested names for the Society was Acton-Tocqueville after two of the main pillars of the Classical Liberal faith.

30. Director and Friedman shuffled off to California by the 1970s leaving his daily contact monopolized to some extent by Gary Becker. However, even before this departure, it was Aaron Director, much more than Milton Friedman, who could always manage to steer or at least arrest Stigler's thoughts. Such an influence did not readily extend to other of his colleagues and friends.

    Milton Friedman: Added to that, well a lot came from Aaron [Director]. I think you had a lot of influence on what he [George Stigler] said.

    Aaron Director: I don't think so.

    Milton Friedman: Between you and me, you were more influential. But of course, you know, people get into patterns of what they say and it doesn't always correspond to what they do. (Conversation with Milton Friedman, Aaron Director and Rose Friedman, August 1997)

31. Characteristically, by the 1970s he would suggest that the Mont Pelerin Society was well past its due date. Namely, that it had accomplished its objective and to continue would be largely an act of vanity. (The self-indulgence aspect has to be interpreted between the lines of Stigler's missives. His barbed insults were not always of the explicit variety.)

    I would like to say, however that you attribute both more unanimity and influence to the Mont Pelerin Society then I do … I should also add, I suppose, that Friedman and I both urged the termination of the society with the present meeting but were over ruled by a large proportion of past officers. I have confidence therefore that although some of us are surely wrong, some of us are not doctrinaire. (Letter from George Stigler to Bertrand de Jouvenal, May 1, 1972)

32. Stigler's early efforts were varied. They ranged from a fairly mainstream review of monopoly (1942a) to a curious piece on deducing a low cost, nutritious diet (1945) that anticipated linear programming and the simplex solution method. His early work also included a more innovative work examining short run cost curves (1939). Immediately after the war, a series of fairly straightforward empirical pieces for the National Bureau of Economic Research appeared (see for example 1946a), as well as the first edition of his textbook (1946b), a work still heavily under the influence of Knight. A somewhat neutral observer would doubtless reckon George Stigler as an academic of promise, but not one with a clear and abiding objective.
33. Given the Cold War environment, the issue of individual choice would cause other methodological concerns of Classical Liberalism to be filed under the quaint and old fashioned category. Accordingly, Stigler would view attempts to achieve an equality of income distribution to be diametrically opposed to individual liberty insofar as it implied freedom of choice. Given his now crystalizing perspective, these

distributional policies were redefined as the inevitable wedge through which collectivists and totalitarians exerted their baleful influence. The continuing focus on income distribution, existing as both the underlying concern of his dissertation as well as one of his first forays into economic analysis while at Northwestern University, is evident in these LSE lectures. Such issues would continue to exist as a defining undercurrent throughout his career. Given the growing Cold War threat, Stigler, by this time, had become frankly suspicious, if not outright repelled by, theories and policies which from his perspective, could only lead individuals astray, by inadvertently encouraging them to become worse than they already might be. Though acknowledging the dangers of trying to mould people into more saintlike positions, this loss of evangelical zeal still left the analyst, in Stigler's opinion, with a clear responsibility not to make humanity even worse by encouraging the wrong set of incentives to be established.

> There is not and cannot be agreement on the precise character of man we seek to achieve … But we are persuaded that an economic system will not help us to move in the right direction unless it grants both opportunity and responsibility to the individual, the very uncertainty of our ultimate ethical goals dictates a wide area of individual self-determination. We are not able to supply a blueprint of the ideal life, but we are persuaded that even if it were known it would be ideal only for the person who individually and knowingly and voluntarily accepted it. It is not necessary, however, to know what is best; it is enough to know what is better. (Stigler 1949d: 8)

In his discussion with his close friend, Milton Friedman, Stigler had been pulled back from the brink of Marshallian evangelizing.

> Re your solution? "the improvement of the individual" is about as ambiguous a touchstone as "equality". I don't know how to define either. You cite Marshall. In him, "the improvement of man" equals the remaking of other peoples into the image of the Englishman, which is warning enough that the slogan has the danger of leading to the narrowest kind of presumptuous provincialism. (Letter to George Stigler from Milton Friedman, February 1948 in Hammond and Hammond 2006: 78)

This suspicion, if not outright abhorrence of income redistribution, would only grow over time. An increasingly cultivated, oppositional position perhaps reaches something of an apogee in Stigler's Tanner lectures (Stigler 1982a, b).

> But, the only thing I can remember him [George Stigler] saying or writing, he wrote it somewhere, but I can't remember where, was that he favoured a capitalistic oriented system. He favoured it because it created the kind of person that he'd like better to live with. And that kind of person was somebody who felt responsibility for himself, and not one who thought, that others were responsible for him. (Conversation with Harold Demsetz, October 1997)

34. Stigler and the Chicago School would take a far less nuanced view of the struggle between the collectivist rule of government and individual liberty. Perhaps the extreme vision, one willingly provided by Margaret Thatcher concludes, "There is no such thing as society". Mill, more pragmatically, recognizes that beneath the rhetoric on either side of the question are deeply imbedded ideologies and prejudgments.

    > There is, in fact, no recognized principle by which the propriety or impropriety of government interference is customarily tested. People decide according to their personal preferences. Some, whenever they see any good to be done, or evil to be remedied, would willingly instigate the government to undertake the business; while others prefer to bear almost any amount of social evil, rather than add one to the departments of human interests amenable to government control … And it seems to me that in consequence of this absence of rule or principle, one side is at present as often wrong as the other; the interference of government is, with about equal frequency, improperly invoked and improperly condemned (Mill 1859/1947: 90).

35. John Stuart Mill is one of the best representatives of this particular approach. Two key aspects of Classical Liberalism, as embodied by Mill, involve first a requirement not to be dogmatic. Within this methodology, scepticism shouldn't remain one-sided in nature, reserved only for the work and analysis of ideological opponents. Here Frank Knight and Jacob Viner set something of an example.

    > Like Marshall, Knight and Viner did not see economics as offering virtually inviolable laws. Instead, they looked at theories as tendency statements as when Knight said that 'economic laws like other scientific laws state as *tendency*, a result which would follow if certain conditions are present. (Medema 2011: 154)

    Moreover, Mill recognized a clear and discernible division between theoretical economics (economics as science) and the very different problem posed by trying to solve specific problems (the art of economics). "… the usefulness of history is not in giving us rules which can be made the basis of inference and prediction, it is not in this respect a science, but rather an art". (Knight quoted in Stapleford 2011: 26)

    > John Stuart Mill wanted to restrict the scope of economic science to the study of the factual and the probable. Senior had forcibly argued for the same view in his inaugural lecture when he succeeded to the newly created Oxford professorship in economics, and he never tired of reiterating the same thesis in his later writings. He expressly stated that the economist's 'conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice'. (Myrdal 1954: 3)

36. The London School of Economics, still under the sway of such luminaries as Robbins and Hayek, (Hayek being instrumental in bringing Stigler to speak there), had along with Chicago, rigorously resisted the allures of Keynesianism.

> That hostility to collectivist restrictions on personal freedom, as well as the liking for a competitive order, were somewhat stronger in the University of Chicago's economics department than at most other places (except, and especially, the London School of Economics). (Stigler 1988: 139)

Little surprise should be elicited then at the positive reaction by the assembled LSE notables to Stigler's five concise, tightly interwoven lessons (the term deliberately used as conveying something of a Christian simile).

> I am writing mainly to swell your head – though God knows it must be big enough already. Hayek reports that your lectures were "brilliant" & successful. Indeed, he said yours were by all odds the most successful series of lectures they had ever had. I didn't realize the state of English Economics had sunk so low – though, come to think of it, Hayek was including pre-war experience, so I guess I'll just have to take it to mean that the English are still smart enough to agree with the rest of us. (Letter from Milton Friedman to George Stigler Tuesday, April 7, 1948 in Hammond and Hammond 2006: 80)

37. In some sense these five lectures, presented before one of the few congenial audiences of that time (sternly anti-Keynesian), are much like the writing of Proust in his *Remembrances of Times Past* or even Keynes in his *General Theory*. Proust in the first fifty pages of his multivolume epic lays out his concerns that are then reworked in a non-linear fashion. The essence of each work is revealed by circling around and disclosing ever more of the core material with each return to the starting point. Keynes uses a somewhat similar technique insofar as his theoretical structure is condensed in the first few chapters with the rest of the volume serving to unwrap this initially compacted theory. Again, Keynes uses something of a non-linear approach to relate his particular story.
38. In these lectures he has not quite shaken the tendency of Knight to sneer at and summarily dismiss even the mildest expedition into the realm of mathematical theorizing.

> It is undeniable that the profession contains many very able economists whose mathematical attainments are meagre or less. If the mathematical economist's results are suggestive or useful, these people have a right to know them. If the results are tentative and conjectural, these people have a right to test them. It is the fundamental obligation of the scholar to submit his results and methods to the critical scrutiny of his competent colleagues in a comprehensible fashion. (Stigler 1949: 45)

Consequently, although Stigler would break from Knight's contempt for mathematical intrusions and his dismissal of empirical evidence, he would definitely not compete on the theoretical or abstract frontier with the likes of Arrow or Samuelson. He would take an alternative scientific path allowing him to jeer at his slightly younger compatriot, Paul Samuelson.

> Rumor has it that Samuelson was quite the unsuccessful suave chairman, a la Schumpeter, at the meetings. Sol[omon] Fabricant said he referred to you as an altar boy or something of the sort. I would have relished being there to see your reaction. It may merely be prejudice, but I'm inclined to write him off as an economist. Two of his recent jobs (the Survey article and his essay in the Hansen Festschrift) were pure mathematical exposition, as is also his current *Economica* item (which, by the way, has already been done better by Wold), and his textbook suggests that he doesn't know anything that hasn't appeared in the *Survey of Current Business*. (Letter from George Stigler to Milton Friedman, January 1949 in Hammond and Hammond 2006: 97)

39. Stigler's break with Frank Knight, which gathered strength after the Mont Pelerin meeting, was not simply a matter of disagreement over the role of empiricism in economics. Ideology seemed one of the key factors driving Stigler's strategy of expanding the reach of economics as science. Unlike his student, Knight didn't kneel at the altar of the price mechanism or stand in awe of the workings of the marketplace. It was, in his view, far from self-correcting on even equitable. A system built with a strong element of uncertainty or randomness promised only adjustments, but not those necessarily welcomed by a large segment of the population. Like democracy, markets were a badly flawed system, but recognizably better than the available alternatives.

    > Late in his life Frank Knight expressed growing dissatisfaction with what he perceived to be the rhetorical excesses of the new generation of market advocates. They were, he wrote to a former student, "hurting the cause by going to extremes." Cannan, Viner and Knight remained dispositional conservatives, inclined to distrust both novel attempts at social engineering and excessive credulousness in regard to the workings of the market and complexity of academic analysis. Their support for the price mechanism was born of dismay at the alternatives than of admiration for its moral, cultural, or informational effects. (Burgin 2012: 54)

40. Both had met and talked to Karl Popper at the Mont Pelerin meeting. The groundwork for the approach was hammered out in a series of letters between Friedman and Stigler prior to the LSE lectures. (Friedman was then at Chicago while Stigler maintained his exile at Columbia.) In a crucial sense, neither one was particularly interested in methodological issues. The purpose seemed more focused on ending such discussions within the profession, particularly those centred on the importance and reality of assumptions.

    > I should like to offer the general proposition that every important scientific hypothesis almost inevitably must use assumptions that are descriptively erroneous. It is of the very nature of a really important scientific generalization that it provides a simpler rationalization of a mass of facts than was available before. It is likely to obtain its objective by an inspiration about the particular basic elements

> of the situation that are important and by discarding what after the event can be shown to have been irrelevant complicating assumptions. In a way, the better the hypothesis the greater the extent to which it simplifies, the more sharply will the assumptions depart from reality. (Letter from Milton Friedman to George Stigler, November 1947 in Hammond and Hammond 2006: 65)

41. If Stigler's progress is followed from his LSE lectures to his presidential address (December 1964—A.E.A. meetings), the end result should neither shock, startle nor surprise. As early as his 1948 lectures, when examining the effect of monopoly on the US economy, Stigler (1949e) seems to indicate that evidence could essentially speak for itself. Levels of monopoly (or levels of competition) were simply correlated with clear indicators of price rigidity and profits. In this light, evidence then dictated policy in a rather simple-minded manner. Thus the problem of evaluating whether a desired policy could affect an interpretation of evidence is never given due consideration. Objective fact existed, which by definition was independent of the investigator. Consequently, his expectations that quantitative techniques would resolve economic issues was a natural outcome of seeing this particular route as the path to scientific veracity. (Needless to say, he also was certain that the evidence would never contradict his vision of market efficiency guided by the unfailing operation of the price mechanism.)

    > The age of quantification is now full upon us. We are now armed with a bulging arsenal of techniques of quantitative analysis, and of a power – as compared to untrained common sense – comparable to the displacement of archers by cannon. But there is much less a cause than a consequence of a more basic development: the desire to measure economic phenomena is now in the ascendant. It is becoming the basic article of work as well as of orders of magnitude, and preferably one should ascertain the actual shapes of economic functions with tolerable accuracy. (Stigler 1965: 17)

42. 'The fox knows many things, but the hedgehog knows one big thing.' The origin of this thought derives from a fragment found in the work of the Greek poet Archilochus (one of the fragmentary Greeks). The idea differentiating two alternative approaches to thinking was made famous by the Philosopher Isaiah Berlin (1953) in his essay, *The Hedgehog and his Fox*.
43. Stigler believed that the post war lack of faith in the market mechanism led to an inevitable turning toward more collectivist structures. Such induced preferences were necessarily degrading the fabric of liberty by destroying individual choice. This firm conviction was installed, grounded, and built upon by that first Mont Pelerin meeting. For Stigler, this durable mindset would only be reinforced by observed evidence in the years that followed.

> The proof that there are dangers to the liberty and dignity of the individual in the present institutions must be that such liberties have already been impaired. If it can be shown that in important areas of economic life substantial and unnecessary invasions of personal freedom are already operative, the case for caution and restraint in invoking new political controls will acquire content and conviction. (Stigler 1975: 18)

44. Classical liberalism lacked a compelling belief in any specifically scientific basis for policy objectives. Consequently, though there was a preference for individual liberty, this attraction did not translate into a scientific demonstration of some innate superiority. An unflagging enthusiasm for markets as the fundamental fount of such individual liberty was also notably lacking.

> Canaan attributed his support for the market mechanism to scepticism about the available alternatives rather than any ardent enthusiasm for its workings. "Modern civilization, nearly all civilization, is based on the principle of making things pleasant for those who please the market and unpleasant for those who fail to do so, he observed in *An Economist's Protest*, "and whatever defects the principle may have, it is better than none". (Burgin 2012: 18)

45. Clearly any extended focus on what differentiated one market from another would not have advanced Stigler's consistent scientific program. The problem though was that for practical policy applications, such ignored differentiations could become vital elements of analysis.

> Well George did not think that differences were so important for economic analysis. You wanted to understand prices, demand and supply. You could use the same kind of model no matter where you applied it and you didn't have to have a really special model for it. I think that was why he was not a fan of the 1930s and 1940s industry studies. They thought every industry was unique. I think that was one of the consequences of the Chamberlin monopolistic competition model and he didn't see any useful generalizations coming out of that. He was always interested in generalizations. And he was not interested in explaining the particular as much as he was in the generalization that you could deduce. (Conversation with Harold Demsetz, October 1997)

46. Alfred Kahn, an economist, was appointed the head of the then Civil Aeronautics Board in 1977. Under him, this nominally regulatory agency spearheaded the effort to deregulate the airline industry. Kahn took great pride in his total ignorance about the specifics of the sector.

> An academic, Mr. Kahn knew almost nothing about the airline business — to him planes were just "marginal costs with wings" — but he quickly mastered the arcana and politics of routes, pricing and costs. (Hershey 2010)

47. Gary Becker, returning to Chicago from his stint at Columbia (much the same path followed by Stigler), was instrumental is nudging Stigler toward an ever broader application of economics and market processes

to an expanding sphere of comprehensive research. Doing so would not have proved difficult given his predilection for developing a comprehensive and consistent theory that could simply be applied wherever and whenever individual decisions were cast.

> Personally, Gary and George were much closer than George and I. Much closer and in many ways, I think, they were much more on the same wavelength, especially in this area, 'how far you could push rational choice,' that kind of stuff. George was absolutely enthusiastic about everything Gary was doing, with the family, with marriage, with this, with that, with everything. But the outside world apparently wasn't. And apropos to the outside world, the graduate students at the end of the year would put on a party where they would perform a play: 'The economics of mud' by Gary Becker. 'The economics of 'fill in the gap," by Gary Becker, you know what I mean. It was a standing joke. It has become a standing joke. But George loved that kind of thing. As I do. I mean, I agree with it. I think it is a very powerful part of the message of economics that there's growth in its thinking, and standards and a reason to take economics seriously. But in terms of actually working in that area, the 'Tastes' paper (Stigler and Becker 1977), I told both of them I thought it was absurd. (Conversation with Sam Peltzman, October 1997)

48. Psychology and the complexities of behavioural economics was for Stigler a needless distraction. Incentives in terms of narrow self-interest were sufficient to explain the world. This insight reflected exactly how the world worked, as buttressed by all available evidence. Since the time of Smith, attempts at developing more complex models of agency had, in Stigler's estimation, merely side-tracked the profession, wasting scarce and thus valuable time. (Ironically, the current Walgreen Distinguished Service Professor of Behavioral Science and Economics, Richard Thaler, was awarded a Nobel Prize [2017] for work in varying degrees directly opposed to Stigler's professional passion. The Chair itself has changed its name from the Walgreen Distinguished Service Professor of American Institutions, its designation when the position was inhabited by George Stigler and then by Robert Fogel, also a Nobel Prize winner—1993.)

> If one were to seek a major economic theory whose existence depended directly and essentially upon prior work in another field, he would find few likely candidates. Putting aside for a moment, the methodological fields of statistics and mathematics, there is in fact no important candidate. A theory of behaviour, such as our profit maximizing assumption implies, could have come from psychology, but of course it did not. In fact Smith's professional work on psychology (in the Theory of Moral Sentiments) bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted despite continued efforts from Jennings [1955] to Herbert Simon and George Katona to destroy it. (Stigler 1960: 44)

Even worse was any time spent on, or consideration allotted to, ideology as a causal value. Stigler found the term to be essentially empty, a signal that some unidentified incentive could not yet be articulated, measured, or put to any serious use. To refer to such essentially vague terminology was to wilfully turn one's back on science, giving way to hand waving and storytelling.

> It is common to interpret it [increase in federal spending after 1929] as a result of what I take it you would call an ideological shift in two respects. First, before 1929, the role of the federal government was inhibited by the strong belief in a balanced budget on the one hand and in limiting the role of government to that of an umpire on the other – what I would call a belief in free markets. Although that belief had eroded in the intellectual community and was gone by 1929, it still held a very strong hold on the public at large. After 1929, it was replaced by an acceptance of deficit spending on the one hand and a belief in government as an effective big brother on the other. (Letter from Milton Friedman to George Stigler, March 19, 1984)

For Stigler, his old friend Milton Friedman had merely demonstrated his lack of understanding of an historical change.

> I don't know how important ideology is, but I think it is unimportant. You don't know how important it is, but think it is important. My position is better because I try – feebly and so often unsuccessfully – to use a trusted theory of human behaviour to explain social phenomena. Your position is worse because you try – with marvellous ease – to explain the mysteries by a deus ex machina. (Letter from George Stigler to Milton Friedman, March 29, 1984)

49. Peltzman is most likely referring to work in behavioural economics which was gaining some steam in 1997. Among others, Daniel Kahnneman, Amos Tversky and Richard Thaler had produced some initial work on the subject. At Chicago, a joint workshop with members from the economics and psychology departments was involved in discussing the possible merits and shortcomings of this approach.
50. The reference here is to the philosopher Thomas Hobbes. In *The Leviathan*, he examines civil society and the place of power. Civil society is possible, according to the Hobbesian view, because individuals relinquish whatever power they might wield to a ruling sovereign. Only by letting go of such power can they insure the individual security necessary for civil society. In essence, only by becoming powerless can they secure the power to operate with a necessary degree of certainty. Without such an implicit concord, life according to Hobbes would be "nasty, brutal and short".
51. In the article, existing as more of a note (Becker 1991), Gary Becker constructs an interesting and very clever theory explaining an observed

anomaly of pricing in the case of competing restaurants. The result is a generalized pricing model that is more widely applicable in explaining a number of apparent observed anomalies. The Chicago method focuses on explaining why any apparent anomaly is only a mistaken perception of what in reality must be an efficient market result. While working on his note, Becker didn't waste time questioning the managers of the restaurant on their pricing strategies. To overstate the case, this would be, at least from the Chicago point of view, as fruitful as questioning a mouse running through a Skinner box. Like the mice, the managers are only reacting to market incentives whether they are cognitive of this relentless relationship or not. To understand the observed pricing regime, an economist examines the way the Skinner box is constructed (the market system) rather than interrogating the mice forced to scamper within its defined boundaries.

52. George Stigler, for instance, venerated Adam Smith yet broke directly with his perceptions (Stigler 1971b) of the role best played by government. What Adam Smith and his subsequent followers understood was that whether or not government had a role to perform within the operations of a market economy was a non-issue. The more productive terrain of debate disputed the nature of the role necessarily played by government and the limits that could and should be usefully imposed. The resolution of these issues, however, did not rest on theoretical grounds.

    > Adam Smith was not a doctrinaire advocate of laissez faire … He had little trust in the competence or good faith of government. He knew who controlled it, and whose purposes they tried to serve … He saw, nevertheless, that it was necessary, in the absence of a better instrument, to rely upon government for the performance of many tasks which individuals as such would not do, or could not do, or could only do badly. (Viner quoted in Van Overtveldt 2007: 90)

53. The description appears in Thomas Sowell's account of the scars suffered under George Stigler's tutelage.

    > Few, if any, areas of economics have as much confusion, circular reasoning, definitional traps, and fervent nonsense as industrial organization. It was the perfect place for Stigler to conduct a Demolition Derby. Nor was he hesitant about the task. Theories like "monopolistic competition" and "countervailing power," which were treated reverently at Harvard (where they originated), were eviscerated by Stigler. (Sowell 1993: 787)

54. Stigler's very consistent and focused insistence that an economist should not be in the business of marketing or even proposing policy

measures is perhaps the only aspect (discounting a sharp and at times venomous tongue) that did not directly repudiate the principles articulated (and usually practised) by Frank Knight.

> You have described his view, accurately in my opinion, that he did not think that what economists said in regard to policies carried very much weight in the formation of policy. That was in fact determined by political interests and those political interests could define their own interests pretty well without the help of economists and did so. If you look at his work, there is really, I would think, two classes of works, two of things that he does. One is he does things that you and I would call scientific work. That scientific work may inform people who set policy, but that is not the open objective of that work. And the other is he argues with his professional colleagues. But there is very little that I can remember, there is nothing that I can remember right now, where he is arguing with politicians about what to do. So he doesn't really do that in his work. He mainly talks to the profession in his books of essays and things like that, and one of the things he tries to do in there is he tries to persuade them to do scientific work. That's what they really ought to be doing. And, all sides to any political dispute will have to use their work, if it has scientific value. So everybody better know, whether you are on the left or the right on a particular issue, that demand curves are negatively sloped. Whether or not he may have put it that way, one time or another, I don't remember. But he definitely had that view and held to it in his own work to a large extent. (Conversation with Harold Demsetz October 1997)

55. Given his premise, Stigler's logic is impeccable. Economic agents respond to market incentives to promote their individual (and narrow) self-interest. Thus having the better economic argument (free trade as one among many examples) is immaterial. Policy shifts when self-interest changes. At best, an economist could try to convince voters that a given preferred policy is in their self-interest. Stigler (1971) would contest the possible efficacy of such attempted political promulgations. Politicians and other sift through economic arguments until they find those that help to rationalize their perceived self-interest, rather than being moved by economic logic to formulate their policy.

> *Aaron Director*: I don't find that very difficult to explain. He just thought there were two different functions to perform. One is just simply to understand. You don't have to be interested in changing anything.
>
> *But, was he interested in changing …?*
>
> *Aaron Director*: No, I'm certain …
>
> *Milton Friedman*: Of course he was.
>
> *Aaron Director*: He would always insist he was not.
>
> *Milton Friedman*: But of course he was. Why would he accept a post on the price …?
>
> *Aaron Director*: Oh, yeah, well the price index I think.
>
> *Milton Friedman*: Why would he chair a price index committee?
>
> *Aaron Director*: Somebody asked him.

Laughter.

Rose Friedman: He didn't do everything somebody asked him to do.

Aaron Director: No, no, but I don't really think you can make something out of so very little. You can believe that your role is really - has very little to do with wanting to change the world. Yet, you know, if somebody wants to re-examine the price index; and they ask you to … I don't think that's really a problem.

Milton Friedman: There's no problem. It's true, that George did want to change things.

Aaron Director: But he preferred to study them, not to change them.

Milton Friedman: He preferred to say that he preferred to study them.

Aaron Director: He preferred to study them. I should quit the argument.

Milton Friedman: It was partly a long-running difference between him and me.

Aaron Director: You're right.

Milton Friedman: And he liked to stress, 'I just want to understand the world and Milton wants to change it.'

Aaron Director: That's right. And predominantly I think that is correct.

Rose Friedman: You would have to have them both psychoanalyzed.

Aaron Director: Yes. That's right.

Rose Friedman:Or hypnotised.

Milton Friedman: Added to that, well a lot came from Aaron. I think you had a lot of influence on what he said.

Aaron Director: I don't think so.

Milton Friedman: But of course, you know, people get into patterns of what they say and it doesn't always correspond to what they do.

Aaron Director: Yes. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

56. For Stigler a working model could not simply explain a given percentage of observed events. Conclusions had to apply equally to each and every outcome. Life was to a practical extent black and white. All actions and observations had to be reduced to narrow self-interest. The underlying logic of that position insisted that if one dug deeply, at least to a sufficient level, sordid greed would bubble up and overwhelm any apparent public good façade.

Milton Friedman:Well, those all reflect a public interest view. A view that you cannot understand government activity simply by looking at the self-interest of the people involved, at least if you are not going to make this whole thing into a tautology.

Aaron Director:No, but if you stop asking us to deal with the economy and you ask us to say something about government, what do you want us to do in that case except to apply the same kinds of analysis in that area that we apply to the others?

Milton Friedman:Yep. But there is also the John Maynard Keynes view that you have government civil servants.

Aaron Director:Yes.

Milton Friedman: Who are pursuing somehow the general public interest. Oh, they're pursuing their own self-interest in one sense because they've got jobs and salaries …

Aaron Director: Yes.

Milton Friedman: … but they are primarily, they are disinterested agents (a) of the general will of public say …

Milton Friedman: Everybody wants to feel that what he's doing is good for the country.

Aaron Director: Of course.

Rose Friedman: OF course.

Milton Friedman: But there's a limit to the extent to which you can rationalize.

Rose Friedman: Of course.

Milton Friedman: And so, it isn't all black or all white.

Yes. And then the issue is, where do you draw the line?

Milton Friedman: And it is true that George would tend to emphasize, more than I would, the extent to which it was - you're just spitting in the wind if you try to advise governmental officials to do something which has not heretofore been in their self-interest to do. It may be most of the time, but where that comes from, I don't know. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

57. This unvarnished and incorruptible belief left George Stigler residing in a rather ambiguous stretch of geography. Despite the purity of his intentions, he wasn't loathe to appear as an expert witness in legal proceedings. His fee for such undertakings spiked sharply upward after winning the 1982 Nobel Prize.

I have seldom been brought to dislike the experts or lawyers on the other side, but I have usually ended up liking those on my side, which comes to much the same thing. That has not led to a conscious manipulation of the evidence and arguments but to a cast of mind that focuses on the arguments favourable to one's side. Is the expert honest? At very best, probably as honest as is possible in a process in which truth is sought by the vigorous presentations of opposing views, and where any admission by one side is heavily overemphasized by the other side. (Stigler 1988: 133)

Though the unfortunate truth about saints, whether economic or otherwise, is that they may also feel the pull of temptation.

When I first came to Harvard in 1935, the university got a grant from Thomas Lamont of the First National Bank, now the Morgan Guarantee Bank, to establish an Institute Professor. … And they had a wide choice of applicants. Now, Aaron Director came to visit me for a weekend at Harvard and he said, 'Why didn't you give it to Frank Knight? He would have liked to have had the job.' And I said, 'Well, would he have accepted it?' Because I had heard the story that when Allyn Young went to England and they had to replace him, Harvard extended a call to Frank Knight who had earlier been Allyn Young's thesis student at Cornell University. … And he said, 'Well, in 1927 Frank Knight refused the call and the reason was that he didn't approve of President A. L. Lowell's treatment of the

> Sacco and Vanzetti case … That's the good part of the story. But then, according to Aaron Director, who shared a cabin in the sand dunes outside of Chicago with Knight, Knight now (1935) said, 'What a fool I was.' I found that sad. (Conversation with Paul Samuelson, October 1997)

58. The underlying reasoning insisted that in a democracy, a political vote was essentially indistinguishable from the type of commercial vote a consumer exercised within a marketplace.
59. If opportunities are seized within the marketplace, they must accordingly be pounced upon within the political sphere. Incentives and self-interest must operate in a parallel fashion wherever individual choice is determinative.
60. Insisting otherwise would appear to contradict his seminal work with Gary Becker (1977).
61. By this time in his career, markets were almost competitive by definition. In the decades spent during the first part of his career, George Stigler had attempted to undermine the legitimacy and importance of seemingly non-competitive markets. By the time Stigler turned his attention to political markets, perfect competition in his research scheme had come to mirror actual markets, at least in their essentials. Deviations were either a case of erroneous labelling or simply inconsequential.
62. Stigler's conclusions were a step too far for many of those normally associated, or running in tandem, with the Chicago School. Most condemned rent-seeking government policies while actively promoting radical changes.

> But there are just things like that which can't be explained. 'Consistency is the hobgoblin of small minds,' somebody once said. He was not consistent. That's clear. And this inconsistency has led to a lot of misunderstanding. There are people outside of Chicago who read him this way. With Becker it is even more powerful. It's all part of Becker's stuff about optimality and redistribution. Outsiders kind of read both of them as, 'This is kind of the senescence of the Chicago school. They have become toadies for big government, apologists for big government.' And I could see why. It is a really subtle kind of distinction we are making here between the two. But look, if you're going to regulate, conditional on wanting to redistribute income, I can't tell you that this is wrong. So, if I don't like it, if I tell you it's wrong, it has to be because I don't like the resulting redistribution. (Conversation with Sam Peltzman, October 1997)

63. The classic Western referred to here is *Shane,* with Alan Ladd alone having the bravery (and ability) to stand up to the hired gunslingers. Though given the heat and dust of the frontier, one wonders at Palance's sartorial choice of black on black.

64. In many ways, Stigler acted as a forerunner of the dreaded Daleks (vacuum cleaners on wheels) intoning "Resistance is useless", on *Dr. Who* in the 1960s.
65. There was something of a democratic aspect to his insults, as George Stigler showed no overt concern over the object of his caustic barbs. To maintain good relations with him, individuals, whether students or professors, could ill afford to be the proverbial shrinking violets. You needed to give as good as you got.

    > He had a very tough exterior and he could be sarcastic and biting with people. George, if it was a choice between a good joke and making somebody feel bad, he'd take the good joke every time. That's how he was. He wasn't deliberately mean to some people, he didn't dislike them a lot, but he had such a terrific wit about him. You had to get used to it. It took me some time to get used to it. It took my wife a while to get used to it. But she began to love him too. (Conversation with Gary Becker, October 1997)

66. Stigler remained irritated that at least during his lifetime he was unable to scourge what he viewed as a theoretical black hole from most first year microeconomic textbooks. However, in an ironic twist of fate, it was Stigler's interpretation of the kinked demand curve that became immortalized in the pages of those introductory texts. Sweezy himself never responded to Stigler's (1947b) demolition job of his paper. His memory was that at the time he was in a particularly rigid Marxist frame of mind and refused to pay attention to orthodox theory (Conversation with Paul Sweezy, November 1997). (For a blow by blow analysis of this now somewhat forgotten theory, see Freedman 1995.)
67. Like the Scarecrow in *The Wizard of Oz*, theories tended to be highly flammable when properly positioned and doused with kerosene (or the equivalent weight of scathing comments). It would only be somewhat unfair to argue that during his long career, George Stigler constructed a number of straw men which would have been sufficient to populate endless summer stock revivals of *The Wizard of Oz*.
68. The relevant works here are: (1) Means, Gardiner (1935). His Washington testimony represents Means' first use of the term "administered prices" and includes his attempt to explain the Great Depression through the prevalence of inflexible pricing policies. (Prices failed to dip down, or rise up, to market clearing levels.) This work forms a natural progression from his previous book with Adolph Berle explaining the decreasing prevalence of competitive markets (Berle and Means 1932). George Stigler sought to demolish Means' obsession in

Stigler, George J. and James Kindahl (1970). This attempt was followed by an intense and pointed exchange between Means and Stigler in *The American Economic Review* in which neither party relented or had any desire to relinquish the field of combat. (2) Leibenstein, Harvey (1966) presented a perspective which attached a level of inefficiency to the price system, at least as it defined and regulated the labour market. Stigler's sardonic dismissal appeared a decade later in a possible attempt to terminally deflate interest in Leibenstein's concept (Stigler 1976b). (3) The next reference is to Galbraith, John Kenneth (1958). Though Stigler combated many of the points raised in this book (one can argue that Stigler, George J. and Gary S. Becker (1977), take direct aim at Galbraith's consumer theory), he didn't directly review that particular volume of Galbraith's work. However, "Ambassador to the Industrial State", *New York Times*, June 26, 1968 and "A Certain Galbraith in an Uncertain Age", *The National Review*, May 27, 1977: 601–604, highlights Stigler and Galbraith as consistent sparring partners. Perhaps Stigler's attempt to shred Galbraith's first influential book (Galbraith 1952) set the stage for all future battles. Stigler's title of the presentation he gave at the December 1953 AEA meetings probably foreshadows all of his subsequent sardonic stances to Galbraith's output (Stigler 1954). (4) Lastly Paul Sweezy's work on the kinked demand curve (Sweezy 1939) was met by Stigler's outright demolition of this model (Stigler 1947).

69. The common thread linking what seems an ill-assorted collection of targets is that each challenged the existence of a unique equilibrium, allowing the possibility of multi, or even a lack of any, equilibrium solutions whatsoever. This result certainly posed a theoretical problem for price theory, at least of the variety which Stigler fervently supported. But, it also undermined the unyielding basis for income distribution, a position to which Stigler subscribed and which he defended dauntlessly. Since everyone was effectively rewarded for individual effort or contribution, not only did this render the market mechanism efficient but also meant that it was quite equitable. (The product was exactly exhausted by these individual shares, at least insofar as it was feasible to apply Euler's theorem to basic economic models.) Stigler perceived the underlying danger posed by these alternative theories as lacking a sufficient degree of sanctification. They all allowed market outcomes to be challenged. In which case, government

intervention in the form of redistribution would become a viable option. In this respect, Stigler explicitly rejected the ideas and formulations of Frank Knight. For his part, Knight had distinctly claimed that income distribution depended not only upon individual effort, but also on initial, and any subsequent, endowment effects (fortunes of birth), as well as simple random luck. An element such as luck would be particularly galling to Stigler as it was almost by definition unmeasurable, while being decisively the sort of effect that might appear to be remediable via government intervention.

> I shall have more to say about acceptable ethical positions shortly, but for the moment I wish only to assert that the appeal of productivity ethics for income distribution commands wide support not only from the public but also from the economists when they are watching their sentiments rather than their words. Ethical values cannot be counted by a secret ballot referendum, but the support for a productivity ethic is indeed widespread. Even Marx, like Pigou, defined surplus value as the part of a worker's product that he was not paid. The fact that more than skill and effort go into remuneration – that in Knight's example bearded women get good circus jobs simply by not shaving – is not enough to dismiss productivity ethics. (Stigler 1982a: 19)

70. Readers with more time on their hands than they know how to deploy might want to look at, "Why Economists Can't Read" (Freedman 1993). The article itself is something of a response to the "economics as rhetoric" debate of the 1980s. Economists, the argument goes, are largely incapable of decent writing because for the most part they were never taught the skill of critical reading.

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A Biographical Perspective

Georgie, We Hardly Knew Ye: Personal Reflections on George Stigler

Craig Freedman

> Today's fashionable notions of teacher-student relations present a warmer, almost cuddly picture. This was not Stigler's vision. "When I was a student," he said, "I never threw my arms around Jacob Viner. He would have killed me if I tried!" I know of no student who wanted to throw his arms around George Stigler but I am sure the consequences would have been equally dire. I was content to salute him from afar. (Sowell 1993: 793)

Clearly, the title chosen to personify this beginning section of the volume is referring to the old Irish folk song as well as having a bit of fun.[1] Whether anyone had the sheer nerve to ever refer to George Stigler as Georgie is entirely another matter. Perhaps a stretch of the imagination would allow his mother to refer to a five-year-old version as Georgie, but even that would appear doubtful.

George Stigler came of a generation that weathered the Great Depression and World War II. The movie stars of the era, like John Wayne or Gary Cooper, tended towards the 'strong, silent type' in their

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movie roles. Hearts were never worn easily on sleeves. These men and women became self-labelled as 'The Greatest Generation' in Brokaw's (1998) volume. A generation then not overly given to navel-gazing but seemingly focused more on accomplishments, on getting the job down without whining.[2] No surprise then that the degree of self-consciousness could be startling low, especially among males of this cohort. They tended not to poke around the entrails of their own ids and egos. Consequently, not only does this mindset raise barriers to fully understanding an economist like George Stigler, but also raises the possibility that such an individual could be highly ideological in his work without being sufficiently conscious of the fact. Assumed toughness of this particular flavour would make George Stigler and his point-men (Milton Friedman and Aaron Director) fit to conduct a successful counter-revolution against the prevailing post-war, mainstream thinking in economics. It would not however provide a landscape conducive to peering under the various facades and poses adopted by someone like George Stigler, who seemed to use a sarcastic swagger to protect a rather shy and sensitive inner self.

> *I know that Claire Friedland wrote that she suspected that it hid a shyness, his always responding with a wisecrack.*
> Rose Friedman: That's right. She's absolutely right there.
> Milton Friedman: Absolutely.
> *He was sensitive and shy?*
> Milton Friedman: No question.
> Rose Friedman: George was a very sensitive person. Though most people didn't appreciate that aspect. They thought of him as being …
> Milton Friedman: Tough.
> Rose Friedman: Right. And he really wasn't (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997).

However, before the various contributors to this volume discuss George Stigler in strictly, or at least to a great extent only from, an academic perspective, some purpose might be served in having the reader gain at least a casual acquaintance with the man himself. As warned, Stigler revealed very little of his inner self, preferring to project a hard-boiled persona, seemingly surviving by trading insults and assuming a somewhat world weary, sardonic stance. He could pull this off because over the years he honed for himself a deadly and razor-sharp wit. Of course, to claim that he was funny for an economist sounds like a classic backhanded compliment, like saying someone is swift of foot considering he is one-legged. But in Stigler's case, no qualifiers are needed. He was simply a quick witted, very funny individual.

> He was one of the truly funniest men I've ever known. He was truly funny. He said remarkable things in a conversation. He would say things in a way that would make you say afterwards, 'How clever'. He was always very sarcastic, very ironic and all that. But, immensely charming, I think. (Conversation with Mark Blaug, April 1998)

Take as given then George Stigler's basic impenetrability, an impenetrability carefully cultivated. Even his own 1988 autobiography focuses almost entirely on academic matters rather than on his personal life.[3] As a result, a mild compulsion exists to shed at least some minimum light on the person himself. Unfortunately, finding appropriate people to write personal reminiscences proved difficult and ultimately impossible. Either those who knew him best were dead, or they preferred not to delve into that often ambiguous and at times downright treacherous territory encompassed by memory. In lieu of this first best offering, this volume has had to settle for the next best possibility, but one that still is able to present quite a few useful insights.

Included in this section then are five conversations with individuals who knew George Stigler quite well in a variety of contrasting roles. I refer to them as conversations since they lacked the formal structure of an interview. The strategy behind these efforts was the view that a relaxed and seemingly casual conversation would be more likely to yield interesting and original insights and ideas. The approach offered more risks and at times self-destructed when an appropriate rapport failed to blossom. However, those that worked revealed quite a bit about Stigler as an individual (as well as providing insights on his work) with the bonus of opening a window on the speaker as well. All but one of the following conversations were conducted in October of 1997. Included among these meanderings is one with a Nobel Prize winner (Roland Coase) as well as a lengthy chat with a former student (Mark Blaug). Another is voiced by a key co-author (James Kindahl), a colleague (Sherwin Rosen) and a family member (his son Stephen Stigler). These are undoubtedly very different voices with quite differentiated views on the man and his work. The conversations are with those who have very different political views and whose degrees of closeness range from James Kindahl (basically a one-off relationship) to Stephen Stigler (a continuing family tie). Each conversation begins with a short scene-setting note. What follows has quite naturally been edited to achieve an appropriate level of readability. Verbatim transcripts convey the charm of cold oatmeal on a frigid winter morning.

Notes

1. The correct title is actually, 'Johnny, We Hardly Knew Ye', a traditional ballad with the same tune as, 'When Johnny Comes Marching Home'. Those who yearn to sit through a rendition of the song have a number of possible choices available on the internet starting with the version sung by 'The Clancy Brothers'. The deeper reference however, is to the memoir of the same name by two of John F. Kennedy's aides, John F. Powers and Kenneth P. O'Donnell (1972). Their book served to promote the myth of an almost magical administration (Camelot) associated with the assassinated Kennedy and his shortened term in the White House.
2. One of the archetype industrialists of this era, Henry Ford II, would become best known for saying, 'Never complain; never explain', using a very Clint Eastwood style of ethos. If the phrase sounds very clichéd British, a stiff upper lipped caricature, the reason is that it is just that. Widely said to be coined by Benjamin Disraeli, probably encouraged by his continual state of bankruptcy, it was later employed by other British Prime Ministers such as Stanley Baldwin and Winston Churchill.
3. No great surprise should be attached to discovering that George Stigler strongly resisted the idea of displaying himself and his assorted private laundry to public scrutiny via a handcrafted autobiography.

Only some considerable pressure from his close friend and colleague, Aaron Director, convinced him to undertake the task. The result is an utterly readable and compelling contribution, full of a number of Stigler-like sly remarks. But it rarely crosses into the personal realm.

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The Curmudgeon as Teacher: Afternoon Coffee with Mark Blaug

Craig Freedman

The very first article that I wrote attempting to analyse and understand George Stigler's work (seeming centuries ago, back in 1993), specifically unravelled the reason behind his sharp-tongued demolition of Sweezy's kinked demand curve.[1] A pleasant surprise arrived in the form of an unanticipated response from Mark Blaug. I was quite naturally flattered that a noted and influential contributor to the study of the History of Economic Thought would take the time to display some interest in my work. As it turned out, his attention, in this case, arose quite naturally. His very first published book on David Ricardo was a distinct outgrowth of the dissertation he had completed under the occasionally watchful eyes of George Stigler while they were both at Columbia University. As it turned out, he had subsequently kept up that connection even after his degree was a long past event. Like most, if not every single individual, who had ever run across George Stigler, he had definite views about the man. However, the aspect that succeeded in ringing a distinct bell for him when reading my article was the connection that I had suggested linking ideology with economic theory.

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So it followed that when on my very first sabbatical leave in 1997, finding myself scheduled to be in London for a few days, I attempted to set up a meeting with Mark Blaug to discuss his understanding of Stigler's methods. Unfortunately, through missed connections and the standard set of mutual muddles, the meeting failed to take place. (These were the days when the internet was still something of a novelty and everyone did not have at least two mobile phones.) However, an unexpected reprieve arrived in the form of a lecture on Henry George, though normally George would be among one's most unlikely candidates to play the role of a deus-ex-machina within any conceivable scenario. Nonetheless, Professor Blaug did fly out to Australia on this occasion (April 1998) and kindly extended his limited time he had in Sydney to include a lengthy conversation over coffee.

Consequently, we did finally manage to chat over coffee during a sunny afternoon in April. It soon became clear to me why Mark Blaug had found George Stigler to be a suitable dissertation supervisor. Surviving and prospering within the domain of Professor Stigler's reach meant being able to give as good as you got. Since Mark Blaug could be as feisty and as sharp-tongued as the master of the trade himself, they got along famously despite distinctly different political leanings. Fortunately for me, during our hour and forty-five minutes together I found him to be affable and extremely informative. A distinct absence of any real disagreements between us might have played a determinative role. Though, like his dissertation advisor, Professor Blaug wasn't shy when it came to contributing his own pointed and opinionated insights. As Lucky Ned Pepper says to Mattie Ross in the book and film *True Grit*, "You do not varnish your opinions." Neither Blaug nor Stigler was ever found guilty of doing so.

The Interview

I immigrated to England at the age of thirteen with my brother, spent the war years in England, and then after the war joined my parents in America.[2] I went to high school in America, then various colleges and I finally ended up doing graduate work at Columbia. In 1950 I arrived at Columbia. After two years of courses, I got a Master's degree, then started on my Ph.D.

I was certainly very interested in the history of economic thought. I took an undergraduate course while attending Queens College, City of New York. I did a BS Econ that included a course on the history of economic thought. I immediately got interested. I was a Marxist.

Economic thought connected with my interests. When I arrived at Columbia the first thing I did was to enrol in the course offered in the history of economic thought. It was taught by Joseph Dorfman.[3]

Within two weeks (at the very beginning of the term) I realised that I had here what I now regard as the most boring lecturer I have ever heard, and I've heard a lot of boring lecturers. He read!- He read !!- his lectures, even though he'd been teaching from them for twenty years. And I, as a student, couldn't stand it. I quit.

The only other person although not teaching history of economic thought, but who had (by his publications), obviously an interest in it was George Stigler. He was teaching what is now called Business Behaviour, but was then called Industrial Organization. It was a very practical course in industrial organization. He's a major figure in that field. Anyway, he taught industrial organization and I don't know that he taught anything else.

Did he teach any history of thought?

No, not at all. He wove it into his lectures occasionally when he was teaching Industrial Organization. He talked about firm behaviour and there would be remarks about the history of the theory of the firm in the nineteenth century, but no, not very much. But he had already published by 1952 a number of articles on the history of economic thought.[4] And, he had already revealed the kind of attitude, which I subsequently realized influenced me enormously. It was a methodological position, which, only when Milton Friedman published his famous (methodology of positive economics) article in 1954 did I then realise he was using the type of argument that sounded exactly like the kind of things Stigler would drop in his articles.[5] It was a kind of (what shall I call it) a poor man's Popperism. I mean it is basically Karl Popper's falsification with a tremendous emphasis on prediction, etc.[6] And I later realised, discovered this because I asked him, that he and Milton

Friedman talked about all these things. Milton however just ran away with it. George Stigler always slightly resented the fact that the entire world learned all this stuff from Milton Friedman, when in fact, if you look at the order of precedence, George Stigler was slightly ahead in this sort of attitude to the testing of hypotheses.

Yes.

It comes out in his articles. By 1952 I had already picked it up. So long before I actually read Karl Popper I sort of knew it all. I certainly knew the essence of it. When I first read Popper it was like, 'Hey, so this is where it comes from.' It didn't come as a surprise. In any case, I was immediately and mainly attracted to George Stigler's lecturing style. I don't think he was a particularly good lecturer. But the lectures were like a lot of his articles; lots of sarcastic jokes, very arrogant, quite cynical. This was much more the kind of lectures that I like. So, I attended the lectures very religiously. As a result, I decided I wanted to do a Ph.D. in this area. Eventually my dissertation became my first book on Ricardian economics, on what happened to the Ricardian system after Ricardo's death.[7]

Yes.

I asked Stigler if he would supervise me, and of course he would. He agreed to be my supervisor and formed a committee. I asked him not to have Joseph Dorfman on committee because I couldn't stand him. This though had unfortunate consequences because when I finally appeared at my doctoral oral to defend my thesis (the doctoral oral at Columbia was open to any member of staff), Joseph Dorfman came. It was immediately obvious that he had never forgotten that I had failed to show up after the first two weeks of his course. He asked some really nasty questions. He asked such nasty questions that I frankly lost my temper. I forgot myself. I forgot that I was being examined. I just really got pissed off.

I always will remember that at some point, while I was remonstrating with Joseph Dorfman, George Stigler grabbed my arm under the table to restrain me. Afterwards, he said to me 'Don't forget, if Dorfman had wanted to, he could just have said later on that under no circumstances should this person be allowed to pass. He still could make a lot of trouble for you. So I just wanted to hold you back, to keep you polite.'

I always thought that was funny. George Stigler telling me to be polite, when he himself was never polite.

Never.

Well he was funny like that, but let me just continue telling you about the relationship. I went to England to do my work on the thesis. But before that happened, I started teaching at Queen's College in 1952 because I had received my undergraduate degree from there. The faculty knew me and the chairman of the department offered me a position. Arthur Goyer, a now almost unknown, but at the time quite well-known sort of Keynesian economist died unexpectedly. So in the middle of the semester the department needed someone to teach all his courses. So I started to teach all of Arthur Goyer's courses. Micro Price Theory, Marketing (which I had never studied), and Consumer Economics (which I had never studied). My first teaching position involved keeping one chapter ahead of the students in the textbook. I was teaching three courses, two of which I had absolutely no idea about and no interest in. Anyway, 1952 is the period of McCarthy. This is not irrelevant in relation to George Stigler.

No.

There was an enquiry into the New York City College system. The McCarthy committee came to New York, interrogated three well-known, left-wing intellectuals (academics) one of whom was a women who taught at Queens College, a labour economist called Vera Schlockman, who had been one of my undergraduate teachers. She was a wonderful teacher, but she was very active in the left-wing teachers union. She and all the others were, of course, asked after some discussion 'Are you now or have you ever been ….'[8]

Yes.

They all refused on the grounds of the 1st and 5th amendments and although they were all full and tenured professors, they were all fired.[9] The students at Queens College (she was very popular) decided to draw up a petition to the President of Queens College, asking him to re-instate her. In order for the students to submit a petition to the President, it had to be counter-signed by two members of the faculty. To give you a feel for the period, the McCarthy atmosphere of 1952 meant that although the students went around the entire Economics Department, consisting of thirty-five people, all of whom were her colleagues, not one person would counter-sign. After they went to the Professors, they went to Associate Professors, then they went to the Assistant Professors and in despair they finally came to the very junior members. I was a tutor, that was my official title. I was only a tutor, but I just felt I couldn't possibly refuse to sign because I really did have a high regard for her. She had never, despite her very left-wing values, she never had indoctrinated her students. She was a wonderful teacher, so I signed it. Literally three hours later I had a personally delivered letter from the President of the university stating, 'You are suspended from teaching as of tomorrow morning. If you refuse, you'll be black listed. I advise you therefore, just quietly to resign.' Well, I asked other people and so on and they said, 'Forget it man. You can't fight. You're not big enough. Resign.' So I resigned.

Then another wonderful thing happened to me, because the next morning I had a telephone call from someone at the Social Science Research Council. He said, 'You have just been given a scholarship to write your PhD thesis. You have been highly recommended to us.' And I said, 'But wait a minute, I haven't even applied.' And he said, 'Don't worry. We'll send you the form today. Fill it out, but it's just a formality, you'll get your money.' I've never found out who it was. I then rang up George Stigler, but he knew already. I knew that we were politically about 180° apart. But he said, 'That's great. Go and do your thesis.' Off I went to England, working in the British Museum reading room which is now defunct.

Yes.

It was the best year of my life. I wrote my Ph.D. thesis in 14 months and all the time (starting within 4–5 months) I would send chapters to George Stigler. He supervised me entirely, in effect, by correspondence. Because by the time I came back to America, a year and a half later, I had finished. He sent me long, detailed comments. They were really vicious. They were just what I needed. I wouldn't have respected you know, 'very nice'. They were very cutting and they made me improve the thesis immensely.

Well, I came back to America, finished the thesis, got my Ph.D., got my first teaching job at Yale. He gave me then very good advice, which

I still give to my Ph.D. students. 'Put your thesis away for a year. Don't look at it. Forget all about it. A year from now, rewrite it as a book.' That's what I did. He gave me then more comments, including the very helpful comments which I would realise were absolutely true. I hadn't really explained what I had set out to do. It had been my objective when I wrote this thesis to decisively explain both the success of the Ricardian system in its day (1820–1875) and its subsequent demise. I fought very hard with 'What is the stock explanation. Do you really ever explain anything and so on?' I realised this shortcoming when I re-wrote the thesis into a book. Although I'd covered it up, I'd never really explained this central issue. At a deep level one wonders whether one can ever explain anything historically. Anyway he said to me, something like this, 'It's a very good book. But then again you haven't really explained your theses.' I realised then and there that what he said was absolutely true.

After that, our relationship was, well I didn't see him all that much in America, but in '62 I got a Guggenheim to go to Europe. He was one of my referees. He obviously gave me a very good reference, because I got the Guggenheim immediately. And after a year spent in Paris, working on the English/French cotton industry, which is what I was working on at the time, I decided I didn't want to go back to America. I got a job at the University of London. After that, whenever he would come to England, every two or three years, he would always let me know and we would always have drinks. We would have lunch. We always kept in touch, despite the fact that he got more and more right-wing in one sense. But still, I read everything he wrote. I loved the way he wrote. He had an enormous influence on my style of writing, my style of thinking. He was one of the truly funniest men I've ever known. He was truly funny. He said remarkable things in a conversation. He would say things in a way that would make you say afterwards, 'How clever'. He was always very sarcastic, very ironic and all that. But, immensely charming, I think. But now, we come to the negative part.

I gradually realised, I don't know when I realised, that he was one of the most fascinating examples, which is why I'm interested in your essays, of how economists act even though they say that economics ought to be value-free (and it is sometimes value free). I find it extremely difficult to resist reading a lot of economic theory, politically.

Now, you can see the way George Stigler had a wonderful nose for attacks on neo-classical economics, let's call it, which were dangerously capable of undermining the very foundations of orthodox economics. It's no accident that he went for Gardiner Means' *Administrative Prices* Harvey Leibenstein's *X-Efficiency*, Galbraith's *Affluent Society* and his other things, the *Kinked Oligopoly Curve* of Sweezy.[10] These are all ideas that are very, very dangerous. They are subversive even of orthodoxy.

Absolutely.

It's those ideas which roused his critical fire. Obviously he thought his views about the markets and so on were identical to Milton Friedman's but much more carefully disguised. He never, unlike Milton, came out with them openly, or wrote about them, as Milton did, in *Capitalism and Freedom*, where Milton sort of let go.[11]

You know, you could read right through all of George Stigler's stuff, even on industrial organisation, and his implicit endorsement of markets, and the condemnation of all government intervention, which of course eventually inspired his work on *The Theory of Regulation*.[12] This proved to be an example where ideology is productive. The trouble with ideology is that it can blind you, but it can also sometimes create a spurt. Who would have thought up *The Theory of Regulation*, unless you were already inclined to regard all government action with deep suspicion, always inclined to believe it does more harm than good.

Similarly ideological is the way he lit on Ron Coase and read the Coase theorem incorrectly, much to Coase's own amazement.[13] Coase never realised there was a theorem there. That's all a wonderful example of ideologically inspired criticism and also a perception of the subtle weakness of economics. This, from an economist who otherwise would, of course, have denied that ideology had any role to play in advancing the role of economics.[14]

Absolutely.

This is why I think he is a wonderful subject for what you are doing. These case studies, these are case studies in how political preconceptions colour, and often direct, one's critical conceptions in economics. I mean I notice myself in that description. I guess there are certain ideas in economics which I may in the end have to swallow, but I swallow them reluctantly. Other ideas I swallow eagerly, because they suit my political belief system. That's why, when I first read your first Stigler article, I was immediately intrigued by it.

He has to a considerable extent, gotten away with murder. Because, I'd say, unlike Milton Friedman, you have to be really very aware when you read George Stigler of these preconceptions. You know, a lot of people that read George Stigler are quite surprised when you tell them how, of course, very pro markets he was. OK, they realise that he was at Chicago and that sort of thing. But he is wonderful in disguising himself, with his wonderful, his very funny, ironic, cynical stance.

Yes.

I remember when he was given the Nobel Prize. The Nobel Prize Committee always gives a brief statement on why they've awarded it. (His work on regulation, the work in industrial organization, and also they mentioned one or two other things.) I sent him a card and congratulated him on his Nobel Prize. He wrote me this very funny card back again. 'They didn't even mention my work in History of Economic Thought.' He knew I would appreciate that.

Yes.

Okay, one more little story.

Please.

I spent six months in Chicago in 1960. Sorry, not 1960, 1965.I was teaching for Mary Jean Bowman. She taught the economics of education. This was a subject I'd gone into in the '60s. She got a sabbatical and they needed someone to teach a course that she usually taught, a Masters course, on the economics of education. I went there for six months to teach the course. At that time I met Milton Friedman and the whole department. Virtually the first day I arrived, George Stigler took me to the Faculty Club for lunch. And he said, 'I know you'll want to meet Frank Knight. Don't argue with him.' When I published *Ricardian Economics*, amazingly enough, a number of eminent economists reviewed the book. I had a review from Hicks and I had a review from Frank Knight. I regarded Frank Knight with great admiration, but as being a very enigmatic figure. Of course I think George Stigler's thesis, his *History of Production, Distribution Theory*, is a great history of thought thesis.[15] It was written under the supervision of Frank Knight. The last chapter is very revealing of George Stigler, but more of that in a minute. I knew that like most people he was in great awe of his Ph.D. supervisor. He says, 'OK. Come and meet Frank Knight, he wants to meet you.' And we did meet Frank Knight. He introduced me to Frank Knight, who by then was seventy-eight, or in his eighties. He was an old man.

Yes.

We chatted. We said this. We said that. I can't remember exactly what any more. But what struck me about the lunch was that, in some ways, whenever I met George Stigler I regressed to infantilism. It's a function of your fear of your Ph.D. supervisor. 'Got to be on my best behaviour. Hope he approves of what I'm doing. Hope he feels that I'm doing okay.' He was exactly the same with Frank Knight. He truly treated Frank Knight as if he was his father. I'd never seen him do this to any other person, this total deference.

This despite the fact that the more you really know about Frank Knight, the more you realise that actually socially and politically he must have made George Stigler very uncomfortable. You couldn't categorise Frank Knight as left-wing/right-wing. He was more complicated. But, he had huge doubts about the whole of economics, orthodox economics. He changed very slowly. By the 1940s and 1950s he no longer believed that economics really had anything to say about the important problems of life. And that must have annoyed Stigler, irritated him, because you know, he really did believe economics had answers to everything. So, I was very touched. I found the whole meeting between the two of them very poignant. To see the man that I was in awe of in front of the man that he was in awe of. I've never forgotten it.

No.

It made a huge impression on me. I know it is funny, but when Frank Knight reviewed my book, he said of the first chapter, the chapter where I explained what Ricardo was all about (this is Ricardo written before Sraffa had ever published). I had worked very hard on that chapter. Frank Knight says in his review 'very good book.' Mmmm. Sort of. And then at the end he said, 'But the first chapter on Ricardo, I don't understand what Blaug says, maybe somebody else will or some sentence like that.' Oops! That was really funny.

I've been told that Frank Knight did not think George Stigler was one of his best students. He had great doubts about George Stigler himself.

I never heard that one.

He was, of course, one of Frank Knight's few students.

Yes. He was impossible to deal with.

Impossible.

In the same way that, at Columbia, I was the only student that George Stigler ever had. This is an important thing to say. Basically the reason was that he mellowed as he got older. But even when he was older, he was extremely bitchy. And he slaughtered his students. Right at the beginning of our conversation together, I realised (I don't know whether I grasped this consciously) that you always had to give back as good as he gave. He'd say, 'Oooh, I think, you'd better watch out for this chapter, you know you didn't take this into account' I'd say, 'Yes I did.' I think I argued with him. And the moment I did that, our relationship was brilliant. No problem dealing with those students who stood up for themselves. But if a student was crushable, he crushed him. Couldn't resist doing it.[16] And he was a bully, you know. You had to stand up to him. I never had any psychological difficulty, but people, other people would call me and say to me 'I don't know how you can stand him as a supervisor, he's terrible.' But, I never had any problems. When he was 60, no, this was his 70th birthday, Chicago threw a big, sort of party for him.[17] He asked me to come, but I didn't go. And Gary Becker wrote to me and said, 'You are one of his few students.' I thought that probably made sense. Even at Chicago he had problems with students.

He had almost none.

You had to really stand up to him.

Yet, there was a very strange, but deep, friendship between him and Robert Solow. And I think that was because they were very much on that same tough level. So they could just rib each other back and forth.

Yes. He played tennis every day, or every other day with Milton Friedman. I remember an amusing conversation I had with him at one time. Milton Friedman was making his *Free to Choose* television program. George Stigler had advised him not to do it.[18] 'Don't do it. It's only cheap publicity and really a television program can't express ideas properly.' And he said it again, when it was very successful. I had the feeling that George Stigler was slightly jealous.

They made a lovely, a most improbable pair. I mean, despite the fact that they had so much in common politically. There was a great harmony intellectually. But they had very different styles. Milton was unlike George Stigler. A non-stop talker, and maybe they never had any arguments, in which case there is no problem about getting along with Milton Friedman. If you argue with him, well you have to be able to do so without getting exhausted. Like the year that I spent in Chicago, less than a year, but the year that I spent in Chicago, Milton Friedman (this is about Milton Friedman not George Stigler) was changing house.

Yes.

They were moving into a new house. So, he and Rose and David moved into a hotel. I was in the hotel because it was very difficult to find anything to rent in Chicago for only six months. They had an apartment on the floor above me. And naturally they would say hello, stop down to see me. Given the era, not surprisingly, we immediately got into an argument about Vietnam.

Right.

I was totally opposed to American involvement. Milton was a firm adherent of the bombing of Hanoi. We would have these incredible arguments. Now, I had read quite a lot about Vietnam. I don't think Milton had read anything. I was much better informed. Nevertheless, we would start these arguments at 9:00 o'clock and by 2:00 o'clock in the morning I would say, 'Milton, I just can't go on. I'm tired. I just can't take any more.' And he would say, 'Let me just give you one more argument.' He was patiently prepared to spend eight or ten hours trying to persuade me of the error of my ways. He knew nothing at all about Vietnam, or Communism. This was outside his knowledge.

Yes.

He was always patient, always polite, never got short tempered like I do in an argument, never got nasty. But he was a horrible person to argue with, just a nightmare. My idea of a nightmare is to stand on a stage and debate with him in front of the public. I watched him debating at Cambridge with Joan Robinson on flexible exchange rates.[19] Unbelievable! I mean, Joan Robinson was one of the world's most aggressive, hostile, debaters. He wiped her analytically, he wiped her rhetorically, he had the entire audience eating out of his hand, after an hour, an hour and a half. An amazing, amazing guy. But a madman, a madman. One of the few people I could strangle with my bare hands. I feel I could actually do it.

Now you tell me a bit about your next article. The next case study.

Oh, well, I still have a few to do. For example, monopolist competition and Chamberlin.

Oh, yes. Quite, oh quite.

That was a key moment.

Characteristic.

Very much so.

Of course.

And if you look at his '49 articles, part of his Five Lectures.

The *Five Lectures*, yes.

Some of the holes he actually poked in Chamberlin's theory, if you go back to an earlier QJE article, Chamberlin had already tried to address some of those concerns.[20] *So, it's very much the same thing. He just focuses in on the points that serves his own purposes, shifts the argument, and then redefines the argument. Then he ploughs ahead, completely devastating his version of Chamberlin's basic theory. Of course, Chamberlin's article was very subversive as opposed to Joan Robinson, which was more or less an extension of Neo-Classicism.*

Absolutely.

The other issue I still have to deal with is Gardiner Means and administrative prices. It was one of the reasons that I talked to one of my old teachers, Jim Kindahl, who worked with him on that particular book he put out on pricing theory. You know of his long running battle with Gardiner Means. It was a fight to the death.

Yes.

If I can complete those case studies, along with the other four, the Liebenstein, which is the recent paper, the Berle/Means, the kinked demand curve, and Galbraith's countervailing power, I think it makes a very solid case of what he was up to.[21] *What I found interesting is that since I wrote the first paper, I have come to understand George Stigler's underlying complexities more. Because I think, a lot of it was very unconscious with George*

Stigler. It's just the way he saw the world. Initially I thought, 'Was he doing this deliberately, consciously, treading a very fine line.' If you asked me today, I would say that I think that his world view was organising his data for him in a lot of ways.

Well, how many people are fully aware of all their preconceptions, how they arrive at their particular attitudes? It is almost too much to imagine. You know, the trouble with conspiracy is that it almost too difficult to maintain a conspiracy. At least for the persons involved. You really have to believe. You have to be inspired, which he clearly was. It's too much to ask if he fully understood the sources of the whole of his inspiration. It is much easier for us, since we are looking back on his lifetime of work. I think when he first went for Chamberlin, in that '49 lecture; it was very difficult to understand what bugged him about Chamberlin. But now we see exactly what it was. I knew Chris Archibald very well, spent a lot of time in his company. We discussed the famous debate Archibald had with Chicago, the article by Archibald and the replies from Stigler and Friedman.[22]

Yes.

Stigler and Friedman managed to win that argument. They shouldn't have won it, but they managed to win it. They got the better of Archibald rhetorically, even though Archibald really had the much stronger argument. It had to do with whether the qualitative calculus gives you enough information to predict prices. When you look at the last part of these documents where you discuss Euler's theorem, you come to the conclusion that the only sustainable assumption (about costs and cost curves in economics) is constant costs and constant returns to scale.[23] If you have increasing returns to scale, well that's very subversive. Marshall realised the problem of returns to scale. It screws everything up.

Yes it does.

Increasing returns to scale requires a dynamic theory of why firms get into trouble when they get bigger and bigger and then get more inefficient.

Yes.

Economics has tended to concentrate on constant costs. Now this is very ironic. Because Stigler's thesis was written in 1938 and then published in, I think, 1947.[24] But Sraffa in the nineteen twenties had already investigated production functions.[25] But Stigler was just too fatally attracted by constant returns and constant costs functions.

Yes.

The same thing holds with Marxism, naturally. I'm really fascinated by this. I think it's funny how the orthodox theory of the firm and Marx's value theory; subjective value theory and the labour theory of value come together. The conclusions of both really have to assume constant returns to scale. The production function becomes a tangent automatically in the case of constant costs. Constant returns to scale in the production function, if there is anything else, you get into analytical difficulties. What I want to emphasize is that there is a strange marriage of opposites.

Expediency.

Yes, expediency. If you throw away standard price theory, then of course you can have economies of scale. But it's just so obvious that economies of scale are just one of the most pervasive phenomena of a capitalist economy. Every theory in which you have to assume that away by assumption cannot generally be a satisfactory way of looking at price determination.

Yes.

It's just that the world is full of these large firms. Most have grown large, at least half for psychological reasons and so forth. How amazing that George Stigler who was always emphasizing the importance of empirical observation relies on such an assumption.

Yes.

You've got this theory that has got to take account of all the leading facts the world presents; product differentiation, economies of scale, advertising. These are phenomena so pervasive in modern capitalism that any theory that precludes them really cannot be very relevant. Maybe we can construct theorems, but we can't really shed much light on how the economy actually works.

He never really faced up to all that. I'd be surprised if he did. In the end, of course, well, he would not have said 'Oh you can't.' I'm sure he would have said, 'The economy is so riddled by the regulation of firms and so on, you can't take a phenomenon like large firms and infer anything from that fact, because they may have grown large to inhibit political interference. It is a reaction against the pressure of political power, rent receiving, da da da.' I imagine that is how he would have answered. He came increasingly to feel that regulation was the key to a lot of economic phenomena.

Yes.

Now, I'd probably have to say that he was probably half right. We tend, the way that we teach economics, to minimise the incredible influence that government has. There are simply myriads of regulatory conventions operating in an economy.

Absolutely.

Everything you look at, including the price of coffee, is regulated. And then, you know, we talk constantly about the sort of price mechanism that very rarely gets a chance to work. Well, have you had any reactions from any of your articles?

I don't know whether those immediately concerned saw the articles or not. I've interviewed about fourteen, fifteen people, all of whom were very close to George Stigler.

And did you interview Tom Sowell?

No. He was one of Stigler's very few other students.

Yes, he was.

George Stigler actually didn't think that much of him.

That's right, yes.

George Stigler was very blunt.

Yes, he always told me that about Tom.

I've read his correspondence and the recommendations that he wrote concerning this person and that. He was very blunt about people.

Yes.

If he thinks they're good, he's very, very strong in his praise. If he doesn't, if they don't come up to his standard of what an economist should be, he is scathing.

Yes, I know.

As you would know.

It would be interesting to hear what Tom Sowell would have to say. I think you should try to interview him. He was much closer politically to George than I was. He's travelled a great distance from where he started. He is currently out of formal economics. He's into the economics of blacks, minorities, the immigration of Asians and Africans, all very interesting stuff.

When the *JPE* did their little commemorative issue, he contributed a short article of George Stigler as a teacher. He was very, very laudatory in that article.[26]

Which always struck me as strange, because …

The article was false in that sense, in his decision to write a nice tribute.

Because when I actually dug around it didn't gibe, with what seemed to be the case.

No. Rosenberg's article in that issue was much better.[27]

Yes.

Have you interviewed him?

I wasn't able to interview him. I interviewed Demsetz, who also wrote in that issue. He wrote a pretty perceptive article, I thought. And I've interviewed a lot of others. I've interviewed Milton Friedman, Rose Friedman and Aaron Director; Gary Becker and Ronald Coase; Sam Peltzman who was one of his few students, his son, Stephen.

Yes.

On the other side, I interviewed Samuelson, Solow and Galbraith. I also spoke to Jim Kindahl and Clare Friedland who worked with him for all those years. She was very delightful.

I never met her, funnily enough.

She's charming. She was also very close to him and respected him enormously, though she differed from him politically. She said, 'George was so conservative that in the '60s he was the only man still wearing a homburg hat. That was exactly it. That was how conservative this man was. That was just George.' But the interviews were very interesting. Most people were quite revealing. It's also clear that George Stigler listened to very few people, I mean, listened seriously to them. He was open to very few people. I was told in later years there was Gary Becker. Earlier on of course there was Milton Friedman and Aaron Director.

Absolutely.

But outside of those very few numbers, he didn't really want to talk deeply, you know, with anyone else. Ronald Coase said his immediate *reaction was to push you away with a barb or a joke. He didn't want to talk to you.*

Yes that is interesting. But I also think the whole story about the Coase Theorem is most extraordinary. This guy writes his article on *The Theory of Social Cost*.[28] I can still remember when I first read it I thought, 'weird'. It should have been in a law journal, not an economics journal. I couldn't read the bloody thing. It was so long. It was fifty-five pages.[29]

I couldn't have passed an exam about it after reading it to the end, not for love nor money. I had no idea what the hell he was on about. Then he goes off to America,[30] gives that session in Chicago, and is told there is a theory in what he has written.[31] It's really funny. He himself describes, you know, his surprise. 'What is this Coase's Theorem?' Then, once he understood it, he says that it is false, that it's not a theory. This is an incredible story of intellectual development, of discovery. A man is shown what he believes. Only then is he fully aware of what it is. After he is shown what he believes, he is kind of horrified by it. To think that transaction costs could ever be zero. It's ridiculous. But out of it all has come the idea of transaction costs.

Yes. I mean it, the 60s article, is simply an extension of his 1937 'Theory of the Firm' article on transaction costs.

No, it isn't 'simply'.

Not simply. But …

Yes, you can see the similarities now. But when I read the 1960 article at that time, I didn't see any connection to *The Theory of the Firm* article. Though I'm not sure I had read the 1937 article then. Because the 1960 article is really about government failure. It's not really that much about transaction costs. In retrospect you can read that into it. So, it was immensely clever of George Stigler at the time to create the Coase theorem, a very strong version of Coase's idea. He turned a long paper into a memorable sentence or two.

Very clever. But again, he never would have thought of it if he wasn't already inclined to think in that way. For example, that the way in which property rights are assigned can determine how the market works. It's something you wouldn't think of unless you were very conservative to begin with and very pro-market. It is a wonderful example of how ideology can inspire. I mean another example of that which is to me even more striking comes out of his hatred for Gardiner Means, 'Administered prices'. He started doing all this work for the National Bureau on price. He collected, with his co-author whose name I now forget, price information.[32] They got the information out of catalogues for commodity goods.

Yes.

They looked at all the prices. It led to the 1962, *Economics of Information*, article.[33] He realised that for the same goddam commodity you can't find a uniform price. Even in catalogues, never mind in shops.

That's because there are an infinite number of variations of what seems to be a simple commodity. My favourite example that Stigler mentions is nail producers. If you actually go and look at the price of nails and you ask for the price catalogue of a famous nail producer (whose name I don't remember any more) the catalogue is this thick, 700 pages with thousands and thousands of different kinds of nails.

If you look at their prices, then as a statement, 'What is the price of nails?' has absolutely no meaning. That's what leads him to the idea, 'Ah, there is no uniform price in the market because of information, you have to search for it.' You have Jevon's famous definition of a market (same quality sold for the same price) which is virtually impossible to find in a concrete example. That is what led Stigler to one of his most important articles, the article on information. It was a real path-breaker. And it was one of the articles they mentioned in the Nobel Prize award. They were quite right. It is an incredibly important article. It led to all the subsequent work on information.

I've asked people, because he was so concerned with empirical evidence, like the way in his '64 Presidential Address where he stakes out his flag, explicitly says 'This is it.'[34] *I've often asked people, 'What empirical evidence would cause George Stigler to change his mind?' And, when I investigated his career, it seems that he will change his mind. But the cases where he changed his mind was if something met with his own deep pre-conceptions. We talked about the Coase theorem, he was very keen for instance on Demsetz's article on franchising, why monopolies don't matter.*[35] *This would be conducive to the dominance of market prices. The old theories of monopoly, were all sort of ad hoc and over there in a corner. They didn't fit in* *with price theory. This was a way to unify economics. So, I would ask you, what would cause George Stigler to say, markets don't work. What empirical evidence?*

The answer to that of course is quite a tall order. I can't say what would make someone change their mind. But whatever you believe, it depends on the strength of your convictions.

These interviews, while I'm sure you would never want to publish them, have you ever thought of reproducing them for private circulation, for interested readers? Or have the people you interviewed made you swear that you would never show them to anybody else?

The promise was, that after I clean up the transcripts I would send it to them. In that way they can x-out anything they don't want to be seen. Because there are some passages that I'm sure at least some of them would rather keep confidential. Then, I may, in fact, attempt to publish them, because I think some of the interviews are extremely interesting. Just two of them that I have cleaned up, ones by Gary Becker and Sam Peltzman are particularly intriguing, because they are very clear and upfront about exactly what they think. I would be happy to make them available, but I'm still waiting to read them through. The transcription process has gone very slowly. But I think they may be of some interest to a wider audience as well.

If you ever did, I would certainly love to be a recipient of any of them, either published or unpublished.

I'd be happy to make them available.

I'd love to read them. I imagine that they are fascinating.

Yes.

I can just imagine Gary Becker.

Certainly the interview with Gary Becker is worth reading. Then there is a sort of counterpoint interview with Robert Solow, which makes a very good contrast to Gary Becker. They approached him in very different ways. Samuelson was quite amazing. His memory for details from the 30s and 40s and 50s about exactly what happened, who did what to whom. I was astounded. You know, he would say, 'Well, you have to understand George did this paper in 1944 when he was with the NBER and I don't know if you would remember the other economist who was working there at the same time. He did this, and so on.' I was fascinated. He just went on for a long, long time.

A pity that he has never written an autobiography.

It would be fascinating. Hearing him talk, he is still very cogent.

Oh, yes. I've interviewed him. I did a video on Keynes. I interviewed six famous modern economists for the video. You know, what was Keynes thinking, what was the legacy of Keynes, and all that. One of the people I interviewed was Samuelson, who was incredibly eloquent. I interviewed Milton Freedman who was also very eloquent, but who pissed me off. Oh, I think it is amazing. It probably happens in other subjects, but we have in economics (still living) really some of the leaders of our profession.

At least the leaders in my lifetime. What extraordinary writers and talkers. I mean, the prolificness of Paul Samuelson. Look at the *Collected Scientific Papers*. Average it out. He starts publishing in 1937 or 1938. In 60 years he produced, on average, a paper every five weeks for 60 years. And every fifth or tenth paper was a classic which everybody still reads today. And every paper of those almost 500 papers I would have been very happy to publish myself. I mean the quality, there are a few papers which are a bit repetitive, but there are amazingly few. I once sat down and went right through all five volumes. It was stupendous.

Incredible.

You feel crushed. I really think, Jesus, how can I possibly ever come anywhere near to that? I mean any time you think that you are some kind of economist, I would advise you to sit down, read *The Collected Scientific Papers* of Paul Samuelson and put your own accomplishments in perspective.[36] There is just no comparison. It's just incredible. I say this as someone who personally believes that he had a bad influence on modern economics. I mean precisely this; I think that the excessive emphasis which he placed on mathematical formulations is where a lot of modern economics has gone wrong. So I don't think that his influence has been necessarily healthy, but I'm not discussing that particularly aspect.

I'm just discussing his extraordinary productivity. This was combined with a fantastic loquaciousness. If you ask any of his students at MIT in the 60s and 70s, they will all tell you that he was a great teacher. His door was always open. You could go and see him at any time. He never closed his office door. He had no office hours, because he went from morning to night. How the hell did he ever write so much under such conditions? He was always opening the door.

And he had so many kids.[37]

Yes, he had a number of kids. But what an attitude. Simply fantastic.

Speaking of Samuelson, I know we've talked about Ricardo. Samuelson in his interview made a point of stating that he had written this article on Ricardo. He thought that he had made a definitive reply to George Stigler's enunciated position.[38] *I think it was published in the JPE, so George Stigler must have read it. And I think that he was still irked that George Stigler wouldn't comment on it. Would never respond to it at all.*

Very interesting, I didn't know that. Samuelson was always very proud, quite rightly, about that article.[39] It was a long title that I can't remember. He was getting back at Sraffa because Sraffa didn't pay attention to the fact that there is demand in Ricardo. How else do you determine the total demand for the total volume of corn? The way Ricardo managed it was to simply make demand a function of population with a perfectly inelastic demand for corn. So demand is there. This was his big comeback to Sraffa. He always said to Sraffa, 'You've studied Ricardo up, down, and sideways but you've missed that. You maintain that there is no demand, but of course there is demand.' And of course he was absolutely and fundamentally correct. And it irked him. You can tell that it irked him all his life that no one ever gave credit to his idea.

George Stigler certainly didn't want to do it. Let me ask you, George Stigler, if you read his articles, he has a very strange relationship to history of thought. For him it really isn't important, at least, not essential. When it's taken off as a requirement for a Chicago Ph.D., he's quite happy to go along with that move. He says, in effect, 'a modern day economist doesn't need it.' Even more interesting, there is one article, I think it might be 'Does Economics Have a Useful Past?'[40] *This is where he says, it actually is counter-productive because it makes graduate students see too many sides to issues, and that is not what we need.*

No, I don't remember him saying that.

Besides his standard reasons given, there is that sort of throw-away line. Now you can never tell when George Stigler has his tongue in his cheek, but he is saying that graduate students would get to see that these problems are *actually ambiguous. They might get too much of a feel for both sides of an issue.*

I think he had an ambiguous attitude to the history of thought and that article is very ambiguous. It produces a defence of the history of economics, indeed of the history of ideas in general which is very weak. It has a sort of, 'oh well, you do learn how to read.' That is exactly what he says, 'Ultimately you learn how to read thoroughly.' But, it is just another example of the power of ideology.

He really believed in modern orthodox price theory, markets and all that. It is perfectly true that if you believe that then (a) you don't need the history of economic thought (b) the more history of economic thought you know the less you'll be inclined to believe the latest modern economics. Therefore, scepticism in any form is to some extent a bad thing.

You know, the more doubt that you acknowledge on any subject, the more sceptical you will be that you really have the truth. The less you know, of course the less you will believe, but you will be sceptical even of your own clearly held beliefs. At some level, Stigler was very tempted by the idea that it might be better that you don't have any doubts that you really believe in things. He is tempted by the allure of the true believer. Of course, he is much too sophisticated to come right out with it. You need to remember all he had invested, an enormous amount of intellectual capital and scholarship, in the history of economic thought.

I mean, on some deep level, you know, whatever I have tried to contribute in terms of the history of economic thought ultimately comes down to the fact that it's fun, I find it interesting. I don't know whether it is useful. I don't know whether it gives you some kind of wisdom. But it is very hard to explain what that wisdom is. It isn't cognitive knowledge. It isn't the number of things you can cite. It isn't the dates. I'm not sure that, like with knowledge of history itself, societies make better decisions, if they really do understand their own histories. I'm not sure. But, anyway, I can't imagine how people think about ideas without thinking about them historically. When I first learned the calculus, almost immediately when I could integrate and differentiate, I said to myself, 'Where the hell did this come from?' Did we always believe this? Did they believe it in the twelfth century? Did they believe it in the nineteenth century? Only when I began to read a bit of the history of mathematics could I say, 'Oh so that's it. That's how it fits into the 17th century.' I mean, I just cannot understand how people can think about things a-historically. Why should I be interested in, for example, the price elasticity of demand as an idea that dropped from the sky just like that? Your first instinct is to say, 'Who first thought of this?'

Yes.

So it's very difficult. For many years, I've been defending the history of economic thought, and beyond economics defending as well the history of ideas. As a subject of study, at a certain level, I think it is defensible.

Yes.

But, I completely agree with you that Stigler must have had an ambiguous attitude to the history of economic thought.

Yes. I think he was wrestling with it all throughout his career. Now, when George Stigler was at Columbia was Maurice Clark still there?

Oh, yes. John Maurice Clark supervised my MA thesis. I wrote my MA thesis in 1952. I later realised I had absolutely no idea of what I was going on about. That is when I really dug into price theory. When John Maurice Clark read my MA thesis, his only comment was, every time I mentioned the word 'entrepreneur', which I did quite often in my thesis, I had always left out the second 'r'. And I did. I'd always written 'en- tre- pre' I'm sorry 'entre-pe-neur', without the second 'r'. And I never make spelling mistakes. But I think it is very interesting that this was his great contribution to my thesis. He'd already written the article on workable competition and he recommended it to me. I read it.[41] It went in one ear and out the other. This was a wonderful example of how you read things when you are young.

Yes.

Now I realise, 'Hey, this is one of the great articles, on the Austrian conception of the process of competition, which he calls workable competition.'[42] But at the time, I had no idea. A quickie on John Maurice Clark which I completely forgot. When I said that Dorfman taught the history of economic thought and no-one else had, John Maurice Clark taught something like the history of economic thought. It was called 'Modern Economic Theory.' It was really sort of history of economic thought since 1870. It had a lot of stuff about institutionalism, but also a lot of stuff about the marginalists.

Anyway, it's 1952, I'm in the library, the periodical room, and I'm looking at the current issue of *Econometrica*.[43] A young man by the name of Don Patinkin has in it an article called 'The Dichotomization of the Classical Pricing Process', with all these general equilibrium equations. And I read it. The classical economists are supposed to have made this terrible mistake, relative prices versus absolute prices. I just couldn't make any sense of it. I said to a friend of mine, 'You look at this Patinkin article. Can you understand it? No. Well let's ask Clark.' So we said, 'Sir, we read this article by Dr. Patinkin in *Econometrica* and we can't understand it. Could you throw some light on it?' He said, 'I haven't looked at the article. But leave it with me and I shall look at it. I'll get back to you next Tuesday.' Next Tuesday comes round. This was one of those moments in my academic careers I've never forgotten. He said, 'Gentlemen, I've looked at this article. My maths aren't up to it. I have no more idea than you do what the hell it means. I figured out equation 1 but equation 2 stumped me.' Now this is funny because these equations were basically a set of excess demand, excess supply functions. Now we would teach this in a course on mathematical economics in the third week.

Yes.

Just shows you how things change. Anyway Clark looked at us, and then he said, 'I'm sorry. I just cannot at my age, master modern mathematics.' And I thought to myself, being an arrogant 24-year-old, 'Boy when I get older I'll never say that to my students. I'll always keep up with everything as it comes along. And I'll never …' It was one of those funny things. You know, many times when I tell my students, 'sorry, but …' I think of John Maurice Clark.

Yes.

Of course, John Maurice Clark was painfully shy. He was very difficult for students to deal with. He was so shy, so reticent, so diffident. You could never get anything out of him, whether privately or publicly. His lectures were dreadful lectures. We all knew about his work on overhead costs.[44] We knew that this guy, and his father before him were important economists, but as a teacher, hopeless. He retired a year after (this was about 1953) I'd had lectures from him. So he was around. I don't know whether Stigler was friendly with him. The only person that I ever noticed that he was friendly with on the faculty was Abram Bergson.[45] As for the rest, I really don't know what his relationship was.

I was just interested because of course, his work would be so …

Of course, absolutely in the same area.

But, in his actual written work, Stigler barely mentions Clark.

Yes, of course.

So he obviously didn't think he was important enough.

No, I mean by the 50s, John Maurice Clark was no longer taken as a serious figure, particularly by young economists. He was an old fuddy-duddy. That's what economists at that time would have said. American Institutionalism had died, and he was regarded as an American institutionalist.

Yes.

People even forgot his article of 1918 on the acceleration principle.[46] You know, he did actually make analytical contributions, which were forgotten.

Yes. I know in his autobiography, when Stigler is quickly summing up his Columbia years, Maurice Clark never gets a mention, never gets a whisper.

There was a great, great guy in that Department. Stigler would have had nothing to do with him. He was the best lecturer I had at Columbia. He was called Karl Polanyi.[47] He taught a course called General Economic History. Whooh. I mean it took you from the Babylonians to the nineteenth century in two semesters. Fantastic. I read myself sick. A terrific lecturer. It was way over the heads of all the students including me with this sort of grand sweeping generalisations about the ancient world. I mean, he could take care of a thousand years in about 2 minutes. Terrific stuff. Don't know who had hired him. This was of a completely different character from what anyone taught in the rest of the department, including even that taught by economic historians. It was an old style, Max Weberian, Continental European approach to economic history, painting with a broad brush. But it was terrific stuff.

Yes. Getting back to Stigler, you mentioned the McCarthy years. Just looking through his papers and letters, what was interesting is that George

Stigler seems to be very quiet about intellectual freedom during the McCarthy period. Yet if you look at his letters and what he writes during the Vietnam era, he is very vehement about the attacks on intellectual freedom that were coming from the Left. Did he ever see any contradiction there? His silence in the 50s and his subsequent outrage.

No comment. I have no idea. It would never have occurred to me in the 50s to suss out what he thought about McCarthy. And the trouble I have was that I don't remember that we ever explicitly discussed it. I really have absolutely no idea. But it wouldn't surprise me that he had a schizophrenic attitude to attacks on intellectual leadership depending on which direction it came. A lot of right-wing people must have found McCarthy to be vulgar, brutal but basically less of a problem than the alternative. Basically he was on the right side of the issues. Not unlike what you read that people said about Hitler in the early 30s, when he was on the rise. Yes, he's a gangster. Yes, he's a hooligan but he's anti-communist. He's going to keep back the communists and control trade unions. These are benefits. And they didn't realise that they were rearing a Frankenstein's monster who would eventually chop off their own heads. A lot of people were sympathetic, who would have been horrified if they had foreseen what would happen.

Yes.

Nevertheless they went along with it as being better than nothing. You have to stop these damn communists, etc. etc. I've met a lot of people who took a very quietist attitude.

Yes.

It didn't mean they loved him. But, as far as they were concerned, he was on the side of the angels.

Yes.

I dare say, I think that would have been Milton Friedman and George Stigler's attitude to McCarthy. One would have wished he didn't do it so loudly, he didn't do it so vulgarly, but they would have said that he was essentially right. In the same way that a lot of people said you know, you have to put up with McCarthy to keep communism in check. Stigler would have regarded McCarthyism as not being a threat.

Yes.

And again, it fooled him only because it went on and on and on. So that I think by the time that they finally got rid of McCarthy, Stigler would have said 'Thank God'. And that's often the case. It's very difficult to control such situations. You know, the forces that McCarthy let loose.

One follow up on that. Stigler's reaction to Marx, in terms of the History of Thought …

Yes.

Did he ever seriously attempt to read …

No, I don't think he ever read much of Marx.[48] He never mentioned Marx to me, and any references to Marx in my thesis he simply ignored. He was studiously uninterested in Marx. And it's funny, John Hicks was the same way. He knew a lot of history of thought. But he would say, if you brought up Marx, in conversation, I knew John Hicks quite well, he'd just sort of say, 'I've never read it. I've never read Marx.' I mean he probably read some of it, but he just made a point of saying, you know, 'I've never read Marx.'

I thought as much.

And I think Stigler's instinct is right because, once you really read Marx, you study Marx, you're never the same. He's one of those types of thinkers. He makes an indelible impression on you and it alters the way you think about things.

Yes.

He is simply, you know, one of the most powerful of intellects ever to be let loose in the social sciences. I just think he was horribly wrong. But, I'd prefer that people had never read him, at least not in the way he was read for a while.

He was a truly profound thinker. God knows what would have happened if he had full intellectual contact with other people instead of having to depend wholly upon Engels. He had almost no intellectual contact with anyone other than Engels. If that man had been in academia and had been criticized, opposed, had been attacked and had to reply to attacks, and really had proper intellectual interchange, he could have gone further. He lacked intellectual discipline.

Yes.

Even Engels never really took him on. Because Engels you know, was overawed by him. And Marx shows what happens when, inevitably, for various reasons, you give up publishing. You know, he gave up at Volume I of Capital. Nobody reviewed it except Engels of course. It got only one review in German. No one wanted to translate it. It was so disheartening, that he didn't go on. He gave up.

Yes.

He left all his notes, his ideas, at Engel's disposal.

Yes. One last thing, as you'll have to run off to lunch soon. I've asked this of a number of people. Stigler's relation to Adam Smith. This is the major figure for Stigler, his main man. Why do you think he couldn't accept Smith's Moral Sentiments as an adjunct to The Wealth of Nations, but wanted to just dismiss it?

Well, I don't think of *The Moral Sentiments* as an adjunct to *The Wealth of Nations*. I think that there was an Adam Smith problem. I think the relationship between these two books is very enigmatic. The two on the face of it are complex and it's not clear of course, what properly holds them together. Skinner, (all the Glasgow scholars), has managed to find a degree of freedom between them, to the extent of asking how come Adam Smith never referred to one book in the other book. It seemed to me when he did a third edition of *The Moral Sentiments*, two years before his death, he never mentioned *The Wealth of Nations*, nor does he ever say that the arguments in one book relates to the arguments in the other. I mean it is normal for an author to tell you how his great masterpieces fit together, particularly when they were supposed to be parts of a three piece work on jurisprudence.

So, he seems to make it all very difficult by not mentioning how it all fits together. And I think that more or less is what Stigler would have said. Not that we ever discussed it. He liked *The Wealth of Nations*. I think *The Theory of Moral Sentiments* is very hard. It is not really easy to marry up with *The Wealth of Nations* because it presupposes that trust is crucial. To put it very succinctly, orthodox price theory works best when markets, when all the traders are anonymous on opposites sides and unlimited. Once there are personal relationships, which of course there are inside any firm, and as there are between traders in any well-defined market, the situation changes. I mean, in many of the well-defined markets they don't even write invoices. They all know each other, and so trust, personal relations, and confidence is extremely important in many, many, if not in most markets.

Yes.

But, it doesn't marry very well with rational price theory in which people are self-interested and as such, have no choice. You don't even say, 'I'll deal with this guy because I know him, we've been dealing with each other for years. I believe that I can rely on him to produce the goods.' No, that is very difficult and yet it clearly plays a role.

Yes.

In the case of Adam Smith, I once said to George Stigler, that it always struck me that the beaver–deer example in Chapter 5 of Book I of *The Wealth of Nations*, demonstrating that in a rude society only labour time is essential isn't quite correct. How come the deer hunter didn't hit the beaver hunter over the head and just steal the beaver. Why go through the act of exchange? Why not join the beast of prey? The answer is, at least the standard answer is 'Oh well, because they trade repeatedly.' Yet in a rude and original society they wouldn't trade repeatedly. You'd grab the beaver and run for it. Even if you say they will trade repeatedly, I've still have to establish my reputation as a deer hunter with the beaver hunters. Reputation is needed to ensure a trust relationship. So right there, this classic parable of price theory has missing in it the role of sympathy. That simply sums up the enigmatic role between the two books.

Yes.

And so in lots of ways, George Stigler was very dishonest, I don't know if he realised the extent, in never discussing the relation between these books. He should really have discussed it. It was part of Adam Smith's thought. If you want to understand Adam Smith, you have to try and understand how it is that he left these two books floating and gave you the job of relating them and I think it is very difficult.

Yes.

And it is a wonderful introduction in economics to the whole question of 'Do we really have to take this relationship within markets and supplement it with some other work?' I say, yes, you do. As a result, it's true that there is something wrong with the way we teach price theory.

Yes.

That is not a good way to understand how the market works, how it actually functions.

Yes. It is the wrong fable.

OK, but that doesn't really answer your question, because I don't know why. But isn't it amazing how he could have done that? How could he have written so much about Adam Smith, even to the point of telling us what Adam Smith really meant, without getting into that Adam Smith problem.

*It interesting, because I asked Ronald Coase that question and Ronald Coase said, 'Well in '76 I was going to a conference in Germany on Adam Smith and I wrote a paper and it dealt with **The Moral Sentiments**, as well.'*[49] *Ronald Coase has a high opinion of **The Moral Sentiments**. 'And I said to George after a while, 'Well, what did you think about it?' And George wouldn't respond.' Finally Coase kept pushing and pushing him and George Stigler responded, 'Well, it's probably better if you had written it in German.' And then that was it. The reluctance was quite explicit, but quite typical. He just didn't want to deal with it.*

1976 was the bicentenary of the publication of *The Wealth of Nations*. At Glasgow there was a conference to which I was invited along with George Stigler. We all went down to the Royal Mall on the Castle to the cemetery where Smith is buried.

Yes.

We stood in front of that tombstone that says, 'Here lies Adam Smith, author of *The Theory of Moral Sentiments*.' It didn't mention *The Wealth of Nations*. And I remember saying, 'Jesus, these people think that the great book which Adam Smith wrote is *The Theory of Moral Sentiments* and not *The Wealth of Nations*?' I know I looked at George Stigler as I said it. That conference included a vile debate between Sam Hollander and George Stigler.[50] Sam Hollander had just published his first book on Adam Smith. Stigler really told him at this conference, 'You're full of shit.' Sam tried to stand up to him, tried to keep it civil as well, but Stigler really laid into him.

Having chatted for almost two hours, we had to end our conversation rather abruptly at this stage so that Mark Blaug could make a scheduled luncheon date.

Notes

1. Stigler's attack on Sweezy's theory of the kinked demand curve is incisive, devastating but in a number of key areas, unrelated to Sweezy's actual theory or what he states in his article. This divergence between Stigler's version of a theory and the actual stated theory is what initially intrigued me about the work of this giant of the post war era. Those interested can judge for themselves: Freedman (1995).
2. Mark Blaug was born in The Hague, Netherlands in 3 April 1927 and died in Dartmouth, United Kingdom in 18 November 2011.
3. Joseph Dorfman (1904–1991) was a Columbia University institution. He received his doctorate from Columbia in 1935 and began teaching there in 1931. In 1948 he became a full professor at Columbia where he remained as an Emeritus Professor. He was a major figure in the History of Economic Thought, especially in the US. Since 1990 a Dorfman prize has been awarded to the best dissertation focused on the History of Economic Thought, a prize clearly honouring his contributions.
4. This was obviously his dissertation completed under Frank Knight that had been published in 1941. But there were also a slew of other articles, George Stigler (1937, 1947a, b, 1950) and perhaps in particular his 1952 article on Ricardo that perhaps to some extent inspired Mark Blaug in his dissertational work.
5. The reference here is to Milton Friedman's famous article which is still a controversial mainstay of economic methodology (Friedman 1953). This was intended as a first and last word on the subject and served for some decades as the unofficial position of the Chicago School. Fifty years after, at the annual January *American Economic Association Meetings*, Professor Friedman saw no need to alter or to adapt his original thoughts. A shorthand version of this stance can be seen in one of George Stigler's *Five Lectures* presented in 1948 to the London School of Economics (Stigler 1949). Previous correspondence between the two makes the interdependency of their ideas even clearer. See Hammond and Hammond (eds.) (2006) for the actual letters.

   How influential were you in each other's thinking on this matter [methodology]?
   Milton Friedman: Very influential. I think there's no doubt that my work would have been different if I hadn't been influenced by George and George's work would have been different if he hadn't been influenced by me. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

6. Karl Popper (1902–1994) is perhaps best known for his reaction to the ideas promoted by logical positivism. He insisted on the idea of falsification. Thus you could never prove something was true. You could only empirically test a proposition to see if it was false. An inability to falsify did not imply the proposition was true, but rather that provisionally it had not yet been proven to be false. Along with Hayek, he was influential in establishing the Mont Pelerin Society, which aimed at reviving Classical Liberalism, given the perceived post war onslaught of collectivist ideology. Attending that first Mont Pelerin meeting were Milton Friedman and George Stigler (under the guidance of Frank Knight and Aaron Director). The young economists (in their thirties) would have met Popper at that time.
7. The book that launched Professor Blaug's career (1958) is still a particularly interesting and valuable analysis of Ricardian Economics.
8. The House Un-American Activities Committee (HUAC) was well known for asking its subpoenaed targets, "Are you now, or have you ever been a member of the Communist Party?" Established in 1938 it lingered well past the McCarthy era (its heyday) until 1975.
9. The obvious reference is to the 1st constitutional amendment guaranteeing the right of free speech and of assembly. Taking the "fifth" as it is more commonly labelled, refers to the 5th amendment which protects individuals from self-incrimination.
10. The relevant works are: Gardiner Means (1935). His Washington testimony represents Means' first use of the term 'administered prices' and his attempt to explain the Great Depression through the prevalence of inflexible pricing policies. This is a natural progression from his previous book with Adolph Berle (1932) explaining the decreasing prevalence of competitive markets. George Stigler sought to demolish this pricing notion in Stigler and Kindahl (1970). This was followed by an intense and pointed exchange between Means and Stigler in *The American Economic Review* in which neither party relented or had any desire to relinquish the field of combat. In contrast, Harvey Leibenstein (1966) presented yet another challenge to Chicago price theory. Stigler's sardonic dismissal appeared a decade later in a possible attempt to deflate interest in Leibenstein's concept of X-efficiency (Stigler 1976). The Galbraith reference is to his take on *The Affluent Society* (1958). Though Stigler would take on many of the points raised in this book (one can argue that George Stigler and Gary Becker [1977] took

direct aim at Galbraith's consumer theory), he didn't directly review that particular volume of Galbraith's work. However, Stigler (1968, 1977), highlights Stigler and Galbraith as consistent sparring partners. Perhaps Stigler's attempt to shred Galbraith's first influential book (Galbraith 1952) set the stage for all future battles. Stigler's title of the presentation he gave at the December 1953 AEA meetings probably foreshadows all of his subsequent assumed postures to Galbraith's output (Stigler 1954). Lastly Paul Sweezy's work on the kinked demand curve (Sweezy 1939) was met by Stigler's outright dismissal (Stigler 1947c). Sweezy, however, was never drawn into the seemingly fruitless endeavour of replying to Stigler's barbs.

> *Now in '47 George Stigler writes what is basically an attack on the validity of that model, also appearing, I think, in the JPE. Do you remember reading that at all?*
> No.
> *So you had no need to respond. You never responded to it?*
> I haven't read it. I don't think I ever did. I don't think I was aware of it actually. I didn't pay much attention to Stigler in those days. I was probably in one of my ultra-left moods, or something like that.
> [laughter] *Not worth replying to?*
> Yes, what's the point of it? (Conversation with Paul Sweezy, November 1997)

11. The book (Friedman 1962) is dedicated to promoting and arguing for a definite economic agenda which embraces distinct policy proposals. The ideological viewpoint of the work is clear and unmistakable.
12. This seminal 1971 article attempts to explain government regulations by a supply and demand framework based solely upon self-interested agents. The work comes as a logical progression from his 1962 article with Claire Friedland, which econometrically demonstrated that regulated electric utilities failed to provide any additional public benefits when compared to their privately owned counterparts. This article, along with subsequent work in this area, put to rest (or attempted to do so) the public service view of government and regulated efforts to improve social welfare.

> *Milton Friedman*: We're talking about the political world, the political market as opposed to the economic one. But in interpreting the political market, George very consistently, interprets the political market as a resolution of opposing self-interests and paid zero …
> *Aaron Director*: No …
> *Milton Friedman*: … tended to give very little attention to the extent to which it arose out of the desire of the people involved in government to promote the public interest. That is, I think a fair statement …
> *Aaron* Director: Yes.
> *Milton Friedman*: … and he took that position to a greater extent than most other people.

> *Aaron Director*: Did he really?
> *Milton Friedman*: Yes, I think that is true.
> *Aaron Director*: Do you?
> *Milton Friedman*: Yes.
> *Aaron Director*: Hm.
> *Milton Friedman*: and you do too.
> *Aaron Director*: I don't know about that.
> *Milton Friedman*: Well, I think we have come to a dead end here. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

13. The textbook formulation familiar to students is not to be found in Coase's (1960) article. What is known as Coase's Theorem is more correctly the creation of George Stigler in his reading of that particular paper. Some economists have objected that such a formulation conveys neither the content of the article nor Coase's intention. Coase himself has raised doubts as to whether Stigler understood his work or the objectives of that work.

> What I showed in that article, as I thought, was that in a regime of zero transaction costs – an assumption of standard economic theory – negotiations between the parties would lead to those arrangements being made which would maximize wealth, and this irrespective of the initial assignment of rights. This is the infamous Coase Theorem, named and formulated by George Stigler, although it is based on work of mine. Stigler argues that the Coase Theorem follows from the standard assumptions of economic theory. Its logic cannot be questioned, only its domain. I do not disagree with Stigler. However, I tend to regard the Coase Theorem as a stepping stone on the way to an analysis of an economy with positive transaction costs. The significance to me of the Coase Theorem is that it undermines the Pigouvian system. Since standard economic theory assumes transaction costs to be zero, the Coase Theorem demonstrates that the Pigouvian solutions are unnecessary in these circumstances. Of course, it does not imply, when transaction costs are positive, that government actions (such as government operation, regulation or taxation, including subsidies) could not produce a better result than relying on negotiations between individuals in the market. Whether this would be so could be discovered not by studying imaginary governments but what real governments actually do. My conclusion: Let us study the world of positive transaction costs. (Coase 1994a: 10–11)

14. Stigler consistently dismissed the use of ideology as an empty box, a response explaining nothing, or even worse, providing an excuse to avoid the hard work of research and analysis. The term in his evaluation neither measured nor quantified, offering nothing of value. Stigler, later in his career would chastise his close friend, Milton Friedman, for resorting to such a meaningless circumlocution.

> I don't know how important ideology is, but think it is unimportant. You don't know how important it is, but think it is important. My position is better because I try – feebly and so often unsuccessfully – to use a trusted theory of human

behaviour to explain social phenomena. Your position is worse because you try – with marvellous ease – to explain the mysteries by a deus ex machina. I have immense amounts to explain. You will have just as much to explain when you try to give us a theory of ideology. (Letter from George Stigler to Milton Friedman, March 29, 1984)

15. Stigler was one of the very few students to complete a dissertation under the guidance of Frank Knight. Others were James Buchanan, another Nobel Prize winner, as well as Homer Jones, one of Milton Friedman's teachers and later a senior vice-president of the St. Louis Federal Reserve Bank. George Stigler's dissertation was later published (Stigler 1941).
16. George Stigler was equally rough on all his students. In this sense he was extremely fair in his willingness to tear them about and provide an unvarnished opinion of their worth. A referee's report written for a former student provides a fair glimpse of his unyielding standards. Stigler always felt the need to provide his honest opinion when asked (or sometimes without prior solicitation). The student below remains unnamed for obvious reasons.

    His defects all stem from a certain lack of either self-discipline or the ability to absorb criticism from others. He is relatively uncomprehending of an alternative line of thought once he has made up his own mind and no one (at least no one at Chicago) is able to persuade him to change his opinion even on fairly technical matters. Nor is he a really rigorous and reliable economic theorist. In short, I think he is a man of good intelligence and high promise, on which he will never deliver. (Letter from George Stigler to Warren Nutter, June 25, 1968)

17. More likely, the birthday party was for his 65th year. There exists a mimeo of a speech he gave at that particular celebration complete with a 1976 date. Born in 1911, that would make him officially 65 at the time.
18. The show *Free to Choose* aired on the Public Broadcasting System starting in January 1980. It derived from a successful book by Friedman and his wife Rose (1980). Consisting of ten, one hour programs, the series starring Milton Friedman became unusually successful. The series was rebroadcast in 1990 with Linda Chavez as moderator. For the idly curious or terminally bored, a bit of hunting on the internet will discover these episodes, displaying an often charismatic aspect of Friedman's character.
19. This debate would have most likely occurred during the 1953–1954 academic year, when Milton Friedman was a Fulbright lecturer at Cambridge.

    At the lecture she [Joan Robinson] announced that Friedman was in the audience and that they had different views about flexible exchange rates. She asked

> him to come to the podium and discuss with her the basis of their disagreement. She said that both were competent technical economists so that their difference did not represent a mistake in logical reasoning, but must reflect differences in their factual assumptions and values and that students would find it instructive to explore those differences. What followed was a wonderful discussion - etched into the minds of all who were fortunate enough to be present. (Dalyell 2006)

20. The relevant article which tackles some of the issues arising out of his breakthrough book is: Chamberlin (1937).
21. The relevant works are: Freedman (1998a, b, 2002).
22. The famous debate began with Archibald's dissection of the Chicago position on monopolistic competition (Archibald 1961). Two years later, the powerhouses of Chicago provided tart and succinct replies (Stigler 1963; Friedman 1963). Archibald's rebuttal nearly equalled, in length, the two replies combined (Archibald 1963).
23. Euler's theorem:

    > A mathematical theorem relating marginal to average products. The theorem states that where a function is homogeneous of order n in its arguments, so that, for example, if $y = f(x, z)$, then $f(\lambda x, \lambda z) = \lambda^n f(x, z)$, the sum of the marginal product of each argument times its quantity equals ny. This implies that if $f()$ is a production function with y as output and x and z the inputs, the amount of factors used times their marginal products equals total output if and only if $n = 1$. Thus if factors are paid their marginal products, only with constant returns to scale does the sum of factor earnings exhaust the total product (Endnotes.com. [2003]. "Eulers Theory", *The Oxford Dictionary of Economics*. http://www.enotes.com/econ-encyclopedia/eulers-theorem, 5 May 2011).

24. A slip of the tongue here, Professor Blaug clearly means 1941 (Stigler 1941).
25. The reference here is to Sraffa's well-known criticism of the then standard theory of the firm and its underlying assumptions (Sraffa 1926).
26. Sowell's very restrained memory of his time with George Stigler can be discovered in the memorial *JPE* number (Sowell 1993).
27. Here the article of interest is one by Nathan Rosenberg (1993). He examines and evaluates Stigler's interpretation and relationship to Adam Smith's *Wealth of Nations.*
28. The well-known work that created an extended amount of debate is Coase (1960).
29. Actually 44 pages, but economists are not noted as having particularly long attention spans.
30. To be precise, Ronald Coase had been residing in the US since the 1950s when he moved from England to take up a position at the University of Buffalo (1951–1958). At the time of the 1960 dinner, he

was a member of the faculty at the University of Virginia. Along with Buchanan, Tullock and Nutter, he was a key member of what became known as the Virginia School in the field of economics.

31. The famous dinner at the house of Aaron Director in 1960 was attended by some twenty economists from the University of Chicago. The dinner and discussion is described by George Stigler in the chapter entitled "Eureka" in Stigler's 1988 academic autobiography. Stigler is himself responsible for finding a theorem in Coase's article and formulating the famous composition of that theorem.
32. The reference here is to the book George Stigler did with James Kindahl (1970) under the aegis of the National Bureau of Economic Research. But at the same time, this reference seems to be confused with an earlier book Stigler edited (1961b), also in its way questioning the price statistics issued by the Federal government. Adding to this inevitable confusion is the fact that both endeavours were bankrolled by the NBER (National Bureau of Economic Research). Additionally, Stigler's "Economics of Information" paper actually appeared in 1961, not 1962 as Mark Blaug recollected.
33. To some extent, Stigler's now seminal paper on information was his long delayed solution to the problem posed by Edward Chamberlin, whose work he attempted to demolish in his 1949 paper. For Stigler, it was never sufficient simply to incinerate an opposing and deemed dangerous viewpoint. He felt compelled to supply something more compelling and rigorous to take its place. Levy and Makowsky (2010) argue that this 1961 paper represented Stigler's attempt to deal with economies of scale.
34. The presidential speech was subsequently published as Stigler (1965). The speech itself was a rousing call to action. Quantification was the engine that would transform economics into a rigorous science.

    > I was sitting with Aaron Director at the time when he gave his Presidential address and we did look at one another at the time to try to see what each one thought about all of this. (Conversation with Ronald Coase, October 1997)

35. Demsetz (1968) basically undercuts the idea of the existence of natural monopolies. Though acknowledging in these cases that there is no competition for the market, an equivalent level of competition can exist to obtain the contract, or franchise, to provide a given service. Competition for the market rather than in the market is what matters in this instance. In essence, if the approach promoted by Demsetz is accepted, a need for a differentiated monopoly theory effectively crumbles.

36. The collected papers, all published by the MIT Press, come to seven very bulky volumes with the last one published in 2011 after his death. It is safe to assume, no additional volumes will be forthcoming.
37. I can only imagine that my odd digression here comes from my own struggle to raise twin girls while advancing a hit or miss career. Samuelson had triplets plus three other children. Fortunately for him, his textbook came out at a strategic moment allowing him to finance his brood.
38. George Stigler made his views clear on David Ricardo in his 1952 and 1958 publications with the latter piece perhaps being the more well-known and certainly the counterpart to Samuelson's expressed views.
39. Samuelson's (1988) article, published in *The Journal of Political Economy,* never received a response from Stigler, despite Stigler's position as one of the editors of that journal. Stigler had also by that time published two (1952, 1958) influential articles on Ricardo.

    > Now the JPE might even have been the first publication. George Stigler de facto was an editor of the JPE at that time. I don't know whether in that year he was a de jure editor because that was a shifting thing. And he accepted the article. The article was accepted and he would surely have been asked since I directly referred it to him as an editor. I never got a note from George saying 'Well, this time around I've got to admit I was wrong. And your reading was right.' Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like 'Well, if you're the kind of person who believes that, then you're just the kind of person who believes that'. (Conversation with Paul Samuelson, October 1997)

40. Stigler's (1969) paper reflects Stigler's ambiguous attitude to the History of Economic Thought. He remained one of the outstanding figures in the field, an economist who produced a dissertation in this intellectual classification. Moreover, one of the few University of Chicago students to successfully complete the task under the infamous Frank Knight. Despite this background, and somewhat ironically, a few years later the department, with George Stigler's unstinted acquiescence and even perhaps with his enthusiastic support, would remove the study of the history of thought as a graduate student requirement.
41. John Maurice Clark (November 30, 1884–June 27, 1963) was the son of another noted American economist, John Bates Clark. He published two classic works, one on workable competition (1940) and another on the accelerator principle of investment (1923). Before returning to Columbia in 1923 (where he had received his Ph.D. in 1910) he was a member of the faculty at the University of Chicago (1915–1923).

42. Though not without an impact at the time, workable competition would be largely unknown except among some of the more decrepit economists of today. Though Clark's ideas did seem to have a later echo in the work of Baumol (1982), as well as Baumol et al. (1977) on contestable markets. This represents more recent work, but is also currently largely uncited. However, given the profession's fixation on recycling ideas, some form of this approach will unsurprisingly be seen as part of some new theoretical wave sometime in the future.
43. Mark Blaug seems to be confusing a 1954 *Economica* paper by Patinkin with his 1951 *Econometrica* effort. Easily done since both papers invoke the idea of 'dichotomizing'.
44. Clark's (1923) book sets out his ideas on overhead costs.
45. Abram Bergson (April 21, 1914–April 23, 2003) did pioneering work on the social welfare function (1938) before becoming one of the leading authorities on the economics of the Soviet Union. He taught at Columbia before winding up at Harvard University. As to George Stigler's attitude toward the rest of the faculty of the Columbia economics department, it was as tart and sardonic as might be expected.

    > There is no one whose advice and company I value more than yours, but there are few other great attractions in the present Chicago economics department. I'm not the least bit inclined to boast of Columbia, which has a fine assortment of damn fools. (Letter from George Stigler to Milton Friedman, June 1951 in Hammond and Hammond 2006: 124)

46. The reference is to Clark's (1917) article in *The Journal of Political Economy*.
47. Polanyi is perhaps best known for his 1944 book, *The Great Transformation,* which attempts to explain the rise of the market economy and the modern nation-state.
48. A lack of familiarity with the works of Karl Marx didn't in any way inhibit George Stigler from denigrating Marx as an economist or as an economic thinker. "Marx is to Smith as Death Valley is to Mount Everest" (Stigler 1988b: 11) makes that particular position more than clear.
49. This is an example of my faulty memory. The relevant article is actually Coase (1994b), namely, "Economics and The Contiguous Disciplines". The paper was presented at The International Economic Association meetings held in Kiel, Germany in 1975.

    > He indicated his displeasure by saying - you know, it had been written for originally, and then given at, a conference in Germany. He said it would have been better if it had been written in German. He didn't care for it. This must mean, I think, and this may be of interest to you, that he didn't understand it. I don't

> think that he seriously examined the whole question of how one goes about accepting theories, although he himself, in his own work, did a lot of interesting things about that very subject. (Conversation with Ronald Coase, October 1997)

50. There is no evidence that Stigler pictured himself as being savage when constructing one of his notable attacks. Nor was it likely whatsoever that he ever intended to personally hurt or humiliate opponents. He simply had the ingrained habit of saying what he thought without the standard benefit of first filtering the more harmful aspects of such remarks.

> I would not enjoy the debate with the inflexible Hollander. Is ideology a question of preferences, if so how can one deny it a role in the market place of ideas simply because it has a somewhat negative connotation. (Letter from George Stigler to Warren Samuels, 13 February 1987)

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Fathers and Sons: A Conversation with Stephen Stigler

Craig Freedman

Meeting with Stephen Stigler had an element of inevitability attached to it. I needed his permission in order to gain access to the George Stigler archives, stored among the treasures of the Special Collections section of the Regenstein Library (University of Chicago). But I hiked over to the venerable Quadrangle Club once again,[1] not to tantalize my appetite with the Club's menu or simply to gain the sought after permission, but rather to glean a few more insights from someone who would, by an almost biological imperative, have a somewhat different perspective than those colleagues who labored beside him or those who opposed his academic efforts.

Stephen Stigler, who holds the post of Professor in the Statistics Department at the University of Chicago, is the only one among three sons who followed his father into the academic trade. Not unlike his unarguably erudite father, he shares a love, or perhaps even a passion, for the history of his discipline of choice. Several people with whom I spoke did, however, contrast Stephen Stigler's temperament with that of his father, George. He was repeatedly described as laid back, a description no one in his or her wildest fantasies ever attached to the

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senior Stigler. The composite picture, pieced together from disparate sources, turned out to be quite reliable. During lunch, Stephen Stigler is by turns helpful and forthcoming. Though perhaps lacking his father's sharp-tongued qualities, he certainly shares the same sort of sharp mind, one given to careful analysis and evaluation.

Meeting though with the progeny of an economist so keen to see the world in terms of rational decision making, inevitably led me to wonder the extent to which George Stigler might have introduced his theory of rationality into the practice of child-rearing.[2] Such a tendency to allow theory to creep into and structure one's personal life is not entirely unknown among leading academics and working scientists. B.F. Skinner became somewhat infamous, at least according to scurrilous rumors, for raising a daughter as if she was one of his laboratory rats trapped within a Skinner box.[3] However, despite George Stigler's well-known gruff and sarcastic exterior, a public face seemingly constructed to scare small children and intellectual lightweights, in practice Stigler remained a loving and compassionate family man with close ties to his family members. Certainly, he appeared deeply attached to his wife. Friends consistently mentioned his acts of kindness and generosity.[4] Stephen Stigler could only reinforce this generally held view while adding a number of other useful and interesting insights concerning his father's actual work. In the many years since this specific conversation, Stephen Stigler has consistently proven to be most helpful whenever I sought his assistance. Another trait, I suspect he shares with his famous father.

The Interview

What would you like to know?

I'm interested in asking you a question that has some personal interest to me as well. Here I'm speaking both as an economist and as a father of young children. I know that in his professional work, his colleagues all make a point of telling me how critical he was. That he was very tough, very critical. I wondered was that simply professional, or did that pervade his more general …

This was a professional attitude. I mean he did have high standards. He had high standards for himself; he had high standards for other people. He also had high standards for his children. But, we had a very good family relationship and benefited from it. We were not constantly under the gun of constant attacks. He dealt with us constructively, I think. And I would have no qualms with that. Now, his attitude in his professional work, I'm trying to remember the point you made to me. You raised some questions about arguments that he got into.

Yes.

I suspect that a lot of them were based on his belief that you could explain large sections of human behavior with price theory. This was a consuming interest, a professional interest, which he maintained. If you look at his papers you'll see that he wanted to extend this to political science, to all manner of areas. Gary Becker for instance is doing this with sociology. When my father encountered something which did not ring true, from the price theory point of view, he would be critical. He would ask the question, 'Is there any evidence to support this?' Generally finding none, except anecdotal evidence, he would sometimes make an investigation of his own. But he believed that such things required the test of other investigations as well. He was not adverse to other people re-doing his work, studies that he had done. Even when they came to contrary conclusions, he was, if not sympathetic with the conclusions, at least sympathetic with the publication of it. He did believe that such things should be played out in the public area of discourse, rather than kept quiet.

I had one interaction with him in one of the areas that you mentioned. This was the administered price question. He had this book with [James] Kindahl, which asked the question 'Well, quoted prices are not the same as transaction prices. We ought to see whether we can determine in some manner by using a large collection of industries, what the actual transaction prices were.'[5] They had a National Bureau of Economic Research grant and by strenuous efforts managed to somehow talk their way into corporate boardrooms. In effect, they got corresponding price series to the Bureau of Labor statistics in a number of areas. They could proceed then to analyze these statistically to determine what was going on. What they found basically, if I understand it and as I recall, is that a tendency for prices to stick was not present in the recorded transactions. The official quoted prices became fictional under certain conditions. Actual transaction prices seemed to reflect the more normal workings of the market place. Well a couple of years after that book (I had no contact or role in that book at all) a couple of years after the book was published there was an article that came out in, I think, the *American Economic Review* by a couple of Texan econometricians or economists, Bohi and Scully.[6] They had discovered the modern technique of Fourier analysis and thought that they would apply it to the same two price series.[7] What do they find? Well they write a paper saying that the behavior of these two series is the same. Though father was, at the time of his graduate education, well trained in statistics, he was not a modern statistician.[8] He vaguely knew what Fourier analysis did, but he was certainly no practitioner of such things. So he asked me for my considered view as to what the merits were in this case. I did look at it and discovered that the only difference between my father and the authors of the paper was my father knew he didn't understand Fourier analysis, while the authors didn't know that they didn't understand.[9] The paper itself was as concentrated a sequence of statistical blunders as you can run into in the published literature. The test they performed had no bearing whatsoever on the questions. It wasn't a question of contradicting or not contradicting, it was a question of it just being basically nonsense.

There was one point where they applied Fourier analysis to a series, which, in a ten-year span of time, changed value only once. Well, that's not in the realm of application of those methods. Even if the series which compared spectral densities had been within the realm of application, the test that they performed, the one that they based on the expected densities of the different series, had absolutely no bearing on the question of whether the series were linked in a dependent way. It was effectively a test that assumed that the two were independent, and tested (through an analysis of variance test) whether they had the same variation.

Well, assuming independence was blatantly incorrect. No one claimed these series were operating independently of one another. It was a question of a finer order of stochastic variation. In his book, my father handled that concern not in a very sophisticate manner, but in a manner that at least led to an understanding of what was going on. He didn't try to submerge it in a sea of advanced statistical methods. Basically, his analysis was sound.

That was not the issue under discussion in the *AER* paper. The issue that was under discussion there was 'Here is a new toy, we'll try it out and see what it says.' Well, the new toy was inapplicable. It answered the wrong question. It was also misleading in several other respects. I made some suggestions to him. The journal had invited him to make a very short comment. I played a role in that, but that's about as far as I got into his work.

That sounds, in the case you've cited, that certainly the referees of that paper would also have been lacking …

I don't think that that is uncommon.

No.

I think referees have less of a stake in something than the author, unless their work happens to be closely related to it. Otherwise, at a certain level, possibly only a superficial level, they just say, 'Oh, yeah, that's interesting'. If they don't disbelieve the findings too much, they may be willing to accept it. That can happen. Fortunately, if it is an important issue somebody else will raise the question and send in an article, which will point out the problems.

I'm interested, would your father have been, do you think, willing to have changed his mind if the evidence had been otherwise. Or how strong would any evidence have to be for him to reevaluate …

Well, I could only quote my experiences with him in other instances. He was open to being convinced by contrary evidence. He did feel that if you have something that has proven itself through one sequence of tests, that the evidence to overturn it had to be conclusive. One did not overthrow theories casually.[10] For example, the way he thought about the question of whether there are upward sloping demand curves.

Giffen goods.[11]

Yes, are there such things as Giffen goods?[12] He regarded this as a question to which the basic answer was 'no' and the argument behind this was that if there were, somebody would have made great hay out of a convincing demonstration of their existence. The evidence for them had been anecdotal. No attempts to seriously document their existence had been successful, or successfully maintained. There was a great incentive for people to provide such evidence, because it would have created a great stir. There was definitely a market for it. So it's not like something that nobody would bother looking for because nobody would be interested if they found it. Yet, no one had succeeded in producing such evidence.

Now, there is of course a theoretical possibility that such things could exist despite all of this. But the absence of evidence seems to be such that one should not spend too much time considering merely theoretical possibilities in real economic arguments.

He believed in the underlying logic of price theory. He believed that it is successful in explaining so much. As a result, one does not necessarily accept naïve, especially naïve, applications, because any analyses of economic problems are known to be very difficult. But he thought that the logic was so strong that the simple plain violation of economic utility maximization was not to be accepted without due scrutiny.

One of the great vices that many economists have is to know what they are going to find before they start looking for it. Now …

Now, wait a minute. It's not a vice. It is something that is true in all of science. You've misstated it a little bit. To *know* what you are going to find versus to *expect* what you are going to find. You cannot statistically design an experiment to learn about the world, and this is not an economic statement I think, it's much more general, unless you have a good idea about what that experiment is likely to produce.

Now that kind of an approach does not mean that you have locked out the possibility of contrary evidence coming up. In fact, you may believe that the experiment is going to come out a certain way. Still, you will plan it to be sensitive to certain types of departures that would be of interest, even if you had put a low prior probability on those departures.

Though it does seem to vary with different investigators. Some investigators are more willing to entertain contrary evidence and some seem more loath, in a sense.

I don't know about that. I don't know how you would quantify that.

I suppose that I think that there may be a greater distrust in the data. There may be a more concentrated attempt to find alternatives.

Well, there is the ultimate example of this. It's a famous quotation from Einstein. This occurs at a meeting in Zurich. Somebody came up to him and said, 'We have just received a report of an experiment that contradicts the theory of relativity.' He shrugged his shoulders and said, 'Too bad for the experiment.'

If you have a strong understanding of, and a belief in, a theory, that doesn't mean there is no evidence that can be produced against it. But, it makes you very skeptical. Suppose somebody has worked hard to try to disprove it. What you can well accept is that the person who has produced this evidence has found themselves quite credulous in accepting it as evidence, because of the desire they have in trying to overthrow somebody else's theory. And that's a very strong attraction. Overthrowing a theory, contradicting some well-known figure, is the way many young scientists get their start. Accordingly, there is nothing wrong with it.

No.

It is quite acceptable. But, that doesn't mean that all young investigators that come forward with evidence attacking some accepted theory of work are accurate, that their work will stand up to scrutiny. Not all of them, frankly, deserve scrutiny. One cannot spend all one's time investigating all claimants. There is a sense in which you can go too far.

In the mathematics department here, they at one time had a special assignment for their graduate students. Whenever anyone came in with a proof of the four-color theorem,[13] or of Fermat's conjecture, his last theorem,[14] they would give the manuscript to a student with a sheet to be filled in. The sheet would be filled out. Then it would be sent back to the person who had sent it into the department. The sheet was a form letter and it said, 'The first error in your proof appears on page so and so.' Well, that takes a certain arrogance, but on the other hand it is not an inaccurate way of approaching that particular problem. Most things that come in over the transom are not the work of cranks. Many of these people are sincere and serious amateurs. Some are even professionals. The chance that an outsider would crack a problem of that sort is not sneezed at entirely. They do look at the papers. But it's such a low probability event that you do not seriously expect them to be successful. So, you have to be practical. But then again, I know some people who make it a practice not to read the literature because they don't want to be influenced by it. That's a mistake, I think. We have to relate what we are doing to what others are doing.

That actually raises in my mind the role that empirical evidence does play in economics. Certainly, empirical work needs to be done. I think your father was very important in this respect. He was in the forefront of this realization. Otherwise, I forget the exact words, I think he said 'All you have is the world of assertion, etc.*' But in many ways, that evidence seems to be a complement to the logic or the force of the actual argument. Economists need to, in a sense, be first convinced by the logic of the argument. Only then do they want it supported by the empirics, whatever empirical evidence there may be.*

No. I don't think it is a one-way street in science generally. I can't speak about economics. Again, I'm not an economist. I've read some economics, but generally speaking, I think it goes both ways. There are times when one entertains a theoretical argument and becomes convinced. Then one has to ask the question, 'Well, let's see if we can gather some data to determine whether there is something we might have overlooked.' Because, any theory has a limited number of inputs in it. For instance, one sets up a utility function with a certain range of inputs. I'm guessing here because I am not an economist. But one cannot include everything in it. You can always point out that. But the question then becomes, 'Well, in that event, are we sure that we have got everything important.' There is a way of trying to determine that. But, if you can't verify by any simple method, or even deem it probably so with any certainty, you can say, 'Well look, there may be something important missing'. I can then try and look at some data to see how much of it's in line with what the conclusions of the theory are and how much is contradicted. This may lead me to go and refine my theory and find that my theoretical apparatus, once I take such and such into account, can indeed take care of the question.

Again, I don't think there is anyone in the economics community that is still seriously entertaining these days the results of questions having to do with administered prices[15] or the kinked demand curve.[16] Big areas of hot dispute today are the questions raised by Tversky and Kahnneman.[17] These are some of the hot topics today.

Yes.

The question is, do the apparent anomalies they discover cause problems with rational choice theory? I have sat in on many of the arguments surrounding this area. You can ask Gary Becker about it. My belief is that it is not really conclusive. They are very clever experiments. They are very interesting. They cause one to be aware of problems with naively gathered empirical evidence, to be sure. But the anomalies that are found do not shed any direct light on the more practical economic behavior that is the topic of the main investigations. Some people want to interpret them as emblematic. But they are certainly interesting and there is a great deal of interest here in talking about these things. A long running seminar goes on here on rational choice that is held jointly between economics and sociology. There was a meeting last night. Gary Becker was talking about some questions involving the art market. Why is it rational, how might one account for the empirically found phenomena? This is after all the way that much of the work is done around here. You find an empirical phenomenon and you ask, 'How do we explain that within our framework?' One of the issues is the tremendous difference in price between an original work of art and a copy. In fact, sometimes the copy is for all practical purposes identical. So why is so much attached to the one and not to the other. One would not even go to the trouble of saying it is intrinsic to the object. So what is the reason? One of the arguments they want to give is that there are sociological factors.

Yes.

They want to try and encompass those sociological factors within their apparatus. There's a new book out by several co-workers, not co-authors. There are different chapters written by different people that go in depth in investigating these questions. They don't have to have complete answers to everything. But, they do have ways of interpreting, of framing, questions in terms of their models. The utility mechanism that would make such behavior and such patterns plausible, that would result in behavior of the sort that can be observed. It doesn't prove that that's what is producing what we observe.

No.

But it's one of the things that one engages in. The debates of this sort have gone on a long time. I finished a paper recently. I haven't published it yet. It concerns an earlier era. It's about an economic debate that took place in 1910 or thereabouts. It was a very heated debate involving Alfred Marshall, John Maynard Keynes, Alfred Cecil Pigou, and Karl Pearson, a statistician.[18] It was Pearson versus the Cambridge economists. It sheds some interesting light on how one should go about interpreting the statistical evidence in so far as it bears on questions involving society. It had to do with the issue 'If you have alcoholic parents, what kind of effect does that have on their children?'

Yes.

Are the children of alcoholics severely penalised by the alcoholism of the parents? The temperance movement for years had been using this as an emotional appeal in order to generate support. They in effect painted a picture, a very grim picture of these children, full of these horribly shrunken, diseased products of alcoholic parents. Pearson had, for his own purposes, for his own reasons, namely his program in eugenics, wanted to attribute everything to heredity. But there was this opposing environmental argument. He was having trouble getting the public eye and ear for his work on heredity. So he said, 'Well, I'll take a look at what these other guys who are successful are doing. I'll see what the evidence is that supports their position.'

I'm not surprised to see that he found no evidence. They didn't have any. It was anecdotal evidence. So he did his own study and published it. It was a preliminary study, as social science goes it was fairly carefully done. I think it stands up pretty well. In fact, one of the interesting things is that his study, although he noticed this but did not make a great deal out of it, was the first real demonstration of what we now call foetal alcohol syndrome. While he was not trying to prove that, he did discover it, noted it, and down-played its importance to be sure. When compared with the kind of effects that the temperance movement claimed, it was not the evidence sought by most. It was not of the same order of magnitude.

Well, Alfred Marshall, then John Maynard Keynes and then Pigou entered in as staunch critics in the pages of *The Times* and in other places attacking him, on theoretical bases. They provided a theory which was, I think, as much as anything a justification of what they presupposed to be true. They had what might be called a Victorian belief that this wasn't a good thing. Alcoholics did harm to their children. So when you see the evidence that seems to say 'Why, they are the same as everybody else. Children of alcoholics are the same as children of non-alcoholics.' There must be something wrong with it.

Yes, with the evidence.

With the evidence. So they pointed out some things which Pearson had already in fact discussed. They pointed out the limitations of the evidence, because it is not easy to get that kind of evidence. But their criticism wasn't largely valid.

Stephen, your father writes, in many cases, about how, in a sense, you have to market your ideas to the profession. He talks about the need to sell them very hard, given the nature of the resistance you might get and the need to put as positive a spin on them.

I don't know. I *suspect* he doesn't put it that way.

Well, he says that you get further by disparaging the work of others. There was a sentence I was reading ….

That phraseology doesn't sound like him. That's all I'm saying.

I think it was in…

He did believe that there was a marketplace in ideas. He did believe that if you wanted somebody to listen to you, you had to, if not raise your voice, at least be very convincing. But, I don't remember him ever speaking in terms of trying to disparage others. He wouldn't, presumably. I wouldn't be surprised to hear that he might have said, if there was some alternative explanation, then it would be incumbent upon you to address that alternative. You would need to see if you could show how your theory would be more successful in explaining the phenomenon than this alternative theory. That doesn't sound out of line. But that is different than taking what could be called a negative approach. You're talking about trying to spin-doctor it, which is not in line with my understanding of the way real scientists work.

He was talking about John Stuart Mill. He said that one of the problems Mill had was that he was too modest of his own accomplishments. He tried so hard for a balanced approach that people in the profession tended to discount, or not to give full credit to John Stuart Mill for his own work. In other words, as far as initially selling the doctrine, too much modesty, trying to be perfectly fair or to assess fully and equally both sides, tended to get it dismissed.

Well I can sort of remember what you are talking about. But was that more to do with historians of thought in terms of their historical assessment of an individual? Or did it bear on the acceptability of the ideas of Mill at the time? I mean, it's one thing to have a tremendous influence, but to have nobody notice it because you're too modest. It's another thing for historians later to say, "He didn't have any influence." Or for somebody else to say, "Well that's because he was misread" and to interpret his modesty to be the cause. Again, I vaguely remember the piece, 'On the Uses of Scientific Biography.'[19] I thought it was more of a comment upon the phases of history of thought rather than a discussion of Mill's influence. I thought that he was very *strong* on Mill having had a tremendous influence.[20]

Yes, in his reading of the work. But it must be like marketing anything else. If there's nothing in what you're selling to the profession, eventually it just won't go anywhere.

No. That's true.

But it seemed to me that you can present a piece carefully, put in all your qualifying statements and all the ways in which it falls short. Or you can accentuate different aspects….

That backfires just as well. If you come in as a strong partisan and do *not* judiciously analyse the issues then people tend to discount it. It depends upon how forceful the product you're actually selling is.

One of the worst things that can happen is if your work is completely ignored, as opposed to becoming a subject of debate. This is when the profession just totally ignores it. I remember George Stigler claimed that one of Milton Friedman's many strengths is that he would infuriate the opposition to such an extent that they would feel compelled to answer him, as opposed to simply ignoring him. It would seem that there is a fine line….

Milton Friedman infuriated them by being so smart that he could answer any of their criticisms. *That's* what infuriated them. [laughter]

That is extremely infuriating, yes.

My father was a great student of the history of thought. He spent considerable time reading and trying to understand it. He thought it was a fascinating topic from his earliest days. He took courses in it and published articles on the history of thought … Have you read the works of Thomas Kuhn?[21]

Yes.

There's a lot of interesting things he has to say there. Some of it, not in his most famous book but in some of his essays, has a distinct bearing on the question of empirical evidence and how to deal with it.[22]

It's a very interesting work. It's interesting that you also became interested in the history of thought of your profession as well.

I couldn't say that there is no inheritance of ideas, but on the other hand, there certainly was never any encouragement. It's just that you grow up in an atmosphere and you absorb some of it. Also, the history of thought is something that you can indulge in only after you're tenured. [laughter]

That's the sad truth isn't it, in all fields?

Gee, do you have tenure then?

Ah, almost. But to be honest, what I'm doing is seen as an indulgence. This is for my own interest.

Listen, I entirely sympathize and understand. I work in the History of Statistics. There's a great interest in it, but on the other hand *nobody* says, "I think I'll try to hire somebody in the History of Statistics."

What regard does it have in the Statistics profession?

Oh, it depends. I think a lot of people have a serious interest in the history of the subject. But there's many more who have a very shallow interest. They want names. They want to gloss up their talks with a casual reference to Gauss or somebody. They're not seriously concerned with the person's work. ….. I've been successful in getting some grant support. I've written and published in the field. I've been indulged by my colleagues. But it's a small fraternity. It's also very hard work as I'm sure you understand, to rethink, to look at that older tradition of investigation. I consider it to be an *immensely* difficult task.

It requires trying to understand things that were common knowledge at the time but are not recorded. It requires a closer look at data. In my case, what I do is I rework the investigations. I try to, in so far as possible, do whatever was done. In the process, I try to understand how it looked and how it was understood. You are going to find people saying one thing and doing something else. In fact it's common to see that. It's not a question of duplicity. It's a question of the difficulty of understanding work at a distance. For example, I find LaPlace analyzing the behaviour of the effect of the moon on the tides.[23] To use modern terminology, writing about doing a regression analysis or an analysis of variance where he's using an estimate of residual error, the residual sum of squares. This is in his discussion. I find that when he is doing this, in an attempt to see whether there *is* a discernible effect of the moon on the tides, he in fact in one important instance uses not the residual sum of squares, but the total sum of squares. So he's not subtracting or correcting for the mean. Now, under the null hypothesis of no difference, the distribution of these statistics is identical. It's OK. You can justify that. But the power of the test, the ability to use certain distributional approaches, this power would be *markedly* less with the approach he took. Yet the reason he did this was pretty clear. He says, "We are looking at it under this hypothesis, so we use that for framing our model." We know now that it's not a shortcoming of LaPlace. It's a demonstration of how much better an understanding of theoretical approaches and of useful data analysis that we have today. But it's easy to look naively at the earlier discussions and read things into them from modern point of view that aren't there.

I think the most difficult thing is to try to come to a work without any preconceptions about what is there. To try to actually read it and figure out what the author wants to tell you. Especially in economics, there's such an informal, unwritten, common knowledge or opinion about what so and so means. A lot of people haven't read the original, but talk about it as if they did. Or, when they do read it, they find in it what they expected to find. That's very difficult to overcome.

But it's also maybe impossible. [laughter]

Yes, I suspect so.

It's not clear that an absence of preconceptions is even the right way to address many of these issues. It might be better to look at the *different* points of view which represent different information sets. In Statistics some people talk about 'non-informative prior distributions', as if one can approach statistical analysis with a clean slate. You cannot. If one has information, one would not. There are reasons for being interested in testing something. There are reasons for being interested in measuring something. Those reasons will play some role in the design of the experiment, the choice of the data to be observed. The background for understanding those reasons can be important, the consciousness of it.

Can I just ask you one last question before we go because this is part of my own personal fascination? I note that some economists like to use economic theory in child raising. Was your father an economist that…..

I don't think so. Not that I can recall.

Do you recall appealing to your narrow self-interest ….

No. Not really. He was conscious of economic incentives and the like. He may have offered us pennies to do certain jobs and the like. This was not an experiment where I can speak of a control. [laughter] Fundamentally, I suspect it wasn't significantly different from other families and other childhoods. As a family, we were not as economically thorough in our upbringing as some people might be. I know some families that would think *everything* was economic. But that's not our family. In many ways my father was, if you wish, inconsistent in some respects. He was very generous with his time, with his money, with his expenditure on his family and towards his friends. He was not somebody who approached most of life with—his personal life—with any narrow, economic constraint. He was fascinated, he was in love with the subject of economics. He was quite passionate about it and always thrilled whenever he found some new area to investigate. When he encountered a setback to his approach he would accept it and move on. He would accept alternatives when they enlarged a theory, when they proved useful. He didn't give up, but he was not unaccepting of contrary views. He was unaccepting of some that he considered to be unsupported by any evidence. That didn't mean that he didn't hear people out. It was more a question of whether the individual was adding to economic theory. He didn't check their views before he talked to them. It depended on whether they could make an intelligent argument which shed light and understanding on something of interest. If they could, he would be fascinated. Even if it was not something that was in line with his own work.

Do you think that some of the force of persuasiveness was the fact that he could present a tightly reasoned argument backed by empirical evidence? That this in fact was the reason for a great deal for his success, besides his own persuasiveness?

I could say 'yes', but I'm not the one that was being persuaded. I'd say an uninformed 'yes'. I was not a part of the economics audience, so I can't answer in a knowledgeable way.

OK. Well, I'll let you go back now.

Notes

1. A previous meeting with Ronald Coase was also conducted at lunch in this venerable institution.
2. In his work, George Stigler often attempted to explain each and every human choice in terms of the rationality attached to narrow self-interest. He certainly would agree with the British economist (Dennis Robertson) that economists economize on love. In essence, though not denying the existence of such sentiments, the reliability and comprehensiveness of love as something approaching a ubiquitous motivation, was considered to be sorely lacking.
3. B.F. Skinner gained initial academic fame by developing an operant conditioning box while still a graduate student at Harvard in the early 1930s. The purpose was to train the behaviour of laboratory rats by exposing them to certain measurable stimuli. In the field of behaviourism, a subject's desired actions could be induced by attaching appropriate consequences, rewards and punishments, to different actions. Within this perspective, individuals were defined as lacking free will. Consequently they could be appropriately trained by reinforcing good actions (those defined as good by the experimenter). Persistent rumours existed, and probably continue to exist, that Skinner's daughter, Deborah, was raised like a laboratory rat in a Skinner box. Such stories

are embellished by stating that she became psychotic and later sued her father. Deborah Skinner has persistently denied such claims, but the stories have proven too enticing to die a natural death, even when confronted by what Deborah Skinner insists are the facts. This may be yet another instance of a story that isn't true, but should be true.

4. Though a pioneer in extending the concept of rational decision making throughout the realm of social science, Gary Becker pointed out Stigler's own thoughtfulness.

   > When George was sceptical as I said about the altruism issue I said, 'Look George, look how generous you are to your children. Are you doing that out of self-interested motives? Who are you kidding? You're not doing it out of that.' And he looked at me and he didn't answer and he knew he wasn't doing that out of self-interested motives. He was actually very generous with his family, as he was with people in general. So, I think George, when pressed hard like I did there, would admit there is a motive beyond simply selfish self-interest. It could include his concern about others and generosity and so on. (Conversation with Gary Becker, October 1997)

5. The reference is to book by Stigler and Kindahl (1970). The NBER ostensibly funded the empirical project to improve the Bureau of Labor Statistics existing price series. These industrial indices, as they then existed, depended on self-reported contributions submitted by the relevant manufacturing firms. In fact, through face to face interviews conducted by Stigler and Kindahl, such an improved series did come about. But as always, more than this was at stake. George Stigler had for seeming decades battled an economist (Gardiner Means) who equally had promoted the need for, and value of, empirical data. Except that Gardiner Means, existing as a sort of perverse doppelganger to George Stigler, consistently drew opposite conclusions from whatever data happened to be available. While for Stigler, markets would be, and could be nothing else than competitive (according to his reading of the data), for Means the US economy failed to operate properly due to its inherently oligopolistic structure. The conflict involved two fundamentally irreconcilable views of the marketplace. Stigler clearly defended a strict belief in classical price theory with Means representing the polar extreme. In essence, from Stigler's perspective, prices by definition had to be flexible given the determinative effect of market forces on supply and demand. This view contrasted irreparably with that of Gardiner Means, for whom prices were administered or set by firms exercising market power. Clearly no middle ground could exist between these two implacably held positions.

> Both of them, both of these men had taken strong positions on this … Maybe - I almost come close to saying, that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist. I could have predicted George Stigler's predictions before I signed up from what I'd learned in graduate school of his writings. And I could have predicted, to a great extent, Gardiner Means' predictions and you could guess what I learned at graduate school about Gardiner Means. (Conversation with James Kindahl, October 1997)

Neither side was willing to give any ground despite the fact that defining the precise boundary between flexible and administered was ambiguous at best. Both Stigler and Means thought each other was simply wrong and dangerous in their presuppositions. Means in fact made every effort to stop the NBER from publishing the Stigler-Kindahl book.

> My quarrel is not with the data but as I have already indicated, with its interpretation. If I am wrong, and I could be, traditional theory can still apply to the modern concentrated economy. But if I am right about the administered price thesis and there is nothing in your findings to persuade me otherwise, then short run economic theory will have to be thoroughly overhauled. In the short run, an economy in which each price changed only once a year (even if the changes for different buyers were scattered through time) would be quite a different kind of economy from the classical economy of flexible prices such as is represented by the Walrasian system of equations. (Letter, Gardiner C. Means to James K. Kindahl, October 26, 1970)

6. The paper is by Douglas R. Bohi and Gerald W. Scully and appeared in June 1975 edition of *The American Economic Review* followed by a terse reply by George Stigler.
7. Fourier analysis is a mathematical technique that allows a general function to be represented or approximated by sums of simpler trigonometric functions.
8. Actually, George Stigler's statistical education came largely after he completed his graduate training at the University of Chicago. As he himself acknowledges:

> The fault of my neglecting what is now called econometrics was no doubt my own. I had not taken any courses with Douglas (who was feuding with my teacher, Knight), and I was not attracted by Schultz's pretentious teaching style (as can be inferred from the tale of four dimensions), but I should have exploited his interest in the field. So I had to acquire my great respect for, and modest skill in, empirical work from fellow economists such as Friedman and Wallis, and from a long apprenticeship later at the National Bureau of Economic Research under Arthur F. Burns, Solomon Fabricant, and Geoffrey Moore. (Stigler 1988: 27)

It is important to remember that as a graduate student, George Stigler was very much in thrall to his dissertation advisor, Frank Knight, and would have adopted many of his positions. Knight was well-known to scoff at allotting any role for statistics or quantitative methods to serious economic analysis. In his view economic science consisted of deductive logic.

> Schultz was an object of derision to Frank Knight and maybe Henry Simons, and this was picked up by George Stigler and Allen Wallis, who were graduate students … The Knight crowd didn't need much to get an attitude of derision towards people and theories. Schultz was a self-trained mathematician and econometrician and a little unsure of his ground. He was a student of H.L. Moore's, and I would say a somewhat limited man, but he did perform a valuable role at that time. Wallis and Stigler used to make him nervous by trading on his insecurity. (Samuelson 2011: 977)

9. This insight calls to mind one of Frank Knight's favorite quotes from the nineteenth century humourist Josh Billings. "The trouble with people is not that they don't know, but that they know so much that ain't so."
10. This perspective is in agreement with Robert Solow's view of the matter as well. (Despite different political leanings, George Stigler and Robert Solow became and remained close friends from 1957 onwards.)

    > I think he would have said, 'It's the better part of wisdom when you come to these really narrow decisions and the data speaks ambiguously. It's the better part of wisdom to accept the long-standing, the long view we've come to accept as knowledge and it's unwise, on the basis of that kind of evidence, to say I should throw over something that has stood us in good stead since 1776.' (Conversation with Robert Solow, October 1997)

11. George Stigler (1947a) commented on the problem of the Giffen good early in his career.
12. Giffen goods slope upwards (higher prices lead to a greater quantity demanded) because not only are the good's substitution effect and income effect opposed to one another (an inferior good), but the relevant income effect, due to the price change, is actually larger than the associated substitution effect. To qualify for such status, the consumption of the relevant good would need to compose a large proportion of total income spent on consumption.
13. The four color theorem states that, given any separation of a plane into contiguous regions, no more than four colors will be required to color the regions of the map so that no two adjacent regions have the same color. The first proof by Kenneth Appel and Wolfgang Haken at the University of Illinois was announced on June 21, 1976. Distinctively, this proof represented the first successful attempt to harness computer assistance as a solution mechanism.
14. Fermat's last theorem, or conjecture, states that no three positive integers; a, b, c, are able to satisfy the equation $a^n + b^n = c^n$ for any integer value of

n greater than two. The cases $n=1$ and $n=2$ have been known to have infinitely many solutions since antiquity. Though ultimately not solved by a paper delivered over the transom, a breakthrough did come by means of two papers published by the mathematician Andrew Wiles and composing the entirety of the May 1995 edition of the *Annals of Mathematics*.

15. Gardiner Means was perhaps best known for his theory of administered prices (first appearing in 1935). This approach appears to be an almost natural inference given his view that the US economy was dominated by oligopolistic power. Using empirical evidence, he ascribed the 1930s Great Depression, and much later the 1970s period of rapid price inflation to inflexible or sticky prices. Though perhaps no longer presented in terms equivalent to those of Gardiner Means, the nature and scope of price adjustments (along with the level of price competition) remains a largely unresolved area of economic research.
16. George Stigler conducted what amounted to an almost lifetime campaign to discredit the kinked demand model, which he considered to be wrongly conceived and unproductive of any future lines of research. His early attack (1947b) was later followed up by another in his 1978 article. He seemed to want to assign the model to a well-deserved place in the historical dustbin, but remained annoyed by the fact that it still lingered on in most microeconomic textbooks. He didn't live long enough to see its general excision, which seems, for the most part, to be the case in the latest such textbooks.
17. Amos Tversky and Daniel Kahneman were both trained psychologists. The 2002 Nobel Prize in economics was awarded to Daniel Kahneman for the pioneering work he had done with Amos Tversky. (Tversky died in 1996 and was thus ineligible for the prize when awarded.) Their work challenged the core assumption of rational decision making in economics, long seen as one of the key foundations of price theory. Their work revived to a great extent behavioural economics by suggesting alternative understandings of human behaviour and decision making.
18. Those readers interested in following up these issues would be advised to dive into an investigation by Stephen Stigler (1999).
19. The paper, 'The Scientific Uses of Scientific Biography, with Special References to J.S Mill' was originally part of a 1976 conference volume focused on the contributions of John Stuart Mill and his father James. The article is easier to find reprinted in the 1982 collection of George Stigler's essays, *The Economist as Preacher and Other Essays.*

20. There is also another slightly earlier 1973 paper on John Stuart Mill which seems also to pertain to this particular discussion.
21. Thomas Kuhn is best known for his 1962 book which explored a theory of scientific progress (the shift to new paradigms in science) based on his exploration of the history of scientific thought.
22. One place to start is with Kuhn's (1961) article that preceded his famous book.
23. Pierre-Simon, Marquis de Laplace (23 March 1749–5 March 1827) maintained a wide range of interests, best exemplifying that period of the European Enlightenment. He made important contributions to the fields of mathematics, statistics, physics and astronomy. Laplace anticipated Bayesian statistical analysis in his 1814 work, *Essai philosophique sur les probabilités.*

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The Way Things Work: The Empirical Bent of Economists—Ronald Coase on George Stigler

Craig Freedman

When people speak about others, they invariably reveal quite a bit about themselves. So though ostensibly the conversation that follows appears to be simply an attempt to explore some ideas about George Stigler, Ronald Coase quite naturally finds it difficult to entirely avoid discussing his own ideas and thoughts as well. Our scheduled meeting was at the Quadrangle Club, which has served as the social venue for the University of Chicago faculty since 1893, only a few years after the opening of the University itself. William Rainey Harper, first president of the institution, was one of its founders. Perhaps like its English counterparts, the atmosphere is intended to exude more charm and good taste than does the typical club food available. The location however did prove to be quite congenial, with Ronald Coase suitably and almost effortlessly performing the role of a very cordial luncheon host. Coase during our session managed to appear as an almost cinematic embodiment of the British Professor. He projected a quirky charm all his own, as well as a very dry and subtle sense of humor. What remains striking

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in this conversation is the essential difference in approach to economics taken by these two major figures. Both Stigler and Coase spent many years at Chicago without travelling down identical roads. Of course, the lazy idea would be to lump Coase together with Friedman, Stigler and Director as generic Chicago economists. Doing so would entirely mislead. Such careless categorization would provide a nod of approval to confusing similar politics and policies with their very distinctive notions of how economics is best practised. More accurately, though Ronald Coase was unmistakably at Chicago for many years, he really never was of Chicago, at least he was not totally in sync with the post-war Chicago School.

> When you say it is un-Chicago, you mean that it is an unmodern Chicago view. Because Frank Knight was at Chicago, and I was brought up more on Knight than I was on any of the others. And my views were quite consistent with what he says. They're not consistent with what George Stigler, Gary Becker, and Richard Posner say. Posner condemns me because I don't think people maximize utility. (Coase in Hazlett 1997: 3)

Coase didn't arrive at Chicago until 1964, well into his very lengthy career.[1] It is often overlooked that Coase was one of the stalwarts of the Virginia School of economics, named after the University of Virginia where he resided from 1958 to 1964. This school, which included such luminaries as James Buchanan, Gordon Tullock and Warren Nutter, has tended to be overlooked, overwhelmed by the struggle waged at that time between the leviathans located at Chicago and the contending giants residing at the Harvard/MIT axis. Coase had much more an affinity to Buchanan, who also harked back to the tradition of such teachers as Frank Knight and Henry Simons. In contrast, Stigler, Friedman and Director broke quite clearly from this older, classical liberal economic position.[2]

I feel obliged to note in advance that there is a crucial gap in the transcription which follows. My almost fatal recording error reflects in no small measure the degree of interest Ronald Coase generated during our discussion. At a crucial stage, I failed to notice that the recorder had clicked off. Words that should have been faithfully recorded were forever lost. Unfortunately, this slip came just as Coase reached the point of discussing the fateful 1960 meeting at Aaron Director's house to argue the thesis contained in 'The Problem of Social Cost'. We also examined during this silent interval (at least on tape) the collateral creation, by George Stigler, of 'The Coase Theorem'. Unfortunately, my short term memory, even back then, was (and remains)highly limited. So I can only supply, by means of an insufficient footnote, some of the gist of what Ronald Coase was willing to relate. However, even with that regrettable break, reflecting my sad amateurish lack of proficiency, the following conversation is still not without considerable value. (Note, I have deleted the opening chit-chat. It is mostly focused on our dining decisions, which conveys nothing even of passing interest except our lunch preferences.)

Conversation

Coase: Yes, now let's see. I don't know whom you've seen. I'll give you my views on different people and what they're good for. Now, have you seen Stephen Stigler[3]?

Freedman: *Yes.*

Coase: On George's upbringing and general habits and so on.

I tried, as far as he would talk about it.

He would be very good on that. On his work habits I would have thought that Claire Friedland[4] is best. Have you seen her?

I've seen her.

Oh, you've seen everybody. Well, if you want, another general appraisal of what his work is worth today, I think Sam Peltzman is the best place to start.[5]

I saw him last week. He was very good.

That I can believe. On recent activities, Gary Becker would probably be as good as anyone else. George worked closely with Gary more recently. As I indicated in my letter, for the sort of overall appraisal of his work, I think Demsetz is the best.

I would have to agree. I actually saw him when I was out in LA and he was very helpful. I would agree with that. I thought that the article he wrote was very perceptive.[6]

Yes, I think it is very good. Thinking of Demsetz in California, there's Milton. Did you see him?

Yes, I actually saw him along with his wife and Aaron Director as well. I saw them all together.

That was really nice because Aaron became really George's best friend. You'll get a good idea from him. I don't really know what Milton would say. I have no idea. I have a good idea of what Aaron might say, but what Milton would say, I don't know.[7] In many ways, their work didn't overlap at all. It was quite separate except in the references say to Milton's methodology.[8] You don't get any reference to Milton. Not any that I'm aware of, not any connection. Aaron was obviously, particularly of recent years, of some influence, but I would be hard put to say exactly what it was.

Aaron Director seemed to be certainly one of the people that George Stigler would always listen to very seriously.

And in more recent times, Gary Becker. He worked closely with him. I don't know always to his advantage but that's what happened.

He did seem as he worked with Gary Becker to push the ideal of self-interest more and more.[9]

And rationality. George didn't like my piece that I wrote on Economics and The Contiguous Disciplines.[10]

Yes, I've read it. It's Yes, I've read it. It's good work.

He didn't think so.

What was his problem?

Well, I didn't actually ask him. He indicated his displeasure by saying—you know, it had been originally written for, and then given at, a conference in Germany.[11] He said it would have been better if it had been written in German.

I suppose he didn't care for it.

He didn't care for it. This must mean, I think, and this may be of interest to you, that he didn't understand it.

Do you think that he really had a hard time getting his mind around certain material? Perhaps there were certain ideas that were alien to his approach?

Well I don't think that he seriously examined the whole question of how one goes about accepting theories, although he himself, in his own work, did a lot of interesting things about that very subject.[12] The interesting thing is that he was probably right.

Yes. So in a lot of ways he seemed to have wanted to hold onto the sort of Friedman-Stigler proposition of the '53 paper.

Yes.

I mean, at least at a formal level, as opposed to informal activity.

Our relations were always very cordial. I always gained a lot from him. If you had a problem, and discussed it with him, you came away afterwards feeling better and understanding better, but not necessarily accepting what he said. So, he's made a lot of difference, at the university at least, to the economics department. He was a real presence.

I can see that he was very influential in shaping the way economics is done. I think probably not only in Chicago, but in a larger sense as well.

Yes, that's true. He was very nice to me and it is interesting. You know the 'Nature of the Firm' that was reprinted in the series which George and Boulding edited.[13] I once asked him about the choice there. And, it was George's choice. They agreed that Boulding should choose half of the articles and George should choose the other half of the articles. So, in a sense it was not a joint effort the selection. And George chose my article. So that, I think, is interesting that he had a sort of general sympathy, a feeling that it was of some importance. And at a time when, I don't think other people did.

But he doesn't seem to have actually, himself, done anything in that area.

No. He makes a number of remarks which are sort of coming to the entry point, as it were, of the subject. When he reprinted that thing of his, that the 'division of labor is limited', or whatever the phrase is, 'by the extent of the market', he adds in a footnote of the reprint that he ought to have referred to my article.[14] Well, I'm not sure that he ought to have referred to the article. I didn't want him to worry. But I think there was a feeling that he might have missed something.

It seems that what you did in that article, and in a lot of your other work was actually, to try to find out what people in firms did. How they organized themselves, what they actually thought and that seems very much not what George Stigler did.

He was not really interested in what firms did or really what the political parties did, although he writes about it, what their effects were on regulation and so on. He sort of studied them from the outside.[15] But why political parties were organised the way they were, which is an interesting question in itself, he never investigated.

Yes.

Do you know the work of a chap called Duncan Black?

Yes.

Duncan Black is now dead. His work is being brought out by this Kluwer publisher in four volumes including his unpublished stuff.[16] I wrote the Preface to the main work. He used—and I refer to this somewhere because I knew him very well you see, and we talked about it when I used to see him—he used transaction costs to explain the emergence of legislative assemblies, governmental departments, political parties, work of that genre, but never published anything on that. What he published was on voting systems where straight mapping would be important. But, it clearly could be used for that purpose.

Absolutely

Although I've never worked on political things.

Somehow George Stigler didn't think it was a very important matter to discuss.

It just didn't interest him I suppose.

That's probably more accurate.

Yes, more accurate. He was a very strange person to sum up because his methods were not those of anyone else. I think he was quite unique. If you put a point to him, he liked to answer it with a joke. Then if you pressed him, he produced some fact or other. You pressed again, he'd give you his answer. But he was sort of an economist even in argument. He used the easiest way.

Why was that?

I don't know. I think he was one of the most difficult people to explain because I mean, there is no one like him. I've described how in an argument he jumps around. He puts in a bit of theory, a bit of statistics, a reference to the earlier economists. It's like no one else's form of argument that you can recall.[17]

I remember in the Harold Demsetz article, Demsetz said he just learned enough about various things to make the argument. But he doesn't seem to go beyond that. If he needs some particular fact about an industry or about a specific instance, he'll get just enough and weave it into his flow of argument, but he stops there and he's just not interested in going any further.

But Claire would be very helpful there.[18] What did she say about his work?

Well, it is hard to say. I mean, he was a very hard worker. He was a workaholic in her opinion, and constantly wanting to take on more and more and to do more and more. But she did say, when I asked her; because in his actual empirical work he always seems to be very confident of whatever results he gets, even if it is not warranted particularly. She just said it was his character and the way he marketed and sold things. He was just a very confident guy pushing his ideals across.

That's interesting because in some ways I always felt that he was sort of unsure of himself, too, in his manner. I always thought that for a person of his stature, he showed less confidence than you would have expected. That was what I felt. Others probably responded quite differently.

Funny you mention that, because at least Claire Friedland always suspected that there was a bit of shyness, or a bit of sensitivity, or a need to sort of keep people at a distance. And that's maybe why he was always making jokes.

Yes, I think that was maybe right. The feeling I always had was that, I don't know, there was a sort of insecurity about the man. At least, I felt that. For which there was no justification at all. But I doubt whether people will be willing to talk about that.

Only a few. I think many people took the image that he projected. You know, quick wit, on the attack, that sort of approach.

His judgments were very good. Very good. As I said, he could get the right conclusion from inadequate data. And, I don't know how you do that. I mean, how you explain that sort of thing.

Some sort of economic intuition?

He had an intuition that was always very good. And I think it particularly helped when he was dealing with the history of economic thought. For then you have to think. There is so much you don't know. You've got to fill in the background. But he somehow managed to fill in the background.

If we consider his history of economic thought work, it seems to me that one approach to doing that sort of research is to go in looking for something, almost knowing what you'll find before you start digging. The other is to hold your judgment and try to put everything together. Do you think he had one approach or the other?

It seems to me that when you get to his later work, say with Becker, you know what the conclusion is going to be before you start the argument. In a sense, you're assembling arguments to support a conclusion.

I mean, that may be unkind and untrue but it's an impression. And, it's even more so in the work of Richard Posner. Have you read any of that? It seems to me that the plot is always the same, and the characters stay fixed.

Yes, that you always know the finale.

It's the same thing, to some extent, one might say about Becker. But his work is so very good. And you learn so much from studying it, that that element in it is not a problem.

I know in your article "How do Economists Choose?" you look at the role of quantitative, empirical evidence in a way which I would say was very different from George Stigler.[19]

Yes

Did George Stigler ever comment on that piece?

No, he didn't. I think I thought it was a somewhat touchy subject. I didn't bring it up. But you know our relationship was very cordial, very friendly, but I didn't know, half of the time, what he thought about me. You know, it was respect, but how far? The fact that he chose 'The Nature of the Firm' himself very much speaks for itself.

I was always curious about that. Because, it didn't, as I said before, it didn't seem to reflect in his own work at all.

No.

And it was certainly not the sort of standard, testable hypotheses, empirical evidence format that he seemed to insist upon. So it's very interesting that it was Stigler who actually chose that article.

That piece of information you might not have got otherwise, except from the editors.

No, I didn't know that. I didn't know exactly how the article got in, because, as I said, reading George Stigler's work there is no indication of any real interest.

I think it is an indication of the way his mind works, though. He almost sensed that it had some importance without perhaps knowing what it was.

[Unintended break in the transcription].[20]

I got so interested. I haven't been recording for the last 15 minutes.

Well, anyway, you can remember all this.

Yes, I'll try.

I'm not getting any younger.

About George.

One of the things about getting old is that your short-term memory gets very bad, so you can't remember what you did ten minutes ago. You're speaking of something very important and you just forget what you're saying.

It starts very early.

Old age starts young.

You were talking about whether George Stigler actually saw that. We were talking about how far the article [the article on Social Cost (1960)] could be applied. Whether you have to know anything a priori*, or whether you had to apply a case by case approach.*

The interesting thing is he was a great enthusiast for quantitative methods.

Yes, always.

So, it doesn't seem altogether consistent. But he certainly was a professed believer in quantitative methods. On the other hand, he knew what the answer was going to be in advance. He just regarded it, then, as a way of persuading other people.

Because, it doesn't seem that George Stigler himself had his mind changed by empirical data.

One of the interesting things about economics is that the subject has been so static over the years. That is, it's not that you don't know much more, but your way of looking at things, your vision doesn't change. One of the things I want to do, if I have time, is to take one passage from the text books in chemistry or biology or physics, say after the War, and compare them to what's happened today.[21] You'd see an immense change.

There would be almost no relationship.

That's right. Now take an economics textbook. It's more or less the same. Exposition improves, techniques improve. There is a lot more illustrative material that didn't exist before. Fundamentally, not much difference. But anyway that's my view. So, the empirical work doesn't seem to change a vision, or hasn't in economics, but I don't know why. It's very tricky, this whole business of how ideas emerge and subjects change, and so on.[22]

It's very difficult.

But it is striking, I think, that economics has shown a certain static quality. You could still learn something from Adam Smith.

You could learn a lot.

How many 18th-century chemists would be worth reading today?

Well, I don't know that you could learn all that much from Lavoisier.

No, that's what I mean. Actually it is very positive.

Puzzling to really figure out what the actual role of empirical data is in economics.

Well, I argue that it is not basically what you find in the physical sciences, and I refer to this work of Kuhn.[23]

Yes.

Now I spent a year with Kuhn, at the Center for, whatever it's called, the Advanced Studies in Behavioral Science.[24] I thought he was the most interesting person there. I, of course, adopted his views before this book of his, *The Structure of Scientific Revolutions*, ever appeared. I was influenced by an article he wrote on the role of measurement in the physical sciences. I can't remember the title or exactly when it was published, but there he says that it's very difficult to get results unless you know what you are looking for.[25]

Yes. That's absolutely true. Look at Galileo.[26] *If he wasn't looking for those specific results, his experiments wouldn't have been evidence of anything in particular. Results are never quite unequivocal.*

Well, I adopt that view and say it is true in economics. But, it means that we have to get the vision first and then do the empirical work. I've just been involved in the starting of a new society, The International Society for the New Institutional Economics.[27] And the inaugural conference was held last month in St. Louis. I wanted to have a small meeting, but we invited some people and they told other people and they told still other people and in the end we had 200 people come from all over the world. No one from Australia, but someone from New Zealand, and I know there were people from Russia, and China, Taiwan, all the European countries and so on. So there is a lot of dissatisfaction with the present state of economics. It's not dominant, but there's a lot of it and it's widespread. However, we mustn't talk about me. Let's talk about George.

You know that's interesting on several counts because I know in that same article which you wrote, 'How Do Economists Choose', you give examples of a number of cases where the profession did seem to move, but the change never came, initially at least, based on any empirical evidence. The Keynesian revolution, certainly had a …

Well, that's a very good example. In fact, people have examined a lot of his empirical statements and they've found they were wrong.

Sure, because Keynes would just make them up, sometimes.[28]

Yes. But that's sort of interesting. Look, the change came so rapidly in England, certainly, and I think in America.

Yes. And it certainly wasn't by appealing to the empirics.

No, it wasn't. The monopolistic competition versus perfect competition controversy really swept economics at that time. That was just due to a feeling that, you know, the existing theory was not particularly good, so they switched over.

I think you could probably make a similar case for recent rational expectations theory. Even though there was a lot of empirical work afterwards, the initial switch had very little to do with any empirical evidence. I think that might be yet another case.

Yes, that's another thing that I would like to work on. I'd like to do some more work on expectations. I don't know whether you knew I worked with Fowler on working out what people's expectations were. Well, if you read Muth's article on rational expectation, you will find a reference to me there, as one of the first people to have a sort of rational expectations idea.[29] What we were concerned with was this. In those days, there was something called a cobweb theorem. The name was invented by Nicky Kaldor.[30] It is based on the idea that people assume that existing prices and costs are going to continue as is. So Fowler and I took the pig industry and we showed that it wasn't true.[31] We showed that it wasn't true, because what you had was a market for two types of pigs. You had breeders and feeders. I think that in a lot of these sorts of industries, you still do.

Anyway, there was a market for young pigs. Now, one knew in a general sort of way what the costs were of feeding. Well, the price that they paid for young pigs would reflect what they thought the price of the pigs, once fed, would fetch. So you could work out what people's expectations were. We showed that when prices were exceptionally high, they thought they would fall, and when they were low, they thought they would rise, and so on. We could have done, these days, much more than we did. And again, I would like to go back and re-work a lot of that stuff and take old data, because with a computer you can do all sorts of things. It would take us a week to work out things which a computer now can do in seconds. We used the machine that was there before the Monroe.[32] Have you seen a Monroe hand-calculator? You go like this, and you go like this. You pull down these things.[33]

Yes, I've seen that.

Well, there was an earlier one, before that. Every figure was a great effort. This limited what we could do. We also wanted to make a study relating to relationships between short-term and long-term rates of interest; to work out what people's expectations were, but it was too complicated. We collected data. But these days those complications wouldn't matter. So, I hope someday to do some work on expectations. It seems to me that the relationship between one's experience and what you expect to happen is a very mysterious thing.

Do you mean the exact relationship?

Exact relationship.

Because our expectations are obviously heavily influenced by our experience. But you want to know the exact relationship? That's the mystery.

Well, it's a jolly important subject in economics, because people are always producing for what they expect.

Right. That's what it means to produce for the market as opposed to producing for orders as they come in. That's the difficulty. That's the risk.

You can't avoid doing that. You have to get ready for the orders when they come in.

Absolutely.

Well, at the moment, I have so many projects that I am hoping to work on, and the trouble is how to get to each one in its turn, how to do them all.

One at a time.

That's true. But I worked out with someone how long it is going to take me to do these things. We worked out twelve years. And in thirteen years I'm a hundred.

Well, do you think you will be able to retire on your hundredth birthday?

I think I will retire then.[34]

I think it may be a good idea at 100 to relax.

Did you find out how it was that old George died? There's some mystery.

Yes. Claire told me a bit.

Because it was a great shock.

That he had gone to hospital?

It wasn't considered serious.

No. And it sounds in fact that it wasn't. They had taken the monitors off. He wasn't being monitored any more, and he was about to be released. And then there was this sudden reversal … So, who knows? Perhaps they didn't really catch the serious underlying problem. It sounds like they possibly misdiagnosed.

Yes.

And there was just a sudden, unexpected turn.

Well, it was a great shock.

I would think so.

I wasn't expecting anything like that. George didn't exhibit any signs of it in his behaviour. I certainly didn't notice anything. He was a bit stooped. But that was certainly nothing out of the way.

No. Apparently he was busy working on lots of projects and arranging conferences and those sorts of things. So, it did come rather suddenly. Very, very suddenly.

Surprised me. I referred to him when I gave this lecture in Stockholm.[35] The first part of it, before I gave my lecture, I talked about George. But I didn't say anything I haven't said to you, probably.

You know you mentioned institutions. What would George Stigler's take or view of the importance of institutions be?

He doesn't seem to have shown any interest at all. He attacked the old institutionalists on the grounds that they didn't have any theory.[36]

Well, that was right. But, how he thought about theory without institutions, I don't know.

Did he show any interest in an idea of path dependency at all, that you knew of?

I don't think he even mentions path dependency.

No, not in his written work.

We referred to it earlier without using that phrase. No, old Harold [Demsetz], from my point of view, has it right. But Gary Becker's views on George in his later days would be very good, because he worked closely with him. And he seemed to me to be much influenced by Becker. The influence went rather that way than from George to Gary Becker.[37]

I know in some place, I can't recall straight off, where Stigler is talking about Adam Smith, he seems to want to completely separate the ***Moral Sentiments*** *from the* ***Wealth of Nations****, and say that the* ***Moral Sentiments*** *simply isn't important. Did you ever discuss that with him, his insistence that you don't really need the* ***Moral Sentiments?***

Well, you know my argument is the opposite[38]?

Yes, I know it is.

By the way, can I just give you this[39]?

Oh, please. That is a great honor. Thank you. I'll carry it all round with me until I get home.

That's one of the difficulties of traveling. You get given things. No, he was sort of critical of Adam Smith in a way that I didn't think was justified, *Smith on the Ship of State* for example.[40] But on the whole, his work on the history of thought is very good.

Tell me when I'm keeping you too long, OK?

Do you know what the time is?

Yes, 1:20.

Well, I think we should only go on a bit longer.

OK. I know in that same piece, 'How do Economists Choose', you start off by referring to Warren Nutter and then you quote the line from Frank Knight about how in order to achieve objectivity you need competence, integrity and humility.[41] *Do you think George Stigler had humility?*

He didn't show it. He may have had it, but it wasn't that apparent. He always appeared confident, sure of himself, but in a way that suggests to me a sense of insecurity. I don't know whether others felt that, but I did. He was always very nice and kind and helpful in many ways. Always. But I often wondered how far he agreed with what I was saying. I think he thought I was all right, but a little odd.

So, what would he have thought, for instance, of your approach to empirical work? It seems your work at times was very different than his. I'm thinking of the piece on the lighthouse.[42] *It's very much a piece of empirical work, but it's bringing together historical evidence to make a case. This is very different than the standard sort of statistical testing* etc. *What would George Stigler think of it? Would he regard it as legitimate empirical work?*

I think he did. Yes. After all, a lot of his work was of that sort. A different field of course and so on. But, no, I think he would have been positive. It was just that he was so enthusiastic about quantitative measures. He thought that he was going to change the world.

You think he ever modified his beliefs on that?

I don't think so. I was sitting with Aaron Director at the time when he gave his Presidential address and we did look at one another at the time to try to see what each one thought about all of this.

It was certainly a call to arms.

Yes. A degree of optimism about the results of events.

I know he saw himself in the tradition of Marshall. Do you think he was actually a Marshallian in approach?

He certainly was always very respectful of Marshall.

Absolutely.

On the other hand, if you read his first book, *History of Production.*[43] He has a lot of highly critical things to say about Marshall there. But then he says highly critical things throughout that book. I once spoke to him about that book and said something like that to him. He indicated he hadn't read it for a long time, nor did he propose to read it in the future. That was something he had done in the past. I think he questioned whether parts of it were right.

I'm also thinking that Marshall was such a qualifier. I mean that statement of Keynes that he knitted in wool,[44] *that he was constantly qualifying, while George Stigler was certainly not a qualifier. He is much more direct. He's very much the chiseller in stone.*

Yes.

Stigler struck me in that respect, to be very non-Marshallian.

Well, that was Marshall's character, which really wasn't very admirable. But it is understandable. I argue it's the way he was brought up by his father. I mean you can always hear the swish of the birch.[45]

So he had this intellectual flinch?

That's right. That's right. Well, we mustn't talk about Marshall.

George Stigler. He started off his career talking about income distribution and theories of income distribution and he had a firm belief in the marginal productivity theory of income distribution. But he also, later on, in his Tanner lectures wants to insist that it's ethical as well.[46] *Not only that it's in an economic sense correct, but somehow that it's fair or just or ethical. And that whole series of these articles seems to want to insist that the tenets of the market-place are actually ethical and just. Could you understand what was propelling him?*

I obviously didn't read them in any depth as I don't recall that. But it fits in with his notion that the sugar subsidies are all right, too.[47]

Or else there would be something different.

Yes, that's right. If there were something better, we would have it. It's a sort of non-moral view of the system. Where are you going, by the way, now?

Just to the library a few blocks away. I'm actually looking through his letters and papers.

Aah, yes I see. He was a great writer. Yes, he wrote lots of letters and things. He'd write little notes to me and so on. Nothing I think that he says there would be of great use to you.

Well, you've been very helpful. Actually you can, if you would, answer a question about your own work that has always been in my mind. If you go back to the '60 paper and then what you wrote in, what was it, '71 or '72[48]*? You re-visited the subject. And, I'm never quite sure about this point, because in the original paper you seem to say that how you assign property rights will have an income effect, and then in the second paper when you re-visited it, there's an argument that has to do with asset values which says that in fact income distribution doesn't change.*

Yes, I think that is a sort of change of view. If you have a given set of property rights then the value put on them will take into account the liabilities attached to it. I can't remember the details now, but it doesn't matter. But I think you are right in suggesting that there was probably a change in view.

Acknowledgements I feel obliged to thank, belatedly, Ronald Coase for proving to me that there is in fact such a thing as a free lunch. I would have happily bought both mine and that of Professor Coase in return for such a delightful 80 minutes. Of course, I cannot say enough about his generosity in giving of both his time and ideas so freely.

Notes

1. Like many other well-known economists, Ronald Coase (29 December 1910–2 September 2013) lived to a very advanced age. (Choosing to be an economist often seems to provide an unacknowledged elixir of life.) His extended career commenced in the 1930s with his first publications appearing in the mid-1930s and extending until his death, a publication record spanning nearly eight decades. When appointed to a position at Chicago he was already a well-established academic entering his mid-career years.
2. Classical liberalism is perhaps best represented by such luminaries as John Stuart Mill. The methodological approach distinguishing this older tradition rested on a willingness to listen to opposing views and take them seriously instead of trying to dismiss them as expeditiously as possible. These economists also held that it was wrong-headed to formulate economic policy directly from theoretical conclusions. Policy by definition had to be applied to specific situations while theory was inherently abstract and general. Effective policy consequently depended on more than economic factors. History, psychology, politics and other such influences could be equally significant.
3. Stephen Stigler was one of three sons of George Stigler. He is currently Professor of Statistics at the University of Chicago and harking back to his father, interested in the History of Statistical Thought. I also talked to him over lunch at the Quadrangle Club.
4. Claire Friedland served as George Stigler's research associate from 1958 to 1991, essentially his entire career as a University of Chicago faculty member.
5. Sam Peltzman was one of George Stigler's first Ph.D. students at Chicago in the early 1960s. "I was George's student, in some sense I'm viewed as his protégé, something like that" (Conversation with Sam Peltzman, October 1997). After a stint at UCLA (Chicago-West) he was lured back to Chicago by Stigler who greatly admired his ability. For Peltzman's evaluation of Stigler's work in industrial organization see Peltzman (1993).
6. Harold Demsetz (1993) provides a very penetrating view of Stigler's role in establishing the domain of Industrial Organization.

7. In fairness, George Stigler focused almost entirely on microeconomic issues. Though in agreement with his close friend, Milton Friedman, on macroeconomic matters, he chose not to do any serious research in this area or make pronouncements concerning these issues. "George would say 'inflation is everywhere a monetary phenomenon.' This is like taking a personality loan from people whom he admires, who believe that kind of thing. More than most, I think, George kept out of things that he felt he wasn't entitled to an opinion on" (Conversation with Paul Samuelson, October 1997).

   Well, I'm a monetarist in the sense of believing that the control over some money supply is important … I've worked primarily in the area of industrial organization and public regulation, but not in the area of monetary policy … So I don't feel very competent on judging that. I've been told by Milton Friedman that one of the perversities of history is that when the quality of the Washington staff [of the Federal Reserve] is high, policy is pretty poor, and in the years in which the policy has been good, the staff has been low-quality. Now, if you want to explore that, you'll have to interview him. (Stigler in Levy 1989: 2)

8. This is a reference to Milton Friedman's famous 1953 paper on positive methodology. A brief forerunner of this theory appears in George Stigler's attack on monopolistic competition (1949). The two discussed the issue prior to any publication in correspondence between them. Friedman, in conversation with the author (August 1997), happily acknowledged Stigler's contribution in forming this methodological framework.

   I had written the methodology paper, which was later formally published. This preceded, by three or four years, the earlier versions. And he [George Stigler] refers in one of those lectures [1948 Lecture at LSE] to the fact that we had been talking about it.
   How influential were you in each other's thinking on this matter?
   Very influential. I think there's no doubt that my work would have been different if I hadn't been influenced by George and George's work would have been different if he hadn't been influenced by me. (Conversation with Milton Friedman, August 1997)

9. Gary Becker is well known for his pioneering work in extending economic analysis to social and political areas. His co-authored article with George Stigler (1977) "De Gustibus Non Est Disputandum", attempts to extend the boundaries of rational choice.

   I think, they were much more on the same wavelength, especially in this area, 'how far you could push rational choice,' that kind of stuff. George was absolutely enthusiastic about everything Gary was doing, with the family, with marriage, with this, with that, with everything. But the outside world apparently wasn't. And apropos to the outside world, the graduate students at the end of the year would put on a party where they would perform a play: 'The economics of mud' by Gary Becker. 'The economics of 'fill in the gap," by Gary Becker, you know what I mean. It was a standing joke. It has become a standing joke. But George loved that kind of thing. As I do. I mean, I agree with it. I think it is a very powerful part of the message of economics that there's growth in its thinking, and standards and a reason to take economics seriously. But in terms of actually working in that area, the 'Tastes' paper, I told both of them I thought it was absurd. (Conversation with Sam Peltzman October 1997)

Coase himself has been at pains to identify himself with the Frank Knight Chicago approach rather than the Friedman/Stigler structure of the post-war Chicago School.

When you say it [Coase's views on rational behaviour] is un-Chicago, you mean that it is an unmodern Chicago view. Because Frank Knight was at Chicago, and I was brought up more on Knight than I was on any of the others. And my views were quite consistent with what he says. They're not consistent with what George Stigler, Gary Becker, and Richard Posner say. Posner condemns me because I don't think people maximize utility. (Coase in Hazlett 1997: 3)

10. As expected, in this article (1994a [1977]), Coase is much more hesitant, if not a touch sceptical, when discussing the reach of economics as an "Imperial Science". For him, economics is not a set of tools or an analytic approach but is rather defined by the relevant subject matter.

    … what is the outlook for the work of economists in the other social sciences? I would not expect them to continue indefinitely their triumphal advance and it may be that they will be forced to withdraw from some of the fields which they are now so busily cultivating. (Coase 1994a: 44–45)

11. Coase refers here to the International Economic Association meetings held in Kiel, Germany in 1975.
12. In his collection of essays, *The Economist as Preacher* (1982a), the second of four parts in that volume is dedicated to what Stigler refers to as "The Sociology of the History of Science" which contains a number of interesting articles on this particular theme.
13. The volume, *Readings in Price Theory* (1952), printed under the imprimatur of the American Economic Association, is something of a classic covering key articles from the first half of the twentieth century and is still worth a careful and considerate reading. Stigler shared very few characteristics with his co-author of the volume, Kenneth E. Boulding. But at the time, they were both clearly rising young (or youngish) stars of the profession. Kenneth Ewart Boulding (18 January, 1910–18 March, 1993) was a British economist as well as a Quaker devoted to advancing peace. He was one of the founders of evolutionary economics with interests extending well beyond the more narrow scope of the economics discipline. He was also one of a handful of economists

who had to contend with a pronounced stutter, which he did quite admirably.

14. The article is "The Division of Labor is Limited by the Extent of the Market", originally published in 1951 in *The Journal of Political Economy*. The article was then reprinted in an edited volume, *The Organization of Industry* (1968) as Chapter 12.

    The exact quote is:

    Reference should have been, and now is made to R. H. Coase, "The Nature of the Firm," *Economica*, 1937, reprinted in George J. Stigler and Kenneth E. Boulding (eds.), *Readings in Price Theory* (Homewood, IL: Richard D. Irwin, Inc., 1952). (Stigler 1968: 141)

    Readers may wonder, along with Ronald Coase, why reference should have been made here to his seminal article.

15. One such article might have been Stigler (1976). Stigler's approach always starts from the idea of how a rational decision maker reacts to a given stimulus. Whether existing as individuals or institutions, they are abstract entities without a complex inner existence. Agents respond to market incentives, they are not envisioned as active innovators that deliberately seek to change their environment.
16. Duncan Black (1908–1991) and Ronald Coase were both colleagues together starting in 1932 at what is now part of the University of Dundee, but was then the newly opened Dundee School of Economics. This essentially was their first appointments as academics. Economists today would probably remember Black, if at all, for his median voter theory. Coase refers to the Brady and Tullock edited volume for Kluwer (1995). Coase's remembrances of Black can be found in his volume of collected articles (1994b).
17. Coase's (1994c) take on Stigler can be found in the same volume as his look at Duncan Black.
18. The interview with Claire Friedland can be found in Freedman (2012). The piece is worth reading just to get a bit of flavor of Claire Friedland's intrinsic charm.
19. The article, "How Should Economists Choose" (1994d) is also included in the aforementioned edited volume. The essay controversially takes issue with the positive methodology espoused by Milton Friedman, as well as George Stigler.

    If all economists followed Friedman's principles in choosing theories, no economist could be found who believed in a theory until it had been tested, which would have the paradoxical result that no tests would be carried out. This is what I meant when I said that acceptance of Friedman's methodology would result in the paralysis of scientific activity. Work could certainly continue, but no new theories would emerge. (Coase 1994d: 24)

20. At this stage we discussed his seminal work "The Problem of Social Cost" (1960). I'm not sure how many economists have actually carefully read through this article in more recent times. I would suspect that many know it through what has become known as "The Coase Theorem". Unfortunately, this particular formulation is misleading in both its label and content. It could more accurately be acknowledged as "The Stigler Theory" based on an article by Ronald Coase. This take on Coase's thought is discussed in Stigler's autobiography (1988). Coase however during this non-taped section of the discussion pointed out that Stigler failed to really grasp his thinking. In fact, of the many Chicago economists at that famous dinner hosted by Aaron Director, Coase concluded that only Arnold Harberger was able to accurately understand Coase's point, namely his use of marginal cost as the key to analysis in the cases examined. Harberger's insight into Coase's approach is clearly stated in a 1997 interview Coase did for *Reason* magazine.

    I remember at one stage, Harberger saying, "Well, if you can't say that the marginal cost schedule changes when there's a change in liability, he can run right through." What he meant was that, if this was so, there was no way of stopping me from reaching my conclusions. And of course that was right. I said, "What is the cost schedule if a person is liable, and what is the cost schedule if he isn't liable for damage?" It's the same. The opportunity cost doesn't shift. (Coase in Hazlett 1997: 2)

    (Notice that this later work on social cost had in common with his earlier work on the firm (1937) a reliance on transaction costs and the use of marginal analysis.) In further discussion, Coase makes the point that the important issue was that transaction costs were never zero. The consequence was that any useful applied work would need to be done on a case by case basis, investigating the specifics reigning in each instance. An economy on this basis could be under as well as over regulated. However Coase did point out that as editor of *The Journal of Law and Economics* (1964–1982) the overwhelming submittals he received that tested the value of specific regulations concluded that the particular government intervention examined served no positive purpose. Undoubtedly, there were other points of interest, but barring the potential assistance to be gained from a hypnotic trance, no additional points can be dredged up from memory.

21. At least to my knowledge, Ronald Coase never got around to this interesting project despite his long life.

22. Coase (1994d) does try to tackle some of these issues in "How Should Economists Choose?"
23. Thomas Kuhn is known for his pioneering work in the History of Science. He attempted to explain major changes or revolutions in scientific theories in terms of paradigm shifts. He is best known for his 1962 book, *The Structure of Scientific Revolutions.*
24. Coase refers to the Center for the Advanced Studies in Behavioral Sciences which is located in Stanford, California. It was founded in 1954, initially funded by the Ford Foundation. The Center became part of Stanford University in 2008.
25. Coase is thinking of the article "The Function of Measurement in Modern Physical Science" which Kuhn published in *Isis* in 1961. That was one year before Kuhn published the book that made him famous, *The Structure of Scientific Revolutions*. Kuhn was at the Center for Advanced Behavioral Studies for the academic year, 1958–1959. George Stigler was at the Center in the previous period (1957–1958).
26. This bit of name dropping refers to the story of Galileo and the Tower of Pisa. To disprove Aristotle's contention that objects accelerated according to their relative masses, Galileo took two balls of similar size and shape, but of different masses, and dropped them simultaneously from the top of the Tower. It is true that this was a thought experiment that is documented in his written work. Whether he actually performed the experiment is an unresolved question as is the supposed result. In one version the two balls hit the ground almost, but not quite at the same time, which either did or did not confirm his hypothesis. Galileo, who had formed a given expectation, would have undoubtedly perceived his supposition to have been upheld.
27. The Society is still going strong and will hold its 18th annual conference at Duke University in June 2014.
28. I am being a bit flippant here. However, Keynes tended to treat economic history and the history of economic thought a bit cavalierly, to say the least. Like other economists of his day, he put great faith in the logic behind his statements and perhaps at moments tended to assert what needed be true. This isn't to deny the insightfulness and value of his work.
29. Muth's famous article appeared in 1960.
30. Kaldor invented the name in a 1934 article in the then newly established *Review of Economic Studies*. The journal grew out of a desire

by young English economists of the time to have an outlet for their more mathematical approach to economics, an approach not then fully appreciated by the editors of the more staid *Economic Journal*.

31. Ronald Fowler was a fellow student with Ronald Coase at the London School of Economics. Not having studied economics at LSE, Coase attributes what he knew about the subject to fellow students like Fowler as well as his most influential teacher, Arnold Plant, whose seminar he attended. Those interested in the pig market should see Coase and Fowler (1935a, b, 1937).

    > I had not studied economics at LSE and had picked up what I knewabout economics from discussions with a fellow commerce degree student, Ronald Fowler, with students who were economics specialists and through attendance at a seminar with Arnold Plant, who was a Professor of Commerce (Coase 2006: 258–259).

32. The relevant instrument might have been a Burroughs Calculator or perhaps even a Compometer. Didn't seem at the time worth any detailed enquiry even had Roland Coase remembered.
33. Ronald Coase is here miming the actions one went through when using the hand calculator. However, those with an undying thirst for knowledge can resort to YouTube for an actual presentation of what is listed as the Monroe Mechanical Calculator and watch someone actually add with it (http://www.youtube.com/watch?v=EozhU6rybHY Monroe Mechanical Calculator). Or, if you are someone who is easily overwhelmed by the romance of such devices, there has been a 1930s Monroe Hand-Crank Calculator L160X Executive Compact on ebay for a mere $144.08. Not as useful as a five dollar calculator, but certainly a conversation piece.
34. This particular hypothesis, "retirement at 100" proved to be false in the case of Ronald Coase. He continued to work past the age of 100 (see Coase 2012; Coase and Wang 2012, 2013). He planned to start a new journal, *Man and the Economy* with Ning Wang of Arizona State University. The first issue was slated for April 2014, to be published by De Gruyter.
35. This refers to Coase's 1991 "Nobel Prize" speech reprinted in *The American Economic Review* (1992).
36. He had absolutely nothing good to say about institutionalists, dismissing them in his autobiography (1988). For Stigler, they served as a punch-line for a variety of his caustic remarks.

> John R. Commons wrote on the legal foundations of capitalism in a book that I believe is impossible to read. Clarence Ayers started a school in Texas that never got beyond the state lines. (Stigler speaking in Kitch 1983: 170)

37. Coase is not alone in holding this valuation of the intellectual relationship between Becker and Stigler.

> I think that George was more broad minded than Gary Becker, and so a collaboration between them would have been good for Gary and bad for George but that's a purely an a priori statement. I really don't know what it was like when they worked together. (Conversation with Robert Solow, October 1997)

38. One place where his argument is clarified is in Coase (1994e).
39. Ronald Coase presented me with his book, *Essays on Economics and Economists*, which has been referred to in various footnotes of this conversation.
40. Coase refers here to "Smith's Travels on the Ship of State" (1982b), to which he might have easily added "The Successes and Failures of Professor Smith" (1982c).
41. The exact quote is "The presuppositions of objectivity are integrity, competence and humility" (Knight quoted in Coase 1994d: 15).
42. In economics, the lighthouse was often employed as a clear illustration of a public good. Coase (1974) examined the archival records of lighthouses in England to determine the extent to which this contention held true. He found evidence that points in quite the opposite direction.

> Samuelson says I was wrong and he was right, and he froths at the mouth when people talk about the lighthouse example. He says Coase is wrong; he doesn't overcome the free rider problem. Who are the free riders? The foreign ships going past the British coast which do not call at a British port. Using Samuelson's approach, what do you do? Do you ask the foreign governments to give you a subsidy? Do you tax people in Britain because the foreign ships are getting help without paying for it? What do you do? My approach is to compare the alternatives. People like Samuelson like to set up a perfect world and say that the market does not bring us to this point and imply that the government should do something. They stop their analysis at that point. (Coase in Hazlett 1997: 6)

43. The reference here is to George Stigler's first published book *Production and Distribution Theories* (1941). The book is based on the dissertation he completed in 1937 under the guidance of Frank Knight, one of the very few graduate students to complete his dissertation with Knight as his advisor. It is a bit of history of economic thought analysis with theoretical approaches analysed from William Stanley Jevons to John Bates

Clark. The work was apparently heavily influenced by Knight, which is perhaps the source of Stigler's later discomfit.

44. Keynes wrote a number of biographical sketches. When describing Jevons, Keynes summed up his approach by using the following metaphor, "Jevons chiselled in stone where Marshall knitted in wool" (Keynes 1951: 241).
45. The flinching reference and explanation (caused by a well-aimed slipper) can be found in a short piece on Marshall's father and mother (Coase 1994f) in his book, *Essays on Economics and Economists*. The volume contains a number of interesting, entertaining and witty pieces on Marshall.
46. Stigler gave his Tanner lectures in 1981 and they are reproduced in his collected volume, *The Economist as Preacher and Other Essays.* The relevant quote is in the second of these articles (Stigler 1982d).
47. In an article published posthumously (1992), Stigler extends the idea of consumer sovereignty to the political market place. The sugar subsidies have been in place for many decades. If the voting public was dissatisfied with these laws, then voters would have elected representatives to repeal such stipulations. Therefore, if voters wish to redistribute income in this way, economists lack any authority to say they are wrong to do so. Consumer sovereignty must hold no matter what the results might be. Stigler referred to this particular conundrum as "the paradox of legitimacy". He was grappling with this problem throughout his last years, sometimes to the dismay of his usual intellectual allies.

    > Then he got into this – I remembered his name for it after talking to you – 'paradox of legitimacy' he called it, or sometimes he called it the '*problem* of legitimacy'. At the time of his death, this was one of the problems he was working on. It was *very* much of a concern to him. You have to remember the kind of public persona he had. If something bothered him a lot *I* saw the side of him that said, "I don't know what to do about this problem!" But the rest of the public saw that other side, "Here's what I've *done* about this problem and isn't it convincing." But he was very much concerned about how you could call something inefficient in the political arena. We do have a democracy, more or less, we have representative government, then how can something that has been allowed to happen be inefficient? After all, that government has allowed some programs to go on and on, year after year. (Conversation with Claire Friedland, October 1997)

48. This comparison is most easily found in the volume of collected articles *The Firm, the Market and the Law* (1988). In this volume, the original article on "The Problem of Social Cost" is followed by "Notes on the Problem of Social Cost" which takes up some subsequent issues raised by the initial analysis.

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The Chicago Battler: Sherwin Rosen on George Stigler, Chicago and Economics

Craig Freedman

This conversation took place in October 1997 on a rather hot day for so late in the year. Sherwin Rosen's office gave proof to the fact that the economics department at Chicago doesn't believe in pampering their faculty. In the background an ineffectual air conditioner was generating noise, but little in the way of cool air. However, despite the heat and humidity, I was met initially with a rather frosty reception. Unbeknownst to me, a then Associate Professor at Murdoch University in Perth, Australia had submitted a controversial paper to the *Journal of Political Economy* edited by members of the economics department at Chicago. The paper attempted to demonstrate that George Stigler and Milton Friedman, the two patron saints of the department, had single-handedly put a stake in the vibrantly beating heart of the Theory of Monopolistic Competition. The two were branded as virtual hucksters.

Whatever its merits might have been, sending such a paper to Chicago was not far different than sending a thesis on the non-existence

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of Christ to the Vatican. The immediate question that arises is why anyone would bother to do so. In any case, also arriving from Australia, I had somehow managed to be tarred with the same brush, at least in the mind of Professor Rosen. (Though Perth was hours away, by plane, from Sydney.) It took then some time and patience before his suspicions were sufficiently allayed and we could begin discussing issues that proved to be mutually interesting. We both become more comfortable with each other as the conversation ensued. At one point [not transcribed for obvious reasons] we even discussed whether Sherwin Rosen should leave early to tend to his wife who was feeling ill. (For the idly curious, I urged him to go home.)

As you will see in the interview, Sherwin Rosen was out of the room when they explained the concept of 'sugarcoating'. If nothing else, his opinions are honest and forthright. He says exactly what is on his mind, which is in part what makes the following conversation of continuing interest to the reader. As stated, there is a distinctly prickly feeling to some of his answers as we both seem initially to be taking in each other's measure. You can also detect that after the unfortunate beginning, we became quite easy with each other. Clearly toward the end, we are both enjoying the conversation and finding it of considerable interest.

The Interview

Just to provide some background information, can you tell me a little bit about how you got to know George Stigler, the influence he had on you …

I was a graduate student here when he was a faculty member.[1] I took a course from him. But I studied his text book before that as a student.[2] It was the first serious economics textbook that I ever read, and I read it very carefully. I followed a lot of his work. My field is microeconomics. I do a little bit on Industrial Organization, but I'm primarily a labor economist. So I wasn't exactly a disciple or a follower of Stigler in the sense of being a full time member of the Industrial Organization community here. Then we became very close personal friends when I moved back to Chicago some twenty years ago.

The two of you then would have general discussions?

Yes, I talked to him all the time. We had discussions during social occasions and while group teaching. We had countless lunch time conversations. Altogether, we discussed a lot of Economics.

You would show him your work?

Sure.

He commented and on it and gave you feedback?

Yes.

I imagine that his comments were very valuable.

Usually I didn't find them too valuable, because he was such a very *critical* person. He would find the flaws in what you were doing, but he would never find any virtue. Or if he did, he would not exactly tell you about it. [laughter] So I found that he was a very difficult person to deal with when we discussed my work.

Do you think that was because …

To begin with, my work wasn't exactly down his alley. So it was a little bit in a different field.

Was he simply not too interested in such problems, or was it…?

No, he had enormously general and broad interests. He was interested in damn near everything. He kept abreast of many, many areas of economics. He was an editor of our journal for twenty years.

Yes, but can we go back to his critical nature? That seems to be a constant throughout his dealings with other people. Did he see himself as being destructive? What in fact drove him to be so negative when reacting to other's work? Do you think that his critical approach had a general negative or positive effect?

Firstly, he was a great intellectual leader at Chicago. He was very essential in creating a certain intellectual atmosphere that existed here at that time. He really stirred things up during our time together in this faculty. He just livened things up. He said what he thought. Sometimes, I didn't necessarily agree with him about what he thought, particularly when he was talking about my work. [laughs] But one got used to the fact that he wasn't going to do a whole lot of back patting. He wasn't going to go ahead and tell you what a great person you were. And he didn't. I don't think he did that to anybody. But he was loyal to me and loyal to everybody he was friendly with. I think he supported us, but probably more on the side, probably through the conversations he held and in other ways. I don't know for sure. I don't think he was trying to put anyone down by making people out to be idiots and showing himself to be their intellectual superior. That wasn't his intention. He just was very critical. He said exactly what he thought and he thought that most things were probably wrong! And you know what? He was probably right! [laughter] But that's part of the culture at the University here also. This is a *very* critical place. So, you need to place him in that culture. I don't know what he was like when he was younger. He was a student here, so he grew up in and was molded by this culture, here at Chicago. Friedman, who was George's very close friend, was in this Department for years. He is a very critical fellow too. He also has very strong opinions and is not shy when it comes to expressing them. And both of them were often right. [laughs] So I guess that's where their reputation and approach was formed. I don't want you to think I'm saying that George was a *nasty* person. He wasn't a nasty person at all. He was very intelligent. He had a tremendous wit, and would make jokes all the time. When he was being critical, he would be smiling about it. So one had to get used to that critical attitude. It definitely had a very positive effect on the intellectual climate here. I miss that a lot. I don't see that we have a similar intellectual figure here today. I'm talking about someone that people gather around and want to be around. Someone who makes other people want to talk about what he is interested in and so on. There's nobody like that today. I mean, we have some pretty top economists here, but I don't think there's anyone comparable to George Stigler. I think he was very unusual, even among his own peers.

I'm interested in what you said, because a number of people have mentioned his loyalty. They claim that he would help not only colleagues, but his *students as well. Often he wanted to remain anonymous. He didn't want to make it directly known to the individual involved. Do you think there was some basic shyness about George Stigler?*

No, he was not shy in any way, shape or form. I'm not familiar with these anonymous gestures you've mentioned. He never shared that with me. I wasn't directly aware of such actions, although I believe I have heard other people say that and they believed that it was probably true. To me it was a generational thing. In his generation, that's what you did. You didn't beat your breast and go on TV. That was not the manly way of doing business. My father was like that. I think a lot of old timers were like that. [laughs] Different cultures. So I think that just would be the *custom* of his generation. I kind of like it better in some ways.

If I can now pick up on some of the things that you pointed out in the JPE article[3] *that you wrote about him?*

Sure.

I found that article interesting on several levels. One is the ideal of specialization in Economics.

Yes.

Certainly Stigler was very clear in his support of the ideal of specialization ….

What do you mean?

He didn't favor broad generalists, but instead a very strong market idea. You need to develop a division of labor as the profession expands and the amount of knowledge and technique you require grows. Major gains are then to be had by narrowing down your focus.

I never came across that myself. I know he had an interest in that idea as an aspect of the theory of industrial organization generally. I mean, *every* Economist does. It's straight out of Adam Smith.

Yes, straight out of Smith.

Not that much actually has been done with the idea. Everyone can see what a great idea it is, but nobody knows how to apply it practically.

George did try.[4] He was successful in some ways and unsuccessful in others. But as an organizational basis for Economics, I don't know if that's the way it really is. I mean, people can't know everything there is. [laughter] But I never heard him talk much about that issue.

There is an article he wrote, or it could have been a talk. I can't recall it straight off.[5] He addressed this ideal of specializing. First he looked at interdisciplinary studies and then within a given field. He took a very standard economic view of it. Yet the problem remains, namely how far can you actually push that line of thought? Or the issue might be, do you want to push it at all? Of course, if you go back to Smith, he would say, 'Yes, it's very useful.' But equally, if you get too narrowly specialized the workers in his famous pin factory become absolute dolts, because they can only do a very limited part of the production process.

No. That was Marx.

No. I think it's in the third book, where Smith talks about the need for education because otherwise they become[6] …

Oh, the need for Military?

The need for Military fitness yes.

No. I've read that. I wouldn't interpret it that way. I would interpret what you just said more as an offshoot of Marx. I've never actually read Marx to any extent. But that's not important. [laughs] I agree with it actually. If you know too little then you're just a putz. [laughs] What I'm trying to say is, if the span of your knowledge is too narrow, then you get shut out of a lot of other things that are interesting and might be useful to your research.

But he also seems to be saying …

Everybody knows that is true, I would think. [laughs] I don't see that is a particularly insightful contribution by George.

But he also then wanted to say that Economists, it seems to me, have nothing to really gain by exchange with other disciplines, other social sciences.[7]

I don't know about that. I think he changed his mind on that because he was *very* close friends with Gary Becker. Are you going to talk to Gary?

Definitely.

Gary is into *everything*, so he's very inter-disciplinary. He and George were much closer in every way, shape and form than I was with George. George was a tremendous advocate and promoter of Becker's work. I don't know for sure, but I don't think he did much of that type of work himself. He encouraged other people to do it.

But that's more of a transfer of economic methods to other disciplines. I suppose the question is whether there is anything to be gained by talking to the actual practitioners of those disciplines?

That's not such a simple question. Economics is an abstract field. This is one of those questions that seems to offer a simple choice. 'Who's for it and who's against it.' But when you do research there are forests and trees. Different levels of work exist out there. Situations vary. There are people who go to the shop floor to interview workers. There is a very good theorist right now, I'm using theorist in the most abstract sense, who's been interviewing CEOs and asking them what they do. So there's that sort of work being done. It's always gone on. Some British guys did surveys asking people whether they were maximizing profits.[8] The response was, 'No!' [laughs] So there was a lot of hand wringing about that. Then people said, 'Aha, maybe they don't *think* they're maximizing profits but they *act* as if they were maximizing profits.'[9] And you know, that's probably a more accurate description and it does tend to work. So I don't think there's been a lot of ideas imported into Economics from other fields. Maybe it is vice versa. Maybe it does go more the other way around. There's been some importation of economic methods into political science, but only very recently.

Basically Anthropologists and Economists have almost exclusively ignored each other. There's been very little traffic between the two fields. Frankly, there could be more. Let me think about the other disciplines. In Sociology I think there's a lot of common interest in the problems of interpreting data from survey work. There's been some traffic there, but largely of an empirical nature. There's also has been some mutually beneficial flows with Statistics. But I don't know of all that much else. And Chicago is recognized as an inter-disciplinary University. We don't interact much with Chemists, Physicists, or any of those guys. Basically there's no exchange there at all.

But the concerns of the Natural Scientists are more removed of course than those of Sociologists and Political Scientists.

Well, I'm not sure. They're involved in a lot of political stuff these days. Look at their social activities.

[laughter] Well, yes. I don't know if that's part of their discipline profile, but yes, you could say that.

[laughter] The fact is that what we've just said is specific to the economics department at Chicago. We're more inter-disciplinary in many ways than other departments, probably, in the world. That is Becker's influence.

Yes.

So I think Chicago has been much more open to these kinds of approaches. Knight talked about it and Knight was a great mentor in the department. Before Knight, apparently Veblen when he was here was attracted to a wide number of different interests as well.[10] [laughter] So this place has been very tolerant.

I was interested in your mention of survey work. What was George Stigler's attitude to survey work?[11] *Did he see any value to it?*

It just never came up. A lot of the data that we had was from surveys. George was intensely empirical. Nobody was as empirical as George, considering how good a theorist he was, one of the most empirical economists of his day. There are very few theorists who have that much of an empirical insight into the empirical side of questions. He certainly used all the data he could get, surveys, whatever. Now, I was thinking in the sociological sense where you've got people asking questions to gather census data.

Yes.

This is absolutely essential in my field. And George would use it. If however I saw data based on responses to questions like: 'How do you feel this morning? What do you feel about Richard Nixon? Did your wife and you have an argument this morning?' I don't pay attention to *that* kind of data. There is this definite bias in Economics. You see what people *do,* not what they say. Because, you can never competently judge their motives, or what is in it for them. [laughs] You've got to study their behavior, pure and simple.

Only what is observable.

Right. I'm not saying that a little bit of that isn't kind of interesting. There's some of that concentration on motives in Behavioral Economics now.[12] I don't think you can go very far with it, looking at other aspects of behavior, at what else can motivate people.

How tolerant or open-minded do you think George Stigler would be to an alternative like Behavioral Economics?

I don't know. It wasn't really around much in those years. It has grown really rapidly. I think it's bullshit, myself, so I don't follow it too much. I think it contains a little bit that is of some interest, but I don't feel that it's gotten anywhere. There's nothing there that is useful for me in the work that I do. I certainly don't use it in my work. But a lot of people, I don't think *too* many, but a significant number of people might feel differently about it than I do. I don't know. But who knows whether we are looking at something that's a trend or simply a fashion? I can't tell.

Sure, there are lots of fashions that come and go.

Right.

Let's get back to the empirical aspect of his work. From very early on, I would say starting after World War 2, George Stigler is pushing the profession very hard to test its conclusions using empirical data, not to simply assert them. Did George Stigler in fact change his mind due to empirical data?

I don't know.

Or was it more…

I don't know where his views came from. That's a hard question. Who knows about things like that? I know he changed his mind. He writes about that in his autobiography regarding anti-trust policies.

Yes.

But a lot of people changed their minds about that, not only him.

Yes.

What that means, I don't know. I can't tell you why he changed his mind. I don't know how his mind worked. How would anyone who didn't know him very well in those days be able to make even an intelligent guess? [laughs] I don't know how people change their minds. I don't even know how I change my mind. I'm an old man now.[13] I know a lot of stuff I've learnt over the years. So, if someone comes up and tells me now that everything I know is wrong I tend to be defensive. I naturally believe that the claim is probably erroneous. [laughs]

Given your lifetime investment…

That's right, given my investment, given what I've read over the years. When somebody tells me now that an increase in the minimum wage increases employment, there's just been a study out on that, I'm very skeptical of that claim.[14] I don't believe it!

To jump to another topic, in your article you talk about George Stigler and his attitude to History of Economic Thought. You mention the fact that although he had a great deal of interest and did a great deal of work throughout his whole career on the history of thought, he was one of the leaders in pushing towards having it dropped as a requirement in Graduate School.

Now, I don't know if he was a *leader,* but he was certainly in *favor* of it.[15]

Definitely in favor of it.

I don't know how that issue first came up. I think it came up because most young Economists had lost interest in it and it was just a big tax on everyone's energy. No-one was working on it. It wasn't really much of a research field. I think it's had a slight come-back, but it's still pretty small. Even now, you see very few Ph.D.s with that particular interest. There's also been a general laissez faire attitude around here about graduate degree requirements. So I think that's how it came up for discussion. In the same way, there was an Economic History requirement which I don't think we have anymore either. That was also dropped. So I don't think he was in favor of putting artificial [laughs] restrictions on a degree. That was probably his motivation. But I don't know. I don't think *any* of us were too happy about that move.

I remember what he wrote in his article 'Does Economics has a Useful Past?'[16] *In one sense he seems to say that studying the History of Economic Thought takes the edge off your ability to promote a theory. Because when you look at history and you see its ambiguous past, you can then understand all the different sides of an argument. He says that in Economics you really have to sell your ideas in a hard headed fashion. Stigler claims that one of the problems that Mill had is that, unlike most other Economists, he was too fair minded.*[17] *As a result, people tended to under rate his work. In several places he pushes this idea of the need to attack opposing ideas without exception in order to push your work. I suppose he is saying that you have to really be a salesman to be successful.*[18] *What's your reaction to that sort of approach?*

I don't know. Look, intellectual activities are a kind of business.

Sure.

Marketing is part of that business. There's no doubt that some people are able to market their work better than others. Or perhaps, it somehow catches on for reasons that are hard to say. I *hope* it catches on when it has something to it. I don't think marketing is all there is to acceptance.

No. But it's certainly part of the business.

If you happen to have a product that is no good you might market it well but it's going to fail. So I do think marketing is very important when undertaking an intellectual activity. It's probably more important *now* than it was before. The market is much bigger [laughs] for one thing. You have people making serious money out of marketing ideas and getting reputations from successfully doing that. But it's not only the money. It's the associated prestige and all kinds of other things as well. It seems to me that there's a lot more score keeping these days. Unlike in previous days, Economists are now reckoned to be in a pretty interesting discipline. So to succeed, you've got to be able to sell your ideas somehow. How do you do that? I don't really know. Maybe you do attack other people. That's been done over the years, but it doesn't always work. It doesn't work if there's nothing behind your attack.

Let me just follow up on that because in your article you talk a bit, towards the end, about George Stigler and his reaction to monopolistic competition, which was a very pitched battle.

There's his lecture in London,[19] which really is all I've ever seen. Leeson claims that a huge fraction of the first edition of his book was devoted to monopolistic competition. That surprised the hell out of me. I had a hard time finding a first edition. When I finally did, Leeson was just dead wrong. There's hardly any discussion on monopolistic competition in the first edition. I have the second edition and the revised second edition. So I didn't see what he was going on about. Sure, there's that vitriolic piece about monopolistic competition in those lectures. But that's about it, isn't it? What else was there?

There's a short reply to Archibald.[20]

Oh that, yeah.

In his autobiography he also talks about monopolistic competition. Surprisingly enough, he said that it was totally dead and buried.[21] *I think you would have a hard time today, claiming that to be entirely correct.*

I wouldn't. No.

It has been used by a number of economists. The model, or at least a variant of the model, has been used in work done on international trade, at least.

That work is dead. That work is burned out.[22]

You think?

Not 'I think'. I know.

You know, OK.

[laughter] Nobody's doing that! [laughs]

Well then I'd say it's dead.

[laughter] Nobody's doing that. I thought that was a very interesting line of investigation. I know one of the originators of it. That was Kelvin Lancaster.[23] But I wouldn't say that what he was doing qualifies as monopolistic competition. Maybe it does a little. But I don't think that came very much from Chamberlin's work (Chamberlin 1933). I don't know. I'm not the person to speak about that. I know that a paper that does get a lot of cites is the one by Dixon and Stiglitz.[24] The reason that their paper gets a lot of attention is that it's a very convenient formulization for theorists to work with. You can put it into a general equilibrium model. The inability to do so is a problem from a general theorist's view. This is true for oligopoly theory in general. That's why monopolistic competition and related models simply are not used much. It's just terribly complicated to manipulate. Then, here comes Dixon and Stiglitz with this nice little example. But it's too damn fragile. I mean it's got an enormous number of assumptions, like you spend the same proportion of your income on all goods. [laughs] That's very important to that model. So is the simplicity of 'all goods have unit income elasticities'and assumptions like that. I mean, it's a nice model. But it raises the question then about the use of such formalisms in economics. Obviously one doesn't really suppose that people spend the same proportion of their income on each and every good.

Of course not.

But that paper has something that people find useful when using a general equilibrium framework. I reviewed that paper for the journal in which it was published. I thought it was a very good paper actually. One of the better ones I ever reviewed for that journal.

So would you see any possibility that some form of monopolistic competition ...?

See, no empirical work has been done with it. It hasn't gone anywhere. That's simply the case if you try using it to do trade theory. The empirical content of it isn't very clear. So, it hasn't been pushed. I think that is why this new trade theory stuff died. And, I think that's why monopolistic competition died the first time. I don't think Stigler had a God Damned thing to do with it. I don't think one intellectual in an academic community has that much power. Sure he was against it, but Jesus, nobody at Harvard was. Or for that matter in England, where the conventional kind of Marshallian price theory went to hell. I mean Marshall invented a lot of that stuff. [laughs] Yet it was gone by World War 2. You couldn't learn it there. You had to come to the US to learn about it. That was shocking.

Stigler certainly saw himself in the Marshallian tradition.

Yeah. Friedman too. All those micro-economists, and I guess I'm a student of theirs, feel that way. I think younger people don't feel themselves so attached to that tradition. They are more concerned with the strategic aspects of theory. They're attracted to political economy, game theory and so on. They've become fairly divorced from that Marshallian tradition.[25]

If we can look at another aspect of Stigler's work … You mention, I think in the same JPE article, that Stigler came to modify his theory of regulation, his idea that regulation could all be reduced down to a question of special interests …?

I don't know if he had that sort of a reductionist view of it exactly. But he certainly thought the issue of special interests was very important. I mean he invented the approach.

Yes.

And it's a very tight theory.

In fact, it seems to me that in one of the very last things he had published, a piece called 'Law or Economics'[26] *he's very, or at least my understanding of the piece is, that he's very strong on insisting …*

I don't know that piece. Where was it published? What was it about?

At least in the beginning of the article, he talks about, say, sugar subsidies. Economists for years have been pointing out the expense of implementing these policies. Stigler himself did empirical work showing how costly these *things are. But in this article he concludes, 'Well, there's a test of time at work here. If it's been maintained all these years, that must mean that for whatever reason these subsidies are a politically optimal way to redistribute income, because otherwise there'd be a shift away from what would be perceived as a policy that does not work. So that, in fact, there's no sense in appealing to the public, as an economist, for policy changes, because that's all a matter of the relevant special interests involved.'*

Yeah. Well, I never went along with him on that. I mean, I appreciated his point. But I think there are a lot of disagreements about this issue. For instance, why did the Soviet Union last so long? Why did it fall when it did? I don't think anybody knows. I think that when the Soviet Union fell, it was very surprising to most people. If you look back at the event and ask why it happened, I think it's very hard to understand. So there has to be a bit of politics involved. I don't think it's all so nice and tidy. When I see Milton Friedman's ideas about market privatization and all that type of discussion, I don't know. How did that happen exactly? Why did the prevailing special interest shift to the right! [laughs]

Well, I'd like to know that too.

So I've never been a complete supporter. My criticism of that stuff was that while a lot of things are regulated, a lot of things aren't regulated. Lawyers have licenses. Why the hell don't economists? I think we *need* licenses. So long as there's a grandfather clause.[27] [laughs] So I thought, for every tariff, you need to try to understand that there were two sides involved in that. It's important to try to understand why there are tariffs on some things and not on others. By the way, I don't think that special interest approach is that successful.[28] I don't know, there just seems to be too many possibilities that are ignored. It basically seemed like a better idea and prospect than it worked out to be. People are still very interested in it, and they're still working on it. In Political Economy it continues to be a real big thing.

Stigler's position always struck me as in a sense odd, since it was made by someone who spent his whole life trying to persuade people, especially people in the profession. I don't think he would say that he was trying to simply shape *special interests. He wouldn't want to think that what he was doing was only appealing to special interests to try to convince people in the profession.*

I think you should talk to Becker about that. Becker and he shared very strong views about that. I mean everyone sees the virtues in the theory. We needed a positive theory of government. That's what economics lacked and still lacks. They pioneered that. Since then, a lot of people have come into that field. Economists were arguing against tariffs for years. Other people didn't find their arguments very persuasive. Why? I think that's what motivated George and still motivates people doing this stuff. I think it's important. You know that the fastest growing sector in the economy in the twentieth century is the Government. It's had an enormous growth. Nobody understands why. That's what I think motivated him. He wanted to try to understand that. That's why you need a theory.

Of course.

Not the standard, here's how things fail and here's what the government should do to correct it. Nobody believes that governments work that way anymore.

No.

If they ever did believe that. I don't think a lot of people try to support that position.

I think that maybe the most important thing Stigler did was to raise the issue. Perhaps that's even more important than whatever was tendered as potential solutions. That way people had to confront the problem.

Yeah, maybe. I don't know. There are other people doing it, so maybe there's something to it.

Can I ask you one last question before I go?

Sure.

It seems, and this comes out a bit when you talk about the complexity of some of these issues, it seems … and I can probably see why … there's a reluctance to accept any sort of ambiguity. This is true in your work and in

Stigler's work. But in fact, to me, the world has always been and people's motives have often been very ambiguous.

I think you're barking up the wrong tree here. All this business about assumptions that was discussed in the '50s.[29] We went around that block back then. Trying to get at people's motives in a better way. That hasn't been very successful. Maybe it didn't work because people haven't tried it properly. Or people haven't tried it properly because they haven't been able to think of any way to make it work. I don't know. So, I think the emphasis on behavior, about what people actually *do* is the only thing you've got. Are people irrational? Probably, sometimes. But I don't know if that's going to concern me when I think about what happens when OPEC raised the price of oil. You can ask whether people really have feelings about the Arabs. You can worry about what these are. But, I don't see where that's going to help you out.

No.

For most of the problems that *I'm* interested in, those issues just don't sort of come up. I don't need 'em! Most people, at least most economists don't need them. That's what gives economics its power. It *is* less ambiguous than other disciplines. [laughs] It has the guiding principle that people do the best they can, given what they've got.

It seems that you can take that as a very reasonable and useful assumption.

Well, that's exactly it. If you don't have that as a starting point, you ain't an economist. [laughs]

Absolutely. But the question is …

It's the organizing principle.

Of course. It's there because it's worked.

Right. Well, at least seems like it's worked, sometimes.

But the question is how far do you actually want to push it?

As far as you can! That's what you're bound to do. That's your obligation as a citizen of a profession. You want to push it until it gets right up against a wall. So you want to push it everywhere and in every way, shape or form. That's the loyalty oath that you take to your profession. I don't think a lot of economists do it. They don't have that sort of loyalty. You've got to take your ideas as far as you can. You've got to push them until they fail. There are a lot of places where I think economics does fail. But you can't just say, 'Well let the Sociologist handle that.' They're *worse!*[30] Or they certainly ain't any better. There's a blood and guts attitude here in Chicago about Economics. That's what makes the Chicago School. That's what I think the Chicago School is about.

Yes.

It's a sort of a 'Marines' approach to economics. Stigler was certainly one of the leaders of the Chicago School. I think that's what distinguished the Chicago approach. We take what we do very seriously. And we take it as far as you can. You reinterpret the world. Becker has made a tremendous career for himself out of doing that with great success. That's very much in the spirit of what's been going on here. I think, to some extent, George was a product of that environment when he was a student here. He certainly had a big effect on the way economics was done, and not only at Chicago.

Very much so.

I've been associated with it for many years. I was a student here in the early '60s, so there's been that stream here certainly since that time. There've been other major streams too, but that particular stream has been pretty important here. It's in the finance stuff that's gone on here and certainly in the micro. So I don't know if George is unique in his association with those ideas.

Perhaps more successful?

Oh! He was very successful. But Friedman was also a very important member of the Chicago School. He was also a very good empirical economist, one of the best.

Pause (Interview resumed)[31]

You tell that Mr. Leeson[32] when you see him, that to claim that Stigler in some sense held back the acceptance of monopolistic competition, if that's the argument he's making in his article, it doesn't make sense. I found that ludicrous in the sense that, did Friedman for example hold back Keynesian Economics? [laughs] Well he didn't!

No.

He criticized it and eventually his criticism gained increasing interest. People got very interested in those types of arguments and other people, like Bob Lucas and others did some work on the problem. As a result, economics is quite a different kettle of fish now than it was back then. To me, the issue with monopolistic competition was that it didn't have any content. It wasn't used. That's what Stigler basically is credited with saying. He said you didn't need it to explain any of the data. If you claimed that you did, then where are the convincing empirical studies that depend on monopolistic competition theory? Where are they? There aren't any! You know, you can argue about these *religious* matters, and there is a certain amount of religion in intellectual behavior. You know what I mean, the 'true way' of thinking, and so on. But there's also another aspect to this issue. The accepted approach might tell you not to go ahead with some particular line of analysis. But if what you're doing is very useful to you, you're going to say 'Fuck you'. You're not going to pay any attention to me, or to people like me. Stigler just didn't have that much power. For Christ sake, he wasn't even in a great university until …

Until 1958.

No. [laughs] Well Columbia was pretty good. It took him a long time to get power and I don't think he ever had that much influence.

Well, put that way, sure.

I think if people are doing something that's useful, then people will naturally pick it up. The fact that Dixon and Stiglitz' articles were important to a number of people arose from the fact that theorists don't know how to deal with oligopoly and their work provided a convenient way to put markets and professions into one single framework. You could say the same thing about trade theory. But that particular theory has come and gone. Nobody's working on it any more. It's dead. Trade theory is a very cold subject right now. I think very few young people are going into it. That's the true test. I don't really know. Maybe in Australia, they've got a lot more going into it. But in the US, that's not true.

Where are the young people going?

Maybe macro, I don't really know for sure. They're doing game theory, Political Economy. But all that interest in trade stuff has gone for some time.

Until the next remodel comes along.

I don't think it was ever that much different than the old model empirically. [laughs]. I guess it has different social welfare implication in the effects of tariffs and stuff like that. Tariffs aren't so bad necessarily. But even Krugman[33] today argues against tariffs.

Very strongly.

So I don't know how he reconciles that with his life's work, his trade theory model. But evidently he can.

I think he uses that model for presenting a theoretical possibility. There doesn't need to be any empirical work to demonstrate it.

That's *exactly* the point!

That's his position.

Yeah, well he pissed off his own work. I don't completely understand what happened. …

He writes for the *New York Times* now. He's a very popular writer… That was another distinctive aspect of George. George was a *terrific* writer. He had real *style.*

Very persuasive I thought.

He wrote all of those books. They're not exactly academic pieces but terrifically interesting. I loved those pieces. There is really no room for that type of writing any more. Maybe some people just do it as a hobby [laughs]. I just don't know. By the time I really knew him he wasn't doing much of that.

A lot of those things in the '50s are extremely interesting.

Yeah. *Why Economists are always Conservative,*[34] *Truth in Teaching.*[35] I wish I could write like George.

We all do. He seems to have been very persuasive. His writing is certainly very persuasive. He seems to have been very persuasive one on one as well. Would that be true?

Well, he was very intimidating in his critical approach. Your biggest fear was that he would make a joke at your expense. So one was always somewhat on guard. [laughter]

Not to be a target.

He had this workshop. People had their knives out. I participated in some of them. I think people were using George's example. No prisoners were taken in other words.[36] [laughs] And everybody just jumped in. It was just *chaos* those workshops. [laughter]

Was a paper actually ever given?

No. A paper was never given. It was just discussed. It was taken apart. And it was *breathtaking.* [laughs] It was totally breathtaking. But, that workshop wasn't all George. There was Stigler, but there were some other guys like Telser.[37] Are you going to talk to Telser? You should.

Sure.

Telser had a very long association with George.[38] Harold Demsetz[39] was also a member of that workshop. No shrinking violets in that group! [laughs] I miss that too. That really made life interesting.

Yes.

I don't know if it's true, but I think it is. There are aspects of economic theory that are very abstract, and not very connected to anything. You know that. They're kind of logical systems and the problems are hard and so on. But once you get things logically arranged, then you're done. But for these Chicago School types, that's not what theory was about at all. It had to be useful. So the *logic* of things, at least at Chicago, was that it had to be right. [laughs] You couldn't make mistakes in logic of course.

But some theories weren't regarded as very interesting because they weren't very useful, like general equilibrium theory.[40] Pushing ahead with that has proven to be probably a mistake in judgment, because people haven't found it to be useful. Although a lot of people still are trying to use those theorems. [laughs] But it's just not useful. That's the blood and guts quality of economic thinking here, the attempt to use theorems. So it's a combination of data and theory. I don't really view Stigler as a theorist, say like Samuelson, Hicks, or Debreu, you know people like that. I would see him more of a theorist like Arrow whose work was a lot more useful, though Arrow's work was a lot more abstract. There's always a lot of very practical interest in his work. Arrow has a much more abstract mind, so he can play with pure maths, which George never could do.[41] I don't think that he was capable of it but I don't think he ever wanted to either.

Just to reflect back on something we were talking about earlier, I was always surprised that there was that strong stand taken that economists shouldn't be talking about policy. Because at the same time George Stigler was very, very insistent that Economics should be practical, should be an applied discipline.[42]

Oh right. I wonder about that. It's a good point. I never talked to him about something like the minimum wage for example.[43] He wrote about that.

In the forties.

He wrote a very important paper on it. It was a *classic* paper really, in many ways. The prevailing theory is still pretty much the same as the one he described. [laughs] He also wrote on such things as rent controls and others. I never talked about that with him. I suppose that he would have been *against* the minimum wage. You know, I did see letters signed by his colleagues. [laughs] I suspect he might have signed one not endorsing the minimum wage. But I don't know if he ever took on that contradiction. Gary might know how it fit in with the approach you were talking about, I just don't know.

Well, I think that you've been more than helpful.

I hope you do a good job on this report because I think that Stigler is a *very* interesting man. Interesting both as a person, he had a very remarkable personality, but also as an economist, the way that he thought. I don't think he got the world completely right, but he made a difference and not too many people do. The more I think about what I do, the more I feel that I personally got a lot out of associating with him. I can't really explain that.

I think the more I dig into it, whether I happen to agree or disagree with his specifics, I'm more and more impressed with his ability to think as an Economist. I'm overwhelmed, that's for sure.

That's the Chicago School. That's what we do.

But I'm particularly overwhelmed with Stigler's abilities, and his mind. I'm not sure that I necessarily agree with him. I still have to think a lot more about the positives and the negatives. But, I find him more and more an interesting and complex character. My work on him has just become increasingly interesting. I just like work that challenges my own ideas, that makes me think.

That's an interesting approach, very interesting. It's quite critical… Are you talking to his son about all this?

Yes. Tomorrow actually.

His son is a *very* interesting man. A very capable person. I like him a lot. He's a real scholar.

I'm particularly impressed by the loyalty in his friends, the friendships he created. I find it particularly significant to see how much so many people actually miss him. That seems to be felt consistently. People who knew him and worked close to him all seem to have felt a great loss with his death.

Yes. That's true.

So there must be something to be someone who can create that.

Yeah. There aren't too many people who can draw a circle around them, the way he did. I think it's vanished from intellectual life. I think this university had several people like that. In physics, there was a guy that everyone just came here to be around. I don't get the impression that there are many people like that in economics today. Maybe there never were. [laughs] I think Marshall was, from what I can gather. He had this huge influence on his students. Friedman had a very huge influence on his students, but he was also a very difficult person to deal with, in a much different way than George. He was a much different type of person than George was. I don't know if Milton attracted as many people around him. He attracted his share, but not in the same way as George did. George was just a lot different.

In what way was he different?

Well George was even more intimidating. [laughs]

Was he?

Oh yeah! [laughs] He was the smartest man I ever met, without doubt, the best working economist that there ever was. He was just the smartest, the very smartest man…. Think about it. How many times did he hit something on the head? It's just unbelievable all the things he managed to do that made a difference. Anyway, that's a different story.

Yes. OK, well thank you very much.

Notes

1. His career as a graduate student at Chicago featured a somewhat rocky start. (Chicago uses a version of natural selection in producing economists. Starting with a large graduate class, numbers are ruthlessly winnowed through means of the initial required classwork.) Rosen failed what is known as 'the Core'. Ever helpful, Milton Friedman suggested that he might not be cut out for a career as an economist and might succeed instead by becoming something useful like an accountant.
2. Despite George Stigler's jokes about the poor sales response for his textbook, clearly a number of students ploughed their way through the volume at Chicago. Since Sherwin Rosen attended Chicago as a graduate student from 1960 to 1966 he would have read the revised edition published originally in 1952. To be more precise he received his Ph.D. in 1966 but actually left Chicago to join the staff of the University

of Rochester in 1964 before returning to Chicago in 1977. Born and raised in Chicago, he continued as a member of staff until his death in March of 2001.

3. This reference is to one of a set of papers in honor of George Stigler published in the *Journal of Political Economy* (October 1993) and edited by his close colleague, Gary Becker. Sherwin Rosen's thoughtful contribution was entitled 'George J. Stigler and the industrial organization of economic thought'. It was that article that prompted the interview.
4. An initial look at this Smithian issue can be found in an early 1951a article 'The Division of Labor Is Limited by the Extent of the Market'. Though as Sherwin Rosen states, any application to the academic realm cannot be found there.
5. The article I was vainly trying to recall is 'Specialism: A DISSENTING OPINION' that appears in a collection of essays (1963b).
6. I was attempting to dredge up from my memory a paragraph in Book V, Chapter One in the section subtitled: *Division of labour destroys intellectual, social and martial virtues unless government takes pains to prevent it*. Smith here makes an incisive point that had struck me when I first read *The Wealth of Nations*, but managed to elude me at this pivotal moment of our conversation. Why Sherwin Rosen had no recollection at all of this section is another question, a curious one of why we remember what we do. Certainly Smith employs this argument to justify certain government expenditures to remedy this observed problem. Government intervention has long been accepted only with great reluctance at Chicago. The attitude might be described as guilty until proven (beyond the shadow of a doubt) innocent. I would also suppose that there is a tendency to attribute anti-market sentiments to Marx (not entirely legitimate).

   The relevant quote is:

   > In the progress of the division of labour, the employment of the far greater part of those who live by labour, that is, of the great body of the people, comes to be confined to a few very simple operations; frequently to one or two. But the understandings of the greater part of men are necessarily formed by their ordinary employments. The man whose whole life is spent in performing a few simple operations, of which the effects too are, perhaps, always the same, or very nearly the same, has no occasion to exert his understanding, or to exercise his invention in finding out expedients for removing difficulties which never occur. He naturally loses, therefore, the habit of such exertion, and generally becomes as stupid and ignorant as it is possible for a human creature to become. The torpor of his mind renders him, not only incapable of relishing or bearing a part in any rational conversation, but of conceiving any generous, noble, or tender sentiment, and

> consequently of forming any just judgment concerning many even of ordinary duties of private life. (Smith 1961: Vol. 2, 302–303)

7. Conclusive evidence that he changed his mind might be somewhat debatable. A credible argument can be constructed that Stigler simply discarded inherited elements that were not consistent with a rational world dominated by efficient, price adjusted markets. Stigler, perhaps through his association with Gary Becker, firmly believed that economic methodology, such as a framework dependent on rational decision making, could be extended to form the basis for all social sciences. Certainly his own later work veers strongly into political science, particularly in helping to redefine political markets. However, his strongly stated earlier opinion precludes the idea of useful cross-pollination.

   > If one were to seek a major economic theory whose existence depended directly and essentially upon prior work in another field, he would find few likely candidates. Putting aside for a moment the important methodological fields of statistics and mathematics, there is in fact no important candidate. A theory of behavior, such as our profit maximizing assumption implies, could have come from psychology, but of course it did not. In fact Smith's professional work on psychology (in the *Theory of Moral Sentiments*) bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted despite continued efforts from Jennings (1855) to Herbert Simon and George Katona to destroy it. Again, the theory of production could be the economist's summary of the technological sciences, but of course it has never been. Economists have produced whatever laws of production they have. (Stigler 1960b: 44)

8. This refers to the article by Hall and Hitch (1939). Stigler himself in his well-known critical analysis of the kinked demand curve (1951b) refers to this work, but saves most of his ammunition for the paper by Paul Sweezy (1939) which appeared in the *Journal of Political Economy* about the same time. The work of Hall and Hitch is basically brushed aside as inherently trivial. An approach to understanding why George Stigler chose to focus on the Sweezy work appears in Freedman (1995).
9. This idea of acting 'as if', enunciated specifically by Milton Friedman (1953), after serious discussions with George Stigler in the years preceding, forms one of the bulwarks of Chicago style price theory.

   > It was a methodological position, which, only when Milton Friedman published his famous (methodology of positive economics) article in 1953 did I then realise

> he was using the type of argument that sounded exactly like the kind of things Stigler would drop in his articles. It was a kind of (what shall I call it) a poor man's Popperism. I mean it is basically Karl Popper's falsification with a tremendous emphasis on prediction, etc. And I later realised, discovered this because I asked him, that he and Milton Friedman talked about all these things. Milton however just ran away with it. George Stigler always slightly resented the fact that the entire world learned all this stuff from Milton Friedman, when in fact, if you look at the order of precedence, George Stigler was slightly ahead in this sort of attitude to the testing of hypotheses. (Conversation with Mark Blaug, April 1998)

In Stigler's preferred framework, individuals, whether in the role of consumer or supplier, react to market incentives in the form of price signals. They need not be fully conscious of doing so. [They do not consciously think of themselves as maximizing utility or profit.] However, the survivor principle, first enunciated by George Stigler in 1958, ensures that only those who respond properly will ultimately succeed. Thus business people need not think in terms of 'profit maximizing'. However, only those that consistently seek and achieve profits will be able to continue to compete in any standard market. Also see Alchian (1950) for an early version of this idea, one that he deftly locates in a supposedly evolutionary context.

10. Frank Knight arrived at Chicago from the University of Iowa and remained until his death in 1972. Thorstein Veblen was a member of the original Chicago department established in 1892. A protégé of the department's first chair, James Laughlin, Veblen made the journey with Laughlin from Cornell to Chicago. He also became the editor of the house journal, *The Journal of Political Economy*, one of the oldest academic journals of economics. Only Harvard's contribution, *Quarterly Journal of Economics*, can claim an older heritage. Refused advancement at Chicago (after 14 years he had only been recently promoted to Assistant Professor) Veblen moved to Stanford University in 1906.
11. Clearly the work he did with James Kindahl (ably assisted by Claire Friedland) in their 1970 NBER book involved intensive shoe leather work, going from firm to firm for pricing data. (At least initially, during the trial period, both hit the road, though later that grunt work was allotted to James Kindahl.) The aim, of course was to illustrate the defects in the existing Bureau of Labor Statistics wholesale pricing series. This formed the foundation for George Stigler's evisceration of the theory of administered prices proposed and eagerly promoted by Gardiner Means.

12. Among others see the work of Richard Thaler, Daniel Kahnneman, Robert Frank, or Amos Tversky to gain an understanding of the ways in which behavioral economists depart from the more standard (and more narrow) assumptions concerning the behaviour of economic agents. Ironically, Richard Thaler currently resides in George Stigler's former position as Walgreen Professor.
13. Sherwin Rosen is exaggerating for effect here. Born 28 September 1938 he would have been 59 at the time of this conversation, certainly not quite ready for the proverbial glue factory.
14. This reference is to work by David Card and Alan B. Krueger (1994, 1995). Their work drew such pointed and negative rebuttals (see for instance, Neumark and Wascher [2000]) that Card was loathe to bear the brunt of the continuing controversy, especially given the perception that such heated exchanges largely missed the point of their work.

    > I've subsequently stayed away from the minimum wage literature for a number of reasons. First, it cost me a lot of friends. People that I had known for many years, for instance, some of the ones I met at my first job at the University of Chicago, became very angry or disappointed. They thought that in publishing our work we were being traitors to the cause of economics as a whole. (Clement 2006: 3)

15. The decision to drop history of thought was the result of discussions in the early 1970s. As noted, George Stigler, who was such an eminent figure in that field, was strongly in favour of dropping the subject as a requirement. From his perspective, the subject failed to offer much in the way of assistance to a young, ambitious economist eager to blaze a career path. Stigler even goes so far as to consider that his own, rather intense, interest may have represented something of a mistake. Though there is a suspicion that his tongue may be firmly planted in his cheek.

    > I did acquire a strong interest in intellectual history at Chicago, as my doctoral dissertation on the history of economic theory attests. So, while I was failing to get the deep training in mathematical and statistical tasks, which would have better equipped me to participate in the increasingly more rigorous economic analysis of the discipline, I was acquiring some mastery of a branch of economics that was permanently declining in professional esteem. This is a modest complaint aimed at myself: I had failed to predict the direction of economic research for the next forty years, although the movement was already under way. My interest in intellectual history has continued to this day. (Stigler 1988: 27–28)

    George Stigler can be contrasted here with his friend and colleague, Milton Friedman, who would have also been part of any such discussions. Friedman had not delved often or too deeply in such historical, intellectual research. Nor, did he particularly value it.

> So today, you either have to be an extraordinarily good mathematician, or else there is nothing else for you to do but the history of economic thought. I'm saying that there is sort of a balance wheel here. If there are exciting things being done in a theory, an interesting and exciting thing to do with the structure of the body of economics, that's what will attract the top young economists. They'll be drawn away from the history of economic thought or similar such fields. On the other hand, if it's a dry period, so far as really adding to the structure of economic thought is concerned, all of a sudden, everybody is interested in such things as the background of Stigler, of Keynes, of Samuelson. (Conversation with Milton Friedman, August 1997)

16. There is a rather half-hearted defence of history of thought in 'Does Economics have a Useful Past?' (1982) However, the quote referred to in the interview actually appears in his Autobiography (1988).

    > In fact, I cannot be confident that it would be profitable for a young scholar to study the history of his subject. If a young economist does immerse himself in the history of economics, he will learn that every proposed innovation is first produced in a highly imperfect form, and only gradually will the larger imperfections be removed. He will also learn that sponsors of the new theory or program exaggerate its merits and just as consistently exaggerate the deficiencies in the previous knowledge they are seeking to displace. (Stigler 1988: 215)

17. Mill is presented as something of an effigy, representing the equivalent of a shibboleth, one that provides a clear warning of the perils of ever slipping into fair minded perspective.

    > The one conspicuous exception to the rule of overestimation of the importance of one's own idea was John Stuart Mill, whose rectitude was so extreme as to be painful: *He* played down his own contributions—and was rewarded for a century with an undeserved reputation for noncreativity. So modesty and respect for received knowledge would be most dubious assets for a scientific innovator. (Stigler 1988: 216)

18. Stigler should be recognized, along with perhaps Milton Friedman, as one of the greatest marketers to have worked in the field of economics. He knew how to package and sell his ideas to his target audience. He was more than aware of the importance of this endeavor.

    > George Stigler, I remember when I was a young person, wired and said 'Selling is very important in your research. So write better. Work on writing because that is important. You've got to sell what you are doing.' I think he's exactly right. You've got to sell what you are doing. (Conversation with Gary Becker, October 1997)

19. This refers to one of his five lectures ('Monopolistic Competition in Retrospect') given at the London School of Economics in 1948 and published in 1949. These lectures do form a coherent whole and shouldn't be read as an amalgamation of separate topics.
20. Archibald (1961) precipitated something resembling a reconsideration of Chamberlin's monopolistic competition. Although fundamentally

critical of Chamberlin's model, he was equally unimpressed with the attacks made on that theory by a variety of Chicago type economists (whether or not actually a member of that faculty at the time of the relevant attacks). Archibald's *Review of Economic Studies* article was met by rejoinders from both Friedman (1963) and Stigler (1963a) as well as a reply by Archibald (1963). It seems clear that neither side was the least bit influenced by any of the opposing arguments.

21. Stigler provides the theory with an early funeral in his autobiography.

> The theory of monopolistic competition had wide influence in economics until the end of the 1950s by which time it became apparent that the doctrine was exhausted. That is to say, numerous economists could play variations on its central theme … but their studies yielded no interesting empirical insights on the workings of industry. The theory was descriptive, not analytical. (Stigler 1988: 162)

22. Though Sherwin Rosen is definite, a former colleague, Harold Demsetz, is a bit more circumspect regarding the senescence of monopolistic competition (though not perhaps in the version concocted by Edward Chamberlin).

> *Because in fact in his autobiography George Stigler refers to monopolistic competition as dead and buried.*
> A bit premature.
> *Never pronounce a death sentence on anything.*
> No, no that's right. (Conversation with Harold Demsetz, October 1997)

23. Though Lancaster might be best known for developing the theory of the second best in an article co-written with Robert Lipsey (1956), Rosen is referring to Lancaster's (1966) work in developing a new theory of consumer demand based on the characteristics of goods and services rather than simply on quantity. He stressed qualities over quantities. Though perhaps not immediately obvious, given some thought, the Lancaster work can be related to the concerns originally expressed by Chamberlin, especially if his work is read carefully.
24. Dixon and Stiglitz (1977) published this widely cited article in *The American Economic Review*.
25. In post war Chicago, the Marshallian tradition came to mean, to a large extent, the use of partial, rather than general, equilibrium models. Whether Alfred Marshall would have managed to fit into this constructed, and entirely constrictive, tradition remains an open question. Not simply out of malicious spite did Dennis Robertson refer to those economists as being loyal, but faithless Marshallians. The Chicago tribe may well have fit Robertson's intention quite snugly.

26. *The Journal of Law and Economics* published this article posthumously in 1992.
27. This comment is reminiscent of that made by Stigler at the end of the paper in which the idea of a politically self-interested market place is articulated.

    > Until the basic logic of political life is developed, reformers will be ill-equipped to use the state for their reforms, and victims of the pervasive use of the state's support of special groups will be helpless to protect themselves. Economists should quickly establish the license to practice on the rational theory of political behaviour. (Stigler 1971: 18)

28. *The Bell Journal* published Stigler's classic paper in 1971. Others connected with the Chicago tradition have also contributed. See works by Becker (1983), Posner (1971), and Peltzman (1976), just to name a few.
29. Machlup (1967) restates the core issues of the debate in defending marginal analysis. Loasby (1971) takes issue with Machlup's approach.
30. This is reminiscent of Frank Knight's comment that, 'Sociology is the study of talk and there is only one law in sociology: bad talk drives out good' (quoted in Samuelson 1946: 188).
31. The pause is due to a knock on the door interrupting the conversation. One of Rosen's colleague with whom he was at that time jointly writing a paper needed to confer over some specific point.
32. At the time of the interview (October 1997), Robert Leeson, an Australian academic, had submitted a paper to *The Journal of Political Economy*. Leeson's analysis was extremely critical of the Chicago approach, especially taking an adverse position to its rhetoric and marketing strategy. He claimed that Stigler helped shape the negative view that the economics profession embraced regarding monopolistic competition.
33. Paul Krugman, formerly at MIT, then at Princeton, and now Distinguished Professor of Economics at the Graduate Center of the City University of New York, established his reputation at an early age with articles that formed the basis for what would became known as 'the new trade theory'. It assumed imperfect competition.
34. The exact title was, 'The Politics of Political Economists' (1959). This was originally a talk given at Harvard and perhaps deliberately formulated to provoke his audience. It was later published in *The Quarterly Journal of Economics*. A.W. Coats (1960) and Murray N. Rothbard (1960) both responded to the piece. George Stigler (1960a) swatted the comments away with a short dismissive note.

35. The article (1973) referred to was 'A Sketch of the History of Truth in Teaching' which appeared originally in *The Journal of Political Economy*.
36. The Industrial Organization workshop became something of a weekly institution. Deirdre McCloskey relates how in this venue, she had seen a grown man, presenting a paper, reduced to tears by George Stigler. As his long time research associate, Claire Friedland relates:

    > As for George's caustic wit, he never let go one of his barbs for the sake of oneupmanship. They were always aimed at the target's ideas, not the target himself: even when a workshop speaker asked whether he should deliver his paper standing or seated and George responded, "With a paper like this, under the table would not be inappropriate." (Friedland 1993: 781)

37. Lester Telser was originally a student at Chicago. (A contemporary of Don Patinkin, the two were later related by marrying sisters.) He would become a long serving member of the economics department. Lester Telser and George Stigler enjoyed a somewhat prickly relationship. Telser, perhaps felt himself to be something of an outsider relative to the inner core surrounding George Stigler.

    > Stigler, Friedman and those around him were Monets. They didn't look as good when examined up close. (Conversation with Lester Telser, October 1997)

38. The tension between Lester Telser and George Stigler might have stemmed from a suspicion on Telser's part that he had failed to receive his full due in terms of the acknowledgment and appreciation for his work. This though, is the variety of assumption that by its very nature must remain in the purely speculative realm. Certainly he was reluctant to go on record with his insights concerning George Stigler, especially those with a more negative edge to them. As he stated, "I still have to live in the department" (Conversation with Lester Telser, October 1997). As his fellow classmate Claire Friedland admitted, their two personalities inevitably clashed, with neither one keen to admit error.

    > Lester and I are old friends. I introduced him to his wife. I know him from the 1950s when we were students together. He says to me, "That George is so stubborn. He is so unreasonable. I told him his research was wrong. I explained it to him. I was right. George was wrong. And he won't listen to me." Lester is just letting off steam. He goes out Stage Left, George arrives Stage Right and says, "That Lester is crazy. I don't know why I pay any attention to him. But let's try it his way." [laughs] Now in that sense, you see I had a private view of George which was different from the one he presented to the rest of the world. He had told Lester he's crazy and that was it. Lester went away totally frustrated. And then, [laughs] George comes to me and says, "Lester is crazy but let's try it his way!" (Conversation with Claire Friedland, October 1997)

39. Although mostly in residence at UCLA, Harold Demsetz was part of the Chicago faculty for a number of years. He was by his own admission greatly influenced by George Stigler.

    He influenced me a great deal, there's no doubt about it. He was a great person to be around, it was fun to be around him because he made professional work fun. And he was a great gossip about the profession. You could always find out what was going on and what George's opinions were about what was going on. Every dinner you had with him was entertaining. He was a man of great wit and great style and I miss him very much. (Conversation with Harold Demsetz, October 1997)

40. Stigler's focus on usefulness is verified by his long time research associate, Claire Friedland.

    Very often George, when he wanted to decide how he felt about something, would ask himself not whether this stands up as a theory, as an intellectual exercise, but whether it had some applicability that moved the profession on, in a useful way. It had to be *useful*. And I don't mean useful in terms of government programs. I mean useful in terms of being productive. Productive of useful predictions, of certain facts, which when you examine the data are confirmed. But it was also a question of whether the profession built on it. (Conversation with Claire Friedland, October 1997)

41. George Stigler was quite open in acknowledging the gap in his mathematical training as an undergraduate. Heavily under the influence of his dissertation advisor Frank Knight, he tended at that time to scoff at mathematical or statistical economists like Henry Schultz who was a member of the Chicago economics department back in the 1930s when Stigler was a graduate student.

    Wallis and Stigler took Schultz's course, which was a very serious assignment because he assigned a lot of work and you had to sign up for a double quarterly credit if you took the course at all. And under the quarter system each subject had a lot of hours attached. You spent a lot of hours on a subject for a third of the year, instead of the way things are divided under the more common two-semester system. Well, because they were contemptuous of Schultz, and kind of mean, they played mean games with him. And in one case they went to Henry Schultz and said, 'Professor, we have an argument between us which we can't settle. We know the formula for the area of a unit square, and we know the formula for the area of a unit cube. But we can't agree on what the general formula is for a four dimensional regular solid. Would you please decide between us?' They knew that Henry Schultz thought that Allen Wallis was a better mathematician than George Stigler, so they gave George Stigler the correct answer and they gave Allen Wallis the incorrect answer. And, as it was basically described to me, Henry Schultz was proven to be a four-flusher. I don't know if that word means anything to you. It means a bluffer. As they saw it, the insecure professor fell into their trap and came out in favour of the Wallis formula. Well, that's a story on three people. And not on Schultz alone. (Conversation with Paul Samuelson, October 1997)

42. See his 1964 presidential speech to the American Economic Association (published in 1965 by the *American Economic Review*) which serves as a clarion call for empirical work to form the backbone of economics.

    > I was sitting with Aaron Director at the time when he gave his Presidential address and we did look at one another at the time to try to see what each one thought about all of this. (Conversation with Ronald Coase, October 1997)

43. His article appeared in *The American Economic Review* in 1946. It formed part of the defence of standard marginal analysis against criticism mounted by Richard Lester (1946) of Princeton University. Read carefully, the two protagonists often appear to be speaking past one another.

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What Price Glory: James Kindahl Airs Some Views on George Stigler

Craig Freedman

In October of 1997, I awoke in the early morning hours to travel by bus from Boston to Amherst Massachusetts where James Kindahl was still teaching in the Economics Department of the University of Massachusetts. I had had the pleasure of taking an econometric series (consisting of two, one-semester courses) with Professor Kindahl while a graduate student in the early eighties. Much to my surprise, he remembered me and still had a record of my achievements (dubious or otherwise) in those courses.[1] My strong memory is of an old fashioned gentleman who was particularly scrupulous in dealing with data. Caution and scepticism were his watchwords and being of a similar bent myself, I found his approach to be particularly persuasive. This methodology (to use an inflated term) provides a stance far removed from the tendency to use statistical analysis merely as a rhetorical device to further one's own preconceived ideas. One particular classroom moment that still sticks with me was a discussion on misleading or spurious correlations. These are variables that seem to be causally related from

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a statistical standpoint, but are clearly not linked. A classic case often cited by introductory statistics textbooks in the relation between larger feet among children and higher intelligence scores. In class, Jim Kindahl examined this issue only to have me claim that he could choose any two variables virtually at random and I would come up with a causal story linking the two together. By the fourth try, he threw in the towel laughing. That is when I became firmly convinced that as an econometrician, I would make an entertaining novelist.

Jim Kindahl, of course, co-authored a classic work with George Stigler, *The Behavior of Industrial Prices* (1970), a painstaking example of empirical work. Subsequently, both authors had to deal with a heated, or possibly torrid, reaction from Gardiner Means who saw the work (quite rightly) as a direct attack on his own theory of administered prices. Neither Stigler nor Means was capable of yielding an inch. A good deal of the discussion that followed the book's publication was listened to with deadened ears by the pair of them. Means himself would grow quite agitated, accusing Stigler (in private correspondence) of breaching standards of honesty and even worse. Means in fact vigorously attempted to keep the work from seeing the proverbial light of day. He was as determined in his particular position as George Stigler was in his. Both sides would have willingly continued the debate in the pages of *The American Economic Review* for a seeming indefinite period had not the then editor, George Borts, called a halt to the proceedings, at least insofar as the journal was concerned. In my discussion with Professor Kindahl, I sought to explore the ways in which the work on the book was done and Stigler's attitude to the actual results. Unlike many of the other interviews I conducted, this was a much more focused interview given that the contact between Kindahl and Stigler was restricted to a limited period and focused on just one particular issue. To a certain degree, this made it easier to concentrate on the way in which George Stigler actually worked. In any case, the contrast between George Stigler's decided conclusions and James Kindahl cautious responses should provide any careful reader a good deal of food for thought.

The Interview

If I was doing this, I wouldn't want to be doing it without a tape recorder.

Yes, because of my faulty memory.

What exactly did he say? Did he say this, did he say that? Did he really say that? Was it that exactly? Did he say it as a joke? Clearly it was a joke. Was it a sarcastic remark, a serious remark, or what?

I know. I find that my memory just isn't good enough. Even if I rushed off and copied every little bit and piece, I could still not remember enough.

It gets worse over time.

Well, I'm already on that downward slope so it's not going to get better.

I think the downward slope begins about twenty-five.

You noticed that too.

Oh, yes.

One minute you're holding all this information in your mind and the next you don't know where you've put things down, what day it is, or who you are.

Like, 'I stuffed it into the computer and I can't remember what directory I put it in.'

I hate when I'm forced to go just randomly through my files.

Dealing with my brain helps me to deal with the computer. I get used to it, as I guess you know. I know it is in here somewhere. But where? That sort of random search takes over. I know the information will come back to me. I know that sooner or later it'll come back into my brain. If I'm just ready for it when it drifts back in and before it drifts back out again, then I'll be okay.

I always think of those old research libraries where you make a request, then someone goes off and you wait and wait and wait. An assistant librarian scurries off and fumbles around and eventually the desired information pops up. But, sometimes it is days later, and by that time you don't know why you wanted the damn thing in the first place.

I know the problem.

OK, so, you were a graduate student then with Claire Friedland?[2] *That must have been in…?*

She was a year ahead of me.

So that was what, in the early 50s?

I was there as a graduate student at Chicago from '55 to '58.

I see.

I think Claire was there. She was in the class one year ahead of me. She must have come in '54.

She said you were there along with Gary Becker, that he was there at that time.

Gary Becker was a new assistant professor in my last year. Something like that, maybe before then. He was a new assistant professor. Chicago had a long tradition with their best students. They put them on the faculty for one, two, three or four years. But then they were never allowed to get tenure directly. Gary Becker went off to Columbia for many years and then came back.[3] I don't know if it was a policy. I don't know if it was a written, agreed-upon policy or just an accident, the way things happened. I can't think of a case where anybody got tenure without first having gone and done distinguished work somewhere else.

Seems to have worked well.

It was a dammed good rule.

Yes.

It's a dammed good rule. First of all it preserved the integrity of the university department, and secondly it just makes moot all kinds of arguments that would be coming out. There would be horse-trading. 'You got your student, now I want my student.'

That's right.

It's the surest way toward inbreeding and inbreeding is the surest way for a department to go down.

Absolutely, because, I do notice that often the very best do come back, if you entice them to return sometime down the road.

It's no accident then if they do come back. They're not just staying around. I mean they are not staying on when they graduate. No matter how much they might like to. It is also good for the people who do come back. That way, it is clear that they made it on their own. They are not there because Professor Grutz wanted them, because they were Professor Grutz' student.

The fact is, I notice in many cases that they have to be enticed very strongly away from their existing department.[4]

Yeah. The typical Chicago attitude would be, if other people don't want this guy, we don't want him either.

The test of the market.

Yes, if other people do want him that doesn't mean we do want him. It's a necessary but not a sufficient condition.

There has to be at least some demand to begin with.

Yeah.

So, when you were there did you know George Stigler? Because George Stigler didn't return until '58 I think.

Yes, he returned to Chicago at exactly the same time I graduated. So I had never met him during my stay as a graduate student.

So, how did you later come to know each other?

Well, I'll tell you. I was teaching at Amherst College at the time. One day the phone rang and a voice said, 'This is George Stigler.' Now, only George Stigler could say, 'This is George Stigler' end of sentence. He would expect that I would know who he was. And I did of course. Any Chicago PhD student knew who he was. We had cut our eye teeth on *The Theory of Price*, First Edition, as undergraduates.[5] And I did work my way through it cover to cover. I was a student in the MBA program.

I see.

Originally I was at one point a student in the MBA program and I left to go to work, then to serve in the army. It was the Korean War days. I got an MBA, worked briefly, went into the army. I decided I wanted to be an economist, came back for a PhD in the department of economics. So I'd never met the man, but in the business school we used his *Theory of Price* volume from cover to cover. And that was my first introduction to the theory of microeconomics. So I of course knew the name, George Stigler. I had never met him. And, he said to me that he had a grant from the National Bureau of Economic Research.[6] He told me about what the grant was to do. He said he would like me to take a year off and come and work with him. This is out of the blue. This is a man I'd never met in my life. He said Milton Friedman had recommended me and Milton's recommendation was good enough for him.[7] He didn't need to know anything more. He asked me if I'd do it.

I then said—but that had nothing to do with George, but about me—and you're not writing about me. The details are irrelevant. It had more to do with me than with George. The upshot of it was that I agreed to come out and talk to him. And I decided to do that. It looked like a good opportunity to me, talking from a professional standpoint. I told him, 'I think I'd like to do this, but it looks to me as though there is no way we can do this in one year. Can you support me for two years doing this?' And he said, 'Two years. Sure.'

A tough bargainer.

And so that's how the whole thing started. That's from my point of view. But for George of course, it was a continuation of his war with people who talked about administered price.[8]

Now, he needed this assistance because it was just a huge project, is that right? Because he intended to go out and get all that price data?

Yes, well, George and I together spent something equivalent to full-time for one person being on the road for a year. In the end I was alone on the road, visiting mostly large, some small and some medium sized manufacturing firms. You've probably seen the data. We selected various products to use as examples. And then I spent lots of time trying to get information on the prices we could use at which the good actually sold. And we found very quickly that one didn't ask questions like that in sales departments. There was no way in which those people were going to tell us about these things. We learned a good deal about how the world works, at least about the administrative aspects. We ran into some disputes with the purchasing agents we talked to. The purchasing agents and account people were always talking about fixed contracts and purchase contracts. I remember the first time this came up. George and I were doing a very early interview. We were doing interviews together for several weeks to get an idea of what kind of questions you would ask, of how to refine our approach to find out the way in which they operated. So during this experimental time, we kept hearing about purchase contracts. We just wanted to say, 'What the hell is a purchase contract?' We eventually found out it is a contract between buyers and sellers, but not for a specified amount. It gave a buyer the right to buy at a price specified for a certain period of time, sometimes adjusted by an agreed upon price index. Anyway, we started with the sales departments. It seemed like the place where they'd have the most information. Of course, they did. But we soon realised there was no way we were going to get the information we wanted. It was because there was psychology involved. Not because of any trade secrets. But we were going in there and saying, 'Look, what did you actually sell this for?' That offended their professional pride. The attitude we conveyed was quite clear—any jackass can sell at list price. So their response was 'I'm sorry, but I can't tell you what you want'. But, we found that purchasing departments were the place to go to find information.

Right.

Because when we went to *sales* departments, we were saying, in effect to them, 'look, we know you guys can't sell this stuff at list price.' And that offended their professional pride.

Of course.

Who wants to admit 'Yeah, well we couldn't do it. We couldn't sell it at list price instead of giving them this price concession. We couldn't stick to it. We had to give up. We hated doing it. We just hated doing it.' They were going to tell us, perfect strangers, about this? No, no way. We did discover though that if we talked in the right way to purchasing agents, then we were asking them, not to tell us about their failures, but to tell us about their triumphs. Some of these guys really opened up. All the details, all the war stories, blow-by-blow descriptions about how good triumphed over evil. Or more accurately, it really was a matter, as the old football legend, Vince Lombardi says, 'Winning isn't the most important thing. It is the *only* thing.'

OK.

We almost played at this strategy. We got absolutely nothing from sales managers. Oh, we got tours. We got a VIP tour of the Inland Steelworks, things like that, which was interesting, but it didn't do a damn thing for us.[9] Absolutely nothing was accomplished. So we learned something about the intricacies of prices and the politics of pricing. We learned the various methods by which something is sold at list price, but not really sold at list price. Nobody ever admitted to kick-backs, under the table deals, doing various other things. 'Well, we did some processing on it. Didn't charge for it.' I've forgotten all the range of schemes.

So, there seems to be at least a couple of irons in the fire here, a couple of projects going on that was driving your survey work. There's looking at the BLS data, the comparison with the BLS data and seeing how close the two are. This comes down to comparing it with the series that you create with your own data, seeing if they run in synch with each other.

The absolute levels are irrelevant in questions of that type.

Yes.

Virtually all our information, we never got in terms of dollars and cents. That was confidential

No. Of course.

But they were willing to compute the price indices for us. They wouldn't let us take the data down. They would give us the resulting price index, which of course, eliminated the key to finding out what they actually charged.

I see, so what you actually collected were just price changes.

Price relatives.

Yes.

Just choose a starting date, call it 100 and divide up the periods.

And you had the frequency of the prices, when they changed. You knew when the series changed as well?

Yes. When the composition of the prices changed.

Yes.

Those were our instructions to them. Now, how closely they were followed was another story. We were not allowed to verify the results for the most part. We always offered to send our own people in to do the work, but we weren't allowed to do so. In private firms at least they wouldn't let us. We had some public sector data also. And there the attitude was just the opposite. If we looked at public information, the response would be, 'I got nothing to hide. I got nothing to hide, you understand. I got nothing to hide.' And then they would say, 'We don't have the personnel to transcribe it for you. You can send your own people in.'

So …

In the public agencies, the purchasing agents it turns out, though I hadn't realised this, they are constantly besieged by disappointed bidders 'Why did you choose him? Why didn't you choose our bid? Our bid, I'm sure, was the best. Why didn't you choose it?' They obviously spend much of their day listening to the complaints of unsuccessful bidders. After listening to them I didn't want to be in public service at any time in my future life.

I wouldn't think so.

Not that I'd ever thought of doing so in the first place.

Not in this lifetime.

I decided that I'd rather go back in the army if it came to that.

It seems to me, as far as I can tell, everyone welcomed the new data. And that wasn't in dispute.[10]

Everyone in economics did.

In the economics profession across the board, or at least close to the whole profession. The more dicey questions came when you took up this administered price question. This seemed to be something of a backfire, to be honest. Why was George Stigler focussed on that? I mean, he could have said, let's look at the price changes. Now, whether they move with this frequency or that, there is always a good profit maximising explanation. Firms aren't changing prices with some ideal frequency, but that says nothing about whether they are operating efficiently or not. He seemed to be focussed on the administrative price question. What was driving that? Was that just a long term battle he was fighting?

You tell me. He was absolutely convinced that prices were flexible from Day 1. That was clearly his a priori intention. It was more than an intention. It was his belief.

His faith

In the true believer sense. He really believed that. The very first interview we did was with a big firm. We have to keep this all anonymous, but it was a large firm in the Chicago area. That doesn't give very much away.

I don't think so. There is one or two of those there.

Yes, perhaps more then than there are now. Anyway, the essence of it was that the people there absolutely denied anything about price flexibility, hidden discounts or things like that. They put out a price book and that was that. Everybody paid the same price. There were quantity discounts that were clearly stated in this price book. Anyone who bought large quantities got the large quantity prices, and small quantities got small quantities prices. George Stigler was clearly disappointed in that. He came back and he told me, 'Well, we'll get to the bottom of this.'

So, would there have been any, I mean what would the evidence have had to be to move him away from that position? What empirical evidence would have dislodged that belief?

That's a good question. People, who know me well, know that's a euphemism which means that I don't know the answer.

Yes. So what is the answer?

I'm not sure there was any answer. It's kind of in the nature of being an economist, I guess. Somehow they get themselves into … many economists take strong positions in policy questions or empirical questions. They often do this—this has nothing to do with George Stigler, but …

Generally.

You have probably observed this kind of thing. People take particular stands early in their career and they're steadfast, I have to admit that. They are not wishy-washy. No amount of empirical evidence will persuade them to change to a different point of view. Would there be some evidence that would have persuaded George on this? I'm not sure. He believed, he really believed that prices responded to short-run market demand. Was there any evidence that would have caused George not to believe that prices responded to short-run market demand? I can't help you on this. I don't have any reason to believe that he did believe there was any evidence for that. But it was not a question that came up in our daily work.

Because, on the other side, it is hard to see what would have persuaded Gardiner Means on the opposite side.

Gardiner Means was also not a man likely to change his views.

So, you sort of …

Take into account the fact that George was 20 years, or more, older than me. George was already a very distinguished economist. There were certain kinds of matters which I simply didn't discuss with him. I knew his views. I was not adamantly opposed to his views. I didn't think they were outrageous in the way that particular economic assumptions, which are made to answer particular questions, are not outrageous. It was a good working tool for some kinds of questions and outrageous for other sorts of questions. But George felt strongly about it. He really believed there was no way in which prices wouldn't respond flexibly to market forces. Well, the issue was not relevant to what we were doing then. I simply followed the policy of not taking on arguments that were not germane to our investigation.

Because I did notice especially that there was a NBER promo attached to the book, which contained some other statements that tended to be very strongly worded. Now was that …?

I don't remember what you're talking about.

Well, you might think there should be qualifiers attached to some of the empirical results cited. Instead there tends to be a sort of confidence, as far as announcing the results concerning flexible prices. Perhaps some other investigators would tend to qualify them a bit more. Now was that basically George Stigler's personality coming through, that he was just confident?[11]

It was George Stigler who wrote those things into the book. Why he wrote them is another question.[12] It reminds me of an interesting anecdote. There was a controversy between Gardiner Means and the *AER*.

Yes.

Essentially between Gardiner Means and George Stigler. As you might expect, ours was not a relationship of equal partners. There was no question of who was the senior economist. At the end of all the work, we agreed jointly on what was going to be published in it. George wrote the first draft and then I was able to get some modifications in. George was the senior researcher and there was never any question about that. I am sure there was no question in his mind or anybody else's, for that matter. I'm not saying anything that people don't know already. And here's the interesting anecdote. At the conclusion of that series, as I recall, the editor at that time, the editor of the *AER,* I can't remember who it was.

Borts.

George Borts unilaterally declared that enough is enough.[13] I don't think he said it that way. I've forgotten who had the last word in this controversy, whether it was Means or Stigler. As everyone knows it was Stigler, I remember now. Anyway, Borts declared that this is enough. That was it. George wrote to me. He sent me that letter from

Borts with a little note that said (I was already here at the University of Massachusetts by that time). The note said something to the effect, 'Let us hope for more attacks on our work. How else can a professor advertise?'[14]

He enjoyed the controversy.

He enjoyed the controversy. He had a very different personality than I have.

Yes.

We were complements and not substitutes. We had very different personalities. We got along fine. Don't misunderstand me.

Oh, no.

You get my point? Complements tend to get along better than substitutes at work.

Now, did he, George Stigler have any respect for the work of Gardiner Means? Did he just think it was rubbish?

I think I'm going to answer that question with 'I don't know'. I have heard lots of things he said. George was not a man who was always over-careful and measured in his remarks and candour about subjects. Differences between people, what I'd have considered to be nuances, to him were canyons. It's hard to know in the case of people, people with whom he disagreed, what he really thought of them. He tended to take an extreme position on most everything. He was not a man to fudge around…

No.

Or to mince words.

I haven't come across one instance where he did so … Now I know that once the book was published, Gardiner Means immediately first responded to the NBER and objected to a certain number of technical points there.

Yes. So I've been told. I didn't hear directly about this.

You didn't?

I take that back. I may have heard. I have a vague memory. I do have a vague memory. The NBER did not contact me about it. I know that. The NBER funded the study and the grant went to George not to me. He already had the promise of the grant and their approval to hire a person of his choice to work with him.

I know that Gardiner Means fired off these long pages of objections, criticisms, etc. Did you eventually get to see them?

Oh, sure. I'm sorry, do you mean the ones to the NBER?

Well apparently he sent some of his objections to the NBER first and apparently he later sent them …

Yeah, to the *AER*.

Was it the later ones that you saw, the ones to the AER?

It's the ones to the *AER* that I saw.[15] I have a vague memory of George saying that Gardiner Means was trying to stop publication at the NBER. And I know nothing more about that. Then came—I've forgotten the exact sequence, you probably know it. I know that Gardiner Means wrote a kind of review article, a scathing review article of the book.

Yes.

And that we responded in the *AER*. Now I was a full partner in the response.

OK. So you had, of course, seen his attack, Gardiner Means' attack in the AER?

Yes, what was written in the *AER* response is partially my work and partially George Stigler's. Now in terms of that article, it seems to me that some of the statements that Gardiner Means made were in general not entirely well grounded. If you want to say something like we responded with some fairly strong statements, some strong denials, that's fine.

Yes.

Is Gardiner Means still alive?

No, he died in '88, I believe.

Yes. Well especially when the man is dead.

Yes.

I have no interest in renewing that controversy. I never met Gardiner Means. I talked to him once on the phone. He called me about three days after George's wife died. Just by coincidence. He didn't know that. He had something that had to be done to protect his reputation immediately and he couldn't get a hold of George and he called me. I said 'Well, George's wife died the day before yesterday. I will forward your objections to him and see what he has to say. But right now I don't think he's interested in responding.' Gardiner Means was still fixated on it … I'd better stop there.[16] There's no point. I have no interest in carrying on this fighting.

No.

I had no interest in carrying it on when he was alive. I thought that some of the things he said about the study were internally inconsistent, illogical and contrary to any kind of economic theory and I drafted some of that response, most of which was changed a little but not that much by George.[17]

Well, there seems to be a huge amount of confusion as to what an administered price could possibly be?

Yes there was.

When does a price start being administered and when does it start being flexible? You know, it seems to be …

Well, except for auction prices, all prices are administered.

By somebody?

Well, yes.

Because I know when Gardiner Means responds, he says you're saying 'absolute administered prices', but I'm talking about 'relative administered prices'. But exactly what does that mean? When does it become relative?[18] *That's why I said, they seem to be engaged in this incredible ….*

Both of them, both of these men had taken strong positions on this … Maybe—I almost come close to saying, that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist.[19]

Yes.

I could have predicted George Stigler's predictions before I signed up from what I'd learned in graduate school of his writings. And I could have predicted, to a great extent, Gardiner Means' predictions and you could guess what I learned at graduate school about Gardiner Means.

I could make that wild guess.

But, as I was saying between the two of us just before, the internal inconsistencies of Gardiner Means' criticisms in that *AER* article just appalled me. Not that he was wrong in his conclusion, but the kind of arguments that were used to get to that conclusion, you see, were wrong. My memory has forgotten the details, but I do remember one of the parts that I drafted personally. He was saying something about the volume of … It's too foggy in my mind, I can't remember the details. It was something about how the price indices were going up and down, or something like that.

Yes.

Essentially what he was saying was really dead wrong, because instead of eight going up or going down, whatever, there was actually sixteen, which made his case even worse than it would have been had he said nothing. I have a vague memory of something like that. If it's important, I could dig it out.

I know he had …

I mean, if it's of pedagogical importance.

I know he had one argument, that you had trended data and that you needed to de-trend the data, (important in his view because he said he was just talking about cycles). Your findings were incorrect because you had mixed in a trend element. He provided that sort of argument. Of course, he had a number of different arguments.

Yes, a whole bunch of them. Some of them were purely technical.

Yes.

And as you might expect, in the division of labour, I worked on the purely technical ones. George worked on the big problems. Or he viewed them that way.

Do you remember any of Gardiner Means's points as being, as having some validity?

For myself? Do you mean 'Did I think?' Or did I see George as thinking?

Well both. You, because you would know what you thought, and George, if you knew what he thought.

I think some of his points were valid.[20] My vague memory, only my very vague memory …

Yes.

My vague memory is that on net, I felt that Means was off the wall. On net we came out with enough evidence to destroy the essence of Means' main criticism.

Right.

But the wall was weak in places. And there was one incident—this is what I tried to remember before but couldn't—Means made an argument which said, 'If you people are right then you should have gotten this series going up.' I can't remember it exactly, but you can look it up in our response in the *AER*. There was some matter where Means said something like, 'Here are three of these series that go down at a certain point. This is not consistent with your theory.' I know I'm not getting this just right but it was something that said, 'If I'm right then this is what should happen. You said they should never have gone down, but they went down.' I can't recall, but it's written out there. You can read our response, where it says essentially, 'Means said, 'OK we should have found that they all were going up, but in fact, they are all going down,' or the majority were going down. But, this was also the opposite of what Means claimed.'[21] In fact, whatever the series was doing was embarrassing to both Means and Stigler and Kindahl. And I pointed out, that it is even more, I remember because I wrote this part, it is even more embarrassing to Means than it is to Stigler and Kindahl, but embarrassing to both of us. I think I said that. At least in the first draft I said something like that. I don't remember what George did to it.

When you think about it, the book certainly … well, if you could ever work out what an administered price was … it raises doubt, certainly with Gardiner Means' position. But it's not clear that if you say, 'Well, this position seems dubious.' It isn't clear that it then naturally supports the idea of flexible prices.

No, not at all. Absolutely not at all. That was it … I was kind of groping for what it was we were saying there. What we said wasn't very convincing. At least the data said so. In my memory at least, the data says, 'Both of you guys are horses' asses,[22] both two sides are off the wall, but Means …' I do remember writing a paragraph that says in effect, 'Well then maybe we are all off the wall, but Means is more off the wall than we are.'[23]

What it seems to suggest is that you just need a rethink on what may be happening with prices.

Could you say that again?

You need a re-think or a reconsideration on exactly how these prices work.

Yes, that's fair.

I know that there is a '87 article, for instance, by Dennis Carleton, who took the same data and separated out each one of the series and basically *said 'Well, you know, what this is saying is that there is a lot of long-term contracting. There is a lot of alternative means of dealing in the market. All of which is perfectly effective and efficient.'*[24]

Yes. I thought that was a good paper.

Do you know if George Stigler ever reacted to it? Was he happy with that?

I never heard him comment on it. After we finished the book, I spent very little time with him. And it's not that we were on bad terms. There was just no reason to do it. He and I had very different personalities. I would think that George Stigler could not possibly work closely with another George Stigler.

It would be inconceivable.

Yeah. It would be a war.

I know there was also—I don't know if you remember—there was, I think, a '77 paper by Leonard Weiss.

I vaguely recall.

My reading is that it came about as a result of that whole controversy, when the editor of the AER blew the whistle and said he didn't want any more of this.

Yes.

That was a sort of the compromise. George Borts said, 'OK, we'll have a third party come in and look at it.'

George was very resentful of that.

Was he?

Yes.

Do you know why?

He thought he was right. And he thought, that he, George [Borts] thought, he, George [Stigler] was right. He felt that the function of journals was to air controversy and to let the profession decide. He had no doubt as to the result.[25]

So he was ready to keep the battle going between him and Gardiner Means?

I think I told you earlier in the conversation what he said. 'Let us hope for more articles, more critical articles. How else can a professor advertise?'

He would have been happy to have Means reply and then have replied back?

He would have gone on until one of them had died—an exaggeration.

But if he had a lot to say …

Yes. I have no doubt that he went to his grave convinced that he was absolutely right and always had been absolutely right on this controversy, in his crusade against administered prices. Long before this stuff that we are talking about, it began in a controversy he had with the chairman of some government bureau of statistics.[26] Indeed it was that experience which led him to ask a few of us who had come to study in Chicago to put an end to all this. To discover once and for all that prices were flexible and there were no such things as administered prices.

And that would be it.

That would be it.

But he did have a statement of what empirical research should tell you. It would tell you definite answers.

Oh, he was a true believer. He wouldn't like that term. But put that in because he thought in that sense. He was absolutely convinced that he was right. It wasn't a doctrinal battle. It was a battle of facts. I almost said good and evil.

And those facts?

He was sure. But then some people are sure that God is on their side.

Yes.

He was absolutely sure the economy was on his side and if research was properly done it would show this. He really believed that he understood how the world works. And the way the world works had been shown to him by the theory of price.

Price adjusted markets … So, he didn't think that there is a great deal of quirkiness in the data?

Quirkiness in the data, yes.

But not in the interpretation?

No.

So, Gardiner Means also had his facts, but they were wrong?

His facts were wrong. Wrong, Wrong, Wrong. And he knew it. How could they be anything but wrong if he thought the economy was not competitive?[27]

So, a priori …

It comes pretty close to it, pretty close to it. My guess is that he came to his conclusion from an a priori position. But then my guess is that most economists make their judgements on a priori beliefs and not on empirical evidence. I want it clearly understood that I'm not singling out George Stigler.

No. I wouldn't think so.

I would apply this to most members of our department [the University of Massachusetts (Amherst)]. I would apply it to all departments of which I have been a member.

I'd have to say in my experience that's true.

So it's not a particularly damming statement about Stigler, it's a damming statement, in my judgement, about the profession. But not about some individual members of the profession as opposed to other individuals.

Yes it's just ironic, I suppose, with George Stigler because he made almost a fetish about empirical research.

Yes.

From fairly early on in his career.

Yes he did.

That, we've got to stop this nonsense and we've got to look at what the data tells us.

Yes.

And yet the data always seemed to confirm what he already knew.

That's your statement but I'm not disagreeing with it.

Again, I'm not saying that that's unique, as we talked about before. But it's just ironic because it was one of his tenets of his Presidential address, his call to arms about empiricism.[28]

Yes.

It's just interesting, the way his particular stance played out.

It's not that he ignored all the empirical work in making his decisions.

No. Not at all.

He would come across empirical work which was contradictory to other empirical work. Somehow it always seemed to him that the empirical work which favored his side was done better than the empirical work which didn't. But this is rampant in our profession. Think about it. Part of the reason for this is the nature of the data that we get. All of it comes from very complicated systems. We have very simple views about how this data is generated, not the data so much but the results measured by the data. The older I get, the more awed I get by the complexity of the way in which the data are generated. I don't mean how it is collected, but the underlying forces which create the measurement of the data. I'm a member of a department where people, many people have the same degree of certainty about the results that come out of this department as George Stigler did about his own. As you probably know, I'm very sceptical about some of what I see as the extreme claims that come out of this department.

Yes.

And I put them and their work on the same level, the same level, but not the same geographic location as I do George Stigler's and Milton Friedman's work. In the final analysis, it's one of the reasons why I do not make sweeping statements about the economy. Well, I could make a few. I can say the economy is large, but I can't go much further than that.

What about complex?

Complex. Large and complex. I will go further. I don't really—I really don't understand. That's a much stronger statement than I don't really understand.

Yes.

And frankly I'm not convinced that anyone else does. I know a lot of people who think they do.

Yes. Great confidence.

They have great confidence. I would say it takes great confidence to think one understands how the economy works.

I often wonder, if I look at the way in which the profession shifts and moves around and forms a consensus, whether any of this is really driven by empirical data, whether that is really ultimately decisive?

I wouldn't want to say there aren't any questions that aren't decided that way.

Well, that would be too sweeping. Too confident.

Put it this way. It's hard to imagine an empirical observation that would convince most members of this Department [University of Massachusetts] and the University of Chicago to change their minds.

It would be difficult to imagine.

My personal view is that if someone holds a view it cannot be dislodged by any conceivable empirical data. Evidence from a data system doesn't convince them. These people have made their decisions already. They've become true believers and no amount of empirical evidence will ever convince them by definition.

Yes, otherwise they would cease being true believers. I know what you mean. I think your course did succeed in adding to my general scepticism about this process which is why I'm …

I consider that a compliment.

It is.

And I thank you for it.

I thank you for it as well. I've been very averse to data analysis because it just bothers me making such definitive statements … I mean I feel too honest to make any claims.

Well, what you saw was part of the ongoing development of my own scepticism. I worked with George Stigler who was one of the finest economists and thought more carefully than most others.

Unfortunately, I don't think the majority of the profession will ever have an attack of scepticism.

Not in my lifetime.

Somehow it's not communicative or contagious.

You might call it a degenerate disease rather than a communicable disease.

I think you're correct, well thank you.

Notes

1. "Indeed I do remember you (whether I will recognize you when you walk in the door is another matter!) To check my memory, I looked up my grade book from long ago. I was correct—you had taken the two-semester econometrics sequence that I used to teach. … The above is largely irrelevant to the question you asked. Yes, I would be happy to answer as best I can any questions you wish to ask about George Stigler and the period of my collaboration with him. That covers only two-plus years of his 80-year life span, and only a small proportion of his lifetime output of papers and books, of course" (e-mail, James Kindahl to Craig Freedman 11 July 1997).
2. Claire Friedland did her graduate work at the University of Chicago, being awarded an MA but never completing her PhD. She became George Stigler's research assistant in February 1959 and continued in that position until his death in December 1991.

    After a while I [Claire Friedland] realized that I was never going to get a PhD. I got a Masters degree instead and stuck around in the Department for another couple of years before I came to the realization that I wasn't going to finish. This was about 1957. I was working part-time for Gary Becker. I realized I had to go out there in the world to get a job. So I talked to Gary. Gary was always a good friend of mine. We went to school together, He happened to be spending one day a week at the Federal Reserve in Chicago and he got me a job. It was a perfectly lovely job. I wasn't learning anything in particular, but I was perfectly happy there. George in '58 had just come to Chicago from Columbia. I had an imaginary conversation in my mind later on, and it was between George and Milton. George got my name from Milton. I imagined George, who was very old fashioned as I said the other day, saying to Milton … "Do you know a girl who would work for me full-time?" I know he would have said 'girl'. He had an enormous grant at that time. And his salary was maybe $25,000 in 1958 dollars. That was what I think he was making per year. *It was one of the biggest salaries in economics*. And he had a grant for a full- time research assistant! So, he asked Milton whether he knew anybody. And Milton would say, "You know, there is a girl who used to be here. She didn't finish her degree, went to the Federal Reserve Bank instead. You might be able to get her back." And he gave George my name. Now the funny thing is that though this conversation was imaginary, George's son Steve found among George's papers, a little piece of paper on which was written my name, spelled wrong, which is perfectly understandable, and then he had written down the phone number of the Federal Reserve Bank. Those were the notes he took of his conversation with Milton, the one that I had imagined. I still don't know if he said 'girl'. Anyhow, he then called me up and I had this wonderful idea that I would work for George, George was already quite famous of course, and have this famous economist's name on my résumé. And I'd do that for two or three years and then I could get a *really good job*. I wouldn't just be someone's research assistant. I started in February of '59 and when George died in December '91, almost thirty-three years later, I was still working for him. (Conversation with Claire Friedland, October 1997)

3. Gary Becker earned a B.A. at Princeton University in 1951 and a Ph.D. at The University of Chicago in 1955. He taught at Columbia University from 1957 to 1968 before returning to Chicago.
4. George Stigler's own prize student, Sam Peltzman, had to be lured back from U.C.L.A. not only with heavy lobbying on the part of Stigler himself, but with a solid commitment of earmarked research funds from Stigler's own Walgreen funds. [Stigler's Walgreen endowed chair came with a large chunk of available research dollars.] In the same way, Gary Becker was enticed back to Chicago, in part, by an attractive package backed up by ample resources from the same pot of Stigler controlled funds.

   The best young economist in the world is Gary Becker … I think it [the pitch] should include

   1. A salary of a least $45,000
   2. The right to bring a colleague or two
   3. Anything else he wants

   (I have already, on my own offered him $50,000 of Walgreen money for research, no questions asked). (Letter from George Stigler to Edward H. Levi, President, University of Chicago, November 25, 1968)

5. The first edition of Stigler's *Theory of Price*, his expanded version of *The Theory of Competitive Price* (1942), first appeared in 1946, with a revised edition appearing in 1952. (A later 3rd edition would see the light of day in 1966. The textbook's last hurrah arrived in 1987 with this 4th and final edition perhaps being driven more by his 1983 Nobel Prize than an up-swelling of demand for yet another intermediate microeconomics textbook.) Here James Kindahl remembers being assigned the first edition as a MBA student in the business school. Considering the time line he provides, it is highly unlikely that he would have been using the 1952 revised edition. In any case, the text would have been a challenge for any student of economics, especially one pursuing an MBA in the business school.

   My only exposure to the Stigler influence at Columbia was studying his book, *The Theory of Price*, in the introductory graduate theory course. The book is probably the least readable thing Stigler ever wrote. It was not a matter of convoluted writing or confused thought – Stigler was never guilty of either of these common academic sins – but of excessive condensation that required painstakingly slow

> pondering over every concentrated thought. If the book had been three times as long, it could have been read in half the time. Still, it remained something of a classic, though Stigler himself made many a wry joke about the supposedly meagre sales. It was the kind of book that teachers of price theory courses read themselves, while they assigned some other text to the class. It was a far better book for reminding you of price theory than for introducing you to it. (Sowell 1993: 785–786)

The first edition of the text did in fact contribute to the tension and bad feelings between George Stigler and Edward Chamberlin. Though, not much was needed to rile Chamberlin. Chamberlin, who was almost obsessional about the due regard awarded to his theory of monopolistic competition, gave Stigler's textbook a catty and somewhat petty review in *The American Economic Review* (1947) due to the book's presentation of imperfect competition. Stigler was far from pleased with Chamberlin's evaluation. His pointed, yet highly controlled, letter to Chamberlin has embedded within it the seeds of his attempt to eviscerate the theory of monopolistic competition in his 1948 LSE lectures. Those lectures were reprinted the following year (1949).

> In any event, it is not a sin to reject your orientation: in this I have very illustrious companions. I am prepared to argue (1) that your theory is indeterminate, and (2) that it is not useful (often) in realistic analysis. I do not recall a single consistent application of it to a real problem, and this is the ultimate failure of a theory. But these are larger issues, which I hope we can thresh out in person. (Letter from George Stigler to Edward Chamberlin, August 1947 in Hammond and Hammond, 2006: 62–63)

Those with an unexplained hankering to learn more about Stigler's textbook, other than by actually reading it, are strongly referred to Kamerschen and Sridhar (2009).

6. The book would become (in 1970) *The Behavior of Industrial Prices*, co-authored by George Stigler and James Kindahl. In some sense this would be an attempt by Stigler to put to rest (in the sense of burying) Gardiner Means' theory of administered prices. Stigler's earlier attempt to eviscerate this same theory in 1961 (*The Price Statistics of the Federal Government*) had failed to drive the stake sufficiently through the heart of the theory. In shadowy places it still roamed, seeming at times to gather strength. In the 1970 effort, Stigler pulled out a cache full of silver bullets and a truckload of garlic to finally rid the earth of the theory's stinking corpse.
7. Claire Friedland told a very similar story of how George Stigler hired her on Milton Friedman's say so. This is just a small reflection of the remarkable trust the two friends and colleagues placed in one another.

> Because George Stigler, who was very critical of people, was almost worshipful of Milton Friedman. And I remember that one of his dicta was that a Milton Friedman theorem was more credible than any other theorem, because everybody picks on Milton. It's an unfair world and so forth, which means that he gets a more rigorous testing than anyone else. Doesn't he have genuinely adulatory remarks in his autobiography about Milton? Now, what you have to understand with somebody like Allen Wallis, and so to a degree those people who were in his circle, is that Allen Wallis had the sharpest priors - I'm using the language of Bayesian probability - of anybody I ever knew. Almost no new data could change his view for this reason. On the other hand, if he thought of somebody as a dangerous, or an incompetent thinker, but Jimmy Savage assured him that the man was very smart and had good judgement that carried more weight with Allen Wallis than a two-year study of the person's vitae and an audit of his writings. (Conversation with Paul Samuelson, October 1997)

8. Much to his chagrin, George Stigler repeatedly discovered that ideas and theories, which he perceived as groundless, did not die so easily despite receiving what he considered to be fatal blows to their credibility. He had conceived that the 1961 volume he edited (also under the kindly eyes of the NBER) had delivered a deathblow to George Means' obvious hokum built upon a foundation of administered (or rigid) prices. Close to a decade later, Stigler was more than willing to climb back into the ring for another round or twelve with his designated (and insufferable) menace.
9. Inland Steel (1893–1998) was born out of the financial panic of 1893. Like its rival, US Steel, it was an integrated steel company with its headquarters and plant located in East Chicago, Indiana (a short hop from the University of Chicago). Its assets were initially taken over by Ispat International and are currently part of the world's largest manufacturers of steel, ArcelorMittal. The corporation is headquartered for tax reasons in Luxembourg. Few companies would bother to locate there for the provided ambience.
10. The usefulness of the price index that the co-authors had constructed was emphasized by James Kindahl in a note sent to George Stigler (July 29, 1972) while working on their reply to George Means.

    > Our study would have value as a study of the validity of the WPI [wholesale price index] even if we were dead wrong about administered prices.

11. Stigler's stance is suggested in such statements as "I'm absolutely confident," Stigler said recently, "that there has been extensive price cutting during the slowdown of the past six to eight months: But these cuts have not showed up in the government's WPI" (National Bureau of Economic Research Press Release 1970, p. 1). 'Still according to Stigler,

the buyers' index proved to be "vastly superior" and "much closer to the truth" than prices reported on similar goods by BLS' (National Bureau of Economic Research Press Release 1970, p. 2). A reader is left wondering why "superior" and "closer to the truth" are not sufficiently descriptive of the Stigler/Kindahl index. That confidence is noted in the book as well. As one respondent to Means' torrent of queries puts it, "It seemed to me on a quick reading that the authors got carried away a bit in their conclusions, that their actual findings were less conclusive than their summaries" (Letter, Otto Eckstein to Gardiner C. Means, August 4, 1970).

12. One motivation behind Stigler's statements may lie in a final quote that ends the *NBER* press announcement. "The evidence of considerable price movement," says Stigler, "makes rumors of the market's death, spread by John Kenneth Galbraith and some other economists, quite suspect" (National Bureau of Economic Research Press Release 1970, p. 3). Of course no love was lost (at least academically) between Galbraith and Stigler who developed a long history of sparring with one another. Upon Gardiner Means informing Galbraith of the controversy centered around the Stigler/Kindahl publication, Galbraith replied, "I don't think it's worth worrying too much about Stigler. He is a superficial man who regrets his inability to command any general interest and attention for his work. If somebody gets satisfaction defending past error, I don't think it should be denied to him" (Letter, John Kenneth Galbraith to Gardiner C. Means, August 19, 1970).
13. George Borts in his reply to Means states, "The reader of your paper, whose comment is enclosed, feels that it does not make a sufficiently strong contribution to be published in the Review. After reading it I must regretfully agree. I will not be interested in the paper but wish to thank you for sending it in" (Letter, George Borts to Gardiner C. Means, November 18, 1975). Means' strong objections to this response led to a review of the decision at the meeting of the Executive Committee of the *American Economic Association* in Dallas on December 27, 1975. The end result was a final letter from Borts. "My decision not to publish the paper was discussed by the Executive Committee of the American Economic Association at its meeting December 27, 1975. They agreed that I am under no obligation to continue publication of the arguments between yourself, George Stigler, and James Kindahl, in view of the papers already published in the

Review by yourself (June 1972) and Stigler-Kindahl (September 1973). This does not mean that the issue is closed, however. In rereading the two papers, I can see room for re-evaluation of the controversy. But I wish to do so in a context which is free of any hint of personal attack or defense. Therefore I shall invite an impartial outsider to write a review article on the controversy. This will take some time, but I am looking forward to publishing an objective evaluation of the issues" (Letter, George Borts to Gardiner C. Means, January 6, 1976). As an absent member of the executive committee cynically replied to Means, "The economists seem to run about as tight a closed shop as the doctors and lawyers and all the other professional vested interest chaps" (Letter, Paul Sweezy to Gardiner C. Means, March 8, 1976). In any case Means was less than mollified. "Your proposal to see an 'impartial' appraisal of the state of the administered-price controversy is obviously no substitute for my rights as an author or your readers' rights to a correction of the misstatements of fact about my work and the misrepresentation of what I have said!" (Letter, George Borts from Gardiner C. Means, March 3, 1976).

14. The published exchanges were quite fierce to say the least. Despite strenuous efforts, Means was unable to stop or rescind the publication of the 1970 book. "I am shocked that the National Bureau would present its scientific data in such a biased and unscientific fashion as that represented by the Stigler report. The basic findings are directly contradicted not only by the published data but by the text itself" (Letter, Gardiner C. Means to Theodore O. Yntema, Chairman, National Bureau of Economic Research, July 24, 1970). After some toing and froing, a final reply closed off any hope of the NBER stepping back. "If, after the exchanges of criticisms, responses, and reconsiderations which have taken place over the past several months, you and the authors still differ with respect to definitions, interpretations, and warranted findings, we believe these differences of view between respected scholars should be put in the public domain and made available to all who may be interested. It seems to us that the best way to do this is for you, if you wish, to send your comments as a note to a scholarly journal; for example, to the Journal of Economic Literature, which publishes review articles" (Letter, John R. Meyer, President, National Bureau of Economic Research, to Gardiner C. Means, March 17, 1971). Gardiner Means was far from pleased with this response. "I have

your letter indicating that the Executive Committee of the Board has decided to take no action with respect to its report on the behaviour of Industrial Prices. Does this mean that the report meets the Board's conception of scientific and impartial presentation and interpretation of economic facts?" (Letter, Gardiner C. Means to John R. Meyer, President, National Bureau of Economic Research, April 17, 1971). Gardiner Means then went on the offensive in a detailed 1972 attack on Stigler's work ("The Administered-Price Thesis Reconfirmed"). The compliment was returned in the joint 1973 paper by Stigler and Kindahl ("Industrial Prices, As Administered by Dr. Means"). George Borts, then editor of *The American Economic Review* had invited Stigler and Kindahl to reply not only to the attack by Means but also to the 1972 note by George A. Hay then at Yale ("A Note on the Stigler-Kindahl Study of Industrial Prices"). The Hay note being uncontroversial was simply ignored.

> Dear Professor Stigler [Professor Stigler crossed out and George written in] I am enclosing proof of the Means article and a manuscript copy of the Hay comment. Should you and your co—author wish to write a brief comment on the Means article or the Hay note, I will be pleased to publish it.

George [Borts]

Both primary combatants proved eager to continue the bloodletting. Such behaviour was quite atypical for George Stigler who usually preferred more of a hit and run approach. If bothering to respond at all to criticism, the reply was usually terse and dismissive. There were then no further rounds issuing from his corner. Something about Means seemed to rile Stigler like few others ever succeeded in doing either before or after. George Borts, however, had had enough. Instead he offered to have Leonard Weiss, then at the University of Wisconsin, evaluate the controversy and give his professional opinion as a means of stopping what might otherwise prove to be an interminable debate. Weiss had done his graduate work at Columbia University, arriving there in 1947 and receiving his PhD in 1954. This would have made him contemporaneous with George Stigler, who was a member of the faculty from 1947–1958. Their contributions to the field of industrial organization shared few points in common. Weiss was one of the leading figures in developing the Structure-Conduct-Performance approach that dominated the field for a time. The actual article Weiss wrote that appeared in 1977 ("Stigler, Kindahl, and Means on Administered Prices") elicited a rather bitter private note from George Stigler.

> Unlike Means, I am not going to pursue the subject in public, or, except for this letter, in private. I assume that your piece in the *AER* is that commissioned by the association's officers to satisfy Means. I hope you succeeded in your task: assuredly you don't satisfy me. (George Stigler letter to Leonard Weiss, September 25, 1977)

15. There is some confusion here. Means sent his correspondence to Stigler and Kindahl as well as the NBER. The NBER forwarded what they received to Stigler and Kindahl as well. In addition there was direct correspondence between Means and Stigler and Means and Kindahl prior to the back and forth in *The American Economic Review.*

    > I have read carefully your comments on The Behavior of Industrial Prices. It seems that since George Stigler and I are jointly responsible for the book, and for the statements to which you take exception, we should submit a joint reply. While I was in the process of preparing a draft of such a reply, I learned that George Stigler's wife died suddenly on August 22. Under the circumstances, I plan to hold my draft for a week or two before sending it to him. I'm sure you will understand. (Letter, James K. Kindahl to Gardiner C. Means, September 2, 1970)

16. An indication of Means' fixation is perhaps indicated in a letter sent to James Kindahl once he was informed of Mrs. Stigler's death.

    > Thank you for your note explaining the delay in dealing with my comments on your price study. I am sorry to hear of the death of Mrs. Stigler and quite understand your hesitation in pressing the matter with him. However, if I am correct in believing that your new data in a high degree validate the BLS indexes and strongly support the administered price thesis rather than the opposite as indicated in the book's "findings", the sooner the false impression given in the book needs to be corrected. (Letter, Gardiner C. Means to James K. Kindahl, October 26, 1970)

17. In a personal communication, Kindahl makes it clear that his opinion of Means' analysis is anything but favorable.

    > I personally find the doctrines (plural) of administered pricing theoretically elusive and empirically dubious. I shall not accept such a view of the world until either a good theoretical justification is developed or the empirical evidence in favor of it is strong. I do not regard our findings as evidence in favor of the variants of the doctrine that I know. Apparently you do. Our published data are now in the public domain, and you are free to interpret them for yourself as you see fit. I am not convinced of the validity of your interpretation. (Letter, James K. Kindahl to Gardiner Means, November 9, 1970)

18. In his criticism of the Stigler and Kindahl work, Means makes it clear that he is focused on a relative criterion. Administered prices are simply less responsive to market forces, namely shifts in demand over a given business cycle.

    > Similarly, since administered prices tended not to fall as much in a recession, they tended not rise as much in recovery … (Means 1972: 292)

> First, the authors admit that their report does not test or even discuss what they call "the hypothesis of relative price insensitivity" yet this is the only general administered price hypothesis I have advanced. As a result, their report has nothing to do with the true administered price hypothesis in spite of the appearance of my name 17 times in its first 20 pages. (Letter, Gardiner C. Means to Theodore O. Yntema, Chairman of the Board, National Bureau of Economic Research, January 15, 1971)

19. The conflict involved two irreconcilable views of the market. Stigler clearly defended a strict belief in classical price theory with Means representing the polar extreme.

    > My quarrel is not with the data but as I have already indicated, with its interpretation. If I am wrong, and I could be, traditional theory can still apply to the modern concentrated economy. But if I am right about the administered price thesis and there is nothing in your findings to persuade me otherwise, then short run economic theory will have to be thoroughly overhauled. In the short run, an economy in which each price changed only once a year (even if the changes for different buyers were scattered through time) would be quite a different kind of economy from the classical economy of flexible prices such as is represented by the Walrasian system of equations. (Letter, Gardiner C. Means to James K. Kindahl, October 26, 1970)

20. There seemed to have been a tendency, at least for Stigler, to have seen all results as vindicating his belief in flexible prices, with Kindahl being far more cautious.

    > I computed changes in WPI indices for the year 1970 and for July '69 – Nov. '70 (peak to trough of the industrial production index). The behaviour of measured prices is about what Means would expect, and claims. I read this as a deficiency in the WPI index, not a vindication of Means. Nevertheless, there seems little to be gained by incorporating the figures into a reply. (Note from James Kindahl to George Stigler June 28, 1972)

21. The way in which it is formulated in their reply is actually:

    > Consider the two recoveries: the decreases of prices in the recoveries account for *more than half* of the 100 observations on Means-defined administered prices in recoveries. These are classified in his tables as "conforming to the truncated administered-price hypothesis." Shall we call this "administered-price deflation"? The extraordinary intellectual imperialism of this label should be clear. (Means 1972: 292–306)

22. As she had since 1958, Claire Friedland (George Stigler's long-term research associate) was an active member of the team. She, like James Kindahl, had a much more modest estimation of the data.

    > Well the truth is the BLS statistics were not very good. Looking at them and seeing how they were collected, there is no question about that. But ours weren't perfect either. Ours were full of holes, but they were better, even if by only five percent. That's what I would have said, but George said, "Look at our data!" and "We have done a beautiful job", which we had done. Ours were better because

> the source of our statistics was a collection of the actual transaction data, whereas the BLS just took any old thing that anyone sent them: contract prices, list prices, etc. And they are now collecting some prices our way. (Conversation with Claire Friedland, October 1997)

23. As Claire Friedland notes about Means' work:

> This all proves that you can prove anything with numbers. This is all really a matter of interpretations. (Claire Friedland note to George Stigler, April 28, 1972)

24. I clearly have the year wrong. The paper was published in 1986 (Carlton, Dennis "The Rigidity of Prices"). In a private correspondence, Dennis Carlton assured me that his colleague at the University of Chicago Graduate Business School, George Stigler, had read the paper and was favorably deposed toward it.

> I am writing in reply to your letter of January 12. George Stigler was extremely supportive of my work with the Stigler Kindahl data set on non-price clearing as well as my other work (otherwise I never would have received a tenure offer). George felt that he and Kindahl had uncovered some peculiar pricing anomalies in their earlier study, but that they uncovered them at the end of their work and was grateful that some of those anomalies (like price rigidity and varying contract length) were being further investigated. My recollection is that George (with whom I spoke frequently – his office and mine were across from each other) gave me helpful comments on my price rigidity paper (I thank him in the published article) and I do recall giving the paper in his workshop and feeling that the paper was well received. I think the key to understanding George's hostile reaction to Means is that George believed markets worked and would determine price. Means never clearly stated what he thought determined price, hence had no theory to explain his price rigidity. George was perfectly content to believe that market forces could explain price phenomenon like rigidity and I believe he liked my work because I presented an equilibrium theory in which I explained how the rigid prices were set. I always found George receptive to using microeconomic theory to explain data that were not what the simple perfectly competitive model predicted. (Letter from Dennis W. Carlton February 6, 1998)

25. Borts, of course, had to keep his personal views to himself and far away from the two inflamed protagonist. He simply chose to sidestep Means.

> I shall have to seek other means of calling the attention of those interested in the subject matter, at home and abroad, to the seventeen serious errors of fact and misinterpretations of my position contained in the Stigler-Kindahl article, as well as the treatment I have received at the hands of my professional association. (Letter, Gardiner C. Means to George H. Borts, March 3, 1976)
>
> I have no desire to stifle debate over the differences between you and George Stigler, but there is no reason why this debate need be the sole property of the American Economic Review.
>
> I have succeeded in enlisting the interest of an economist who has worked in this field. He is willing to write an objective appraisal of the controversy. If you wish I shall be pleased to send him a copy of your notes on the seventeen

> "misinterpretations". (Letter, George H. Borts to Gardiner C. Means, March 15, 1976)

As noted above, George Borts could have certainly chosen someone more favorable to Stigler's point of view had he deemed to do so. George Stigler was certainly unhappy with Leonard Weiss's article when it appeared.

26. George Stigler headed the Price Statistics Review Committee set up by the NBER in 1960 for the Bureau of the Budget. As mentioned above, the result was the 1961 book: *The Price Statistics of the Federal Government* (ed.).
27. George Stigler always marketed his views confidently, trying to push his ideas until they squeaked. It was for others to prove him wrong. This rhetorical approach comes through clearly in a note written by his long time research assistant, Claire Friedland, at the time (June 9, 1972) when George Stigler and James Kindahl were preparing their reply to Gardiner Means' criticism.

    > I realize that you've exercised as much self-restraint here as I have by giving up chocolate, but I think "never before employed", "wholly novel", and "utterly" should be deleted on the ground that sentences employing words "wholly", "never" & "always" are always wholly wrong & never right (a rule I learned in Forest Hills High School for use in multiple-choice exams).

28. His 1964 Presidential address appeared as 'The Economist and the State', *The American Economic Review* (1965). It is perhaps his equivalent of Miranda's 'Brave new world', speech in *The Tempest.*

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George Stigler's Career Moves: The Roles of Contingency, Self-Interest, Ideology, and Intellectual Commitment

David Mitch

George Stigler is commonly associated with the Chicago School of Economics and is generally seen as one of the central figures in such a school (see Nik-Khah 2011, p. 116). However, he did not actually take a faculty position at the University of Chicago until the age of 47. Although his time there as a faculty member of 23 years occupied the largest part of his academic career (and admittedly 30 years plus if one includes his time after stepping down from the Walgreen Professorship) he had previously spent 22 years at other institutions. Moreover, his return to Chicago was by no means inevitable. Not only was the first serious attempt at bringing him to Chicago spurned by Chicago's central administration, the Economics department had reluctance in at least one subsequent attempt and Stigler himself then turned down multiple subsequent attempts to get him to return. The final successful effort originated from the Graduate School of Business, not the Economics Department. Examining the factors behind this circuitous

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return to his graduate school alma mater provides perspective both on the diversity of views present in the Economics department at Chicago at various points in time and on the evolution of Stigler's own aspirations and loyalties.

This essay will provide a narrative account of George Stigler's various career transitions from graduate school through his "retirement" (he was in fact quite active in both scholarly and organizational activities after stepping down from the Walgreen Chair). This narrative structure will be employed to bring out what archival material implies about a number of general themes regarding Stigler's career.

A first theme concerns the role of contingency. Chance appears to have played central roles in his initial academic appointment at Iowa State, in the abortive attempt to recruit Stigler to Chicago in 1946, and the successful attempt to recruit him to the Graduate School of Business in 1958.

A second theme concerns the intellectual diversity of the academic milieus in which Stigler operated counter to the conventional view of a monolithic free market-focused Chicago school. Heterogeneity was certainly characteristic of the Chicago department in 1946 that failed to make Stigler an offer. But it was arguably true of the Chicago department in the early to mid-1950s that did make Stigler an offer which he declined. And his decline of Chicago offers also points to his mixed loyalty towards both Chicago and his academic home for about a decade, Columbia University.

A third theme concerns the extent to which Stigler was a partisan or a scientist in his academic endeavors (see Nik-Khah 2011, pp. 117–118) and whether he viewed the economics profession as more swayed by the social environment of its times or whether it made independent scientific and intellectual contributions to social policy. The view taken here is that Stigler probably saw both partisan elements and scientific endeavor in play in the profession of economics as suggested by both his own written statements and by his organizational endeavors.

Stigler (1983, p. 533) recognized the possibility that economists like all self-interested agents have responded to the environments in which they have found themselves, yet he also noted that "A science required for its very existence a set of fundamental and durable problems."

He states in his essay "The Intellectual and the Market Place" (p. 92):

> I have seen silly people –public officials as well as private, by the way — try to buy opinions but I have not seen or even suspected cases in which any important economist sold his professional convictions.

Stigler did think Economics was a science (see Stigler 1982/1983, p. 35):

> Please do not read into my low valuation of the importance of professional preaching a similarly low valuation of scientific work…The most influential economist, even in the area of public policy, is the economist who makes the most important scientific contributions.

A final theme will concern the extent to which Stigler as Nik-Khah (2011, p. 117) among others (see Fourcade and Khurana 2013) was an empire builder, especially during his tenure as Walgreen Professor of American Institutions and then in establishing the Center for the Study of the Economy and the State (Wallis 1993). A review of both Stigler's management of the Walgreen Foundation and his years with the Center for the Study of the Economy and the State suggest that the breadth of Stigler's intellectual interests and his collegiality offset any empire-building proclivities. A related issue concerns the role of personal and institutional loyalties throughout his career; at various points they were notably present and at others notably absent.

While the essay will survey Stigler's entire career, particular attention will be devoted to the 1946 episode in which Chicago failed to make him an offer and the 1957–1958 episode in which W. Allen Wallis successfully induced to him take over the Walgreen Foundation and Walgreen Professorship.

Iowa State and Minnesota

Stigler's first faculty appointment was in 1936 at Iowa State College, its name then, in the Department of Economics and Sociology. The chair of the department at this time was T.W. Schultz. According to Stigler's account in his memoirs, this job only opened up after Homer Jones, his classmate from Chicago turned down the position. Indeed Stigler claims he told Jones that if the latter had accepted the Iowa State post, Stigler would "probably have become a Seattle real estate dealer" (Stigler 1988, p. 39). However, Frank Knight's recommendation letter to Schultz indicates that Knight recommended Homer Jones only after Stigler indicated to Knight that he was not interested in the position; and Knight indicates that Stigler subsequently changed his mind:

> I do want to write to you about the man who was first on my mind before, and whom I believe I did mention, namely George Stigler. He is the man I thought would be most interested in your prospective position, and perhaps most suitable for it, but at that time he did not wish to be considered. Now, however, he has changed his mind. (Knight's Letter to T.W. Schultz 1936)

Knight describes Stigler as follows:

> He is distinctly a theorist, in the general price theory sense, and is one of the very few very best who have ever worked with me. He now reads German quite competently, and uses mathematics far beyond my own ability to do so…In comparison with Jones I would say that Stigler's interests are slightly more along purely intellectual lines, but he also takes an active interest in questions of public policy, has already spent some time in Washington, "New Dealing," and expects to go into some of the government departments if a satisfactory teaching position does not open up. I do not know of his having any special interest in agricultural economics, except possibly the forestry branch; his father is a lumber man in the Northwest. (A copy of Knight's letter was kindly provided by Stephen Stigler)

Knight's characterization of Stigler as having engaged in "New Dealing" presumably refers to an episode Stigler mentions in his memoirs (Stigler 1988, p. 52) in which he mentions that he spent part of 1935 in Washington as an assistant economist in the National Resources Planning Board. He describes his assignment as "to help estimate the portion of the benefits of federal works that might properly be charged against state and local governments because of the local benefits they received." One should also note that Knight's characterization Stigler's father as a lumberman seems at odds with Stigler's account in his memoirs of his father's career as a brewer and then in real estate (Stigler 1988, pp. 9–10).

Stigler spent two years at Iowa State primarily finishing his Ph.D. dissertation (Stigler 2009, p. 83). When Schultz was subsequently department chair at Chicago and Stigler's name came up on the short list of faculty candidates in 1946, Schultz only ranked Stigler 4th out of 5 on the short list. And D. Gale Johnson, who Stigler mentions as one of his students at Iowa State ranked Stigler only 4th out of 5 on his short list rankings as well. Friedman in his correspondence with Arthur Burns reports that in early 1946 negotiations to hire Stigler, Knight thought Schultz was only lukewarm about hiring Stigler and that Knight did not trust Schultz on this.[1]

Thus, in his deliberations over accepting his first academic appointment, Stigler appears to have displayed some ambivalence about pursuing an academic career. And subsequently his then Iowa State colleagues and students appear to have displayed some ambivalence about Stigler.

Stigler (1988) notes that he had both good colleagues and good students at Iowa State (including D. Gale Johnson as a student), but that just as he was getting established there, he met Frederic B. Garver at a meeting of economists in Des Moines and Garver invited him to take an appointment at the University of Minnesota which Stigler accepted. Stigler indicates that a possible rival for his position at Minnesota was Oskar Morgenstern. Alvin Hansen had just departed Minnesota for Harvard before Stigler's arrival. Stigler lists Garver, Francis Boddy, and Arthur Marget as his closest colleagues. While he rose through the ranks as Assistant, then Associate, and then Full Professor at Minnesota not officially leaving until 1946, he spent the years 1942–1945 on leave first at the National Bureau of Economic Research and then with the Statistical Research Group at Columbia University doing military-related research.

Stigler's correspondence while at Minnesota shows that he did lobby for a faculty position for Milton Friedman at Minnesota in 1945, after Friedman's difficulties with an appointment at the University of

Wisconsin. (See Stigler's correspondence with Arthur Burns; Arthur F. Burns papers, Duke, Rubenstein Library, Box 3, George Stigler correspondence file.) Friedman in his memoirs (Friedman and Friedman 1998, p. 147) also acknowledges Stigler's role in securing an appointment for Friedman at Minnesota.

In 1941, Yale appears to have made an effort to recruit Stigler. In a April 21, 1941 letter to Stigler, W. Allen Wallis who had recently been teaching at Yale, strongly discouraged Stigler from moving from Minnesota to Yale. Wallis acknowledged that Yale would likely pay more than Minnesota and that Yale had considerably more "social prestige" than Minnesota. However, he also thought that so much of the Economics faculty at Yale were "dead timber (not old timber, unfortunately, so there is no possibility of making any material progress in clearing away the debris within the next decade)."

The subtext of the correspondence between Stigler and Wallis in 1941 would seem to be that they both put a premium on having intellectual stimulating colleagues and on academic achievement rather than social prestige or generous compensation. By the mid-1940s, Stigler clearly seems to have been aspiring to a position in the top tier of economics departments.

The Abortive Effort to Recruit Stigler to Chicago in 1946[2]

By 1945, Stigler was clearly on the radar screen of the Department of Economics at Chicago as a potential hire as the department was seeking to build up its own faculty. His name is one of the 20 economists on a list of "of possibilities for new appointments" in a 1945 strategic planning memo on postwar plans for the department of economics at Chicago prepared by then Chair Simeon Leland. Among others, John Hicks and W. Allen Wallis are also on this top 20 list, while Milton Friedman is only on a follow-up list for a possible joint appointment with an Institute of Statistics and Friedrich Hayek and Paul Samuelson only warrant mention as possible visiting appointments. Interestingly, Stigler's fields are listed in this document as "Theory and Foreign Trade."

Henry Simons, in an August 20, 1945 memo to Leland gives higher priority to hiring Friedman over Stigler, but he strongly favors hiring Stigler as well:

> If Lange is leaving, we should go after Milton Friedman immediately.
>
> It is a hard choice between Friedman and Stigler. We should tell the administration that we want them both (they would work together excellently, each improving what the other did), Friedman to replace Lange, Stigler to replace Knight and to be with us well ahead of Knight's retirement.

In early 1946, when Chicago's department faced having to replace the departing Jacob Viner and more generally build up its faculty in economic theory, Stigler's name featured prominently in deliberations. In Mitch (2016), I argue that Chicago's Economics Department at this time was split between two factions. Frank Knight led one faction. Knight saw Stigler as the natural successor to Viner and Knight in taking over the teaching of Economics 301, the department's core graduate price theory course. Jacob Marschak led the other faction which included his Cowles Commission colleagues. They favored someone with a more technical approach to economic theory such as John Hicks or Paul Samuelson. The following provides further detail on how this disagreement played out as it relates to Stigler.

Henry Simons and Frank Knight both issued memos supporting the ready appointment of Stigler:

> Memo that can be ascribed to Simons:
> Viner also urges, as I have long urged, that we move promptly to get George Stigler and Milton Friedman (both now at Minnesota). Viner regards Stigler as the ideal person for Economics 301; Knight regards Stigler as be far the best person to take over Knight's courses at, and partly before, his retirement. The complete agreement of Viner and Knight on the Stigler appointment should, I think, be decisive. Action in this case seems urgent; Stigler would not be hard to move from Minnesota; but he is likely soon to be offered posts from which he might not be easy to move.

Frank Knight in a January 31, 1946 memo to absent department faculty stated:

> On the theory side, Viner suggests George Stigler, but he is opposed by a minority of the group in favor of J.R. Hicks.

However, Jacob Marschak of the Cowles Commission and John Nef, a rather heterodox presence in the department were far less enthusiastic about a faculty appointment for Stigler.

Jacob Marschak in a counter-memo to Knight's and dated February 1, 1946 also to absent faculty states:

> … my impressions differ somewhat from those of Knight.
>
> This applies in particular to the question of the relative merits of Hicks and Stigler. My impression is that, while granting that Stigler would be a good teacher of the course 301, there was little disagreement on the superiority of Hicks as a leader and discoverer pushing the frontiers further; and there was evidence of Hicks having been a very inspiring teacher when at the London School. My impression was therefore that Hicks was regarded by some of us as the man to be put at the top of the list, or possibly after Robbins, the latter's assets being those of an all-round man, a charming personality and an experienced practitioner of international economic policy.

John Nef was noticeably even less impressed by Stigler than Marschak. In a February 5, 1946 letter to Knight, Nef states:

> I do not profess to have a profound grasp of economic theory, but I have had as students—and have to some extent followed the careers of –Messrs. Friedman, Hart and Stigler. I have no hesitation in saying that none of them can be regarded as satisfactory to replace Viner. They not only lack his intelligence and drive; they have no real conception of the meaning and value of careful historical scholarship, which he understood and practiced; nor are their interests in knowledge anything like as broad as his. In short they are simply not up the same street, as the English say. I write not to disparage them. They are very pleasant, bright young men, and excellent material for Oberlin, Columbia or Northwestern.

Department Vote and Rankings

Records of the actual department vote in early February indicate that Stigler was strongly preferred by Frank Knight and colleagues associated with him. Lloyd Mints and H. Gregg Lewis along with Knight all ranked Stigler first of five finalists (the others being Milton Friedman, Albert Hart, John Hicks, and Paul Samuelson). And Simons ranked Stigler equally with Friedman and Hart, giving all three 2's and no one 1's. However, others ranked Stigler considerably lower. Only Jacob Marschak ranked Stigler in last place, but Hazel Kyrk, D. Gale Johnson, T.W. Schultz, and Robert Burns all ranked Stigler as 4th out of 5. Interestingly, Tjalling Koopmans who had strong ties with Marschak on the Cowles Commission, indicated in his rankings that he had insufficient knowledge of Stigler to rank him.

John Nef, Paul Douglas, and Oskar Lange were all absent from the February 11, 1946 meeting but in submitting their rankings in absentia, Nef declined to rank any candidate as acceptable except Hicks, while Douglas ranked Stigler as 4 out of 5 and Lange as 5 out of 5. At a February 14 meeting, when the results of the absentees were considered, Stigler, Friedman, and Samuelson were all perceived as close together in the rankings and further voting showed that Stigler ranked first of the bottom three followed by Samuelson and then Friedman at the bottom. At the February 14 meeting, the department approved offers to Hicks, Hart, and Stigler.

Hicks and Hart turned down their offers. Stigler visited Chicago to meet both with department faculty and with central administration. However, as Stigler himself has reported, when it came time for Stigler to interview with administrators in Central Administration, the Chancellor, Robert Hutchins was sick with a cold that day and so the President, Ernest C. Colwell interviewed Stigler instead. Stigler reports that Colwell thought that Stigler was too empirical. However, Colwell's notes as reported to Hutchins suggest a different perspective:

In a February 22, 1946 memo to Hutchins, President E.C. Colwell states the following about his interview with Stigler:

> Schultz brought in George Stigler in Economics. He was educated here in classical theory economics. He has moved towards empirical economics since. He is well educated. He is intelligent. But he is not brilliant for my money and I think he lacks the drive of either Viner or Schultz or the really strong men in the field.

In an April 9, 1946 letter to W. Allen Wallis, Friedman states about the Stigler interview with Colwell:

> I got from Schultz indirectly the impression that George may have misread Colwell and have taken some remarks he meant ironically and to draw George out as serious.

Friedman had noted to his mentor Arthur Burns in an earlier letter dated March 3, 1946:

> The main possible source of a slip lies in with George's case. George was down a week ago. Unfortunately Hutchins was sick, so George saw Colwell (the President—also present at my interview at which he said not more than 10 words) & Gustavson (Vice-Pres.). Apparently, he performed very badly from their point of view–& gave them such a strong negative reaction that it is very doubtful they will agree to his appointment. Schultz told Knight that he felt now that a direct request for approval would meet with a direct refusal & would kill it for good, & beyond he was very reluctant to make the request. Kn. feels it should be pressed, & is very likely, one way or another, to go direct to Hutchins. I don't know what should be done. Knight suspects Schultz of being lukewarm to George & doesn't trust him. The thing is an awful mess, which is, of course, terrible for George, as well as for his supporters down there, of whom Knight is the chief. And it does seem as if the clear wish of the Dep't ought not to be vetoed on the basis of a half-hour's chat.
>
> Mints & Simons feel that George's case has no connection with mine, that both appointments can, and so far as the dep't is concerned, would be made. George & I feel that Schultz was compelled to call me so hurriedly because he was counting George out & I was the next on the list to take over the work in theory. The most probable interpretation seems to me

> to be that that was why he called me; but that, having called me & gone so far as to get administrative approval and having been pretty well sold on me himself (I had never met him & again, I'm citing Kn. & Mints on his having been sold) he would be very likely to go ahead on me even if George's were to come through.

As is well-known, after the Central Administration refused to approve Stigler for a position an offer was made to Friedman. Stigler (1988, p. 40) quipped "So the professorship was offered to Milton Friedman, and President Colwell and I had launched the Chicago School."

Thus a major chance factor in Stigler's career at this stage turned out to be intervention by Chicago's Central Administration, an act that can be attributed to the accident of which administrator happened to interview Stigler on his campus visit. However, as well as the role of chance, the episode also underscores the ideological heterogeneity present in Chicago's department at this time. But it should also be noted that Stigler was clearly the favored candidate of his graduate school mentor, Frank Knight, as well as by two of Knight's colleagues.

Stigler at Brown and Columbia

Though by 1946, Stigler was a full professor at Minnesota, he decided to leave for an appointment at Brown. He only stayed there one academic year. During Stigler's first term at Brown in Autumn of 1946 he was also teaching at Columbia, commuting from his home in Providence (Hammond and Hammond 2006, p. 149, note 56). In a November 1946 letter Stigler to Friedman (p. 42 of Hammond and Hammond) he explains why he might have been interested in leaving Brown:

> Brown is very pleasant. The only, but big, defect is that there isn't a single really good person on the faculty and not a single really good student.

And correspondence between Stigler and Friedman that same fall suggest that Stigler was angling for a full-time position at Columbia

Stigler to Friedman, November, 1946:
p. 42, There was a meeting to pick a successor last week, with Arthur conjecturing that he and Angell (for me) would be outvoted by Clark, Goodrich, and one other (Haig?) (for Hart). Apparently he argued well, and Goodrich arranged a lunch this Wed for me to meet the econ. Dept, which I did (yesterday). (Hammond and Hammond 2006, p. 42)

Friedman to Stigler, December 2, 1946:
I'm delighted to hear about the turn at Columbia. Arthur told me that his tactics were to work for two app'tments & it looks as if he may have succeeded. I would, of course, have been even more delighted had you gotten the first offer as you so clearly should have. But I had gathered that was extremely unlikely & was afraid nothing at all would come through. (Hammond and Hammond 2006, p. 47)

In December of 1946 Stigler was offered a professorship in the Economics Department at Columbia, confirmed in a further letter dated February 21, 1947 both from Carter Goodrich, Executive Officer of the Department of Economics. His appointment was effective July 1, 1947 at a salary of $8250 per year (Stigler papers, addenda, Box 46, 2016-279, Correspondence on the move from Columbia to Chicago).

According to a memo by T.W. Schultz to R.W. Tyler, Division of Social Sciences at the University of Chicago dated May 15, 1947, Milton Friedman was offered an appointment at Columbia which according to Schultz was sufficiently attractive that "it will be necessary for us to advance his salary substantially to induce him to remain." Schultz recommended a salary of $7500. Friedman's starting salary in 1946 was $6250 a year and Friedman indicates that he considered that relatively high since his starting salary at Minnesota the year before had only been $3500 (Friedman and Friedman 1998, p. 186). Friedman attributed his high Chicago salary to the notorious 4 E contract Chicago required all faculty to sign at that time, by which any outside faculty earnings would revert to the university.

So there was a real prospect that Friedman and Stigler could have been reunited at Columbia rather than Chicago. In a letter dated July 11, 1947, Stigler to Friedman states:

> Naturally I'm disappointed at Columbia's delay, but I'll continue to hope. (Hammond and Hammond 2006, p. 59)

Hammond and Hammond (2006, p. 6) indicate the delay did refer to Columbia's offer to Friedman. Possibly Columbia was not offering Friedman as much as Stigler. Stigler's Columbia salary seems to have been relatively high. Columbia hired Stigler as a full professor while Chicago hired Friedman only as Associate Professor. This may reflect that at that point, the profession considered Stigler a more established scholar.

In contrast to his comments on Brown, Stigler seems to have been an admirer of Columbia University both prospectively on joining its faculty in 1947 and retrospectively. In his 1988 memoirs (p. 42), he stated:

> When I came to Columbia University in 1947 I felt that I was joining one of the truly great universities, as indeed I was. {the following sentence does state however, "The next decade, however, was not one of progress for Columbia."). He does go on to observe (p. 43), "The Columbia I came to, in any event, was a great university with a strong and varied faculty in economics.

Stigler's decisions to move to Brown and then Columbia seem to have been motivated by academic, intellectual considerations rather than ideological ones, at least so the material just reviewed would suggest.

Further Abortive Efforts to Recruit Stigler to Chicago in the Early 1950s Reflecting Reticence of Both Parties

The Economics Department at Chicago deliberated over offering a position to Stigler a number of times in the early 1950s. The first episode, as recorded in Economics Department faculty meeting minutes, was in 1950. Along with Stigler, the department also considered Abba Lerner, Kenneth Boulding, Leonid Hurwicz, Kenneth Arrow, and Lawrence Klein. There was apparently no great enthusiasm for any of these candidates including Stigler. Department minutes for its May 30, 1950 meeting state that:

> Several members of the Department stated that none of these men had all of the qualities sought: a good mind reaching out fruitfully in new directions in economics. It was agreed, however, that there were no likely candidates possessing these qualities in a high degree.

A poll taken at the meeting indicated that Stigler was the one candidate that did not have more votes against than votes in favor, but even for him, votes were evenly split at 6 in favor of an appointment and 6 opposed. On further polling of those absent, Schultz found the same split and he himself abstained from voting to break a tie on grounds that support was not strong enough to advance any candidate for approval by the administration (Department minutes, June 6, 1950). Interestingly, the department approved Schultz' proposal to request a one-year visiting appointment for Gunnar Myrdal (Department minutes June 6, 1950). From Dept. of Econ records, Box 41, Folder 2.

By mid-1951, there was a stronger meeting of Department faculty minds much more clearly in favor of a permanent appointment for Stigler in comparison with alternatives. Minutes of a May 23, 1951 faculty meeting indicate that:

> Schultz reviewed briefly the discussion of the Spring, 1951 which had led to two slates of candidates, with George Stigler clearly heading one slate and Lerner and Hurwicz the other. The Central Administration, Schultz, said, was now prepared to finance a full professorship with tenure, the resources to be used for continuing the tradition of Professor Knight.

Then at a May 28, 1951 faculty meeting Under the Heading Major Departmental Appointment:

Schultz corrected the impression held by some members of the Department that a chair is being established in honor of Professor Knight and that the appointment under consideration was to fill such a post. The post in question, he said, was that of a full professor with tenure and the field of candidates was open.

In voting at that meeting, Stigler secured 8 first-place votes compared with only 1 for Arrow and 3 for Lerner with 0 first-place votes for Hurwicz. A motion was passed authorizing the chairman to negotiate with the Central Administration the appointment of Stigler as a full professor with tenure. The vote of the members present was nine for the motion and four against the motion; the vote of the absent members was [blank].[3]

While the vote of those present was substantially in favor of an offer to Stigler, there were 6 faculty reported as absent. Mints who was absent would almost surely have voted in favor of an offer to Stigler; he had ranked Stigler number 1 in the 1946 vote that brought in Friedman. Nef may have been the most likely to have cast a negative vote. In any event, there was substantial opposition among those present but the offer went through all the same.

Both the May 30, 1950 vote and that of May 28, 1951 suggest that there was not overwhelming consensus supporting an offer to Stigler. Thus it is of interest to consider the particular faculty who participated in each vote.

May 30, 1950 minutes show that those in attendance included T.W. Schultz, R. Burns, D.G. Johnson, E.J. Hamilton, F.H. Knight, L. Metzler, R. Blough, F.H. Harbison, A. Rees, H.G. Lewis, T. Koopmans, J. Marschak, M. Friedman. Those absent included H. Kyrk, P. Thomson, L. Mints, J. Nef, and R. Goode. May 28, 1951 minutes show that those in attendance included: T.W. Schultz, D.G. Johnson, M. Friedman, H.G. Lewis, F. Harbison, E. Hamilton, F. Knight, L. Metzler, G. Tolley, J. Marschak, A. Rees, H. Kyrk, T. Koopmans.

Absent: J. Nef, P. Thomson, B. Hoselitz (out of city), R. Blough (on leave), L. Mints, R. Goode. The clear Stigler supporters from the 1946 vote present in both the 1950 and 1951 votes would have included F. Knight, H.G. Lewis, and L. Mints. Presumably M. Friedman would also have been strongly in favor of an offer to Stigler. This constitutes only 4 out of some 18 faculty potentially voting on this appointment. One would anticipate that Marschak and Koopmans would have been at best lukewarm about an appointment for Stigler and would have strongly preferred some of the alternatives under consideration such as Arrow or Hurwicz. This still leaves some dozen faculty whose votes and rankings could have gone in various directions and thus points to both the diversity within the department and the role of chance in determining voting. The changes evident in the composition of faculty in the two years was that Robert Burns, an industrial relations specialist was present at the meeting in 1950 but was not listed as either present or absent in 1951, while Bert Hoselitz was not listed as either present or absent in 1950 but was listed as absent in 1951. Burns only ranked Stigler 4th out of 5 in 1946, so his departure presumably enhanced Stigler's ranking but it is not evident how Hoselitz, a wide-ranging generalist who combined interests in economic sociology with economic development would have voted. This lack of any evident major shift in the composition of the department despite a clear shift in support for Stigler further underscores its diversity.

Stigler turned down both this offer and a subsequent one by the Chicago department in 1954, deciding to stay at Columbia. In his correspondence with Friedman, the reason he cited for his 1951 decision was financial sacrifice entailed of giving up his NBER earnings and for his 1954 decision, the difficulties of uprooting his family to move from New York to Chicago. He notes in both instances that the decision was a difficult one for him given the attractions of having Friedman as a colleague. Nevertheless, he indicated that in both instances the professional advantages of returning to the Chicago department were not strong enough to outweigh the other considerations. In his letter of June 1951 to Friedman he states:

> On the professional side, it may be that the balance is ambiguous. There is no one whose advice and company I value more than yours, but there are few other great attractions in the present Chicago economics department. I'm not the least bit inclined to boast of Columbia, which has a fine assortment of damn fools, but the N.B. crowd – if one may average a universe ranging from Arthur to Mills—has a lot of sense and knowledge. Arthur has perhaps too strong a desire to formulate a program of research — the thing which, if successful, is called an architectonic sense—but I wouldn't want to go beyond this. And if I can formulate a really significant study, I'm confident he'll further it.

> This decision is most unhappy in that it disappoints good friends and does not elate me. It seems fundamentally improper for us to be at different schools and I don't like to continue the impropriety. But so be it. (Hammond and Hammond 2006, p. 124)

In his October 19, 1954 letter, he states:

> To return to your letter, I do not think it probable that at present I would accept a generous offer—as the last one was –at present.
> Nor do I think that you should delay a decision when the rewards of that delay are estimated so low. (Quite aside from myself, however, I see no reason why Chicago should make any hasty decisions in any area—a year or two is nothing in the life of a famous department of economics.)
> Whether it is necessary or not, I should like to add that these latter paragraphs are not easy to write: there is no one anywhere I would rather have as a colleague than you, and no one soon at Chicago who I would not enjoy as a colleague. Since I cannot say as much of Columbia, I should be logical in my conclusions and actions. If I am not, it is because I am loathe to uproot a family for less than major professional preferences. It is not reassuring to me that the writing of this letter leaves me less confident I am acting properly than I was when I started! (Hammond and Hammond 2006, p. 133)

It thus appears that as of 1954, Stigler did not view "the major professional preferences" associated with a move from Columbia to Chicago as sufficiently strong as to warrant moving his family. Was lingering resentment about the collapse of Chicago's 1946 offer a factor as well? I have found no documentation to support this, but then lingering resentments may be especially difficult to document.

Stigler and the Walgreen Foundation

In the early 1930s concerns were raised by Chicago businessmen that University of Chicago faculty were advocating communism and socialism. The activities of Paul Douglas, Professor of Economics, drew particular attention (Boyer 2015, pp. 262–267). In early 1935, Hamilton Fish

III, a congressman from New York, further reinforced such concerns in a national radio broadcast. In his talk, Fish charged that the University of Chicago was one of a number of prominent universities that were "honey-combed with Socialists, near-Communists, and Communists, teaching class hatred, hatred of religion, and hatred of American institutions, including the American flag" (Boyer 2015, p. 267; Mayer 1993, pp. 147–149). The controversy was brought to a head when in April of 1935, Charles R. Walgreen, founder of the eponymous drugstore chain, wrote to Robert Maynard Hutchins, President of the University that he was withdrawing his niece, Lucille Norton, from the University. Walgreen had become alarmed after conversing with his niece, who was staying with Walgreen while attending the University. His niece implied that in her classes at the university she encountered advocacy of communism as well as the views that the family as an institution was deteriorating in America and that free love was an acceptable lifestyle (Boyer 2015, p. 268; Mayer 1993, pp. 153–165).

The Illinois State Senate appointed a special committee to investigate "the subversive communistic teachings and ideas advocating the violent overthrow of the established form of government of the United States and the State of Illinois" in "certain tax exempt colleges and universities in the State of Illinois." The committee held hearings in April and May of 1935. The committee voted to exonerate the University by a vote of 4 to 1 (Boyer, History U of C., p. 269). In 1937, Charles Walgreen made a donation of $550,000 to the University to "promote familiarity with the American Way of Life" thus establishing the Charles R. Walgreen Foundation for the Study of American Institutions. The memo on the letter of gift, states that it is "for the purpose of providing instruction in and extending the field of knowledge concerning the history, development and current state of American institutions" (Stigler papers, Box 3, Folder on Walgreen Foundation, History).

Whether or not there was any official statement that the donation was an act of penitence by Walgreen for previous charges of subversive teaching at the university, at least one historian of the university has interpreted it as such (Boyer 2015, p. 273). An obituary biography of Walgreen simply states regarding the donation:

> Personally, he was a quiet modest man with a deep and abiding faith in the principles of democracy. To help spread a deeper understanding of the American way of life (in which he fully believed) he contributed $550,000 to the University of Chicago in 1957 to establish the Charles R. Walgreen Foundation for the Study of American Institutions. (Walgreen Foundation Papers, Box 3, Folder 12)

This obituary makes no direct mention of the controversy with the University involving his niece. The same is true of the statement about the Walgreen Foundation in the finding aid to its papers at the University of Chicago Libraries.[4] Interestingly, in his 2016 piece, Boyer focuses on the hearings involving Walgreen's niece and does not mention Walgreen's subsequent donation as an aftermath to this episode in contrast with his 2015 history of the University cited above.

During the 1940s and early 1950s, the Walgreen Foundation funds were used to fund visiting lecture series and to fund specific projects of existing faculty. By February of 1954, Morton Grodzins, as Dean of the Social Sciences Division was endeavoring to shift the use of the Walgreen Foundation funds to "support a long-term integrated program of instruction and research" under the broad category of "American Institutions."[5]

Counter to the claim by Nik-Khah (2011, p. 122), and Stigler (1988) himself, the Walgreen Foundation was not based in the Political Science department, even if Jerome Kerwin, who managed the Foundation, had an appointment in that department; previously William T. Hutchinson in the History Department had purview over the Walgreen Foundation. The Walgreen Foundation had its own budget line within the university; faculty secretaries who came from a range of departments including history and political science administered this budget.

By June of 1954, an offer of a faculty position funded by the Walgreen Foundation had been made and turned down by Oscar Handlin of Harvard with another offer pending to Richard Hofstadter of Columbia (which also seems to have been declined) (Office President, Kimpton Papers, Box 224, Folder 12), Memo Grodzins to Kimpton. Despite the lack of success in filling appointments, Grodzins was "heartened" that "we can continue to seek personnel with Walgreen funds without consideration of issues that do not bear upon scholarly qualities."

However, writing to George Stigler, over 25 years later, as Stigler was seeking a replacement for himself on his retirement as Walgreen Professor, W. Allen Wallis claimed that:

> Larry Kimpton, who was then Chancellor [of the U. of Chicago], was on the verge of turning the money back to Walgreen, as he was being urged to do by Morton Grodzins, then Dean of Social Sciences, and others. Chuck Walgreen had turned down several candidates supported enthusiastically by Grodzins and company including Clinton Rossiter. Grodzins took the line that no academically respectable appointment could be made, since it was subject to Walgreen's Veto. He also argued that accepting the money with such a string attached to it was disgraceful, etc. When Kimpton told me about this, I commented that if he would assign the Foundation to the Business School, I would guarantee that creditable appointment would be made before very long —which as you will be the first to concede, it was. (George Stigler papers, Box 3, Folder on Walgreen Foundation)

Charles R. Walgreen, Jr. relays the following account of how the Walgreen chair was established in the Graduate school of Business in a March 16, 1992 letter to Hannah Gray related to a memorial service for George Stigler:

> My father established the Chair for the Study of American Institutions shortly before he passed away in 1939. Initially, the chair was under the auspices of the Department of History. Rather than filling the chair, eminent individuals gave series of lectures from time to time.
>
> I am sorry to say that some of these individuals had beliefs which were extremely liberal and differing substantially from those of my father's. Following discussions with Larry Kimpton, the Chair was moved to the School of Business. My Dad would have been delighted with the appointment of George Stigler. (Source Stigler papers Box 28, George Stigler Memorial, correspondence)

The administrative history of how the Walgreen Foundation "was moved to the School of Business" would appear to be incomplete. As well documented further below, Wallis described the Walgreen Professorship to Stigler as "all-University professorship for which the school of business simply has the right of nomination." John Wilson in a letter of November 13, 1972, questioned the identification of the Walgreen Foundation with the School of Business while Richard Rosett, then Dean of the School of Business in a letter of July 26, 1978 to then President Hannah Gray stated:

> You will find that I am taking a stronger position now on the side of keeping the Walgreen Fund in the Business School than I did when you and I spoke face to face. That is because my subsequent review of the record convinces me that the parties to the transaction intended that the shift from Social Sciences to Business be permanent. (Stigler papers, Box 45, correspondence)

In a letter to T.W. Schultz dated October 17, 1956, D. Gale Johnson floated the possibility of finding an economic historian to fill the chair. Johnson in his letter notes the prospect that the Walgreen funds would be made available to the School of Business. He notes that in a meeting with Chauncy Harris, a university official that "I was given the impression that, if the Department of Economics were to come forward with a rather strong candidate for the directorship, part or most of the funds might become available to us. It seems to me that, if we could come up with a strong name in the field of American economic history, we might have some chance. The difficulty is that I can think of no strong names in that field." Johnson then goes on to suggest a joint arrangement with the School of Business in the "general area of American Business, emphasizing questions of growth, productivity, industrial organization, and the relationship between government and business" as another possibility. Letter from D. Gale Johnson to T.W. Schultz dated October 17, 1956. Department of Economics Records. Box 41, Folder 9. Special Collections Research Center. University of Chicago Library.

Given that Wallis was a graduate school friend of Stigler's and Walgreen, Sr. and Jr. conservative propensities, the appointment of Stigler to the Walgreen Professorship for the Study of American Institutions may seem almost inevitable. However, by Wallis' own account, both James

Lorie, Associate Dean of the Graduate School of Business at Chicago under Wallis and Leonard Read, head of the Foundation for Economic Education, played key intermediary roles in the appointment of Stigler to the position. According to Wallis in an interview he did with William Meckling, Leonard Read's approval was required for the recipient of the Walgreen Chair because Charles Walgreen relied on Read for advice in such matters (p. 84). And by Wallis' account, Lorie and Wallis managed to gain Read's confidence to such a degree that Read trusted their judgement in such matters (p. 85). Correspondence between Lorie, Wallis, Read and Kimpton in the 1950s suggest that Wallis and Lorie built up that confidence with a series of lunches with Read as well as with donations to Read's Foundation for Economic Education. In a letter dated Aug. 2, 1955, Lawrence Kimpton, then Chancellor of the University of Chicago, wrote to Leonard Read:

> Chuck Walgreen and I are looking forward with a great deal of interest to the recommendations that you and your group will make to us. We badly need some help in what I am sure will agree is a very important appointment.
> I hope that someday when you are in Chicago we can work out a meeting involving Frank Knight, Milton Friedman, Friedrich Hayek and some others about here, who are very close to your viewpoint. It ought to be an interesting meeting.

In his August 5, 1955 reply, Read states:

> Nothing would please me more than to have your suggestion materialize, namely that you, Frank Knight, Milton Friedman, Friedrich Hayek, and I get together. Getting all five of us together in one spot at one time may be difficult, but I shall hope for it.

Then in a letter the following year, June 1, 1956, Kimpton wrote to Read:

> Did it ever occur to you, incidentally, that the only [first-rate—inserted in pen] academic people in the country who really share your views are

> on this faculty? I think of Hayek, Milton Friedman, Aaron Director, and Frank Knight. I might add that there are a lot of young people on the faculty who are coming up with the set of ideas, Carry On! (See Office of the President, Kimpton Administration, Box 210, Folder 9, Leonard Read)

The well-known irony here, is that Read had previously objected to Stigler and Friedman's pamphlet, Roofs and Ceilings on grounds that it acknowledged the desirability of more equality of income in society though Foundation for Economic Education did ultimately publish it [Also see Hammond and Hammond 2006, pp. 20 and 147, note 17, 34–38]. Friedman for one did eventually reconcile with Read at a session at Orly Airport in France after a Mont Pelerin Society meeting. Friedman states of this meeting with Read that [we] "resolved our dispute and I discovered what a charming and principled person Leonard was, and from then on he was one of our fast friends" (see Friedman and Friedman 1998, pp. 150–151).

George Stigler was NOT Wallis' first choice for the Walgreen Chair. Wallis had initially decided with Read's approval to offer the chair to Dexter Keezer. Keezer had formerly been President of Reed College and at the time in the mid-1950s was a high-level executive with McGraw-Hill Publishing. As a McGraw-Hill executive, Keezer had established a reputation for his annual surveys of business activity and he seems to have been involved as well with editing Business Week, a McGraw-Hill publication at that time. His June 25, 1991 *NY Times* obituary describes him as a "pragmatic economist" and cites a McGraw-Hill associate describing Keezer as "the No.1 person in the business economics field." The offer to Keezer would thus seem to indicate that Walgreen was willing to approve an appointment to someone without a clear conservative bent. It was only after Keezer turned down the offer, that James Lorie suggested George Stigler to Wallis as a possibility for the Walgreen Chair. Wallis indicated in a letter to Milton Friedman in 1957 that Stigler occurred to Lorie as a possibility after Lorie had met Stigler at a Wabash College summer conference and Stigler had conveyed his interest in working on broader philosophical and policy issues rather than just on narrower issues in technical economics (from Friedman papers, copy in Stigler papers, see further citation below).

The Walgreen Professorship offered a salary of $25,000 per year. Stigler's 1956–1957 salary at Columbia was initially to have been $12,000 per year (April 2, 1956 letter, Grayson Kirk to Stigler, Box 46, Correspondence, *Move to Columbia*). Then in Oct. 1956, he was offered a research professorship with a salary of $16,000. In a Nov. 20, 1957 letter, which would have been after Stigler would have received the Walgreen offer, Columbia offered him a salary effective July 1, 1958 of $17,000 a year (letter Carl Shoup to Stigler dated Nov. 20, 1957, Box 46, correspondence on move to Columbia). So while Columbia does appear to have raised Stigler's salary in response to the Walgreen offer, it by no means came close to matching it.

Could Columbia have retained Stigler in 1958, if it had made a counteroffer more effectively? Evaluating this counterfactual requires taking under consideration both Stigler's deliberation process and that of the Columbia department and central administration and thus is particularly challenging. However, Gary Becker in October 1997 interview with Craig Freedman opined in the affirmative about this counterfactual:

> I wasn't privy to what was going on at Columbia. They handled it very badly. I did think they in fact had a good shot at keeping him [Stigler], but it was terribly handled, as Columbia handled a lot of things and he decided to go. But it wasn't an open and shut decision on his part.[6]

One factor that may have hindered Columbia's ability to retain Stigler is that in the academic year 1957–1958, he was not physically at Columbia. He was visiting at the Institute for Advanced Studies in the Behavioral Sciences at Stanford. His geographic remoteness from colleagues and administrators at Columbia may have made it easier for him to severe ties and resist entreaties to remain. (I thank Stephen Stigler for this observation.)

Stigler's acceptance letter to Wallis of the offer was in his inimitable style:

> Nov. 30, 1957,
>
> Dear Allen:
>
> It always hard for me to begin a letter which conveys bad news to a friend. I know that it is preferable to be explicit and direct, "to get it over with," as the popular expression has it, but it is easier to give such uncongenial advice than to follow it. Especially is the task more difficult when the bad news is of the writer's own making, and to a large degree, but not exclusively, it is in the present case. In short, I shall be happy to accept the professorship at Chicago. (Stigler Papers, Box 46. Also cited in Wallis 1993)

Did Stigler's occupancy of the Walgreen Professorship redirect the mission of the Walgreen Foundation and transform business education at Chicago and beyond?

Nik-Khah makes the claim that

> Shortly after taking control of the Walgreen Foundation, Stigler completely revised its mission: He shed the obligation to educate undergraduate students, creatively reinterpreted its mission to promote study of the 'American way of life,' and announced his intention to devote the Walgreen resources to a study of the 'causes and effects of government control over government life'. (Nik-Khah 2011, p. 126)

Nik-Khah makes the further claim (2011, pp. 119–120) that

> In addition to standard channels of influence [by occupying administrative roles] at the GSB [Graduate School of Business], Chicago economists were involved in rather novel (and underappreciated) forms of alliance formation and institution building and repurposing, efforts that would have consequences for the GSB and ultimately for the entirety of U.S. business education. The central figure in these efforts was the most prominent hire that Wallis made during his tenure, his former classmate at Chicago, George Stigler.

Did Stigler revise the mission of the Walgreen Foundation?

Regarding Nik-Khah's claims, it is not at all clear that the mission of Walgreen Foundation before Stigler became director was that well-defined; this was indeed one of the sources of discontent of both University officials such as Morton Grodzins as evidenced in his statement and of Charles R. Walgreen as cited above. Furthermore, under Stigler's tenure, the Walgreen Foundation did not exclusively focus on government regulation of the economy. The Walgreen Foundation sponsored lecture series addressing much broader issues related to the American way of life, such as the series of lectures by the sociologist Edward Shils on "the attitude of the intellectual classes toward private enterprise," the subsequent one of Hayek and then a series in 1976 to commemorate the bicentennial of publication of *The Wealth of Nations*. Charles R. Walgreen, Jr. observed in a letter to Stigler dated July 27, 1981, in acknowledging Stigler's letter stepping down as Walgreen Professor:

> Our family have been very happy and proud of your achievements as Charles R. Walgreen Professor of American Institutions at the University of Chicago. Your contributions through your research, teaching –your publications and your lectures –have had far-reaching effects in academic, business, and governmental areas.

And in a July 31, 1979 letter to Stigler, Walgreen stated:

> The goal of the Foundation is that it might act as a catalyst for progress. Surely, again, your tenure as Walgreen Professor has made the goal a reality…. I need only refer to your own words: "widened…launched…continued…begun." In or out of context, they describe an intensity and a dedication to progress that makes us proud that George J. Stigler has been Walgreen Professor. (Stigler papers, Box 45, correspondence with Charles Walgreen)

As already noted above, Stigler was not Wallis' first choice for the Walgreen Professorship. In fact by Wallis' own account, Stigler only occurred to Wallis as a possibility for the professorship after James Lorie, Associate Dean of the GSB at this time, suggested this. And

Lorie's grounds for this according to Wallis was not Stigler's ideology but that Stigler had conveyed to Lorie an interest in broadening his intellectual reach beyond technical economics.

Wallis conveyed this account in a letter to Milton Friedman dated Nov. 8, 1957:

W. Allen Wallis to Milton Friedman, Nov. 8, 1957 (frame 4218 in pics), p. 2,

> The second thing we can offer him professionally is related to the incident that gave us the idea of approaching him for the job. At some meetings at Wabash in June, George happened to mention to Jim that after twenty years in technical economics, his mind begins to wander to other, though related subjects, such as history, politics, and philosophy. This job is tailor-made to give any scope he desires to such interests, and at the same time to keep him solidly planted in his own field.

Although Wallis was able to base the faculty appointment in the Graduate School of Business, as a professorship of "American Institutions" he also emphasized to Stigler the sense that the incumbent should reach beyond narrowly defined professional training. Thus, he noted to Stigler in a letter to Stigler dated October 18, 1957,

> One of Mr. Walgreen's hopes was that the Walgreen Professor would have some impact on undergraduates, so we hope that you would at least be receptive to, and perhaps even occasionally seek out opportunities of that kind…

Moreover, Wallis noted to Milton Friedman, in the letter already cited November 8, 1957 (frame 4218 in pics), p. 2 (Stigler papers, box 30).

> About the only worry George expressed about the job was the fact that his interests somehow are economics, not business, though they do happen to fall in large part into the field of industrial organization, which overlaps both. He wondered whether he might not be bothered, however little we were bothered in the Business School, about doing too little for us and too much for the Economics Department. I tried to emphasize that we anticipated this, and more basic, that the Walgreen professorship

> is an all-University professorship for which the school of business simply has the right of nomination. I told him that we would be satisfied if he would simply keep his office in our premises. Actually, as I told George, I think we do have a number of activities which will interest him from time to time, such as participation in the Executive program.

Wallis' characterization of the professorship as an "all-University professorship for which the school of business simply has the right of nomination" contrasts with the characterization of Richard Rosett to President Hannah Gray in 1978 cited above:

> that the parties to the transaction intended that the shift from Social Sciences to Business be permanent.

In his November 30, 1957 letter to Carl Shoup of Columbia explaining his decision to accept the Walgreen Professorship at Chicago, Stigler stated:

> The compelling factor in the decision was the fact that the Chicago post offers unlimited opportunities to pursue my research interests wherever I wish, quite independently of financial considerations. (Box 46, Correspondence on move from Columbia to Chicago, 2016-279)
>
> Chancellor Kimpton's offer of the Professorship to Stigler dated October 18, 1957 states:
>
> It was the intention of Mr. Charles R. Walgreen, Sr. in establishing the Walgreen Foundation about twenty years ago that we appoint a distinguished professor who would teach, write, and speak for a variety of audiences both within and without the University regarding American economic, political, and social institutions. The Foundation fund now exceeds eight hundred and fifty thousand dollars. This permits us to put at your disposal, in addition to your salary, funds for such things as lectures, travel, and research that would further the purposes of the Foundation whose director you would be.
>
> And W. Allen Wallis in his own letter to Stigler of the same date (Oct. 18, 1957) states:

> The essence of the matter is that you would have complete freedom from assigned duties—almost complete freedom, anyway, the only restraint being your own conscience. Furthermore, you would have resources at your disposal for whatever use seemed to you appropriate for furthering the very broad purposes of the Foundation. You could teach such courses, or give such lectures, as you chose, either in the School of Business or in the Department of Economics. (Addenda Box 46, 2016-279, Correspondence on move from Columbia to Chicago)

Stigler sent regular reports to the Walgreens on his activities as the holder of the chair throughout his tenure (May 11, 1979 memo to Rosett). The reports were not lengthy, generally one or two typed pages, but they were regular, generally once a year. The reports indicate that in addition to his own salary, Foundation funds were used to fund graduate student fellowships, support for research of other faculty including summer stipends and for lecture series (for example, Hayek gave 5 lectures on the history of economic thought in October of 1963; Stigler papers, Box 3, Walgreen Foundation, history of). Stigler also seems to have met Walgreen for lunch on occasion (see Stigler papers, Box 18, Charles R. Walgreen, Jr. Correspondence folder).

Internal university sources did on occasion monitor Stigler's use of Walgreen funds. Thus Stigler was sent a memo dated February 22, 1974 by Harold Bell, of the university's comptroller department noting that

> information we have raises the question of whether the proposed The Wealth of Nations Lecture Series meets the purposes specified by the donor … the lecture series seems to have a European flavor as opposed to that of American Institutions. No doubt you can connect the two, but I believe it would be desirable to clarify this point for the permanent record.

Stigler replied that

> two of the lectures will be narrowly American…. The others, on Adam Smith, Blackstone, Burke, and Bentham, are of course on four philosophers who were immensely influential in and on the United States … It would be inconceivable that this set of subjects by considered irrelevant to the 'history [and] development' of the American Society. (Box 3, Folder on Uof Ch Walgreen correspondence)

Stigler in a report prepared for GSB Dean Richard Rosett dated May 11, 1979, described his vision of the Walgreen Foundation during his tenure as director:

> The goal of the Walgreen Foundation, as I understand it to have been presented to the Walgreen family, was to foster the study of American Institutions. I have interpreted this to mean the study of the American private enterprise system and of the increasingly extensive regulatory activity of government under which the enterprise system works. The focus of the Foundation's work has been on increasing scientific knowledge of this area. The Foundation seeks to mobilize the resources within the entire University so far as they can contribute to this limited area of research. (Box 3, Folder, GSB Dean's office correspondence)

On the one hand, Stigler's 1979 statement to Dean Rosett would seem in accord with the passage that Nik-Khah extracts from a 1959 letter that Stigler sent to Walgreen regarding study of the "causes and effects of government control over economic life" (Nik-Khah 2011, p. 126). The study of the causes and consequences of government regulation was certainly a central theme of Stigler's academic career. Nevertheless, by omitting surrounding passages of Stigler's statement, Nik-Khah considerably overstates his claim that this passage indicates that:

> Shortly after taking control of the Walgreen Foundation, Stigler completely revised its mission: He shed the obligation to educate undergraduate students, creatively reinterpreted its mission to promote study of the 'American way of life,' and announced his intention to devote the Walgreen resources to a study of the 'causes and effects of government control over government life'. (Nik-Khah 2011, p. 126)

The full paragraph from which Nik-Khah takes the passage above states:

> I have been moving rather slowly in establishing a program of activities because it is much easier to enter into activities than to get out of them and I wanted to be reasonably sure that they were worthwhile. Aside from my own work, the chief activity of the Foundation at the present is the sponsoring of a workshop in the area of industrial organization. It

> consists of about eight faculty members who are professionally interested in the structure and behavior of the American economy, and of an almost equal number of students. The primary focus of the workshop is on the understanding of the causes and effects of government control over economic life. In the current academic year the Foundation has awarded four Charles R. Walgreen Fellowships in Industrial Organization to the following young men:
> Fleuck, John
> Kassing, David
> Moore, Thomas
> Saving, Thomas.

The letter then goes on to describe arrangements for Edward Shils, who he describes as "one of the most distinguished American sociologists" to give a series of lectures on "the attitude of the intellectual classes toward private enterprise." He then elaborates on the contribution he thinks the lectures series will make:

> Professor Shils is both widely informed and very acute in his analysis of the extent of the hostility toward private enterprise by the so-called intellectual classes and the causes of this attitude. I think these lectures will perform a signal service. You will, of course, be notified of them well in advance of their delivery.

He goes on to note that his chief current activity is in an area he would not have predicted, which was chairing a committee reviewing the price statistics program of the Federal government. He concludes the letter with the "hope that sometime after the holidays we can have a luncheon together" (Stigler papers, Box 18, Charles R. Walgreen, Jr. correspondence).

George Stigler would appear to have shared ideological predispositions towards free markets with Charles R. Walgreen, Jr. and this likely contributed to their cordial relationship during Stigler's tenure of the Walgreen Professorship. And of course a different incumbent, such as for example, Dexter Keezer, would surely have taken the activities sponsored by the Walgreen Foundation in different directions than Stigler did. Stigler's successors to the professorship, considered below, clearly did so. Nevertheless, the material reviewed here indicates that Stigler pursued aims in directing the Walgreen Foundation fully congruent with the intentions of its founder and that of his heir.

Did Stigler's appointment to the GSB transform business education at Chicago and beyond?

Business schools' curricula in the first half of the twentieth century did include some economic content and some business school faculty had training in economics. Nevertheless, the late 1950s and the 1960s saw a fillip in the percentage of business school faculty who had doctorates in economics as well as in other social science disciplines (see Fourcade and Khurana 2013; Khurana 2007; Overtveldt 2007). Khurana (2007, Chapter 6) argues that this change reflects the efforts of the Carnegie and Ford Foundations to reform business education. And Wallis in his interview with Meckling (Wallis interview with Meckling 1993) suggests that this reflected adopting the Flexner model of grounding medical education in mastery of key natural sciences to the business training setting in which business practitioners should have familiarity with relevant social sciences. Wallis in the same interview acknowledges that it was quite unusual in the late 1950s for a business school to have hired such a prominent economist as George Stigler for its faculty.

Nik-Khah acknowledges that Stigler's appointment was hardly the only important factor leading to changes in the Graduate School of Business at Chicago. Nevertheless, he also claims that this set the stage for coming transformational changes in business education at Chicago and elsewhere

> Although it would be an exaggeration to attribute all the developments at the GSB to him alone, Stigler's efforts had set the tone at the Chicago GSB, even in areas normally outside his areas of expertise. (Nik-Khah 2011, p. 128)

Although Van Overtveldt's (2007) chapter on the Chicago Business School bears the subtitle, as Nik-Khah points out, "A Great Economics Department," the chapter in fact makes a case that many other disciplinary influences than simply economics were at play in changes at the business school. Moreover, while Stigler's influence may have been one contributing factor, numerous other prominent economists were involved as well. Moreover, James Lorie continued to play an important role even after Wallis left the business school in 1962. In addition, Wallis' successor as Dean, George Schultz, arguably contributed more to sustaining the changes initiated by Wallis than did Stigler.

Stigler and His Successors as Directors of the Walgreen Foundation

The successors to Stigler as directors of the Walgreen Foundation would further seem to belie any claim that Stigler was an empire builder focused on promoting anti-government economic policies. None of those who were offered the chair had clear free-market or anti-government regulation proclivities. Indeed there is irony in each of Stigler's subsequent successors. Robert Fogel his immediate successor had been a communist party organizer as a young adult, counter to the anticommunist impetus leading up to Walgreen's donation to the University. And Richard Thaler's claim to fame and the basis for his Nobel prize award in Economics is as a behavioral economist, in contrast with Stigler's apparent antipathy towards behavioral, nonrational choice approaches to economics and to the social sciences more generally. Indeed, one Ford Foundation program officer expressed concerns in 1957 that if Stigler accepted the Walgreen Professorship, this would result in a neglect by the Graduate School of Business at Chicago of behavioral science and related interdisciplinary approaches in favor of economists' rational choice perspectives (Fourcade and Khurana 2013, pp. 145–146).

Over the period, 1978–1979, Stigler headed the search committee for his replacement as Walgreen Professor of American Institutions. As noted above, Richard Rosett as Dean of the Graduate School of Business argued forcefully that the Chair should remain with the GSB since he claimed that the shift from the social sciences had been intended to be permanent.

The search conducted in 1978 with Stigler as chair, John Jeuck, Sidney Davidson, Gary Becker, Richard Posner serving on the search committee lead to a short list of ZviGriliches and A. Michael Spence. Griliches was offered the chair but turned it down on grounds that he did not want to disrupt the lives of his children by moving from Harvard to Chicago (letter to Stigler, March 6, 1979, Box 3, Walgreen Committee, 1978–1979).

In 1979, a new search committee of John P. Gould, James Lorie, Merton Miller and Arnold Zellner was convened. Richard Rosett, Dean of the GSB played a key role in advocating that Robert Fogel to be offered the chair which he accepted (interview with Richard Rosett 2005). Stigler did contact Charles R. Walgreen about the choice; Walgreen indicated familiarity with *Time on the Cross* as well his approval of the choice of Fogel (letter to Stigler from Walgreen, Jr. dated December 30, 1979. Box 45, Correspondence, Addenda, 2016-279). Interestingly, as part of the search process, Stigler sent letters to a number of prominent economic historians both in the United States and abroad inquiring as to whether the controversy over Fogel's book on slavery with Engerman had diminished Fogel's standing in the field. The general consensus was that it had not. The most negative statement about Fogel appears in Stigler's notes of a conversation he had with the prominent African American historian and Professor in the History Department at Chicago, John Hope Franklin. According Stigler's notes:

> The main basis of the criticism was an acceptance of the arguments of some critics (e.g., David, Temin, Sutch).

Stigler goes on to note:

> Franklin had no personal criticisms of Fogel, and got along well with him. Fogel had not been active in the history department when he was at Chicago. (Stigler papers, Box 3, Folder on Walgreen Foundation Committee 1978–1979)

After Fogel passed away in 2013, Richard Thaler was appointed Charles R. Walgreen Distinguished Service Professor of Behavioral Science and Economics. He has reported having no contact with the Walgreen family in accepting or subsequently holding this position. (conversation with Thaler, May 18, 2017). Professor Thaler indicated that he was not familiar with considerations behind the apparent renaming of the Professorship from that of American Institutions. Given Nik-Khah's claim that Stigler's research program considered behavioral and bounded rationality approaches to business behavior to be of little value (Nik-Khah 2011, p. 128) as already noted there would seem a certain irony in the appointment of his subsequent but one successor.

Center for the Study of the Economy and the State

In the late 1970s, Stigler was still Director of the Walgreen Foundation as he was in the process of searching for his successor in this capacity but was also involved in establishing the Center for the Study of the Economy and the State. A memo he sent to Dean Richard Rosett of the GSB dated May 15, 1978 gives a rough sense of the relative sizes of the two operations at that point in time. Stigler reported an income for the Walgreen Foundation of around $115,000 per year. The uses of those funds included his own salary, funding the Industrial Organization workshop, grants to other faculty, especially as summer stipends, pre- and postdoctoral fellowships, and special lectures and conferences. The Center for the Study of the Economy and the State was projected to spend about $200,000–$300,000 per year on five components: (i) support for "local" faculty, (ii) postdoctoral fellowships, (iii) predoctoral fellowships, (iv) visiting professors for specific research projects, and (v) conferences (Stigler papers, Box 3, GSB Dean's office correspondence).

In a series of letters to Richard Larry of the Scaife Family Charitable Trust in April and May of 1977, Stigler described the proposed aims of CSES. Attached to his letter of April 28, 1977, on a statement about the Center, Stigler describes its research program as follows:

> The study of the politics and law of economic policy has been a traditional part of economics for literally centuries. Until remarkably recent times, however, the nature of the workings of the political system has not been so much studied as assumed by the economists: the state was

> given duties —often difficult or impossible duties —and told to get them done or, if their failures could not be overlooked, told to devote more people, laws, and money in fulfilling the duties. Only recently have the hard questions begun to be asked:
> – what groups determine the economic policies of the state?
> – how can the actual goals of these policies be discovered?
> – what have the effects of these policies been?
>
> It is the systematic and comprehensive study of questions such as these that will form the central case of the research program of the center. (Coase Papers, Box 33, Folder 10, Stigler correspondence)

Attached to a follow-up letter to Larry, dated May 31, 1977 in anticipation of a June 14 meeting, Stigler provided a statement on "The Roles of the Center." The statement's opening paragraphs are:

> The ultimate purpose of research in economics is to improve the workings of the economic system. No matter how indirect the route, economic research must finally explain how an economic system works, and therefore give us a rational basis for improving the performance of our system.
>
> This function of research has three components: i) fundamental research to provide the general theories necessary to understand a problem (rate regulation, antitrust, advertising, unemployment); 2) applied research involving specific policy issue under consideration (e.g.) the current energy proposals); and 3) effective dissemination of the results to legislators and administrators, and ultimately to the public. All of these steps are essential to fulfilling the task of economic research.
>
> Particular institutions emphasize different components of the research function: no one (except Milton Friedman) can do all these things equally well. Partly this is a matter of temperament: the person who has the patience and single-mindedness to perform high grade professional research may not possess the fervor and enthusiasm to be a first class salesman of ideas. Too strong a commitment to immediate policy problems may distract a scholar from the fundamental scientific issues that still need to be studied.

> Our Center has one indisputable advantage in the preservation of the market economy. We are part and parcel of one of the world's great economic communities, and so acknowledged by the remainder of the profession. That means we can attract the best young scholars from other schools (*and, as I will explain below, that is precisely what we hope to do_). That means that our work is quickly received, analyzed, and criticized by major scholars; we are a force that the scientific community does not and cannot ignore. That means that our work also receives prompt and respectful attention from the media and other opinion shapers and policy makers. (Coase papers, Box 33, Folder 10, Stigler file)

In later parts of the statement (pp. 3–4), Stigler indicates an intent to both engage in the production of knowledge and to "assure the wide dissemination of the results of our studies" and he notes three channels of influence: (1) publication in major academic journals in Economics (JPE, JLE, AER), (2) work taken up by more policy-oriented organizations—he gives the example of the American Enterprise Institute's pamphlet series, and (3) "Through the close relationships between the economists and the law school at Chicago, our work has percolated to members of the most influential profession in the U.S. in the formulation and administration of public policy" (lawyers?).

In a July 13, 1983 memo to Dean Richard Rosett of the GSB, Stigler describes further the aims of CSES at that date:

> The basic goal of the Center is to increase our knowledge of the reciprocal effects of government and the private economy upon each other. We view the governmental policies broadly to include taxes and subsidies, direct operation of economic enterprises, and regulatory activities. We seek to understand not only what the effects of these policies are, but also why they were instituted.
>
> We pursue this goal primarily by supporting the research activities of economists and lawyers (and we hope soon also political scientists) who are at the University of Chicago, and of course the galaxy of talented economists here is our great resource. In addition, we wish to have one or two post-doctoral fellows, and sometimes support doctoral candidates.

> Obviously our mandate is wide; it includes macroeconomics (where we are supporting Barro this summer) as well as microeconomics and "public" economics (the application of economic analysis to political institutions and behavior) (George Stigler papers, Box 3, GSB Dean's office correspondence).
>
> A statement prepared by Stigler in January of 1984 on new CSES grants committed in calendar year 1983 and through January of 1984, indicated that these totaled $524,500 of which $200,000 (38%) came from the Scaife Foundation, $242,000 (46%) came from Lilly-FREE, $50,000 (9.5%) from the Olin Foundation with the remaining percent coming from Amoco, Proctor and Gamble, and Getty. These are the same sources reported by Nik-Khah 2011, 136, though these percentages indicate that the bulk came from Foundations rather than direct corporate contributions. And Nik-Khah reports that from 1985 to 2005 a further $5 million came to the center from the Lynde and Harry Bradley foundation, the John M. Olin Foundation, and the Sarah Scaife Foundation. (George Stigler papers, Box 3, GEB Dean's office correspondence)

While Stigler did use Walgreen Foundation funds and then subsequently those raised for the CSES to support the research and activities of other scholars, he would not appear to have sought to dominate or monopolize research on the relationship between the economy and the state. Indeed, as Stigler acknowledges in his memoirs, a rather distinct research program in that direction had been developed by James Buchanan and Gordon Tullock first in the location at the University of Virginia, then at Virginia Polytechnic and finally at George Mason. Rather separate sources of funds appear to have been used for that operation. MacLean (2017) makes just one reference to Stigler in his capacity as President of the Mont Pelerin Society. He does not feature prominently in her narrative. Nor do Buchanan and Tullock feature prominently in Stigler's memoirs or in documents related to the Walgreen Foundation or the Center for the Study of the Economy and the State. Stigler's center was thus not "the only game in town."

Finally, it should be noted that after stepping down from the Walgreen Professorship in 1981, Stigler remained in residence at the

University of Chicago for the rest of his life. This was in sharp contrast with Milton Friedman, who immediately moved to a position with the Hoover Institution at Stanford in 1977 upon Friedman's retirement. Stigler (1988, p. 48) observes that he led an "active committee life" both at Columbia and then at Chicago, despite the conventional disdain he acknowledges he himself often expressed regarding such activity.

Conclusion

As suggested in the title and outset of this essay, George Stigler's career moves can be seen as featuring contingency, self-interest, ideology, and intellectual commitment.

Contingency

George Stigler's career history identifies a number of paths not taken. If Homer Jones had accepted an academic position at Iowa State in 1936, Stigler could have pursued a career in real estate. If Robert Maynard Hutchins had not come down with a cold in February of 1946, Stigler not Milton Friedman may have taken a faculty position then and Friedman in turn could have ended up at Columbia. If Charles Walgreen's niece had not been staying with the Walgreen family in 1936 or if Dexter Keezer had accepted Allen Wallis' initial offer, then Stigler may not have returned to Chicago in 1958. Nevertheless, chance, as well as, God helps those who help themselves.

Self-Interest

Self-interest or at least family rather than collegial concerns seem to have been involved in Stigler's decision to turn down Chicago offers in the early 1950s and to accept the Walgreen Chair in 1957 rather than prolong negotiations over a counteroffer from Columbia.

Ideology

The Walgreen chair as envisioned by Walgreen, Jr. seemed well suited for someone of Stigler's ideological inclinations. Ideological factors do seem to have contributed to why W. Allen Wallis was able to secure the Walgreen Foundation for an appointment in the Graduate School of Business rather than elsewhere at the University of Chicago. Nor were ideological considerations likely absent from why Stigler was able to cultivate a cordial relationship with Charles Walgreen, Jr. during his incumbency of the Walgreen professorship and in his ability to establish and fund the Center for the Study of the Economy and State.

Intellectual Commitment

Yet other features of Stigler's career choices imply that he did view his endeavors along with those of colleagues as economists as more than just as a supplier of propaganda. Scientific and more generally intellectual considerations motivated Stigler as well. This is especially evident in the selection of his successors to the Walgreen chair. This chair seemed best suited for someone with a large vision of society whether it be Stigler's view of the role of markets and their interaction with government activity, Fogel's view of long-run trends in health and technology or more recently Richard Thaler's behavioral perspectives on economic decision-making. The Walgreen Foundation and the Center Stigler subsequently founded has commanded ample funding which in turn facilitated impact. Yet in the discretion, judgment, and most importantly vision required to have such impact in turn required an abiding intellectual commitment.

There is certainly tension if not contradiction in grouping together the features just listed. Stigler's Tanner lectures delivered at Harvard in 1980 published under the title *The Economist as Preacher* provide one attempt by Stigler himself to reconcile such tensions and contradictions. He recognized that economists as human agents are subject to influence by the society in which they find themselves. Yet he also argued (p. 34) that ideas that have compelling intellectual merit have a survivorship value that will tend to eventually prevail.

He made the argument that whatever doubts he had about whether the intellectual merits of arguments would dominate over ideological predilection, a policy of preferring intellectual merit would lead to a more suitable ideological balance than one of explicitly aiming at intellectual balance in a letter to the President of the Board of Trustees of Stanford University in 1966. The letter appears to have been in response to a proposal the Trustees of Stanford were considering to establish a position for recruiting a prominent economist with conservative views as a way of offsetting the prevalence of liberal, New Deal-Great Society views among the economists on Stanford's faculty.

Stigler stated:

> Speaking only as a conservative, and ignoring my professorial role, I would consider it a deplorable development if a Board of Trustees could be brought to push the appointment of a conservative professor, and for a simple reason. It is much more likely, given the energy of the left-wing groups, the ascendency in intellectual circles of collectivist ideals, and the growth of the federal governmental role in higher education, that a Board of Trustees acquiescent to outside pressures would foster non-conservative appointments and not conservative appointments. Or if conservatives won the battle this year, they could easily lose it five years hence. The long run acceptance of conservative ideas, I pray, will depend upon the validity of our reasoning and the cogency of our evidence, in which I have more faith than I have in our ability to mobilize the right kinds of conservative influence.

He concludes the letter by observing:

> Professors are not endowed with any special objectivity, and as a group they conform much more closely to 'standard' views that (sic) would wish. But faculties are not perverse or misanthropic or hostile to the search for excellence. They will respond to this drive for academic excellence, and in the process both conservatives and radicals will appear a bit more often than in the typical faculty. And this is all that I, as either a Trustee or a conservative would desire.

Perhaps Stigler would have offered the same advice to a present-day "progressive" president of a board of trustees wishing to increase the proportion of like-minded progressive faculty on university campuses to offset the infusion of funding from conservative and libertarian sources for faculty positions.

Stigler was notably modest about the actual impact of economists. In his essay "Do Economists Matter?" he states (p. 63) "My central thesis is that economists exert a minor and scarcely detectable influence on the societies in which they live." He notes in the same essay the tension between two views of the economist. One is that "economists are the expert critics or defenders of any and all economic policies" (p. 57) while the other view is of "the economist as a customer's man" (p. 59) and that "consumers generally determine what will be produced, and producers make profits by discovering more precisely what consumers want and producing it more cheaply" (p. 57). However, Stigler's career endeavors would seem to suggest that he thought economists mattered both as preachers and as scientists.

Acknowledgements Stephen Stigler graciously provided a copy of Frank Knight's reference letter for George Stigler to T.W. Schultz and provided access to his copy of the Meckling interview with W. Allen Wallis.

Notes

1. See Mitch (2016) for details of the rankings by Johnson and Schultz; and the online appendix to Mitch (2016) for the text and source of Friedman's letter to Arthur Burns.
2. The sources for this section are provided in Mitch (2016) and the online appendix for that article.
3. Those present at the May 28, 1951 faculty meeting included: T.W. Schultz, D.G. Johnson, M. Friedman, H.G. Lewis, F. Harbison, E. Hamilton, F. Knight, L. Metzler, G. Tolley, J. Marschak, A. Rees, H. Kyrk, T. Koopmans. Absent: J. Nef, P. Thomson, B. Hoselitz (out of city), R. Blough (on leave), L. Mints, R. Goode.

   It was agreed upon to rank the top three of the four names: George Stigler, Leonid Hurwicz, Abba Lerner, and Kenneth Arrow. The results of the ranking were:

Number of members giving candidate			
Candidate	1st position	2nd position	3rd position
Stigler	8	0	2
Arrow	1	7	3
Lerner	3	4	3
Hurwicz	0	1	3

From University of Chicago, Department of Economics Records Box 42, Folder 2.

4. For more on the 1935 episode and the larger context of concerns about anti-American teachings at the University of Chicago at this time see Boyer (2016, pp. 27–65).
5. Feb. 1, 1954 memo from Morton Grodzins to Lawrence Kimpton in Office of the President Kimpton papers, Box 224, Folder 12.
6. This interview segment with Gary Becker was kindly sent to me in an email from Craig Freedman dated September 17, 2017.

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Voyages on the Seas of History and Economic Thought

Historical Ambiguity: Reshaping the Snows of Yesteryear

Craig Freedman

History is bunk (Henry Ford)

History of economic thought may have been George Stigler's first love, but he turned out to be something of a feckless lover as the years rolled by. (It raises the question of why at crucial moments Stigler obviously didn't seem to give a feck.) At times, his attitude to the field, which greatly benefited from his efforts, parallels the closed-minded perspective of the mechanically gifted but entirely cantankerous Henry Ford. These moments reveal an academic who considers his passion to be something of a guilty pleasure, like someone catching up on his macramé during an academic conference. The pursuit has hardly been (for many decades) a field that garners even a modicum of respect.

Those readers with literary pretensions might recognise the allusion to François Villon's well-known poem. The line of course that forms the work's refrain: '*Mais où sont les neiges d'antan!*' 'Oh, where are the snows of yesteryear!'.

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> … the low state of repute in which the subject has fallen in the United States discourages me from pursuing that interest any further. People positively jeer at you when you state your speciality … it should be a side line, they argue. (Mark Blaug letter to George Stigler, December 20, 1963)

Despite his eminence in the field, that George Stigler in the 1970s did nothing to resist the subject's (history of economic thought) permanent termination from graduate education at the University of Chicago should hardly come as a shock. Intermixed with his sense of guilty pleasure is a continuing ambivalence of attitude with which he surrounded the subject.[1] Whatever his innermost reflections and emotions might be at any given moment, Stigler was never one to hint at, let alone exude, any degree of sentimentality. His heart was never carelessly displayed on his sleeve. Though it is true that while at Columbia, he supervised Mark Blaug's dissertation on Ricardo, equally clear is his continued unwillingness to actually promote the subject, whether through making a conscious decision or by simple neglect alone.[2] However, he clearly dismissed the importance of such knowledge as forming a vital, or even useful, substratum of an accomplished, academic economist.[3]

> Some historians of economics – Schumpeter is an eminent representative – believe that an understanding of the evolution of a science helps to understand its present structure. This claim may be conceded and restated as the plausible hypothesis that correct knowledge never has a negative marginal product. Nevertheless, one need not read in the history of economics – that is, past economics – to master present economics. (Stigler 1982: 107)[4]

In Stigler's actual work in the field, he seemed incapable, at moments, of preventing ideological concerns from seeping into his research. His work, scientific or otherwise can be evaluated, without too much strain, as a continuing defence of market efficiency. The core of his analytical framework rested unreservedly on a base of rational decision-making.[5] Doing so, allowed him to equate consumer sovereignty with individual freedom and liberty. This ploy necessitated a determined rejection of any conflicting influences, whether psychological, historical or political. In this sense, history for Stigler was indeed bunk.[6]

> In fact Smith's professional work of psychology (in the *Theory of Moral Sentiments*) bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted despite continued efforts from Jennings (1855) to Herbert Simon and George Katona to destroy it. (Stigler 1965: 28)

He revered Adam Smith as the alpha and omega of economics for recognising that narrow self-interest, when attached to competitive forces, led to unintended but desirable societal ends. Stigler was nonetheless disappointed at Smith's reluctance to carry the campaign to its logical end.[7] ('Well, you know my argument is the opposite? No, he [Stigler] was sort of critical of Adam Smith in a way that I didn't think was justified". Conversation with Ronald Coase, October 1997.) The insistence that everything had to reduce to self-interested motivations, including in particular political markets, separated him from his version of the unarguable superiority of Adam Smith's insights. To a clear and certain extent, Stigler remained dumbfounded at his hero's inexplicable lapse.

> We're talking about the political world, the political market as opposed to the economic one. But in interpreting the political market, George very consistently, interprets the political market as a resolution of opposing self-interests and tended to give very little attention to the extent to which it arose out of the desire of the people involved in government to promote the public interest. (Conversation with Milton Friedman, August 1997)

Apart from his own work, George Stigler was grudging in his praise for, and even dismissive of, the field in which he himself had so extensively contributed. In his evaluation, History of Economic Thought harboured more insidious danger than the *Postman Pat* children's series, but lacked its scholarly impact. If young economists were not to become fatally distracted by mulling the ambiguities thrown up by a careful examination of the subject's history, then the only real prophylactic was complete abstinence. He was then perhaps heartened in his later years by the increasing ignorance of most economists when it came to an understanding of the history and evolution of their profession.

> The young economist who reads some of the early controversies with care will surely learn one lesson, and he may learn two. The inevitable lesson is that after studying previous controversies one cannot become quite so engaged in the current controversies – one cannot become quite so convinced of either the correctness or the importance of one's new ideas. The more subtle lesson is that it does not pay to learn the first lesson: the temperate, restrained, utterly fair-minded treatment of one's own theories does a disservice to these theories as well as to one's professional status and salary. The scientist is loath to buy new models which have not been well advertised. I therefore accept the proposition of Bishop Stubbs that the study of history probably makes a man wise, and surely makes him sad. (Stigler 1988: 111)

George Stigler apparently remained quite comfortable throughout his career in permanently exiling the field described by the history of thought to the dank anterooms of economic research. Despite his own distinguished efforts, starting with his Northwestern MBA[8] and relentlessly exploring noted unresolved issues in the field, Stigler remained ambivalent of its worth. He even showed a willingness to emblazon it with a warning label sufficient to deter the unwary who might wander into these unrewarding thickets, 'abandon hope all ye who enter here'. One does wonder, in light of his treatment of economists long buried, whether George Stigler was really Adam Smith's best friend or even much of a supporter of History of Economic Thought.

> There is a well-known story, still worth retelling, about a word game George invented for young children. He would give them, he said, a million dollars if they correctly answered three questions. The first, "Who was buried in Grant's tomb?" and the second, "Whose head was on the Lincoln penny?" inspired great confidence in the child for a prospective life of luxury and leisure. Invariably, all hopes were dashed with the third question: "Who was Adam Smith's best friend?" Except one time the son of a friend responded, "Why, you are, Uncle George". (Rosen 1993: 809–810)

Notes

1. Stigler's exact role in banishing History of Economic Thought from the graduate curriculum is not unfortunately clear. In a published piece, Sherwin Rosen claimed that

   > … history of thought, like all other fields, is well enough served by its own specialists. These were the reasons why Stigler proposed and supported the decision of the Economics Department at the University of Chicago to abandon its history of thought requirement in 1972, before many other departments did. (Rosen 1993: 811)

   However, in private conversation, Rosen preferred to qualify this unequivocal claim shifting some of the core responsibility away from the determined shoulders of George Stigler. Whether or not George Stigler agitated for such a move, what remains clear is that he certainly did not see fit to attempt to impede such a development.

   > Now, I don't know if he [George Stigler] was a *leader* [in abolishing the HET requirement], but he was certainly in *favour* of it. I don't know how that issue first came up. I think it came up because most young economists had lost interest in it and it was just a big tax on everyone's energy. No one was working on it. It wasn't really much of a research field. I think it's had a slight come back but it's still pretty small. Even now, you see very few Ph.D.'s with that particular interest. There's also been a general laissez faire attitude around here about graduate degree requirements. So I think that's how it came up for discussion. In the same way, there was an Economic History requirement which, I don't think we have anymore either. That was also dropped. So I don't think he was in favour of putting artificial restrictions on a degree. That was probably his motivation. But I don't know. I don't think *any* of us were too happy about that move. (Conversation with Sherwin Rosen, October 1997)

2. Though at times tossing titbits of history of economic thought in his classroom lectures, he was never known to have taught the subject despite the renown his work in this field gained. Whether this was by choice or by necessity remains a mystery. Certainly at Columbia, that particular teaching spot seemed permanently owned by Joseph Dorfman. [Joseph Dorfman (1904–1991) was a Columbia University institution. He received his doctorate from Columbia in 1935 and began teaching there in 1931. In 1948 he became a full professor at Columbia where he remained after retirement as an Emeritus Professor].

> Economic thought connected with my interests. When I arrived at Columbia the first thing I did was to enrol in the course offered in the history of economic thought. It was taught by Joseph Dorfman. Within two weeks (at the very beginning of the term) I realised that I had here what I now regard as the most boring lecturer I have ever heard, and I've heard a lot of boring lecturers. He read! - He read !! - his lectures, even though he'd been teaching from them for twenty years. And I, as a student, couldn't stand it. I quit.
>
> The only other person although not teaching history of economic thought, but who had, (by his publications), obviously an interest in it was George Stigler.
>
> *Did he teach any history of thought?*
>
> No, not at all. He wove it into his lectures occasionally when he was teaching industrial organization. He talked about firm behaviour and there would be remarks about the history of the theory of the firm in the 19th century, but no, not very much. (Conversation with Mark Blaug, April 1998)

But this failure to ever formally teach history of thought doesn't adequately explain Stigler's acquiescence in banishing the subject at Chicago. However, whatever his wider misgivings might have been, he was known to have performed as a mainstay of the subject, having presented such work at numerous conferences, for instance the bicentenary celebration of Smith's *Wealth of Nations*.

> 1976 was the bicentenary of the publication of *The Wealth of Nations*. At Glasgow there was a conference to which I was invited along with George Stigler. We all went down to the Royal Mall on the Castle to the cemetery where Smith is buried. We stood in front of that tombstone that says, 'Here lies Adam Smith, author of *The Theory of Moral Sentiments*.' It didn't mention *The Wealth of Nations*. And I remember saying, 'Jesus, these people think that the great book which Adam Smith wrote is *The Theory of Moral Sentiments* and not *The Wealth of Nations*?' I know I looked at George Stigler as I said it. That conference included a vile debate between Sam Hollander and George Stigler. Sam Hollander had just published his first book on Adam Smith. Stigler really told him at this conference, 'You're full of shit.' Sam tried to stand up to him, tried to keep it civil as well, but Stigler really laid into him. (Conversation with Mark Blaug, April 1998)

3. The sense that the subject lacked any crucial importance is best reflected in Stigler's tone. He often joked as a way to dismiss from consideration a subject he either wasn't interested in or simply found far too ambiguous to tackle.

   > A young economist who believes that Adam Smith was the Smith who founded the Mormon faith will only provide innocent amusement outside of Utah, at no cost to his professional status. (Stigler 1988: 215)

4. Certainly George Stigler's close colleague and friend, Milton Friedman could be similarly dismissive of forays into History of Economic Thought. Such endeavours for Friedman reflected a certain ennui or even stagnation within the profession itself.

   > I'm saying that there is sort of a balance wheel here. If there are exciting things being done in a theory, interesting and exciting things to do with the structure of the body of economics, that's what will attract the top young economists. And then they'll be drawn away from the history of economic thought or similar such fields. On the other hand, if it's a dry period, so far as really adding to the structure of economic thought is concerned, all of a sudden, everybody is interested in such things as the background of Stigler, of Keynes, or of Samuelson. (Conversation with Milton Friedman, August 1997)

5. At times George Stigler seems determined to reduce Smith to a fervid supporter of this formulaic motivation. Doing so nicely strips Smith of his complexity. Though in his autobiography (1988), Stigler honours his former teacher, Jacob Viner, in recognising his scholarly and near encyclopaedic knowledge of the economics discipline, he seems to discard or simply not remember Viner's views on Smith.

   > The important thing for the interpreter of Smith is to note how low down … reason enters into the picture as a factor influencing social behaviour. The sentiments [that is, the instincts] are innate in man … Under normal circumstances, the sentiments make no mistake. It is reason which is fallible. (Viner quoted in Coase 1994: 116)

6. For the world to work according to Stigler's prescribed manner, self-interest had to be the sole, not only the dominant, motivation for political legislation and everything else.

Yet he would often criticise others. I know he would often criticise people for trying to explain every observation and not accepting the fact that there was a certain degree of error. But when in the middle of a lunch time conversation, if you confronted him with something like that, with some anomaly, he could have just said, "Look, that's the error. Let's just go on and talk about the substance". But he would in fact say, "No. You're going to find that some interest was in there on the political front. You'll find it if you look hard enough." You wouldn't find it. (Conversation with Sam Peltzman, October 1997)

7. Ronald Coase, in this regard, as in others, differed markedly with George Stigler in his understanding of Adam Smith. Stigler seemed in his writings more animated by what Smith should have said rather than deciphering what Smith actually meant. At points, Stigler gave into the temptation of finding within Adam Smith what he needed to discover. Coase, while at Chicago, never became of Chicago in the same sense that Milton Friedman or George Stigler did. Coase instead saw himself as a throwback to Frank Knight, taking a more pragmatic methodology as his guiding light.

   It is wrong to believe, as is commonly done, that Adam Smith had as his view of man an abstraction, an "economic man" rationally pursuing his self-interest in a single-minded way. Smith would not have thought it sensible to treat man as a rational utility-maximiser. He thinks of man as he actually is: dominated, it is true, by self-love but not without some concern for others, able to reason but not necessarily in such a way as to reach the right conclusion, seeing the outcomes of his actions but through a veil of self-delusion … if one is willing to accept Adam Smith's view of man as containing, if not the whole truth, at least a large part of it, realization that his thought has a much broader foundation than is commonly assumed makes his argument for economic freedom more powerful and has conclusions more persuasive. (Coase 1994: 116)

8. There is a possible assumption which is easily leaped upon, namely that Frank Knight's influence when George Stigler hit Chicago led him to write his History of Thought dissertation which would become his first published book (1941). Less well known is that he had already completed two lengthy papers (1931, 1932) working with issues that might be considered as preliminary work forming a natural bridge to his doctoral efforts.

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George Stigler's Adam Smith: Successes and Failures

Jeffrey T. Young

Elsewhere in this volume David Levy and Sandra Peart have discussed Stigler as a reader of Adam Smith. I propose in this chapter to examine Stigler as an interpreter and user of Adam Smith. In a letter dated August 17, 2001, David Raphael wrote to Andrew Skinner:

> The first suggestion of a Glasgow edition of the works came from Laurie Hunter in October 1961, when he was a graduate student at Chicago. He wrote to Macfie saying that Professor Stigler thought there should be a collected works edition to celebrate the bicentenary in 1976, and that he (Stigler) would not want to intrude if Glasgow were to do it. (Raphael 2001: 1; see also 2007: 3)

Ronald Meek was the "prime mover" at Glasgow. In a memorandum dated, October 31, 1961, he set out two arguments in favor of a collected works:

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> (a) because others [one such "other" would have been the University of Chicago] would do it if we didn't, and (b) because there was scope for going beyond Cannan in considering Smith's thought in the light of modern interest in economic growth and development. … Following upon Meek's memorandum, a committee was set up (with Meek in the key position of secretary) and a meeting was held on 30 November 1961. (Raphael 2001: 2)

Thus was born the committee to oversee the project, with Ronald Meek in the lead. (He was later replaced by Andrew Skinner.) I relate this anecdote here for two reasons. First, we may observe the ecumenical appeal of Adam Smith. As economists, Stigler and Meek would be strange bedfellows. Stigler was a founding member of the modern Chicago School and Meek was a self-proclaimed Marxist. Yet as scholars they had great professional respect, and shared a deep and abiding appreciation of Smith's work. Second, it brings to light Stigler's role in bringing about the *Glasgow Edition of the Works and Correspondence of Adam Smith*, the single most important publishing event for the advancement of Smith scholarship.

As an interpreter of Adam Smith, Stigler published two major articles, "Adam Smith's Travels on the Ship of State" (1971), and "The Successes and Failures of Professor Smith" (1976). He also edited a reissue of Cannan's edition of the *Wealth of Nations* (WN) (Smith 1976b), and he produced an abridged version of the book.[1] As what I would call a user, Smith was part of Stigler's "extended present" (Boulding 1971). As such we find frequent references to Smith throughout Stigler's writings. These are more than just passing references or textbook sound bites. Stigler is interacting with Smith as if he (Smith) was a contemporary colleague.

Before turning to these we might first briefly note Stigler's general approach to the history of economic thought. Beginning with his doctoral dissertation, published as, *Production and Distribution Theories* (Stigler [1941] 2013), written under the mentoring of Frank Knight, he produced major historical studies during his career. As a historical scholar one should not be surprised that Knight's approach to historical texts instilled in him a highly critical approach. Knight opened his famous two-part article on Ricardian economics with the following observation:

> On the assumption that the primary interest in the "ancients" in such a field as economics is to learn from their mistakes, the principal theme of this discussion will be the contrast between the "classical" system and "correct" views. (Knight 1935: 3)

In fact, Stigler and Knight pioneered this sort of critical history, which Mark Blaug has called the "absolutist" approach to the history of thought (Blaug 1996: 1–2). Modern theory, Knight's "'correct' views," are the standard of judgment. It is the standard Stigler employs. Indeed, his dissertation was a seminal work in the advent of absolutist history.

I should also mention that Stigler developed a criterion for scientific exegesis to help determine which interpretation of an author's words is the correct one when the historian is faced with contradictory passages. He concluded:

> The test of an interpretation is its consistency with the main analytical conclusions of the system of thought under consideration. If the main conclusions of a man's thought do not survive under one interpretation, and do under another, the latter interpretation must be preferred. (Stigler 1965a: 448)

This is of course consistent with absolutist history of thought. Both privilege scientific analysis as the basis of judgment on the grounds that correct and incorrect views can be successfully distinguished. However, I will not be using it in what follows as establishing consistency in the face of contradictory statements by the same author does not arise in his interpretations of Smith.

"Adam Smith's Travels on the Ship of State"

The article begins by announcing Stigler's essential interpretive stance vis-à-vis Smith: "The *Wealth of Nations* is a stupendous palace erected upon the granite of self-interest" (Stigler 1971: 265). Having stated the maximizing principle, which modern economists trace back to Smith's "palace," Stigler detects a significant paradox in Smith's thought: "If self-interest dominates the majority of men in all commercial undertakings, why not also in all their political undertakings?" (Stigler 1971: 265). In other words, why would a legislator obstruct the system of commercial self-improvement?

Yet Smith did acknowledge the self-interest of particular economic groups in obtaining favorable legislation, and Stigler assembles an impressive table of 26 specific instances (Stigler 1971: 267). There follows another list where the supposed beneficiaries obtained a policy, but they were mistaken, the policy actually hurt them. Stigler states two propositions:

A. Sometimes (often) economic legislation is passed at the request of economic groups who hope to benefit by the legislation.
B. On occasion a group is mistaken in the consequences of the legislation and receives no benefit or even positive harm from its legislative program (Stigler 1971: 268).

But, these are immediately dismissed as either platitude, once corrected for Smith's errors, or as insignificant. Stigler would replace Smith's "sometimes" with "all," and

C. All legislation with important economic effects is the calculated achievement of interested economic classes (Stigler 1971: 268).

Appropriate or not, Smith implicitly rejected the use of self-interest as a general explanation of legislation (Stigler 1971: 268). What evidence is presented in the article, having just stated 26 seemingly counterexamples? "The most important evidence is that for most legislation no group is identified which could have fostered the law and would benefit from it. The most important area of neglect is the discussion of taxation" (Stigler 1971: 269). The second piece of evidence relates to laws requiring that wages be paid in money, not in kind. Stigler claims that Smith is posing a nonexistent legislative puzzle. I find this point baffling. First, I do not detect a puzzling attitude in Smith's text (which I will examine again below). Second, Stigler, of all people, should know that payment in the medium of exchange puts a utility maximizer on a higher indifference curve than payment in kind. Thirdly, as I will argue at length, the issue, for Smith, was a matter of justice, and Stigler seems to be blind (dismissive) of any justice claims or ethical underpinnings to the self-interest model.

Stigler's third, and last, piece of evidence is the role Smith grants to pure emotion in politics. Here it is the ignorance and stupidity of the landlords that proves the point.

Stigler goes on to suggest that Smith paid too little attention to the political process itself, as opposed to the legislation it produced. Stigler quotes approvingly Smith's handling of the motives of the American revolutionaries, and how this might be taken advantage of to motivate Smith's proposed peaceful solution of union with Great Britain. However,

> In general, … Smith's attitude toward political behavior was not dissimilar to that of a parent toward a child: the child was often mistaken and sometimes perverse, but normally it would improve in conduct if properly instructed. (Stigler 1971: 272)

I shall not discuss Stigler's objections to Smith's analysis of taxation. Rather Stigler discusses Smith's treatment of the Oxford professors as evidence for his naïve treatment of the agents of the public sector. Smith's unhappy 6 years at Oxford taught him that

> In every profession, the exertion of the greater part of those who exercise it is always in proportion to the necessity they are under of making that exertion. (WN V.i.f.4, p. 759)

Consequently, "In the University of Oxford, the greater part of the public professors have, for these many years, given up altogether even the pretence of teaching" (WN V.i.f.8, p. 761). Simply, their salaries were not tied in any way to their performance. For Stigler, this demonstrates his point. If Smith was so astute about human nature as to not even let professors off the self-interest hook, why should he not apply the same logic to legislators?

In fact, he does apply it to law enforcement. Amid a lengthy, Smithian story of unintended consequences Smith observes that, "Public services are never better performed then when their reward comes only in consequence of their being performed, and is proportioned to the diligence employed in performing them" (WN, 241). The story is how an independent judiciary emerged in England, which is a prerequisite for that impartiality which is itself a prerequisite for judges to rule justly. Since Smith is here talking about the public services, it may be that Stigler has overlooked the fact that Smith does apply the model of self-interest to the agents of the government, if not to the political process and lawmakers themselves.

The article ends with a discussion of market failures that we find in Smith. Stigler is not impressed with Smith's catalogue of failures of self-interest, which he categorizes as being rooted in ignorance, agency problems, and public goods provision. Stigler's major contributions have centered on his theory of search and information acquisition, and his analyses of the economics of regulatory action and its efficacy in achieving its stated goals. In this light his dismissal of all market failure claims is understandable.

"The Successes and Failures of Professor Smith"

This essay was published in the bicentennial year, 1976. It is Stigler's mature assessment of Smith's great book. His criterion of judgment is historical: were Smith's theories taken up and used by subsequent practitioners of the science or were they discarded as wrong-headed? "In any event, it is the judgment of the science that is decisive in judging a scholar's achievements" (Stigler 1976: 1200). To this Stigler attaches his own judgment and further subdivides successes and failures into "proper" and "improper." We might note here in using science as the criterion of judgment, Stigler is not using the principle of scientific exegesis of texts. He is not trying to reconcile real or apparent cases of inconsistency. Here, while still approaching an historical text as a scientific treatise, he is engaged in assessing the text's scientific value. As such, the judges are the economists, including Stigler, who either accepted, rejected, and/or built upon Smith's analytical system.

> Smith had one overwhelmingly important triumph: he put into the center of economics the systematic analysis of the behavior of individuals pursuing their self-interest under conditions of competition. This theory was the crown jewel of *The Wealth of Nations*, and it became, and remains to this day, the foundation of the theory of the allocation of resources. The proposition that resources seek their most profitable uses, so that in equilibrium the rates of return to a resource in various uses will be equal, is still the most important substantive proposition in all of economics. …
>
> I do not know whether to list as a second triumph one enormously successful application of this theory of competitive prices, namely, Smith's theory of the differentials in wage rates and profit rates among occupations. …
>
> The third and final major success of Smith was his attack on mercantilism. (Stigler 1976: 1201)

But,

> There is a fourth considerable success to be credited to Smith: the formulation of the wages-fund theory. … there is no doubt that it dominated the next 100 years of English economics. (Stigler 1976: 1203)

Thus, the allocative, optimizing properties of the competitive economy, the sources of equalizing wage differentials to explain wage inequality, and free trade have all become "permanent parts of economics" (Stigler 1976: 1204). I concur that these are indeed proper successes.

Stigler proposes one main category for an improper success, an influential doctrine which should have been discarded. This is the idea that Smith got from the Physiocrats: the distinction between productive and unproductive labor. It is "improper" because it is an "error or infertile … subject or method of analysis" (Stigler 1976: 1204). Certainly, Smith has been widely and frequently criticized for this. As is well known he advances two definitions: a value definition (productive labor adds value to materials) and a physical definition (productive labor leaves behind some tangible good) (WN II.iii. 1, p. 330). Consequently, Smith and the classical economists, including Marx, have been criticized for ignoring the role of services.

First, Stigler recognizes the kernel of truth in Smith's text. "The purpose of the distinction is clear: if we identify productive labor by the characteristic that its product can be accumulated, then capital formation can take place only out of the product of productive labor" (Stigler 1976: 1204). His critique, then, ignores the definitional problems, but, second, he points out two other issues.

> The difficulties with the distinction are two. Even if Smith is correct, the extensive employment of productive labor merely *permits* the accumulation of capital, and the actual formation of new capital requires a wholly independent act of saving. …
>
> There is a second difficulty: there are investment acts which are not the result of productive labor. Investments in what we now call human capital do not become incorporated in a tangible, saleable commodity as commonly understood. (Stigler 1976: 1204)

We see, then, that human capital, which Smith did recognize as part of the capital of a nation, presents a particular problem. Producers of education should be productive laborers. However, since they produce only services they run afoul of the physical definition of productive labor. In fact, Smith lists teachers explicitly as useful, but unproductive workers. Again I tend to concur with Stigler's judgment here.

"A proper failure contains analytical error" (Stigler 1976: 1205). Here the award goes to Smith's hierarchy of capital investment. Based on the criterion of employing productive labor, Smith argued that from most productive to least productive was agriculture, manufacturing, transport, and retail.

> That Smith was in error is unequivocal. He allowed a system of financing to conceal the facts of economic life. If the consumer, instead of paying

> the retailer for the corn, had paid the farmer for raising it, the millwright for grinding it, the ship's captain for transporting it, and the retailer for stocking it, then everyone's capital would have gone exclusively to the direct support of production. But nothing essential would have changed. (Stigler 1976: 1206)

Closely related to the hierarchy is the hierarchy of social usefulness of different forms of trade: domestic, foreign, and carrying. There is no doubt that history has passed a negative judgment on Smith's hierarchies. The famous invisible hand comes into play to help private owners of capital choose the socially correct place in the second hierarchy by preferring to invest in domestic trade over foreign or carrying trades. History threw out the theory behind the invisible hand, but not the principle of beneficial unintended consequences.

While the historical verdict may be true, Samuel Hollander in his book on Smith's economics has attempted at least a partial vindication of Smith (Hollander 1973; see also Young 2001). Briefly, Hollander argues that Smith's true position can be discerned by looking at his applications of the principle. Here we find the hierarchy of investments, which in the natural progress of opulence govern the process of economic development, couched in terms of the principles of resource allocation based on factor price equalization, operating in the context of changing relative scarcity. The dynamic model of development is built on the model of static resource allocation and is fully consistent with it. The natural advantage of agriculture, for example, depends on the relative abundance of land compared to labor, thus generating a relatively high profit rate in agriculture (pp. 280ff.). The free play of self-interest is sufficient to direct the first investments of capital into agriculture, and it is only when land becomes scarcer relative to labor that the first investments in manufacturing will come forth. This process generates Smith's "natural balance of industry" which plays a leading role in his critique of mercantilism (WN IV.vii. c. 43, p. 604). Hollander concludes:

> It is, in brief, not merely the elaboration of the mechanisms of resource allocation which requires attention, but also the particular uses to which

> the analysis was put, and it is in the course of Smith's treatment of the historical sequence of investment priorities according to the principle of profit-rate equalization, that a fundamental equilibrating mechanism is utilized, namely resource allocation governed by the differential pattern of factor endowments between economies. (Hollander 1973: 307)

Moving along, Stigler, then raises Smith's labor commanded theory of real price. Smith proposed an hour of labor as the proper unit to correct money prices in the face of long-run inflation and/or deflation. In my view Stigler, again, shows sound judgment not to get involved in Marxian metaphysics about value and labor as the sole "creator" of value. "A very different error, and possibly not an error at all, is Smith's measure of value-which came from the same source as that which may have led him to overvalue agriculture" (Stigler 1976: 1206). Modern price indexes fix a bundle of commodities which yield equal utility as the unit of value, while Smith proposed "the disutility of 1 hour of ordinary labor" (Stigler 1976: 1207). Stigler lists at least three reasons why economists might legitimately object to Smith's standard, and much work has also gone into measuring and correcting sources of error in the modern approach as well. The economics of price indices is beyond the scope of this entry, but I will point out that Smith's labor standard has been fruitfully applied in some modern research. William Nordhaus's celebrated work on the price of light, for example, develops a Smithian index of labor commanded (Nordhaus 1996). His research suggests that the labor measure may perform better than conventional index numbers, especially when measuring price over extremely long periods of time. The upshot is that I concur with Stigler's judgment on labor commanded. It may not have been an error even though history has largely selected against it.

Stigler's final candidate for a proper failure is Smith's monetary theory. I will simply defer to Stigler's conclusion that Hume's theory was better, because it was more "general" and had more "predictive power" (Stigler 1976: 1208).

The last section of the paper takes up the one remaining category of improper failures. Here he asks the question, "Where has the scientific judgment of future economists been wrong about Smith?" Malthus, for example, is charged with taking up the subsistence wage theory, which

Stigler claims Smith rejected. Smith did, in fact, argue that British wages were above subsistence. Moreover, in the progressive state real wages would rise continually, depending on the rate of accumulation of capital. Malthus, then, is charged with perpetrating the error by implicitly assuming that the accumulation parameter in the wage equation was zero (Stigler 1976: 1208).

We might note, that a considerable body of recent scholarship absolves Malthus of this charge, but Stigler's point with regard to Smith is well-taken.[2]

The theory of rent, Stigler suggests, is another failure that should have been a success. It did eventually find its niche in contemporary economics once neoclassical economists came to understand that rent was a surplus in the aggregate, but an opportunity cost in each specific land use:

> He consistently treated the rent of land as it should be treated: any one use of land had to pay a rent, which was a cost of production, to draw the land away from other uses; whereas for all uses combined, rent was a residual. (Stigler 1976: 1209)

Lastly, Stigler regrets that no theory emerged to explain the division of labor, but more on this below. Stigler's conclusion is that

> Smith was successful where he deserved to be successful-above all in providing a theorem of almost unlimited power on the behavior of man. His construct of the self-interest-seeking individual in a competitive environment is Newtonian in its universality. That we are today busily extending this construct into areas of economic and social behavior to which Smith himself gave only unsystematic study is tribute to both the grandeur and the durability of his achievement. (Stigler 1976: 1212)

Having surveyed Stigler's main contributions as an interpreter of Smith, I offer a brief discussion of his uses of Adam Smith. Having just noted Stigler's dissatisfaction with the way Smith failed to develop a theory of the division of labor, Stigler offered a theory of Smith's famous theorem that the division of labor is limited by the extent of the market. Smith's theorem suggests a positive feedback loop which generates increasing returns to scale. The theoretical problem, of course, is that increasing returns to scale is not compatible with the competitive organization of industry. This vexed Marshall who developed the concept of external economies. Stigler's contribution toward a solution of the problem was to propose a theory of vertical integration.

> But, with the expansion of the industry, the magnitude of the function subject to increasing returns may become sufficient to permit a firm to specialize in performing it. The firms will then abandon the process (Y_1), and a new firm will take it over. (Stigler 1951: 188)

Increasing returns lead to the development of specialist firms, which can operate at different points along the supply chain. Thus,

> Smith's theorem suggests that vertical disintegration is the typical development in growing industries, vertical integration in declining industries. The significance of the theorem can therefore be tested by an appeal to the facts on vertical integration. (Stigler 1951: 189)

Not surprisingly, Smith's theorem is also invoked in Stigler's celebrated *Price Theory* text (revised edition, 1966).

> Then a famous theorem of Adam Smith comes to our rescue: the division of labor is limited by the extent of the market. Smith pointed out that small villages could not support highly specialized occupations, but that large cities could. (Stigler 1966: 168)

This reference is accompanied by the following footnote: "*Wealth of Nations* (New York: Modern Library ed., 1937: 17–21). I earnestly recommend that all of this book except p. 720 be read" (Stigler 1966: 168n). In fairness to Stigler, I could not avoid at least one reference to his famous sense of humor. So, of course, I looked up page 720 in the Modern Library edition, and this is what I found: "If the teacher happens to be a man of sense, it must be an unpleasant thing to him to be conscious, while he is lecturing his students, that he is either speaking or reading nonsense, or what is very little better than nonsense [and so on]" (Smith 1937: 720).

Smith also gets significant space in the text on the topic of speculation, and there are numerous passing references throughout Stigler's work, which do not need comment. Stigler interacts with Smith as a scientific colleague, which in a nutshell captures Stigler's sympathetic, critical approach to Smith.

Stigler's Adam Smith: Successes and Failures

As a consummate neoclassical economist Stigler's absolutist approach to Smith has significant merit. As a writer of a somewhat broader approach to historical texts, I nonetheless have a great deal of respect for well-done rational reconstructions, and the use of modern theory as a judge of older theory. There is an analytical core which can be rationally understood and criticized from the perspective of a search for the truth. There are analytical mistakes in the history of economics, and it is worthwhile to point them out even after they have been eradicated from the discipline. (Chances are they have found refuge somewhere else, and are in need of continual refutation.) Smith, after all, was trying to correct what he perceived as the incorrect analytical underpinnings of mercantile commercial policy. His assessment of the Physiocrats was also absolutist in nature: "The capital error of the system … seems to lie in its representing the class of artificers, manufacturers, and merchants as altogether barren and unproductive" (WN IV.ix. 29, p. 674).

I have already rendered judgments concerning Stigler's catalogue of successes and failures with respect to Smith's analytical economics, and his failure to extend the model of self-interest to the analysis of the public sector. Certainly Smith's vision of the automatic functioning of the competitive economy, analysis of wage differentials, and the demolishing of mercantilist fallacies are success for Smith. Stigler's negative judgment on productive vs. unproductive labor is also well-taken, and I have offered a somewhat more sympathetic view of the hierarchy of capital investments. It was not until well into the twentieth century, with Stigler as once again a pioneer, that economists began to develop a theory of the state and the economic functions it was taking upon itself. Thus Stigler's negative assessment of Smith's handling of the public sector would seem to have merit. Although, as I argued above Smith would counsel the legislature that the public servants who are employed by the state will behave according to whatever incentive structure the law constructs for them. I also appreciate Stigler's positive use of Smith's analyses in developing his own theory of the dynamics of the division of labor, and his incorporation of significant references to Smith in his *Price Theory*.

However, these successes also form the basis of what I think is the single most significant failure of Stigler as an interpreter of Smith: namely his almost complete neglect of any other aspect of Smith's thought in general and of the *Theory of Moral Sentiments* (TMS) in particular. Amartya Sen, for example, in referring to the same set of Stigler's papers has recently commented that

> There is no room in the beliefs of this "as if Smith" for moral values of various kinds, from altruism to social commitment-values the reasonableness of which Smith discussed in considerable detail in *The Theory of Moral Sentiments*. (Sen 2016: 294–295)

Stigler's mentor, Frank Knight, argued repeatedly over a long period of time that economics as a set of analytical propositions was limited both in its ability to explain social phenomena and in its role as guide to policy making. Social questions required economics and ethics. Economics was a science of means, and we need to look to ethics via free and open discussion to discover a set of values to serve as worthwhile ends.

Indeed, Stigler once shared Knight's perspective in his 1943 critique of the "New Welfare Economics." Arguing that new welfare economics was consistent with paying thieves not to rob, Stigler noted that, "surely the primary requisite of a working social system is a consensus on ends" (Stigler 1943: 357). Sandra Peart and David Levy refer to this essay as part of a "Knightian moment" in recent history of economic thought (Levy and Peart 2017: 47ff.). Indeed, at the time Stigler went so far as to suggest that economics was a form of applied ethics (Stigler 1943: 358). As with the discipline, so with Stigler, this moment passed.

The absolutist approach to history similarly narrows the focus of historical inquiry to this analytical core and its evolution. It is potentially valid in its own sphere, but good history requires a broader approach. Like Knight, and briefly Stigler, Smith also was keenly aware of the ethical dimension, but unlike Knight he actually produced a significant treatise.

Stigler once remarked

> If an economist is to be a moral philosopher, however-and I have no doubt that we would do this well too-he should develop his philosophy to a level where its implications for policy become a matter of logic rather than a vehicle for expressing personal tastes. (Stigler 1975: 44)

The Knightian period is now long gone, and the irony of this statement should be obvious. Smith, Stigler's longtime "good friend" (Stigler 1976: 1200) was a moral philosopher who became an economist, and he did economics better than any of his contemporaries. (Witness history's judgment of Sir James Steuart's book.) The neglect of TMS of course, is part and parcel of the modern Chicago School's ambiguous Knightian legacy (see Emmett 2009). While eager developers of Knight's price theory they systematically relegated ethics to the realm of personal tastes, which were simply assumed as given, and not open to rational discussion. Consequently, in his Tanner Lectures at Harvard University in 1980 Stigler noted that:

> Economists have no special professional knowledge of that which is virtuous or just, and the question naturally arises as to how they are able to deliver confident and distinctive advice to a society that is already well supplied with that commodity. (Stigler 1980: 145)

Thus, it is not surprising that Stigler's attitude toward TMS is highly dismissive:

> In fact, Smith's professional work on psychology (in the *Theory of Moral Sentiments*) bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted despite continued efforts from Jennings [in 1855] to Herbert Simon and George Katona to destroy it. (Stigler 1965b: 28)

Stigler did acknowledge one connection between Smith's economics and TMS. In explaining why Smith might have rejected a bundle of consumption goods as his measure of real value, Stigler noted that

> Smith's rejection of consumption in fixing on a measure of value is attributable to his belief that luxuries are frivolous and yield illusory pleasures that vanish in the act of realization. This view is extensively argued in his *Theory of Moral Sentiments* … and receives adequate expression in the *Wealth of Nations*. That Smith should attribute to almost all economic actors an illusion that greater wealth yields greater satisfactions, an illusion that is perhaps never pierced, is one of his greatest idiosyncrasies. (Stigler 1976: 1207)

In what follows I shall take issue with two central points in Stigler's view of Smith, both rooted in Stigler's neglect of TMS. First, I would like to shed somewhat different light on Stigler's negative assessment of Smith's treatment of politics. While not specifically disagreeing with any of Stigler's specific points, I think Smith's whole treatment of the state and the political process needs to be set against some illuminating remarks found in TMS. Second, I wish to take issue with the foundational principle of Stigler's interpretation: that self-interest is the bedrock upon which the WN is erected.

Consider, first Smith's view of the state and the political agents that make its laws. In Part IV of TMS, Smith develops an original and complex view of the role of utility in the way humans make moral judgments:

> But this fitness, this happy contrivance of any production of art, should often be more valued, than the very end for which it was intended; and that the exact adjustment of the means for attaining any conveniency, or pleasure, in the attainment of which their whole merit would seem to consist, has not, so far as I know, been yet taken notice of by any body. (TMS IV. 1. 3)

Smith claims that utility as means (as opposed to ends) and their fitness for accomplishing their intended ends is frequently more valued than the ends themselves. Having applied the principle to the desire for material wealth, Smith notes that

> The same principle, the same love of system, the same regard to the beauty of order, of art and contrivance, frequently serves to recommend those institutions which tend to promote the public welfare. (TMS IV.1.11)

Specifically, he has in mind constitutions of government, which, "are valued only in proportion as they tend to promote the happiness of those who live under them" (TMS IV.1.11). Thus, the purpose of the government is to promote the general welfare, but Smith goes on

> From a certain spirit of system … from a certain love of art and contrivance, we sometimes seem to value the means more than the end, and to be eager to promote the happiness of our fellow-creatures, rather from a view to perfect and improve a certain beautiful and orderly system, than from any immediate sense or feeling of what they either suffer or enjoy. (TMS IV.1.11)

> In the same manner, if you would implant public virtue in the breast of him who seems heedless of the interest of the country, it will often be to no purpose to tell him, what superior advantages the subjects of a well-governed state enjoy; that they are better lodged, that they are better clothed, that they are better fed. (TMS IV.1.11)

In this case we value the beauty of the system of policy more than we do the material comforts of the majority of the people. Going on Smith notes that

> You will be more likely to persuade, if you describe the great system of public policy which procures these advantages, if you explain the connexions and dependencies of its several parts, their mutual subordination to one another, and their general subserviency to the happiness of society …. (TMS IV.1.11)

> Upon this account political disquisitions, if just, and reasonable, and practicable, are of all the works of speculation the most useful. (TMS IV.1.11)

This, I take it, is a self-description of Smith's own *Inquiry into the Nature and Causes of the Wealth of Nations*. Now we have already seen

Stigler's sneering attitude toward the idea of using moral suasion in achieving policy reform, where he would prefer that moral advice could be made a matter of logic instead of personal taste. Yet surely Smith and Stigler share a similar view of current policy: that it is the result of special interests being able to capture the political process. Merely explaining that is not going to solve the problem. What appeal could there be other than to rise above the faction and self-interest in politics to appeal to something greater? This would be Smith's public virtue. I would suggest that the systematic, analytical structure of WN is Smith's conscious application of his original insight about the beauty of means. Smith has erected a "stupendous palace" in order to arouse public spirit. Indeed, the automaticity and the efficiency of the system of natural liberty, which virtually all of Smith's readers have admired, is an appeal to this same principle in the way humans make value judgments.

Does this absolve Smith of his failure to apply the self-interest principle to the state? Perhaps not, but it does suggest that such a theory might not have served the purpose of arousing public spirit. I might also point out before moving on that Smith intended to write a treatise on law and government, but never finished it:

> I shall in another discourse endeavour to give an account of the general principles of law and government, and of the different revolutions they have undergone in the different ages and periods of society, not only in what concerns justice, but in what concerns police, revenue, and arms, and whatever else is the object of law. (TMS VII.iv.37)

This comment was allowed to stand when Smith revised TMS in the last year of his life. The 1790 edition was prefaced with this: "In the *Enquiry concerning* [*sic*] *the Nature and Causes of the Wealth of Nations*, I have partly executed this promise; at least so far as concerns police, revenue, and arms" (TMS Advertisement.2). Whatever would have been in the law and government treatise has been lost save for the two sets of student notes published as the *Lectures on Jurisprudence* (Smith 1978, hereinafter (LJ)). However, I doubt that Smith's treatise would have satisfied Stigler's objections.

Having argued that the "stupendous palace" was a conscious rhetorical strategy that Smith believed would be the most effective way of arousing public spirit to bring about the reforms necessary to establish the system of natural liberty, I turn to the "bedrock" of the palace. Consider the following two quotations:

> But *man has almost constant occasion for the help of his brethren*, and it is in vain for him to expect it from their benevolence only. He will be more likely to prevail if he can interest their self-love in his favour, and show them that it is for their own advantage to do for him what he requires of them. … It is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, but from a regard to their own interest. We address ourselves, not to their humanity but to their self-love, and never talk to them of our own necessities but of their advantages. (WN I.ii.2; emphasis added)

> It is thus that man, who can subsist only in society, was fitted by nature to that situation for which he was made. *All members of human society stand in need of each other's assistance*, and are likewise exposed to mutual injuries. Where the necessary assistance is reciprocally afforded from love, from gratitude, from friendship, and esteem, the society flourishes and is happy. … But though the necessary assistance should not be afforded from such generous and disinterested motives, though among the different members of the society there should be mutual love and affection, the society, though less happy and agreeable, will not necessarily be dissolved. Society may subsist among different men, as among different merchants, from a sense of its utility, without any mutual love or affection; and though no man in it should owe any obligation, or be bound in gratitude to any other, it may still be upheld by a mercenary exchange of good offices according to an agreed valuation. … Society may subsist … without beneficence; but the prevalence of injustice must utterly destroy it. (TMS II.ii.3.1–3; emphasis added)

The first quote is, of course, the locus classicus, of the self-interest bedrock of WN. The second, from TMS, is Smith's statement of justice as the necessary virtue for society to exist. In the first Smith is contrasting benevolence (the will to do good) with self-interest as motives to action. In the second, he is contrasting the virtues of justice and beneficence (the act of doing good). The point being that justice and self-interest seem to occupy the same position in the two passages. If self-interest is the bedrock, then justice is a substratum. If the division of labor is limited by the extent of the market, then the market is limited by the extent of justice; no justice, no market. Smith the moral philosopher turned economist does have special knowledge about what is just.

I have explored Smith's theory of justice and its relevance for WN in other works, so I will offer a very condensed treatment here (Young 1997, 2008, 2018). First, we may note that Smith's discussions of justice are set within the natural jurisprudence tradition, associated with Grotius, Pufendorf, and Hutcheson, extending back to the medieval Scholastics and to Aristotle (Hont and Ignatieff 1983; Young and Gordon 1992; Young 2008). As Smith was aware, the word "justice" had several meanings in the writings of his predecessors, and accordingly, he was always careful to distinguish distributive from commutative justice (see Haakonssen 1981: 99). Hence, when he comes to define justice in *The Theory of Moral Sentiments*, he first notes that the word "justice" has different connotations in the languages with which he is acquainted. "In one sense we are said to do justice to our neighbour when we abstain from doing him any positive harm, and do not directly hurt him, either in his person, or in his estate, or in his reputation" (TMS, VII.ii.1.10). This is commutative justice, and it is the sense in which Smith normally uses the word. The laws of justice deal exclusively with commutative justice.

Continuing, Smith notes another meaning of justice which coincides with certain earlier writers' concepts of distributive justice, and it "consists in proper beneficence, in the becoming use of what is our own, and in the applying it to the purposes, either of charity or generosity, to which it is most suitable…that it should be applied" (TMS, VII.ii.1.11).

These passages suggest that Smith has relegated distributive justice to the category of a personal moral virtue, namely, the duty of charity. Such a classification is in accord with the Protestant natural law tradition (Hont and Ignatieff 1983). Indeed, Smith followed this tradition closely in the *Lectures on Jurisprudence*, the student notes of Smith's moral philosophy course, which have survived and been published, where the distinction between commutative and distributive justice is explained in terms of perfect and imperfect rights:

> The common way in which we understand the word right, is the same as what we have called a perfect right, and is that which relates to commutative justice. Imperfect rights, again, refer to distributive justice. The former are the rights which we are to consider, the latter not belonging properly to jurisprudence, but rather to a system of moralls as they do not fall under the jurisdiction of the laws. (LJ (A) i.15)

These passages give rise to a common interpretation, which is being increasingly challenged, that Smith held to a narrow definition of justice as commutative justice, and what we now call "economic justice" or "social justice" or sometimes "justice" Smith called distributive justice, which was the virtue of beneficence. I cannot go into this debate here except to note that I am on record supporting a revisionist view that distributive equity may rightly be seen as part of Smith's conception of justice (Young and Witztum 2006; Young 2018).

I will simply note a few instances of justice in WN. First,

> All systems of preference or of restraint, therefore, being thus completely taken away, the obvious and simple system of natural liberty establishes itself of its own accord. Every man, as long as he does not violate the laws of justice, is left perfectly free to pursue his own interest his own way, and to bring both his industry and his capital into competition with those of any other man, or order of men. (WN IV.ix, 208)

> According to the system of natural liberty, the sovereign has only three duties to attend to … secondly … the duty of establishing an exact administration of justice. (WN IV.ix, 208–209)

This is, of course, the well known and often quoted end of Book IV of WN. The point is that justice is a constraint on self-interest, and thus the motive of self-interest which is indeed so prominent in WN is circumscribed and underpinned by justice. But justice having codifiable laws and punishments also underpins a system of justice, which transforms the principles into law.

Another instance would be Smith's defense of the high wage, progressive state:

> No society can be flourishing and happy, of which the far greater part of the members are poor and miserable. It is but *equity* besides, that they who feed, clothe, and lodge the whole body of the people, should have such a share of the produce of their own labour as to be tolerably well fed, clothed, and lodged. (WN I.viii, 89; emphasis added)

To my knowledge Smith does not use the word "equity" anywhere in his account of justice in TMS. I have, however, found it in the *Lectures*, where I have concluded that equity would seem to constitute a fairness norm in Smith's theory of justice. The upshot is that he could have just as easily said that "it is but *justice* besides that they who feed, clothe and lodge the whole body of the people, should have such a share of the produce of their own labour as to be themselves tolerably well fed, clothed and lodged."

I suggest that justice underpins Smith's account of wages and wage inequality at the microeconomic level. The chapter in the *Wealth of Nations* on the wages of labor begins with a seemingly obvious point that, "The produce of labour constitutes the *natural recompence* or wages of labour" (WN I.viii.1; emphasis added). Before going on I wish to establish two points. Wages are not actually paid in produce, because the law requires them to be paid in money:

> Whenever the legislature attempts to regulate the differences between masters and their workmen, its counsellors are always the masters. When the regulation, therefore, is in favour of the workmen, it is always *just and equitable*; but it is sometimes otherwise when in favour of the masters. Thus, the law which obliges the masters in several different trades to pay their workmen in money and not in goods is quite *just and equitable*. (WN I.x.c.61; emphasis added)

Hence, payment in money is just, and perhaps it is the moral element of Smith's argument that explains why Stigler treated this passage so negatively, as discussed above.

Note that the real value of the wage is the natural recompense for labor. Now this is natural law language, which I have argued reflects the natural law roots of Smith's value and distribution theory (Young

2008). The wage payment is rooted in a just claim on the revenue of the employers. Specifically, it is an instance of retributive justice. As Raphael has pointed out, Smith was one of the few to apply the principle of retributive justice to reward as well as to punishment (Raphael 2001: 118) Given that the liberal reward of labor is just and equitable, what does Smith say about the structure of wages at a point in time?

Smith attributes inequality in the labor market to two broad sources: the nature of the employments and the "policy of Europe." In other words inequalities may be either natural or artificial. Those which arise from the nature of the employments we may consider just, while those resulting from policy are unjust, although there is one case where the results are nonetheless beneficial. There are five broad categories of natural inequalities in the labor market:

> First the agreeableness or disagreeableness of the employments themselves; secondly, the easiness or cheapness, or the difficulty or expence of learning them; thirdly, the constancy or inconstancy of employment in them; fourthly, the small or great trust which must be reposed in those who exercise them; and fiftly [*sic*], the probability or improbability of success in them. (WN I.X.b.1)

It is not necessary to go into these five cases to understand the point: "The five circumstances above mentioned, though they occasion considerable inequalities in the wages of labour … occasion none in the whole of the advantages and disadvantages, real or imaginary, of the different employments …" (WN I.x.b.39). And the conditions necessary to achieve this equalization among other things there must be "the most perfect freedom" (WN I.x.b.40). The labor market functioning under conditions of perfect freedom (itself a principle of justice) will establish equality in the whole of the rewards minus costs. For example, the expense of acquiring an education creates a just basis for additional compensation—an instance of retributive justice in practice.

That this does present a picture of a just labor market is evident from Smith's condemnation of certain practices, such as long apprenticeships, which the policy of Europe enforces.

> The property which every man has in his own labour, as it is the original foundation of all other property, so it is the most sacred and inviolable. The patrimony of a poor man lies in the strength and dexterity of his hands; and to hinder him from employing this strength and dexterity in what manner he thinks proper without injury to his neighbour, is a plain violation of this most sacred property. It is a manifest encroachment upon the just liberty both of the workman, and of those who might be disposed to employ him. (WN I.x.ii, 136)

This hardly needs elaboration. Freedom exercised within the rules of justice is just liberty, which all should enjoy. A labor market in which freedom equalizes the whole advantages and disadvantages of all employments is a just labor market. Interferences are violations of natural justice. As matters of policy they are, of course, enjoined by the positive law, reforming which is the point. Thus, a just labor market will create equality at a point in time, not an equality of real wages but one of net advantages.

To recapitulate this rather long excursion into Smith scholarship, to view self-interest as the bedrock of the book is to take a superficial view of the book, its place in Smith's larger project, and the role of moral philosophy in economic analysis. Justice, I have argued, is the true bedrock. It not only makes markets possible, but it also constrains self-interest, and consequently makes it possible for the division of labor to function in commercial society. I cannot appeal to the self-interest of a person who stands ready to harm me. Stigler's superficial view of WN is symptomatic of the separation of economics, viewed as positive analysis, from moral philosophy, viewed minimally as providing a theory of justice. I might also point out that the theory of justice informs both the positive analysis and the normative ethical commitments of WN.

Conclusion

Stigler was a consummate neoclassical economist, and where neoclassical theory is indeed superior to Smith's classical theory it is right and informative to point this out. Stigler nicely brings Smith into the conversation when he is teaching material that contains Smith's original insights. As such Smith, the theorist is part of Stigler's "extended present." Stigler, as is true of other of Knight's students, has only taken half of what Knight was teaching. His separation of ethics and economics, while in some sense Knightian, misses the point. It is the ethics arrived at through discussion which has a Smithian antecedent in TMS that is really important. It is foundational for social life. The result is a regrettable neglect of Smith's moral theory in general, and the whole idea of justice and its relation to Stigler's beloved competitive economy. In short, I conclude that Stigler was half a Knightian and Smith was a whole Knightian. Or perhaps more accurately Knight was a better Smithian than was Stigler.

Notes

1. References to all of Smith's texts will be to the *Glasgow Edition* in the standard scholarly format.
2. The reader could do no better than to consult Anthony Waterman's work on Malthus (1991, 2012).

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George Stigler as a Reader of Adam Smith

David M. Levy and Sandra J. Peart

A theory of behavior, such as our profit maximizing assumption implies, could have come from psychology, but of course it did not.

In fact Smith's professional work on psychology (in the Theory of Moral Sentiments) bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted despite continued efforts … to destroy it.

George J. Stigler (1960, 44)

Earlier versions were presented at the International Adam Smith Society conference in Palermo, June 2017, at "George Stigler in the twenty-first Century" in Chicago and at the International Adam Smith Society conference in Viña del Mar, Chile. We benefitted from all the helpful comments. Stephen Stigler solved a mystery for us and Barry Weingast's comments helped us to focus the argument. Craig Freedman gave us useful suggestions. The remaining mistakes are ours.

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Introduction

We propose to consider George Stigler as a reader of Adam Smith by examining his position on how editors of complicated texts assist their readers. In Stigler's view, such assistance was considerable. Stigler characteristically did not suppose the economists he studied were inconsistent any more than he supposed choosing agents were inconsistent. Indeed, in a 1969 paper Stigler put forward the challenging thesis that anyone who understands David Ricardo's theory of production and distribution should be able to develop his theory of taxation (Stigler 1969).[1] Thus, Stigler's report of a radical inconsistency in Smith's treatments of private and public activity (Stigler 1971), presents two possibilities: Adam Smith's inconsistency; or a lack of editorial support that establishes the relationships among his works. We propose to develop the latter possibility as an extension of one of Stigler's memorable contributions, his economics of information (Stigler 1961). Famously, he demonstrated why consumers would apparently violate rational choice and fail to avail themselves of the lowest price. As Stigler pointed out, such choices might actually be rational if the cost of obtaining information about the price distribution were too high relative to the benefits. In the context of costly information, we can appreciate that the editor's role is to reduce the reader's cost of understanding the argument. In a world of dispersed prices, there is no reason to believe the consumer will find a minimum cost bundle. In a world of complicated texts without editorial support, there is no reason to believe the reader will appreciate how the argument is developed.

In Stigler's view, Ricardo's consistency followed from his approach as a commentator on the *Wealth of Nations.* By focusing exclusively on a subset of the problems in Smith, Ricardo did not need to address the relationship between distant parts of Smith's argument. Indeed, Stigler distinguished between the classics and their heirs on the scope of problems considered. The classics worked on a vastly larger scale than did their heirs.[2]

Stigler's 1960 judgment on Smith's *Theory of Moral Sentiments*, quoted above, reflects Stigler's view that the problem of consistency might be our problem, rather than Smith's. As a psychologist, Smith worked in an area that is distant from the competence of the modern economist. The point is simple: a modern reader might be less well-informed than Smith and so fail to understand critical steps in the argument. Since this would be true of any classical author, not just Smith, we open the paper by examining Stigler's reviews of proposed standard editions of economic classics in which he addresses at some length what services an editor ought to perform for the reader. Here, Stigler gives us a theory of editorship. We shall pay attention to Stigler's worries about the "psychological" barriers that even the best editor might erect when the texts are complicated.[3] His review of John Robson's edition of J. S. Mill's *Principles* offers the most pronounced worries since the texts are very complicated, the edition was prepared with such care, and Stigler made significant contributions to Mill's scholarship (Peart 2015).

What might help, in Stigler's account, is the authors' correspondence with their peers. The principle that nearer is clearer than the distant is an easy inference from Stigler's celebrated explication of search theory (Stigler 1961). If something puzzles us about Adam Smith's work, perhaps, it also puzzled those able to ask Adam Smith about it.

In the second part of the paper, we formulate Stigler's inconsistency reading as rooted in Smith's claim that individuals are persuaded to act contrary to their interests. Then we trace Smith's thoughts on persuasion across his texts. First, in *Theory of Moral Sentiments* Smith explains how "systems" are time economizing methods of persuasion. Next, in his lectures, Smith explains trade itself by persuasion. At this point Smith is perhaps the furthest from the neoclassical supposition that individuals know their interest prior to trade. There is no suggestion here that persuasion is used for deceit. In *Wealth of Nations*, by contrast, those who specialize in calculating their interests are able to take advantage of those who do not.

Toward a Theory of Editorship: Replication

One aspect of Stigler's career that is now largely forgotten is the thankless task of checking proposed standard editions of classical texts. Stigler's replication was quite extensive.[4] The few who thanked him, Piero Sraffa, John Robson, and Jacob Viner, might have been all who mattered to him. Stigler would have known the Smithian principle that praise from the praiseworthy is terribly important. Moreover, a reputation for scholarship with great care might have induced the editors of the Glasgow edition of Adam Smith's works to ask for his advice on a delicate matter. Stigler's candid answer proposed a modification of the edition, a correspondence volume. His recommendation, that Aaron Director assume responsibility for the edition, signifies the importance Stigler attached to the proposed volume.

We consider four instances in which Stigler attempted to replicate the editorial claims in collated editions: Sraffa's Ricardo, Guillebaud's Marshall, Robson's Mill, and the Glasgow Smith. We know the reaction of Sraffa and Robson. What Guillebaud might have thought is easy to imagine.

Sraffa's Ricardo. In his study of the first edition of Ricardo's *Principles*, Piero Sraffa noticed something odd about the printing. The issue is complicated so we quote his statement:

> This second case concerns the chapters 'Taxes on Raw Produce' and 'Taxes on Rent' which are respectively numbered VIII and VIII*, the asterisk appearing both in the chapter-heading and in the table of contents. Our suggestion is that these two at one time formed a single chapter (numbered VIII and entitled 'Taxes on Raw Produce') and that they were separated, not in the revision of the proofs, but at a much later stage—after the Index had been compiled and indeed after the book had been printed off: so that the pages affected had to be reprinted, and substituted by the binder in every one of the 750 copies of the edition. (Sraffa 1951, xxviii)

Sraffa adds a note that contains a test of his hypothesis:

This is the case in all the copies examined. It would be of interest if a copy were to be found in which the binder had failed to carry out the replacement. (Sraffa 1951, xxviii)

We reproduce a page from the volume 10 of the *Works* to explain what happened next. (Sraffa 1955, 403)

SUPPLEMENT TO VOLUME I

New Evidence on the Subdivision of Chapter VIII of the 'Principles' 1817

In the Introduction to the *Principles* (above, I, xxvi–xxx) the relation between the double-numbering of chapters in ed. 1 and the fact that three leaves are 'cancels' was discussed. It was suggested that, after the book had been printed off, Ricardo divided what had previously been Chapter VIII, Taxes on Raw Produce, into two chapters by making out of its last few pages a separate Chapter VIII*, Taxes on Rent; which involved the reprinting of those pages. It was also conjectured that the need to start the new chapter on a fresh page was met by pushing forward the existing matter into a half-empty page at the end of the original chapter.

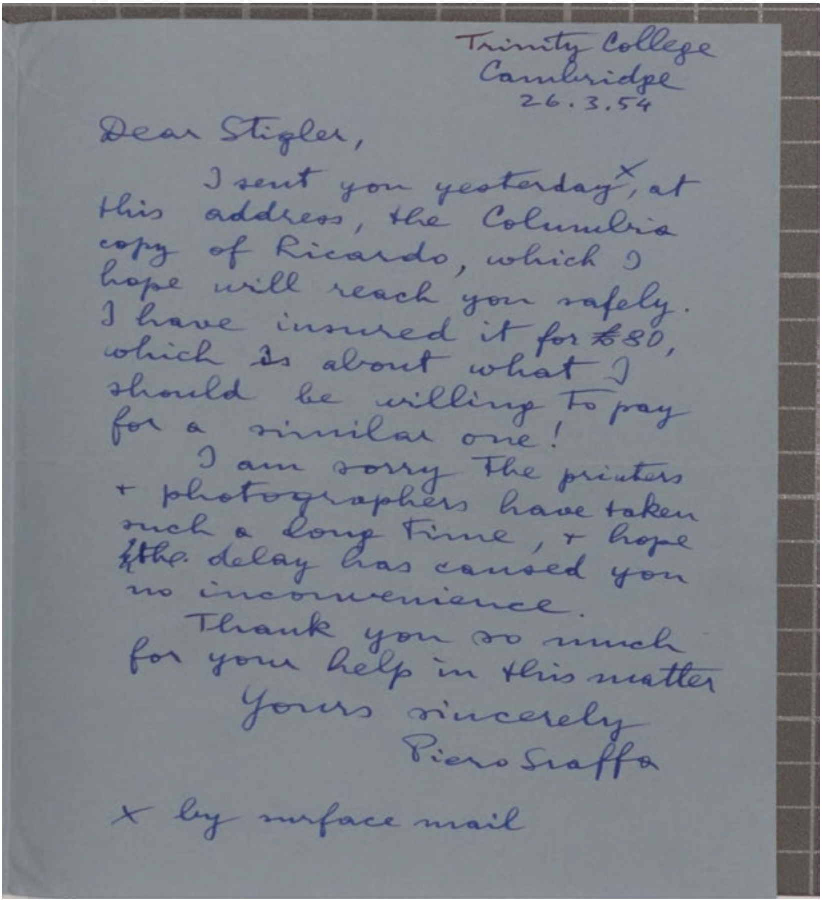
This operation required that the binder, when folding the printed sheet, should cut off three leaves (six pages) and substitute for them three newly printed ones. The possibility was mentioned that a copy might be found in which by an oversight the original leaves had not been replaced.

Such a copy has now come to light, and confirms the hypothesis as to the cause of the cancels and as to the way in which the subdivision of the chapter was carried out. This copy has been found by Professor George J. Stigler in the Library of Columbia University[1] and is referred to by him in a review-article in the *American Economic Review*, September 1953, p. 587 n.

In this copy the first two of the leaves that should have been cancelled (signatures P6 and P7, being pages 219–222) have been left in the original position, although slit and torn by the printer

[1] Pressmark D 110 R 361. It was acquired by the Columbia Library in 1931 with the books of the economist Henry R. Seager. Acknowledgement is due to Professor Stigler for his kindness in making this copy available to the editor.

Stigler's archives preserve the note of thanks from Sraffa. Sraffa's decision of how much to pay for insuring the Columbia University copy puzzled us until Steve Stigler explained that Columbia University Library would not send the copy to Sraffa by interlibrary loan. Accordingly, his father simply checked the copy out on his faculty card and mailed it to Cambridge. If it had to be replaced, there was no reason to believe that the Columbia Library would understand how valuable this possibly unique copy was so insuring it for the price of an ordinary first edition seemed appropriate.



Trinity College
Cambridge
26.3.54

Dear Stigler,

I sent you yesterday×, at this address, the Columbia copy of Ricardo, which I hope will reach you safely. I have insured it for £80, which is about what I should be willing to pay for a similar one!

I am sorry the printers + photographers have taken such a long time, + hope the delay has caused you no inconvenience.

Thank you so much for your help in this matter

Yours sincerely
Piero Sraffa

× by surface mail

Guillebaud's Marshall. Stigler opens his review (Stigler 1962, 282) with a brief judgment of the collated editions of economic classics. Cannan's edition of Adam Smith's *Wealth of Nations* (definitive), Ashley's edition of Mill's *Principl*es (dubious), Sraffa's edition of all Ricardo (superb), Jaffé's edition of Walras's *Elements* (only worthy of compliments), Guillebaud's edition, in the works for almost thirty years (disappointing). Spot replication found too many important mistakes. Perhaps more serious was the failure of the editor to replicate the author's claims:

> Guillebaud stops short, however, with the comparison of editions, and does not even go outside the *Principles* to compare Marshall's quotations and references with the original passages. This decision drastically reduces the value of the new edition. Marshall lived in an age and place of in-formal scholarship, and his references are notoriously slovenly, so it is especially regrettable that Guillebaud ignored the customary task of an editor. (Stigler 1962, 283)[5]

The Guillebaud review marked the beginning of Stigler's discontent with collated editions. He came to believe that Sraffa would have been better served had he included the chapter Ricardo changed most dramatically in an appendix (Stigler 1962, 285).

The Toronto Mill. After a short introduction, Stigler offered summary judgment of the Toronto edition of Mill's *Principles of Political Economy*:

> I gladly supply a truthful sentence for advertisements: Every serious student of the history of economics must have this edition—a nice market of almost a dozen in the United States! (Stigler 1966, 90)

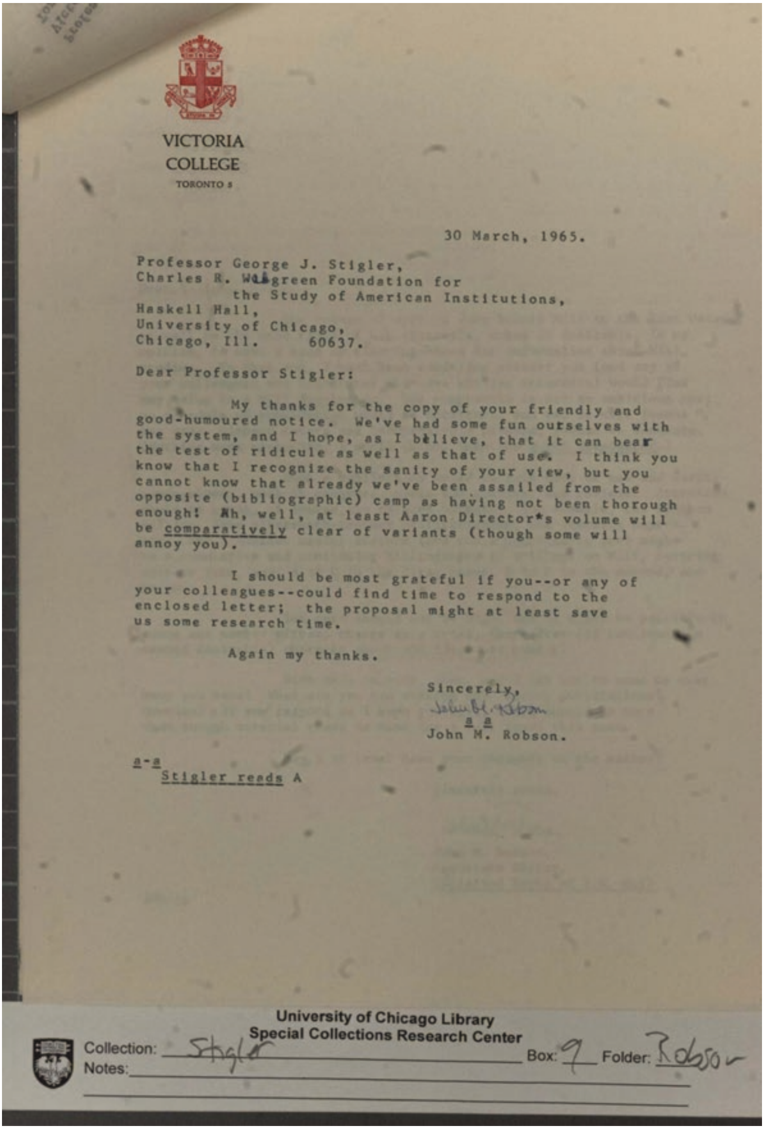
Then Stigler provided a demonstration of how useful it was to be close to the editors of the *Journal of Political Economy.* One is able to ask for typographical work that might be out of bounds for ordinary contributors. First, from the text of the review in which the distinction between logical and psychological barriers is pressed:

> The textual editor, [a]Professor[a] John Robson, has performed his work with [b]exorbitant care[b] and a tenacity that the RCMP would do well to [c]emulate.[c] Changes between editions of a typographical sort are [d]not reported[d], but some 3,500 other changes—varying from a word to half a dozen pages—are presented by an apparatus of footnotes such as I am using [e]here[e]. [g]In [f]a few[f] cases of very extensive rewriting, the earlier versions are presented in appendixes.[g]
>
> The density of my illustrative footnote references runs rather heavier than the average in Mill (about three per page), but they convey the atmosphere. The reader will have to decide whether he will find these references burdensome in ordinary reading; this is their cost. The specialist will have to decide whether he can reconstruct the evolution of the text—not logically, for Robson *is* careful—but psychologically. The annotations of Mill's fourth proposition on capital illustrate the problem (I, 87):

Then from the notes:

> a—a, +3 (The material between the letters was inserted in the third draft.)
>
> b—b, 1 meticulous scholarship (So in the first draft.)
>
> c—c, 1, 2 rival
>
> d—d, 1, 2 omitted
>
> e—e, 1 misspelled hear
>
> f—f, 1 some
>
> g—g, +2, 3

This prompted a note from John Robson which somehow has escaped publication. One notes that Arron Director is scheduled to be the editor of Mill's economic essays. The importance of this will be clear later.



VICTORIA
COLLEGE
TORONTO 5

30 March, 1965.

Professor George J. Stigler,
Charles R. Walgreen Foundation for
the Study of American Institutions,
Haskell Hall,
University of Chicago,
Chicago, Ill. 60637.

Dear Professor Stigler:

My thanks for the copy of your friendly and good-humoured notice. We've had some fun ourselves with the system, and I hope, as I believe, that it can bear the test of ridicule as well as that of use. I think you know that I recognize the sanity of your view, but you cannot know that already we've been assailed from the opposite (bibliographic) camp as having not been thorough enough! Ah, well, at least Aaron Director's volume will be comparatively clear of variants (though some will annoy you).

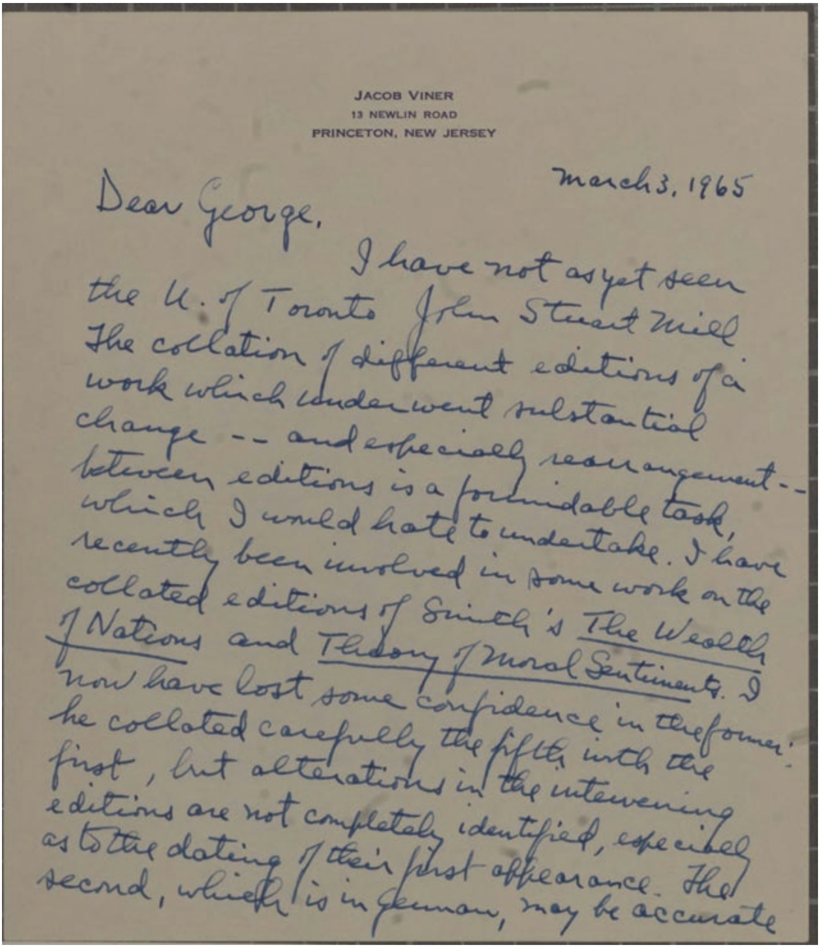
I should be most grateful if you--or any of your colleagues--could find time to respond to the enclosed letter; the proposal might at least save us some research time.

Again my thanks.

Sincerely,
John M. Robson
a a
John M. Robson.

a-a
Stigler reads A

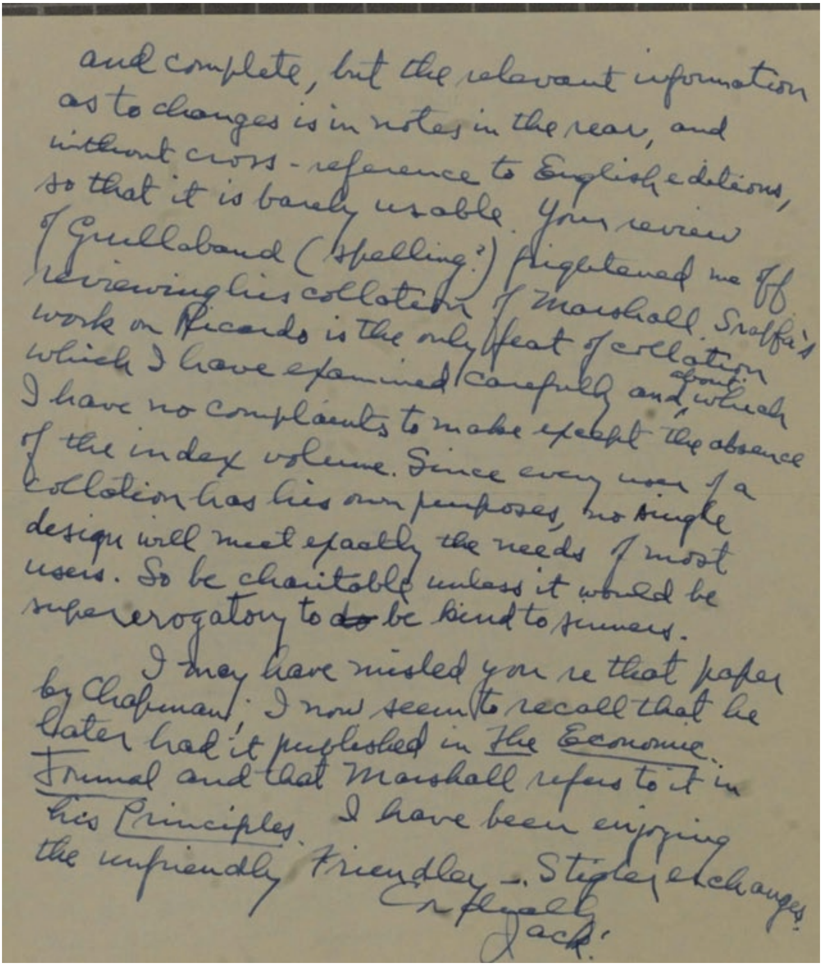
The two reviews seemed to have prompted a note about collated editions from Stigler's old teacher, Jacob Viner. Perhaps Guillebaud was fortunate to escape a Viner review. He would have remembered that even Keynes responded to Viner.


JACOB VINER
13 NEWLIN ROAD
PRINCETON, NEW JERSEY

March 3, 1965

Dear George,

I have not as yet seen the U. of Toronto John Stuart Mill. The collation of different editions of a work which underwent substantial change -- and especially rearrangement -- between editions is a formidable task, which I would hate to undertake. I have recently been involved in some work on the collated editions of Smith's <u>The Wealth of Nations</u> and <u>Theory of Moral Sentiments</u>. I now have lost some confidence in the former: he collated carefully the fifth with the first, but alterations in the intervening editions are not completely identified, especially as to the dating of their first appearance. The second, which is in German, may be accurate



and complete, but the relevant information as to changes is in notes in the rear, and without cross-reference to English editions, so that it is barely usable. Your review of Guillebaud (spelling?) frightened me off reviewing his collation of Marshall. Sraffa's work on Ricardo is the only feat of collation which I have examined carefully and about which I have no complaints to make except the absence of the index volume. Since every user of a collation has his own purposes, no single design will meet exactly the needs of most users. So be charitable unless it would be supererogatory to ~~do~~ be kind to sinners.

I may have misled you re that paper by Chapman. I now seem to recall that he later had it published in *The Economic Journal* and that Marshall refers to it in his *Principles*. I have been enjoying the unfriendly Friedman–Stigler exchanges.

Cordially
Jack.

Glasgow Smith. As this seems to be the last of the collated editions that Stigler reviewed, we quote the opening judgment as well as his considered opinion as to what service any acceptable edition of a classical author ought to provide:

> The University of Glasgow, where Adam Smith was Professor of Logic and then Professor of Moral Philosophy, is commemorating the bicentenary of his greatest work by bringing out a new edition of all his works, as

> well as a new biography and a volume of correspondence. This is the first title to appear, and it has been edited with scrupulous care. It will be the standard source for all historians of economics.
>
> There is a larger audience, however, and the question can be raised: what does this general professional and academic audience wish in the presentation of a great classic? The average professor or student will want a pleasant, readable text plus a variety of editorial aids:
>
> 1. A report of changes among editions. These changes indicate the evolution of the author's own views or modes of expression. It is not probable, however, that trivial changes are worth the distraction they cost. The common reply is that the editor dare not decide which changes are trivial. We deny that this decision is even debatable in any large fraction of cases.
> 2. Improve the author's cross-references. The author, and in particular Smith, will often discuss a subject several times, and it is useful, whether by footnote or index, to catch other significant references.
> 3. Indicate errors in references or quotations. The presence and magnitude of these errors help the reader to judge the writer's scholarship, and sometimes better to understand the views he is discussing.
> 4. Provide explanations of facts no longer possessed by the reader, e.g., obsolete weights and measures.
>
> The present editors provide all four of these services, although not systematically with respect to obsolete units (e.g., shekels of silver, I, 41). The cross-references are splendid, and the quotations are consistently verified. The only lament, at this level, is the heavy reporting of minor changes among editions. In a sample of 77, textual changes, at most four were of substantive significance: 6 were changes of tense in a verb, 2 a change in paragraphing, 7 were changes in spelling, etc. These minor changes average about one per page.

The psychological barrier that Stigler noted in his review of Robson's Mill now has a name: distraction.

The Importance of Correspondence

If the barrier between the author of a text and the reader is a function of distance in time and space, then one great service an editor provides to the reader is to report the author's correspondence with his peers. Stigler singles out this aspect of Sraffa's Ricardo in his review. He points to a large number of interpretative myths that were shattered by the correspondence in the Sraffa edition.[6] In this context we report a part of his correspondence with Ronald Meek about the new edition of Smith's works.

Meek reported a conversation with Lord Robbins about the plans for a great Smith edition. Robbins recommended that William Letwin, Aaron Director, and Stigler himself should be involved. Meek asked for Stigler's judgment of Letwin and Director. Stigler's response is a testimony to the futility of reform (although he could not know when he wrote that Director's edition of Mill's economic essays would never be produced!). He emphasized the importance of Smith's correspondence by proposing it should be published as a separate volume, instead of being lumped in a miscellanea volume. That, of course, was the solution the Glasgow editors adopted. That Stigler would recommend Director to edit the correspondence volume suggests he thought the volume would be important. As it turned out, the *Correspondence* volume (Smith 1977) without Director's hand was a disaster. The review by M. A. Stewart is devastating (Stewart 1979). Oxford evidently agreed and soon after published a revised edition. Those who know the Glasgow edition only from the Liberty Fund paperback (Smith 1987) will not have seen the first edition of the *Correspondence* volume.

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EXT. 11

THE UNIVERSITY
GLASGOW, W.2

DEPARTMENT OF POLITICAL ECONOMY

28th December, 1961.

Professor George J. Stigler,
Charles R. Walgreen Foundation for the Study of American Institutions,
Haskell Hall,
University of Chicago,
Chicago 37,
Illinois,
U.S.A.

Dear Professor Stigler,

I am writing to you in my capacity as Secretary of the Adam Smith Bicentenary Committee which has recently been set up by the Court of Glasgow University. I have before me your very kind letter of 6th December to Professor Macfie, which encourages me to write to you for information on several specific points.

But first let me give you a rather more detailed account of our Committee and its projects. The Court's representatives on the Committee are the Principal, Sir William Robieson and Professor Fordyce; and the other members are Professor Macfie, Professor Maclagan, Professor Checkland, Professor Wilson, Professor Raphael, Professor Robertson and myself. Sir William Robieson is Chairman of the Committee, and I am its Secretary.

The Committee's main job is to prepare for the Bicentenary of the Wealth of Nations in 1976. We feel that Glasgow's major contribution to the Bicentenary should be the publication of a collected edition of Adam Smith's works, and we are at present engaged in a search for suitable editors, negotiations with the Oxford University Press regarding publication, and similar matters. A Board of Provisional Editors has been set up, consisting of Professor Macfie, Professor Checkland and myself. The job of editing the Theory of Moral Sentiments has been taken on by Professor Macfie, and the Wealth of Nations is to be tackled jointly by Professor Checkland and myself. Otherwise no definite arrangements have yet been made regarding editors.

The/

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The Collected Works which we envisage will comprise six volumes, roughly as follows:

(1) The Theory of Moral Sentiments

(2) The Glasgow Lectures and associated documents

(3) & (4) The Wealth of Nations

(5) Essays, Miscellaneous Pieces and Correspondence

(6) Biographical and Bibliographical

We also propose to publish a commemorative volume of essays on Smith and his work, to be written by British and overseas scholars. So far as the timing of the volumes is concerned, this naturally can't be settled yet, but we anticipate that the Moral Sentiments will appear first, within perhaps five or six years (since Professor Macfie has already been working on the book for some time), and that the Wealth of Nations will appear last, in 1976 (partly because of the magnitude of the task and partly because it would seem appropriate for the Wealth of Nations to appear in the Bicentenary year). The other volumes will I imagine appear at various dates in between these.

You will appreciate that the project is rather a considerable one, and we have no hope of doing a decent job unless we can get the co-operation of large numbers of people from several countries and from several disciplines. For this reason we are trying to establish contact with as many scholars who might be interested in the project as we possibly can, with a view to getting their comments and criticisms and wherever possible their practical assistance.

We very much hope that American scholars will play a prominent part in the preparation of the volumes, and we are naturally anxious to obtain as soon as possible some idea of the panel of American experts upon whose interest and ability we may be able to draw. I discussed this matter a month or two ago with Lord Robbins, who mentioned the names of three American scholars in particular who would probably be interested in the project - yourself, Professor Director and William Letwin of M.I.T.

So far as Mr. Letwin is concerned, Lord Robbins thought he was at the moment engaged on two Smith-projects - a new edition of the Moral Sentiments, and a new biography of Smith. We are particularly interested in the biography, since the last volume of our edition is to be "Biographical and Bibliographical". We haven't yet worked out the detailed contents of this volume, and will probably not be able to do so until we come across a suitable editor. But we should very much like to find someone who would be prepared to work on a new biography - relating especially to the early and late periods of Smith's life which are not

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covered in W.R. Scott's book. I have written to Letwin asking him to let us know the sort of work he is contemplating, but I haven't yet had any reply from him. Our main difficulty, of course, is that we know very little about Letwin over here - all we have to go on, in fact, is his article on North in Economica and a strong recommendation from Lord Robbins. It would help us very much if you could let us know frankly just what you think of Letwin and his work. Do you think he would be a suitable person to approach regarding the editing of the last volume in our set, or at any rate the biographical part of it?

So far as Professor Director is concerned, we are in a somewhat similar position. We are particularly interested in him as a possible editor of Smith's correspondence, in which, as appears from your letter, Professor Director has a special interest. We are indeed very concerned to collect and publish Smith's correspondence, which at the moment is scattered all over the place. At present, as you can see from the plan of the volumes set out above, we are proposing to include the correspondence along with the Essays and a few miscellaneous pieces such as the letter on Hume, but these arrangements are very provisional and may well be modified. May I ask you, then, whether Professor Director might be interested, were we to approach him formally, in editing the correspondence? It would help us very greatly if you were able to tell us something about Professor Director and his work, so that our Committee will have the fullest possible information before it when it considers the question of editorship.

May I conclude by expressing the great pleasure with which your letter was received by the Committee? We were delighted to know that you and your colleagues were prepared to co-operate with us, and I am sure that there will be many ways in which you can give us practical help. Any advice which you care to offer us will be most gratefully received.

Yours sincerely,

Ronald L. Meek

Ronald L. Meek.

file: Glasgow, U of
(cc: file Letwin
file Director)

, U.S.A.
January 29, 1962

Professor Ronald L. Meek
Department of Political Economy
The University
Glasgow, W.2, Scotland

Dear Meek:

I am not at all certain that I can answer your questions about Letwin. He took his degree here before my time, and the opinions vary greatly. The men in the Committee on Social Thought, Hayek and Nef, think very highly of him, but the men in the law school, who played a major part in supervising his work on English common law, are extremely critical of his analytical abilities. With this stand-off, one must form his judgment chiefly on Letwin's recent work, which has not been extensive. I am not enthusiastic; Hayek will, I believe, offer a higher estimate.

My judgment of Aaron Director is to be treated critically for the opposite reason: he is a close friend, and I probably know him better than anyone else. He is a broadly erudite man, a thorough worker, well-versed in the history of economics possessing an excellent analytical mind, and is, I may add, a gentleman. One might expect, with these qualities, that he would be internationally famous. He would be if he could bring himself to write, (as it is, some of his remarkable work in industrial organization has been published by his students). I have weakened this characteristic, -- fttends are remorseless "reformers",-- and he is bringing out a volume of John Stuatt Mill's essays in econmmics.

If the life of Smith is to be a new edition of Rae, with additions, as I think it should be, Letwin may be adequate (Hayek thinks so), but I myself would think that an English scholar would be much better located for the research.

The oorrespondence should be a most valuable addition to your series, and depending on the supply it may deserve much more than a part of a volume. I think Director would be admirable on this -- he is resourceful, meticulously careful, and would exercise the same kind of unobtrusive skill as Sraffa in the editorial work. I have not idea whether he is interested in doing it -- he happens to be away for a month.

I am delighted to know that Smith is in such good hands.

Sincerely,

George J. Stigler

GJS:pma

The importance of correspondence comes into play again when James Buchanan wrote to Stigler about the inconsistency between Smith's view of private and public activity (August 8, 1975). Buchanan accepted Stigler's view of the published work, but he pointed out that John Rae had reprinted a letter from Smith that offered a sophisticated interpretation about incentives and public policy.[7]

The Problem of Consistency

Stigler saved a paper—"The theory of value from Adam Smith to Stanley Jevons"—that he wrote for a 1932 class at Northwestern. In it, he focused on the problem of value and traced how Smith's doctrine developed over the next century. The paper reveals his concern with consistency, and how inconsistencies would be detected and eradicated, that would characterize so much of his later work. Stigler distinguished between Smith's causal theory of value and Smith's account of the measurement of value.

From this Smith steps into his theory of (exchange) values

> The value of any commodity to the person who possesses it and who does not mean to consume it himself, has to exchange it for other commodities, is equal to the quantity of labor which it enables him to purchase or command. Labor is the real measure of the exchange value of all commodities.

This is obviously a labor command or measurement theory, yet he continues along different lines:

> The real price of everything, what everything really costs to the man who wants to acquire it, is the toil and trouble of acquiring it. … Labor is the first price, the original purchase money that was paid for all things.

Smith in this latter sense considers labor as the cause of value. This failure to distinguish between labor as a cause and as a measure of value is due to his belief, perhaps, that both are controlled in like fashion and decree by the same cause, for

> …the quantity of labor commonly employed in acquiring or producing a commodity, is the only circumstance which can regulate the quantity of labor which it ought commonly to purchase, command, or exchange for (4)

Of the two concepts, cause and measure, the former is undoubtedly the more fundamental, for it is the labor (Stigler 1932, 11).

> I have pointed out that Smith often confused the cause and measure of value—this latter phase deserves here the added emphasis it drew from Smith. His major theme has already been stated—the measure of value of a commodity is the quantity of labor it can command. Labor is the fixed, the invariable unit, he declared for the laborer's efforts alone are fixed, all other things, including the necessities he purchases with his labor, are variable (Stigler 1932, 12–13).

In this early essay, we see Ricardo read as The Commentator on the *Wealth of Nations*, a theme to which Stigler would return decades later[8]:

> Ricardo's work is often difficult to understand, much being stated too concisely, and important thoughts are often merely mentioned and left unrepeated. Ricardo was profoundly influenced by the "Wealth of Nations", being Adam Smith's foremost disciple and acknowledged commentator. (Stigler 1932, 16)

Forty-four years later, Stigler considered Smith "successes and failures" and he returned to the question of value. Now, he pointed to an issue he regarded as unresolved. In a passage which Stigler did not quote but only referred to in 1932, Smith claimed that there is a fixed psychic burden of time forgone.

> A very different error, and possibly not an error at all, is Smith's measure of value—which came from the same source as that which may have led him to overvalue agriculture. Smith was acutely sensitive to the instability of monetary measures of value, and an appreciable fraction of the *Wealth*

> *of Nations* is devoted to the chronicle of currency debasement and inflation. He proposes as the ultimate measure of value the disutility of 1 hour of ordinary labor:
>
> Equal quantities of labour, at all times and places, may be said to be of equal value to the labourer. In his ordinary state of health, strength, and spirits; in the ordinary degree of his skill and dexterity, he must always lay down the same portion of his ease, his liberty, and his happiness. The price which he pays must always be the same, whatever may be the quantity of goods which he receives in return for it. … Labour alone, therefore, never varying in its own value, is alone the ultimate and real standard by which the value of all commodities can at all times and places be estimated and compared. [1976, 1: 50–51]
>
> Smith's error, if indeed it is an error, was to assume that the psychological cost of performing 1 hour of labor is more stable, in its significance to a person, than the psychological pleasure from the consumption of some bundle of goods (Stigler 1976, 1206–1207).

Such a claim is not part of modern economics, and Stigler provides reasons not to believe it, but he allows for the possibility that Smith is not wrong. Psychological claims are outside the scope of economics.

The Problem of "Mistakes"

When Stigler collected his papers on economic regulation he offered his considered view on the problem of policy mistakes—"To say that such policies are mistaken is to say that one cannot explain them" (Stigler 1975, x). This is of course the same problem he worried about in a textual context. When we find a "mistake" in the classical authors, is it their fault or is it ours? Concern that the fault might be ours explains Stigler's concerns both for psychological barriers in standard editions that stand in the way of assigning blame to "mistakes" and the importance of reading how an author responded to those near in time and place.

Stigler's eventual paper for the Glasgow celebration—"Adam Smith's Travels on the Ship of State"—considered many instances in which Smith's explanations do not fit neatly within a paradigm of self-interested choice.[9] Of Stigler's many examples, we address the failure of an actor to predict the consequences of the choice, instances when the means selected do not attain the ends desired. It seems unproblematic to describe this action as a "mistake." In this instance, Stigler reads Smith as offering a paternalistic view of economic policy.[10] Contrary to Stigler's reading, we suggest that Smith *did* offer a model consistent with his general approach in which a mistaken choice is to be distinguished from a successful choice.[11] What Stigler saw in *TMS* as foreign to the economics he taught late in the twentieth century, is central to our reconstruction.

Where does the disconnect between chosen means and desired ends enter into the discussion of policy? First, Smith explains the role of system in economic policy in *TMS*. Next, in his lectures he explains that trade is rooted in persuasion. In the lectures, there is no reason to doubt that the persuasion is truthful and the trade is indeed beneficial. Then, as noted above, Smith argues in *WN* that persuasion about public policy is rooted in deceit.

For Smith, the role of system in public policy speaks directly to the confusion of ends and means. Smith writes about system in the first (1759) edition of *TMS*:

> The same principle, the same love of system, the same regard to the beauty of order, of art and contrivance, frequently serves to recommend those institutions which tend to promote the public welfare. When a patriot exerts himself for the improvement of any part of the public police, his conduct does not always arise from pure sympathy with the happiness of those who are to reap the benefit of it. … The perfection of police, the extension of trade and manufactures, are noble magnificent objects. The contemplation of them pleases us, and we are interested in whatever can tend to advance them. They make part of the great system of government, and the wheels of the political machine seem to move with more harmony and ease by means of them. We take pleasure in beholding the perfection of so beautiful and grand a system, and we are uneasy till we remove any obstruction that can in the least disturb or encumber the regularity of its motions. (TMS IV.i.11)

System is, in Smith's formulation, a heuristic to interpret distant objects, whether it be planetary bodies or policy guides.[12] Smith's celebrated words on the dangers of the man of system would need to wait until the final (1790) edition but the confounding of ends and means is there in the beginning:

> All constitutions of government, however, are valued only in proportion as they tend to promote the happiness of those who live under them. This is their sole use and end. From a certain spirit of system, however, from a certain love of art and contrivance, we sometimes seem to value the means more than the end, and to be eager to promote the happiness of our fellow-creatures, rather from a view to perfect and improve a certain beautiful and orderly system, than from any immediate sense or feeling of what they either suffer or enjoy. (TMS IV.i.11)

As noted above, Smith's lectured during the time between *TMS* and *WN*. From the surviving students' notes we know that he addressed what he later referred to as the "instinct" to "truck and barter" in *WN*. In the lectures it is clear that trade is rooted in persuasion. The role of persuasion is to make the case that it is to the trading partner's interest to make the exchange. Unlike his neoclassical heirs, Smith does not assume that traders know how to obtain theirs. Thus, language is important:

> If we should enquire into the principle in the human mind on which this disposition of trucking is founded, it is clearly the naturall inclination every one has to persuade. *The offering of a shilling, which to us appears to have so plain and simple a meaning, is in reality offering an argument to persuade one to do so and so as it is for his interest.* Men always endeavour to persuade others to be of their opinion even when the matter is of no consequence to them. … In this manner they acquire a certain dexterity and adress in managing their affairs, or in other words in managing of men; and this is altogether the practise of every man in the most ordinary affairs.–This being the constant employment or trade of every man, in the same manner as the artizans invent simple methods of doing their work, so will each one here endeavors to do this work in the simplest manner. That is bartering, by which they adress themselves to the self-interest of

> the person and seldom fail immediately to gain their end. The brutes have no notion of this; the dogs, as I mentiond, by having the same object in their view sometimes unite their labours, but never from contract. (LJ 352) [emphasis added]

For Smith, persuasion is akin to dexterity in that one acquires both by practice. He stressed the role of "dexterity" in the critical example of policy "failure" in *WN*. Stigler knew the kindred *WN* passage (WN 1.ii.2) well. Indeed, he used it in class (1966) to make the point that for Smith language drives trade. With a note of regret, he added that economists had learned something in 190 years, Smith here is wrong. Agents with well-defined preferences in an Edgeworth box trade without language. That, of course, was before the discovery that humans are not the only agents with well-formed preferences. Rats have them too and, although experimentalists have found Giffen segments, they cannot demonstrate that rats trade (Battalio et al. 1991). By the time this was clear (Levy 1992), Stigler had passed away.

Conclusion: An Implication of a Theory of "Mistakes"

Stigler's central empirical challenge for Smith scholarship is what distinguishes a "mistake" in Smith from a success. To answer this challenge, consider Smith's explanation for why merchants and masters exploit the other orders in society. His extended polemic against the mercantile *system* suggests how important this is for Smith. He tells us that the masters have more practice calculating their self-interest than other orders in society. The reader might be presumed to remember that specialization improves productivity because of the increase of dexterity that repetition provides

> Merchants and master manufacturers are, in this order, the two classes of people who commonly employ the largest capitals, and who by their wealth draw to themselves the greatest share of the publick consideration. As during their whole lives they are engaged in plans and projects, they

> have frequently more acuteness of understanding than the greater part of country gentlemen. As their thoughts, however, are commonly exercised rather about the interest of their own particular branch of business, than about that of the society, their judgment, even when given with the greatest candour (which it has not been upon every occasion) is much more to be depended upon with regard to the former of those two objects, than with regard to the latter. Their superiority over the country gentleman is, not so much in their knowledge of the publick interest, as in their having a better knowledge of their own interest than he has of his. (WN I.i.5)

Then the link between trade and persuasion enters. Here, unlike the passage in his lectures, persuasion is for the purpose of deceit:

> It is by this superior knowledge of their own interest that they have frequently imposed upon his generosity, and persuaded him to give up both his own interest and that of the publick, from a very simple but honest conviction, that their interest, and not his, was the interest of the publick. (WN I.xi.p)

For Smith those who are practiced in knowing how best to obtain their wants are practices in persuasion, and that persuasion takes on the form of system. If everyone costlessly knew their interests, then their choices would have been mistaken just as if information were costless, everyone would buy at the minimum price. Since neither information nor knowledge is obtained costlessly, however, such choices are simply the result of scarcity. Stigler did not see this in Smith, because the route Smith traveled between *TMS* and *WN* was not clear in the editions Stigler depended upon. His theory of editorship would have produced an edition that would have filled the missing steps.

Notes

1. The classroom version was somewhat sharper when he proposed a student edition of Ricardo's *Principles* with the taxation chapters blank save for the topic to be addressed; hence, a proposed solution was produced (Levy 1976). If one takes the view that individuals behave

in an optimal fashion, mistakes would seem unlikely. This is the basis of Stigler's position that it is not sensible to believe that single individual, whether it be Charles Babbage proposing the computer or a graduate student proposing a novel interpretation of standard texts, could be right and the profession wrong. This would be plausible if the profession worked independently but if economists take Stigler's doctrine to heart and depend upon the consensus of the profession not being in common error—they fail to work independently. They will simply repeat what they read (Levy and Peart 2008). Stigler's lectures, something he evidently never published nor worked from detailed notes, come into play in section 'The Problem of "Mistakes"' on Smith's "mistakes."

2. Stigler (1949, 46): "On the other side, I should be inclined to argue that most of our modern economics of price was deemed by the classical economists to be pedestrian stuff, inappropriate to a treatise. The problems of the firm and industry are important, they would say—no doubt of it. But they are tolerably well handled by the journalist and the businessman. For us, they would say, there are greater problems: the true basis of value, the laws of distribution of national income, the foundations of national prosperity, the growth and decline of nations. Why should we fuss over the minutiae of a firm's costs, or over its timid elements of monopoly? These are proper subjects for later, and lesser, men. As for us, we seek the great eternal truths and the laws of history. Pray leave us to our grand pursuit, in which, perhaps, we shall succeed."
3. Stigler found errors in Sraffa's edition, but none in Robson's!
4. Stigler tells the reader that he checked about 10% of Sraffa's printing of the first edition of Ricardo's *Principles*. The result: "The impression of precision is well-founded; I found only one large error. Any economist who does not appreciate how extraordinary such accuracy is, should spend an hour or two checking published quotations" (Stigler 1953, 587). Guillebaud's edition of Alfred Marshall's *Principles* was so filled with inaccuracies that Stigler could simply look into the sections on which he had worked previously (Stigler 1962).
5. Edwin Cannan pointed to a particular example: "Marshall in a fine flight of imagination says of Adam Smith that 'after insisting on the advantages of division of labour and pointing out how they render it possible for increased numbers to live in comfort on a limited territory, he argued that the pressure of population on the means of subsistence

tends to weed out those races who through want of organisation or any other cause are unable to turn to the best account the advantages of the place in which they live' (*Principles*, ed. 1, p. 300, ed. 8, p. 240). Future editors of Marshall will be puzzled to find this argument in the *Wealth of Nations*" (Cannan 1929, 89).

6. Perhaps, in Stigler's view, the most important part of Sraffa's newly published correspondence was that between Ricardo and James Mill. The context is a famous missed opportunity. Alexander Bain lamented that he started his biography of James Mill so late. He tells us that he thought that John Stuart Mill's autobiography would have an extensive discussion of his father's life so he stopped working on his own biography. It was only after the publication that Bain realized how little of James's life was discussed in *Autobiography.* Restarting so late was costly: "Time had already been lost; those that, from personal knowledge, could have given information as to Mill's early history, were nearly all dead. Several invaluable collections of letters have been destroyed. Instead of making a selection from a copious mass of documents, I have been obliged to use almost everything that came into my hands" (Bain 1882, vii).
7. Buchanan (1976): "a letter to Henry Dundas, Smith supports free trade for the Irish, but follows this up with the statement below: 'Whatever they (the Irish) may demand, our manufacturers, unless the leading and principal man among them are properly dealt with beforehand, will probably oppose it. That they may be so dealt with I know from experience, and that it may be done at little expense and no great trouble. I could even point to some persons who, I think, are fit and likely to deal with them successfully for this purpose.' The letter is cited in full in John Rae's *Life*, and Rae seems clear as to his own interpretation. With reference to the statement above, Rae says: 'I cannot explain the allusion in the closing parts of the letter to the writer's personal experience of the ease with which the opposition of manufacturers to proposed measures of public policy could be averted by sagacious management and a little expenditure of money. Nor can I say what persons he had in view to recommend as likely to do this work successfully; but his advice seems to imply that he agreed with the political maxim that the opposition of the pocket is best met through the pocket.' The passages occur on p. 355 of the Augustus Kelley Reprint." Buchanan's widening interest in Smith is detailed in Levy and Peart (2019). It is insufficiently

noticed that Buchanan came to identify the teachings of John Rawls with those of Adam Smith.

8. Stigler (1958, 358): "Ricardo's formulation of his theory of value was much influenced by his desire to correct what he believed to be the major errors of Adam Smith's theory."
9. His first proposed paper was "Adam Smith's use of empirical evidence to support theoretical positions." Judging by the working table of contents, several papers were proposed but not published: "Adam Smith's growth theory" (J. R. Hicks); "The Rousseau, Smith Marx linkage in the theory of alienation arising out of capitalism and division of labor" (Jacob Viner); "A sketch of the economics of Theory of Moral Sentiments" (F. A. Hayek); and "Adam Smith's relation to Ricardo" (M. H. Dobb).
10. Stigler (1971, 272): "In general, however, Smith's attitude toward political behavior was not dissimilar to that of a parent toward a child: the child was often mistaken and sometimes perverse, but normally it would improve in conduct if properly instructed."
11. Stigler (1971, 277): "No principle is apparent by which one can distinguish these failures from the many decisions which effectively advance these various persons' self-interests: …."
12. This argument seems to be an extension of George Berkeley's theory of vision, something Smith explicitly avows (TMS III.3.3)—we have to learn to perceive distance, to distinguish between smaller things close to us and bigger thing at a distance.

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Stigler on Ricardo

Heinz D. Kurz

Introduction

Writing on Stigler on Ricardo exposes one to a formidable difficulty. In the preface to the *Principles of Political Economy, and Taxation*, first published in 1817, Ricardo famously identified the "principal problem in Political Economy" to consist in unravelling the "laws" that regulate the distribution of the product between capitalists, workers and landowners in conditions in which capital accumulates, the population grows, the scarcity of some natural resources increases and there is technical progress (*Works* I: 5).[1] Eventually, after long debates with Thomas Robert Malthus in particular, Ricardo felt that he had elaborated "a very consistent theory" (*Works* VII: 246). Then along comes Stigler generations later who in his treatise *Production and Distribution Theories* contends boldly: "In 1870 there was no *theory* of distribution" (1941: 2; emphasis in the original).[2]

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Readers will rub their eyes. Stigler supports his claim in terms of the following assertion: "Most English economists after Smith devoted separate chapters to rent, wages, and profits, but without important exception such chapters were *only descriptive* of the returns to the three most important social classes of contemporary England" (1941: 2; emphasis added). He goes on: "This type of analysis may have had its uses in the England of Ricardo and Mill, but its analytical shortcomings are obvious. Extended criticism is unnecessary at this point[3]; the fundamental defect was clearly *the failure to develop a theory of the prices of productive services*" (1941: 3; emphasis added). Such a theory required solving the (in)famous imputation problem (*Zurechnungsproblem*). Stigler (1941: 156) could therefore couch his criticism of the classical authors also in the following terms: "Smith and his followers never confronted the problem of how a given product may be imputed to the resources which cooperate in its production nor did they consider distribution as a value problem or discuss the pricing of productive services."[4] This shows neatly that Stigler assesses the contributions of the classical economists strictly in terms of marginalist theory. It also explains why he was of the opinion that "The branch of economics which was in most urgent need of reformulation was, in fact, distribution" (Stigler 1941: 2). In Stigler's view marginal productivity theory filled the lacuna he contended to have discerned in the classical authors.

How can one possibly maintain that one of the most celebrated heroes of political economy failed to produce precisely what he explicitly set out to produce—a theory of distribution? Is it because Stigler and Ricardo attribute vastly different meanings to the term "theory"? Is it because one of them requires an economic theory to be presented in mathematical form, whereas the other doesn't?[5] Or is it because one of them defines a *theory* of distribution in terms of its particular content, which differs from the content the other one had delivered? In my final judgement, which is supported by what we have just heard and what we are still going to hear in the sequel of this paper, it was first and foremost the issue of content, and no fundamental differences about what a theory is and whether it has to be formalized.[6] In fact, when reading Stigler's works on Ricardo I could not help thinking that he did not really mean what he had written as a young (and perhaps not very well informed) Ph.D. student. Ricardo clearly *had* a theory of distribution. It may have been less than perfect, incomplete, insufficiently general and so on, but a theory it definitely is. Stigler himself comes close to admitting this in the concluding section of his main work on Ricardo's theory of value and distribution. There he writes:

> Ricardo, with his great power of abstraction and synthesis, was a master-analyst. Population, natural resources, capital accumulation, and the distribution of income – these were woven into a sweeping theoretical system [sic!]. Measured by the significance of the variables and the manageability of the system, he fashioned what is probably the most impressive of all models in economic analysis. (Stigler 1952: 206–207)[7]

I take this eulogy to mean that Ricardo had in fact elaborated an impressive theory of a dynamical economic system dealing with the production, distribution and utilization of the wealth of a nation. No talk anymore that in 1870 there was no theory of distribution! Around the time mentioned a fundamentally different explanation of distribution was gradually taking shape which the Stigler of 1941 apparently took to be the only one that deserves the name "theory". I am inclined to think that things have not changed much later. This follows from the fact that Stigler did not really take on board, or refute, Piero Sraffa's interpretation and reformulation of the classical theory of value and distribution, as we will see below. However, Stigler, unlike several other marginalist commentators, saw clearly that central properties of Ricardo's theory did not fit the marginalist perspective. The fact that Ricardo, among others, had invented the marginal principle when dealing with intensive diminishing returns and thus intensive rent apparently prompts Stigler to ask himself why he failed to apply this principle indiscriminately to all factors of production alike—land, labour and capital. This would have led Ricardo to the elaboration of the sought "theory of the prices of productive services", which the marginalists later developed. When reading Ricardo, Stigler is on the lookout for anticipations of basic marginalist concepts, such as the elasticity of demand for labour or for corn (see, for example, Stigler 1982: 68–71), and since he does not really find them there is inclined to take it as reflecting a shortcoming of Ricardo's theory rather than as evidence of its different nature.[8] Ricardo's treatment of wages as a given magnitude in his explanation of profits as a surplus is a case in point, as will be seen below.

The composition of the paper is the following. Section 'A Watershed in the Interpretation of Ricardo—The RES Edition' sets the stage for what follows by emphasizing Sraffa's edition of Ricardo's works and correspondence as a watershed in interpreting Ricardo and more generally the classical economists' approach to the theory of value and distribution. Section 'Stigler on the Ricardian Theory of Value and Distribution' discusses Stigler's interpretation of Ricardo's theory of value and distribution in his 1952 paper. Section 'Stigler on "Sraffa's Ricardo"' turns to his eulogy of Sraffa's Ricardo edition in his 1953 review article. Section 'Stigler on Ricardo's "93% Labor Theory of Value"' deals with Stigler's 1958 essay on Ricardo's alleged 93% labour theory of value. The final section contains concluding remarks.

A Watershed in the Interpretation of Ricardo—The RES Edition

The Royal Economic Society edition of *The Works and Correspondence of David Ricardo* in eleven volumes (Ricardo 1951–1973) marks a watershed in the interpretation of Ricardo's contributions to political economy and indeed a watershed in the history of economic analysis more generally. In the introduction to volume I, which contains Ricardo's *Principles of Political Economy, and Taxation*, Piero Sraffa, the editor (in collaboration with M. H. Dobb), put forward a novel interpretation of Ricardo's approach to the problem of value and distribution, now known as the "surplus approach", which has revolutionized the way we see Ricardo today. The core of this interpretation was actually not new. It had been advocated by contemporaries of Ricardo, including James Mill and Robert Torrens, by some of his later critics, most notably Marx, and then by authors who formalized aspects of the theory, in particular Vladimir K. Dmitriev and Ladislaus von Bortkiewicz. But as Sraffa pointed out in his 1960 book, an understanding of "the old classical economists from Adam Smith to Ricardo has been submerged and forgotten since the advent of the 'marginal' method" (Sraffa 1960: v).[9] Sraffa deserves the credit for having rediscovered the classical approach from under thick layers of interpretation, which frequently amounted to misinterpretation. And he has shown conclusively that it was not an early and primitive version of demand-and-supply theory. His view has not gone undisputed, but in my judgement emerged from the debates unscathed.[10]

George Stigler was an eminent Ricardo scholar, who thought very highly of Sraffa's Ricardo edition. He did not join the camp of the critics, despite the fact that because of his marginalist training and outlook he was much closer to it than to that of the followers of Sraffa. He was critical of Samuel Hollander's point of view, for example, and criticized him for being "interested only in Ricardo's inner convictions" (Stigler 1990: 765). This is tantamount to saying that Hollander's view is based on beliefs, the correctness of which cannot possibly be established. Stigler published several articles on Ricardo in the 1950s: a major one at a time when he had not yet had access to Sraffa's introduction to *Works* I (Stigler 1952), others before Sraffa's 1960 book had come out (Stigler 1953, 1958).

The book contains a logically consistent reformulation of the "classical standpoint" in the theory of value and distribution and solves many of the conundrums with which Ricardo had struggled. It is therefore interesting to see how Stigler tried to cope with the situation. He wrote in a period of transition from received views on Ricardo, shaped first and foremost by Alfred Marshall, to Sraffa's novel one. His acute scholarship prevented him from falling victim to a fairly common attitude, namely to see Ricardo as one's preconceptions wish to see him. Different from some other interpreters, he did not substitute fantasy and inventiveness for meticulousness and scientific sobriety, but tried as best as he could to make sense of Ricardo. This was a difficult task, not least because when Ricardo passed away his theory of value and distribution was still in the making, in *statu nascendi*, as his manuscript fragments on "Absolute and Exchangeable Value" (*Works* IV) show. Not knowing the logical terminal point of Ricardo's thoughts and theory—the "higher standpoint"—made it difficult, if not impossible, to judge the various processing elements in it. It cannot come as a surprise, therefore, that Stigler was not always right in his assessment of Ricardo's efforts and in some places, was seriously wrong. At the same time, he deserves credit for having seen things in Ricardo that escaped the attention of other scholars and confirmed implicitly Sraffa's point of view.

Stigler on the Ricardian Theory of Value and Distribution

In June 1952, Stigler published what may be seen as his main article on "The Ricardian theory of value and distribution" (Stigler 1952). He informs the reader that "A draft of this paper was completed before the magnificent edition of Ricardo's works edited by Sraffa and Dobb began to appear" (1952: 187n). And while all references to Ricardo's works are to this edition, there are no signs that Stigler had been able to absorb Sraffa's new interpretation of Ricardo's theory of value and distribution contained in his introduction to volume I of the edition (Sraffa 1951). This is unfortunate because it could have served as a foil to Stigler's own interpretation, its hits and misses. The fact that he did not confront it with Sraffa's may perhaps be interpreted as indicating how confident Stigler was at the time that his interpretation, or at least central elements of it, would not be questioned. Be that as it may, in this section we provide a critical account of Stigler's essay.

Law of population. Stigler calls Ricardo "the most influential economist" of nineteenth-century England, which was "an extraordinary achievement of an extraordinary man" (1952: 187). He dubs the theory of population "the first pillar of the Ricardian system" (1952: 187). However, subsequently he points out rightly that while for some of his argument Ricardo accepted the law of population, which implied a tendency towards a long-term fixed real wage rate, "when he came to analyse wages, the Malthusian theory was virtually ignored" (1952: 194). So no pillar anymore!

This is a valid observation, which raises, of course, the question why Ricardo assumed a given subsistence wage in one part of his economic analysis, but abandoned it in another one. Stigler refrains from entering directly into a discussion of this issue.[11] Close scrutiny shows that

Ricardo distinguished between the determination of the rate of profits and relative prices in *given* economic circumstances, that is, at a given time and place, and the movement of all distributive variables, including wages, in *changing* circumstances over time. In the former case, Ricardo insisted, the rate of profits and relative prices are fully determined in terms of the given system of production and a given level of real wages. For an essentially tactical reason he was prepared to come partly Malthus' way by assuming the law of population, because then the real wage rate could be taken as a given ("subsistence") magnitude. This rendered the explanation of profits residually in terms of the surplus product a great deal easier and should have prevented Malthus from escaping the logic of Ricardo's reasoning.[12]

When Ricardo in his theory of capital accumulation and economic development then turned to a system incessantly in movement and transformation from within, he emphasized that the real wage rate can no longer be taken as given and constant and explicitly distanced himself from the Malthusian law of population.[13] He stressed the historical and social dimensions of the natural wage (*Works* I: 96–97) and that "population may be so little stimulated by ample wages as to increase at the slowest rate – or it may even go in a retrograde direction" (*Works* I: 169). "Better education and improved habits" may break the connection between population and necessaries (*Works* II: 115). Workers may get "more liberally rewarded" and thus participate in the sharing out of the surplus product (*Works* I: 48). If this were the case for a prolonged period of time, a sort of ratchet effect may be observed. The higher real wages become customary and define a new level of "natural" wages.[14] As early as in the *Essay on Profits*, Ricardo stressed that "it is no longer questioned" that improved machinery "has a decided tendency to raise the real wage of labour" (*Works* IV: 35; see also VIII: 171; Jeck and Kurz 1983). It follows that the concept of "natural wages" in Ricardo is defined with reference to the wealth of a society and the growth regime it experiences and must not be interpreted as indicating a given and constant real wage rate—nothing of this sort. An implication of this is that Ricardo felt the need to replace the real (that is, commodity) wage rate by a share concept, or "proportional wages" (Sraffa 1951: lii), that is, "the proportion of the annual labour of the country … devoted to the support of the labourers" (*Works* I: 49). It was on the basis of this wage concept that he asserted his fundamental proposition on distribution, that the rate of profits depends inversely on proportional wages (see Gehrke 2011).[15]

To conclude, the surplus explanation of profits applies both in a regime in which the law of population holds, and in a regime in which it doesn't. Interestingly, Stigler (1952: 194) qualifies Ricardo's view on wages explicitly as "correct", because it did not postulate a given and constant real wage rate, but allowed for an increase over time. The previous discussion should have made clear, however, that Stigler's adjunct remark "Ricardo did not know how to incorporate [it] into his theoretical system" (ibid.) cannot be sustained. We come back to this below.

Rent theory. According to Stigler, the second pillar upon which Ricardo's system was erected was the theory of differential rent. However, also in this regard Stigler considers Ricardo "chiefly a borrower [who] did not improve upon either theory in any basic respect" (1952: 200). I wonder whether this harsh judgement can be sustained in view not only of the evidence available to us but also in view of what Stigler writes in the rest of his essay. Sir Edward West and Malthus had anticipated the theory, or parts of it, the latter "with much less incisiveness and clarity"; Malthus is even said, not without some justification, to have managed to "invent two errors for each truth" (1952: 198). Yet, if the form, in which Malthus had put forward the theory was muddled and if "Ricardo went beyond West" as regards "the analysis of the effects of improvements [in agriculture] on rent" (1952: 199), then Ricardo obviously deserves greater credit than Stigler is willing to give him. Stigler is, however, right in insisting: "In the synthesis of these theories into a general theory of value and distribution, [Ricardo] struck out on his own. The peculiar combination of doctrines that makes up his system is truly original" (1952: 200).[16] Ricardo's main achievement, as I see it, was indeed to have studied the problems of value, distribution, capital accumulation and economic development for an open economy characterized by a division of labour in which money serves as a means of payment in a *general* framework of the analysis.

There is one element in Stigler's interpretation I consider to be misleading. This concerns the multifarious theme of technical progress.

First, Stigler contends that Ricardo, like Malthus and West, "gave little thought to technical improvements" (1952: 196) and failed to see that "improvements in agricultural technology were neither negligible nor sporadic, [and that] technological progress in non-agricultural industries could offset diminishing returns in agriculture" (1952: 204). Secondly, he contends with reference to Wicksell (1934) that "the celebrated chapter on machinery rests upon a logical error" (1952: 206; see also Stigler 1953: 587). As will be shown, both claims are untenable.

Different types of technical progress. Ricardo was clear that technical change was an essential part of the development of modern society and that different types have different effects. He saw the historical course of an economy as largely shaped by two opposing forces: the "niggardliness of nature", on the one hand, and man's ingenuity and creativity reflected in new methods of production and new commodities, on the other. Such inventiveness was seen to be the result of competitive conditions. Ricardo also saw that while technical change in industries producing "necessaries" (that is, wage goods) or in industries supplying these industries with inputs will increase the general rate of profits, given the real wage rate, technical change in the production of "luxuries" will not have this effect, but only reduce their prices. He saw the essence of foreign trade to consist in providing access to new methods of production and commodities abroad and compared it to technical progress. The Corn Laws, he insisted, implied interrupting this access with regard to the core necessary of the English economy, which entailed a fall in the general rate of profits, a consequent fall in the rate of capital accumulation and a fall in the real wage rate as a consequence of a reduced growth of the "demand for hands". The only beneficiaries of the law were the landlords.[17] And he understood that new technical knowledge may at first not be adopted, because it would not be profitable to do so, but may be adopted at a later time as a consequence of the economic environment having changed from within. This is the case of what John Hicks later called "induced technical change".[18]

Already in *The Essay on Profits* of 1815 Ricardo wrote: "we are yet at a great distance from the end of our resources, and… we may contemplate an increase of prosperity and wealth, far exceeding that of any country which has preceded us" (*Works* IV: 34). In a letter to Hutches

Trower on 5 February 1816 he concluded from a fall in grain prices since 1812 that "we are happily yet in the progressive state, *and may look forward with confidence to a long course of prosperity*" (*Works* VII: 17; emphasis added). Also, in his entry on the "Funding System", published in September 1820, he stressed with regard to England that "it is difficult to say where the limit is at which you would cease to accumulate wealth and to derive profit from its employment" (*Works* IV: 179). The widespread view (see, for example, Rostow 1990: 34, 87; Blaug 2009; Solow 2010) that Ricardo saw the stationary state lurking around the corner cannot be sustained. It mistakes Ricardo's method of counterfactual reasoning—What would happen if there was no technical progress, but capital accumulated and the population grew?—for a factual statement about economic development.

Ricardo studied various types of technical progress and their effects. In chapter II of the *Principles* the focus is on (i) land-saving and (ii) capital alias labour-saving forms of improvements in agriculture. Stigler is, of course, aware of the existence of Ricardo's discussion. He argues that the class of improvements that fall under case (ii), in which "the amount of labour necessary to produce a given product from given land" is reduced, "is surely vacuous under [Ricardo's] definition" (Stigler 1952: 199). He maintains that "Under [Ricardo's] usual assumptions his conclusion should have been that *improvements always benefit the landlords*: the marginal product curve of capital-and-labor is higher relative to the cost of capital-and-labor" (1952: 199–200; emphasis added).

Ricardo's chapter has met with considerable difficulties of understanding and some serious misunderstandings. Edwin Cannan ([1893] 1967) made a start. Scholars including Harry Johnson (1948), Mark Blaug ([1967] 1997), Denis O'Brien (1975) and Paul Samuelson (1977) followed him. However, as Gehrke et al. (2003) have shown, Ricardo's argument is essentially correct, only its presentation is (partly) problematic.[19] Stigler (1982: 110) in another context, and with explicit reference to Cannan, rightly speaks of the "blinding effect of hypercriticism", which prevents one from understanding what can be deduced from an analytical system.[20] Stigler deserves credit by being always concerned with learning from and understanding the thrust of Ricardo's argument and not mistaking imperfections of presentation for irremediable errors. Alas, with regard to chapter II of the *Principles* his criticism, which is different from that of Cannan et al., can also not be sustained. The fact he mentions regarding the marginal product curve is not sufficient to disprove Ricardo's view since it does not touch upon the question of how in the new situation the social surplus is divided between the recipients of property income, capitalists and landlords, that is, profits and wages. While the sum total of profits and rents is larger, given a constant level of wages, it is quite possible that profits are larger and rents smaller. This is the case if the *same* product can be produced cultivating a *smaller* number of qualities of land. (Stigler's above formulation "… from given land" lacks clarity in this regard.) A simple diagrammatic illustration can show this. The backbone of Stigler's argument is a diagram he borrowed from Marshall ([1890] 1977: Appendix L) in which the effect on rents of a parallel upwards shift of the marginal productivity curve as a result of technical improvements is discussed; see Stigler (1941: 90–91 and 90 fn. 2). However, the argument does not disprove Ricardo's argument, in which certain qualities of land, some of which will have been intra-marginal in the original situation, will no longer be cultivated as a consequence of technical progress. Hence the sum total of the rents of land will be smaller.[21]

To conclude, the following clarification is in place. In summarising Ricardo's theory especially in the *Essay on Profits*, Stigler interprets him as saying: "the rent of land [Q] will be equal to the total product [X] minus the amount of agricultural capital [K] times its profit rate [r]" (1952: 201). Hence according to this definition $Q = X - rK$. This is only correct, if by "total product" Stigler means what Ricardo called "neat" or *surplus* product. Ricardo leaves no doubt that the latter equals what he, Ricardo, called "total produce" (in quarters of wheat) "after paying the cost of production" (*Works* IV: 17, Table). With total capital consisting only of wages, W, an assumption Ricardo entertained in his example, the correct equation would be $Q = X - W + rW = X - (1 + r)W$, since the wages of labour must be subtracted from the total product. I wonder whether Stigler felt entitled to identify neat and total product because in his view Ricardo did not provide a "complete system", for, he maintained, "in the absence of more explicit theories of population and capital accumulation, the aggregate output of the economy is not determined" (1952: 201).

However, as Ricardo's numerical example in the *Essay* shows, aggregate output is known at each stage of the cultivation of different qualities of land. And if it were not known, this would certainly not justify dropping cost of production from economic accounting. Furthermore, assuming a given and constant wheat wage in the example strikes me as a legitimate simplification that does not render the entire exercise futile. It also does not contradict Ricardo's conviction that in the course of actual development the real wage may be expected to change. His reluctance to forecast in which direction and by how much, rather than being a sign of the weakness and incompleteness of his theory, reflects his awareness of the complexity of the issue at hand and the limits of our knowledge. To "elucidate" an economic principle in given conditions is one thing (here: the inverse movement of the rate of profits and the rents of land), to venture predicting the actual course of events is an entirely different thing. Stigler is repeatedly inclined to request being given the latter, where perhaps only the former is possible.

On the basis of his reasoning Stigler (1952: 199) contends: "Ricardo was prone to exaggerate the conflict of interests between landlords and other economic classes". We have seen that the argument Stigler puts forward in support of his claim does not stand up to scrutiny.

On machinery. In the machinery chapter the emphasis is on improvements in the production of necessaries (see Kurz 2010). While Adam Smith viewed the manufacturing sector as essentially producing only luxuries, Ricardo glimpsed its emerging key role as an engine of growth.[22] It is remarkable that he even contemplated the limiting case of mechanization, that is, a fully automated system of production, and observed: "If machinery could do all the work that labour now does, there would be no demand for labour. Nobody would be entitled to consume anything who was not a capitalist, and who could not buy or hire a machine" (*Works* VIII: 399–400).

Technical progress, Ricardo was clear, was not always an unambiguous blessing for all members of society. The system, he maintained, may experience prolonged periods of what later was called "technological unemployment". In short, maximizing profits did not ipso facto amount to maximizing employment levels. He explained:

> My mistake arose from the supposition, that whenever the net income [profits (and rents)] of a society increases, its gross income[23] would also increase; I now, however, see reason to be satisfied that the one fund, from which… capitalists derive their revenue, may increase, while the other, that upon which the labouring class mainly depend, may diminish, and therefore it follows… that the same cause which may increase the net revenue of the country, may at the same time render the population redundant, and deteriorate the condition of the labourer. (*Works* I: 388)

In the chapter "On Machinery" Ricardo constructed an example that was designed to illustrate precisely this possibility. Since the progressive replacement of circulating capital (wages or rather labour) by fixed capital is a characteristic feature of modern economic development, the case under consideration is of great relevance.[24] The numerical example is judiciously constructed. The resulting reduction of employment of labour follows from the stipulated assumptions, which include for simplicity a given real ("subsistence") wage rate. The argument does not rest upon a "logical error", as Stigler surmises. When Wicksell (1934) showed that displaced workers may swiftly find employment again, he did so by stepping out of the framework of Ricardo's analysis. Wicksell in fact assumed in a fully fledged marginalist manner a downward flexible real wage rate and ample opportunities of substitution between labour and machines. And he pointed out that his different results followed from the different assumptions he entertained. Wicksell's reasoning was not designed to question the logic of Ricardo's argument, but to show that premises matter.[25]

There is also the problem of how to read Ricardo's famous statement that "Machinery and labour are in constant competition and the latter can frequently not be employed until the former rises" (*Works* I: 395). This has typically been interpreted in straightforward marginalist terms as referring to the usual static mechanism of substitution among factors of production as (relative) real factor prices change: with a rising *real* wage rate relative to the rate of profits machines will be substituted for labour. Stigler is inclined to read the statement in this perspective and rejects some alternative interpretations. Eugen von Böhm-Bawerk's attribution to Ricardo of an early version of the concept of the superiority of more roundabout processes of production, Stigler (1941:

284–285) dismisses as being without foundation. However, the conventional interpretation cannot be sustained. As I have shown (Kurz 2015), an interpretation of Ricardo's above statement that is faithful to his doctrine has to take into account the fact that with the accumulation of capital and the growth of population, *money* wages will have to rise in order to prevent real wages from falling as corn prices rise. This rise in money wages eventually triggers a shift to methods of production that use machinery: it is a case of induced innovations in a dynamical setting and not a case of static factor substitution.

Stigler concludes his respective considerations in the following way: "Depending on the relative strengths of technological progress and diminishing returns, the dismal stationary state lies near or far in the future". He adds: "Ricardo pays little attention to this final, historical equilibrium, so we are entitled to infer that he did not believe that it was near" (Stigler 1952: 204).

Both statements, I think, are correct and contrast Stigler's Ricardo pleasantly with the Horseman of the Apocalypse as which Ricardo is frequently portrayed, apparently confounding him and Malthus. But if the latter statement portrays adequately Ricardo's position, and I have no doubt that it does, is it then possible to accuse Ricardo of paying too little attention to technical progress? I wonder.

Stigler calls Ricardo rightly "a theorist who wished to answer definite questions (presented by economic problems), and he made his theory no more general than these questions required" (1952: 200). He rightly praises Ricardo's great intellectual powers and his capability of abstract reasoning and, as we have already heard, sees him as having elaborated "what is probably the most impressive of all models in economic analysis" (1952: 207). Ricardo incited "order and precision" in political economy: "This was the basic 'Ricardo effect'; and … we must thank him for it" (1952: 200).

Stigler on "Sraffa's Ricardo"

Shortly after the appearance of the first nine volumes of *The Works and Correspondence of David Ricardo* in 1951 and 1952, Stigler publishes a review article of them in the *American Economic Review* (Stigler

1953).[26] The article contains three main parts: an assessment of Sraffa's work as an editor; Stigler's view on James Mill's role in prompting Ricardo to write the *Principles*; and Stigler's interpretation of whether Ricardo or Malthus advocated "Say's Law" in one form or other. Here we focus attention on the first and the third theme.

Sraffa's editorial achievement. Stigler is full of admiration for Sraffa's editorial achievement.[27] He calls Ricardo "a fortunate man", not least because "he has been befriended by Sraffa – who has been befriended by Dobb" and calls "Sraffa's *Ricardo*" a "work of rare scholarship". He explains:

> The meticulous care, the constant good sense, and the erudition, make this a permanent model for such work; and the host of new materials seems to suggest that Providence meets half-way the deserving scholar. (1953: 586)

Can a greater amount of praise be heaped upon a scholar? Yes, it can. Stigler stresses the "extraordinary accuracy" of the edition. He calls Sraffa's editorial notes "superb" and adds: "They seem unbelievably omniscient; they are never obtrusive or pedantic; and they maintain unfailing neutrality. Their presence not only *clarifies much of Ricardo's work* but also provides a vast fund of information on the economics of the period" (1953: 587; emphasis added). Stigler notes that Sraffa limited his observations in his introduction to volume I, the editorial prefaces and notes to the statement of facts and refrained from providing his interpretation of them. Stigler comments: "This severe self-abnegation was wise: the facts are relatively timeless but even the best analysis of a predecessor will change with the interests and knowledge of the science" (1953: 587). While this is true, it deserves to be mentioned that by simply stating the facts Sraffa was able to draw the attention to a Ricardo, who had nothing in common with the received picture of Ricardo.

Stigler concludes the section in the following way: "Others may be as uncomfortable as I at undiluted praise", but putting forward the few insignificant criticisms that can be made would only "emphasize more subtly the superlative quality of the scholarship. But usual rules must bow to unusual events: here is a task that need not be performed again" (1953: 587). He ends the essay with the remark: "We are still to receive from Sraffa a biography of Ricardo. We shall wait for it with the patience which he munificently rewards" (1953: 599). Volume XI, which contains a short biography, was published in 1973.

The arguably most controversial statement of Stigler's essay is the claim that the volumes of the edition "often amplify and sometimes modify our understanding of [Ricardo's] doctrines, but *they do not change it in essentials*" (1953: 586; emphasis added). As a matter of fact, Sraffa's interpretation (and then his 1960 reformulation) of Ricardo's theory of value and distribution changed our understanding fundamentally. Sraffa brought it back towards the way in which the theory was seen by some of Ricardo's contemporaries, Marx, Vladimir K. Dmitriev and Ladislaus von Bortkiewicz, to mention but a few. Sraffa removed it from the interpretation of the marginalists, who tried to understand Ricardo strictly in marginalist terms and thus missed the gist of his approach. Most importantly, from a history of economic thought perspective, Sraffa's interpretation turned out to be faithful to what Ricardo actually had written, whereas marginalist interpretations, old and new, faced insurmountable obstacles in establishing a correspondence between interpretation and textual evidence.[28] Alas, Stigler also had difficulties to escape received modes of interpretation (more of which will be seen below). Luckily these did not completely overshadow his perception of Ricardo.

Ricardo's achievements and failures. While Stigler's praise of Sraffa's edition can hardly be topped, his praise of Ricardo's achievements as an economist is moderate at best. Interestingly, he writes: "I think Ricardo's policy recommendations were profoundly good but his theory was not of the highest quality" (1953: 586). This may come as a surprise to many readers. First, one may ask whether Sraffa has wasted his talent and energy on a mediocre scholar, and if so, would his edition deserve to be held in such high esteem as Stigler displayed? Would it not rather be a sign of a misallocation of scarce intellectual resources and therefore a welcome object of scathing criticism? And if Ricardo's theory was not of the highest quality, how come that his policy recommendations could be "profoundly good"?

Joseph A. Schumpeter was of a radically different opinion than Stigler. In several of his writings and most notably in his *History of*

Economic Analysis (1954) he chastised Ricardo for what he dubbed the "Ricardian vice". This accusation was intended to highlight Ricardo's alleged habit of introducing excessively bold assumptions into an already oversimplified picture of the economy and treating these as known magnitudes when in fact they are unknowns. According to Schumpeter (1954: 569), Ricardo's "fundamental problem" was that he had only one equation, but four variables: "net output equals rent plus profits plus wages".[29] Operating with this perspective, Ricardo was bound to treat three of the variables as constants. In addition Schumpeter deplored Ricardo's alleged habit of "piling a heavy load of practical conclusions upon a tenuous groundwork" (1954: 1171). In short, Schumpeter insinuated that Ricardo's policy recommendations were not derived from a fairly coherent economic theory, but were rather ad hoc and opinionated. Ricardo was more of a political animal than an unprejudiced scholar.

I disagree with both commentators, but my disagreement with Schumpeter is greater still.[30] Let me explain the reasons for it. As far as I can see, Stigler bases his judgement that Ricardo's theory "was not of the highest quality" on the following reasoning. While he does not claim that Ricardo's theory is underdetermined, he maintains that several elements of it cannot be sustained. These include in particular: (i) "Ricardo's mistaken theory of the effects of machinery (fixed capital) on wages" (1953: 587); and (ii) Ricardo "postulating full employment in dealing with a theory of unemployment" (1953: 598). As regards the machinery case, we have already seen in section 'Stigler on the Ricardian Theory of Value and Distribution' that Stigler's criticism does not stand up to close scrutiny. Here the case may be related to the second criticism, which turns upon the compatibility, or otherwise, of "Say's Law", as Ricardo advocated it, and unemployment.

Prior to that we have to deal briefly with Schumpeter's "Ricardian vice" criticism (see also Kurz 2008). As regards the way Ricardo reasoned, Schumpeter and other critics have not taken seriously his statement that in all his argument and numerical illustrations "I have been desirous only to elucidate the principle, and it is scarcely necessary to observe, that my whole basis is assumed at random, and merely for the purpose of exemplification. The results though different in degree, would have been the same in principle…. My object has been to simplify the subject" (*Works* I: 121–122). Hence, while it is true that Ricardo typically employed bold cases to "elucidate" the principle at hand and draw attention to what in his view were the most important aspects of the problem under consideration, he invited his readers to try out less restrictive assumptions and investigate their implications. Some later commentators rightly praised Ricardo for having heralded an approach in economics that requires a clear statement of the assumptions on the basis of which certain propositions are taken to be valid within a given analytical context. This is now considered an indispensable prerequisite of scientific communication. Therefore, what in Schumpeter's view is a vice, nowadays is taken to be a virtue. Stigler actually emphasizes in this vein: "The triumph of Ricardo over Malthus cannot be regretted by the modern economist: it is more important that good logic win over bad than that good insight win over poor" (1953: 599).

We now come back to the machinery chapter. It was a turning point in Ricardo's understanding of an economic system penetrated by labour saving and fixed capital increasing methods of production. Ricardo had to admit, as we have already heard, that certain types of technical progress "may at the same time render the population redundant, and deteriorate the condition of the labourer" (*Works* I: 388). While Ricardo previously had thought that displaced workers would swiftly find employment again, either in the same or some other industry, he now felt that this was not so. There was no presumption that labour compensation was almost automatic. The question now is: Does this new view get in the way of Ricardo's concept of "Say's Law"—is it the source of an inconsistency of his theory?

The answer is no. To see this, we must be clear about the precise meaning of Say's Law in Ricardo. The important point to be made is that Ricardo's conception of the law implied, against Malthus, that there cannot be a "general glut" of commodities and a redundancy of capital, that is, there cannot be an overproduction or excess supply of things that are being produced out of the profit motive. Since labour is not so produced, the law does not extend to it. The law does not eo ipso imply the full employment of labour. Stigler (1953: 595) quotes Ricardo, who emphasizes "that there is no limit to demand – no limit to the employment of capital while it yields any profit" (*Works* I: 296). If this is the case, then there is no presumption that the ordinary state of affairs in the economy is characterized by full employment. Stigler is one of the few interpreters of Ricardo, who has clearly seen this when he writes with regard to the quoted passage: "In this form the law of markets is no longer a truism, it is the proposition that general equilibrium of the economy, with prices equal to costs (including 'profits'), *is compatible with any level of real income*" (1953: 595). It is thus also compatible with a level of employment below full employment. Stigler adds: "It would be more appropriate to call this the [James] Mill-Ricardo Law than Say's Law" (1953: 595).

Since in Ricardo's conceptualization Say's Law did not in itself imply the full employment of labour, the net displacement of labour consequent upon the introduction and diffusion of new methods of production, will increase unemployment. If the wages of those employed were fixed at a rigidly defined subsistence level, they could not support the unemployed. Unemployment would gradually disappear because of workers' attrition. In Ricardo such a scenario is at most used in order to illustrate the logical implications of an extreme hypothesis, but not in order to describe the ordinary state of affairs of a developing economy.[31]

Stigler on Ricardo's "93% Labor Theory of Value"

Analytical vs. empirical labour theory of value. Stigler (1958: 357) introduces his essay bearing the same title as this section with the question: "Did Ricardo have a labor theory of value – did he believe that the relative values of commodities are governed exclusively by the relative quantities of labor necessary to produce them?" As against those who have given "a flat affirmative answer to this question" he insists "that there is not the slightest basis for such an answer" (ibid.). This view may be confronted with Sraffa's whom Paul Samuelson once asked, whether Ricardo held a labour theory of value. Sraffa is reported to have answered: "He did and he didn't" (see Samuelson 2000: 139). What might at first sight be considered a sibylline response turns out to reflect properly Ricardo's point of view, which, for example, in the third edition of the *Principles* comes to the fore when Ricardo speaks "of labour as being *the foundation of all value*, and the relative quantity of labour as *almost exclusively* determining the relative value of commodities" (*Works* I: 20; emphasis added).

Stigler derives his concept of a "93% labor theory of value" from a numerical example Ricardo discusses in the *Principles* (see *Works* I: 35–36) in order to illustrate the dependence of relative prices on income distribution, given the system of production in use. Stigler's interpretation elicits the following remarks. In the example Ricardo exemplifies how a *change* in income distribution, that is, a rise in wages and the corresponding fall in the rate of profits (see below), affects relative prices of commodities produced with different proportions of fixed and circulating capital. Ricardo concludes against the background of his numerical example: "The greatest effects which could be produced on the relative prices of these goods from a rise of wages, could not exceed 6 or 7 per cent". The reason he gives in support of what he calls a "comparatively slight" effect, is that "profits could not, probably, under any circumstances, admit of a greater general and permanent depression than to that amount" (*Works* I: 36). Which amount does he stipulate? "[O]wing to a rise of wages, profits fall from 10 to 9 per cent" (*Works* I: 35). Here Ricardo invokes his "fundamental law of distribution"—the inverse relationship between the general rate of profits and wages (in modern theory also known as the *wage curve*). Alas, since he is not possessed of an analytically precise specification of this relationship, he can only put forward a guesstimate by how much the rate of profits will "probably" have to fall as a consequence of the postulated rise in wages. He *stipulates* that it falls by just 1%. If it fell by more (or less) than that, the change in relative prices would be different.

More importantly with regard to Stigler, since the rate of profits was assumed to be positive in the initial situation (10%), relative prices will already then have deviated from relative labour values. Hence, what Ricardo in fact discusses, and what Stigler's interpretation misses, is not a deviation of prices from labour values, but a *change* in the deviation due to a change in income distribution. And this *change in the deviation* Ricardo considers to be relatively small, in the range of 6–7%.

The *whole* deviation is an entirely different thing and may, for a given distribution of income (that is, a given wage rate), be a great deal larger with respect to some prices and a great deal smaller with regard to some other prices.[32] If the chosen standard of value exhibits a medium proportion of fixed to circulating capital, Ricardo was convinced, its price will ex hypothesi not change and remain equal to its labour value, independently of income distribution. Yet commodities that are produced with a higher proportion than the medium one, will fall in price relative to the standard consequent upon a rise in wages (and the corresponding fall in the rate of profits), whereas commodities produced with a lower proportion will rise in price.[33] In view of these facts, which Ricardo stresses, it makes little sense to talk of a 93% labour theory of value.

Secondly, and closely related, for a given system of production and a given rate of profits not 93% of *all* prices will be "explained" by labour, some prices will deviate more strongly and others less strongly from labour values. Depending on the measurement device applied with regard to labour values and prices, some prices will exceed and others fall short of the labour value![34] Would one then have to say in the second case that labour values explain *more* than the price under consideration? One may construct at will numerical examples in which the average percentage will be higher or lower, as the case may be. And with a change in the general rate of profits (and an inverse change in the real wage rate), given the system of production in use, the percentage with regard to each commodity will typically change more or less. Hence, attributing to Ricardo a definite percentage labour theory of value, while suggestive, is neither faithful to his argument nor does it make analytically much sense.[35]

Yet the important point Stigler wishes to make, and which I endorse, is this: "I can find no basis for the belief that Ricardo had an *analytical* labor theory of value, for quantities of labor are *not* the only determinant of relative values". According to Stigler, Ricardo "held what may be called an *empirical* labor theory of value, that is, a theory that the relative quantities of labor required in production are the dominant determinants of relative values" (1958: 361; emphases in the original)

While I consider the first statement to be correct and faithful to Ricardo, the status of the second one is somewhat dubious not least in the face of numerical examples that can be constructed. However, the problem is somewhat deeper and derives from the fact that Ricardo did not manage to elaborate a fully correct theory of value and distribution, as Stigler rightly mentions (1958: 361). Yet without such a theory the deviation of relative competitive prices from relative labour values for a given system of production and a given distribution of income cannot be ascertained. Therefore, Stigler's attribution to Ricardo of a 93% labour theory of value is somehow hanging in the air, not least because with a change in the system of production and in the real wage rate (or the share of wages) the percentage will, in general, change too. Since Sraffa elaborated a correct theory in his book *Production of Commodities by Means of Commodities* (1960) by adopting, correcting and extending Ricardo's approach to cover all important phenomena in the theory of production (fixed capital, joint production, scarce natural resources), we may look at the issues at hand from this higher standpoint.

Two senses of "labour". Sraffa began to develop his theory as early as the second half of 1927. In a quick succession he came up with important insights that throw fresh light on old controversies and actually show the way how to resolve them. A first such insight concerns the concept of "labor", which in many contributions, old and new, is treated as a simple and straightforward thing, although it is anything but that, as Ricardo knew well. The following note Sraffa wrote in November 1927 may be read as a comment also on Ricardo's above statement (see *Works* I: 20):

> It is the *whole* process of production that must be called "human labour", and thus causes all product and all values. Marx and Ricardo used "labour" in two different senses: the above, and that of *one* of the factors of production ("hours of labour" or "quantity of labour" has a meaning only in the latter sense). It is by confusing the two senses that they got mixed up and said that value is proportional to quantity of labour (in second sense) whereas they ought to have said that it is due to human labour (in first sense: a non measurable quantity, or rather not a quantity at all). (Sraffa Papers D3/12/11: 64; emphasis in the original)

A confusion of the two senses was widespread in the literature on Ricardo, as Stigler (implicitly) stresses. Actually it still is. Recently Mary

Morgan wrote with regard to Ricardo's theory: "it is labour alone that creates value, and … there is a direct relationship between labour input and value" (Morgan 2012: 60). As we have just seen, Ricardo was decidedly *not* of this opinion. While he assumed *for simplicity* (see *Works* I: 36–37) that relative prices are proportional to relative labour quantities, he clearly did not advocate the view that "it is labor alone that creates value"—it was Marx who did it most prominently.

So what precisely was Ricardo's view, what was his *analytical* theory of value, as interpreted in terms of Sraffa's reformulation and generalization of it?[36]

"Natural prices". This theory may be stated with the help of some little formal analysis. The system of "natural prices" or "prices of production" in the simple case of circulating capital only (and setting aside scarce land on Ricardo's assumption that prices and the general rate of profits are determined on no-rent bearing, or marginal, land) and in which each and every commodity is used in the production of each and every commodity, either directly or indirectly,[37] is given by

$$\mathbf{p} = (1 + r)(\mathbf{Ap} + w\mathbf{l}), \tag{10.1}$$

with $\mathbf{p}$ as the n-dimensional price vector, $\mathbf{A}$ as the material input matrix, $\mathbf{l}$ as the n-dimensional vector of (homogeneous) labour inputs, r as the general rate of profits and w as the wage rate (see Kurz and Salvadori 1995: Chap. 4). All value magnitudes are expressed in terms of some standard of value, a single commodity or a bundle of commodities $\mathbf{d}$, that is,[38]

$$\mathbf{d}^T\mathbf{p} = 1. \tag{10.2}$$

Solving (10.1) for $\mathbf{p}$ gives

$$\mathbf{p} = (1 + r)w[\mathbf{I} - (1 + r)\mathbf{A}]^{-1}\mathbf{l}, \text{ where } 0 \leq r < R, \tag{10.3}$$

with R as the maximum rate of profits of the system corresponding to a zero wage rate, $w = 0$, and $\mathbf{I}$ as the n x n identity matrix. For a given system of production represented by ($\mathbf{A}$, $\mathbf{l}$), and a given real wage rate (that is, a wage rate expressed in the standard of value and compatible with a non-negative rate of profits), the $n+1$ unknowns—n prices and r—are determined by Eqs. (10.1) and (10.2).[39] Some simple manipulation of the equations gives the inverse relationship between the real wage rate and the rate of profits—Ricardo's "fundamental law of income distribution":

$$w = \left\{ \mathbf{d}[\mathbf{I} - (1+r)\mathbf{A}]^{-1}(1+r)\mathbf{l} \right\}^{-1}. \tag{10.4}$$

Obviously,

$$\partial w / \partial r < 0,$$

or, as Ricardo put it: "The greater the portion of the result of labour that is given to the labourer, the smaller must be the rate of profits, and vice versa" (*Works* VIII: 194).[40]

Before we continue, a remark on Stigler's (1952: 203) following proposition is appropriate: "Ricardo's basic theorem on distribution … is strictly dependent on his measure of value". To be clear, depending on the chosen standard of value, that is, the vector **d** in the above formalization, the shape of the w–r relationship will change. But Stigler seems to suggest (see 1952: 202) that a change in the standard of value might also affect the mathematical properties of the economic system, especially whether the wage rate and the rate of profits are inversely related. However, as Ricardo already made very clear, the choice of a standard of value (or numeraire, as it was later called) is a question of convenience, but not of substance. The fact that he paid so much attention to the problem reflects his search for a correct theory of value and distribution and in this regard some standards of value are more illuminating and useful than others (see Sraffa 1960: Appendix D). In systems without pure joint production, the two distributive variables are negatively related, independently of the standard of value chosen.

As Sraffa made clear, the general rate of profits and relative prices can be fully ascertained in terms of the independent variables, or data, Ricardo assumed: the system of production in use (**A**, **l**) and the real wage rate (w). Hence prices depend on two givens—technology and income distribution.[41] No other data are needed, as the marginalist critics of Ricardo from William Stanley Jevons to Léon Walras (but not Knut Wicksell!) wrongly contended. This finding pays tribute to Ricardo's remarkable intuition. He was basically on the right track, although he was unable to follow it down to its end.

Labour values. Now what about labour values? The labour value of a commodity equals the amount of labour directly expended in its production plus the sum total of the amounts of labour needed indirectly. These are "embodied" in the commodities productively consumed directly or indirectly in the course of the production of a unit of the commodity under consideration; these commodities are taken to transfer the labour contained in them into the produced commodity. In matrix notation

$$\mathbf{v} = \mathbf{A}\mathbf{v} + \mathbf{l} \text{ or } \mathbf{v} = (\mathbf{I}-\mathbf{A})^{-1}\mathbf{l}, \tag{10.5}$$

with $\mathbf{v}$ as the vector of quantities of labour embodied in the different commodities.

We may now compare the equations giving natural prices (10.3) and the equations giving labour values (10.5). The ratio v_j/p_j gives the percentage of the price of commodity j ($j=1, 2, \ldots, n$) "explained" in terms of the amount of labour "embodied" in the commodity, *given* the system of production actually in use and *given* the real wage rate (the rate of profits). (To be able to say this, we would have to normalize prices in such a way that they are directly comparable to labour values, an exercise we leave to the reader.)

It can be seen at a glance that prices are proportional to labour values in the special case in which the general rate of profits is equal to zero, because then

$$\mathbf{p} = \mathbf{p}^* = w(\mathbf{I}-\mathbf{A})^{-1}\mathbf{l} = w\mathbf{v}. \tag{10.6}$$

Prices $\mathbf{p}^*$ are just a special solution of system of Eq. (10.3) in which there are no profits and the entire social surplus is distributed in terms of wages. In this case, and opposed to Stigler's view quoted above, quantities of labour are the *only* determinant of relative prices. There is another case in which they are the only determinant, but now irrespective of the level of the rate of profits r, $0 \leq r \leq R$. It is the case known as "equal organic compositions of capital" throughout the economy. Since from a technological point of view this case is very special it need not concern us here.

Reduction to dated quantities of labour. In Ricardo we do not find any of the above equations. What we find, however, are numerical examples in which the prices of commodities are "reduced to dated quantities of labour" that are appropriately discounted forward. Such a reduction can also be carried out with respect to Eq. (10.1) by replacing time and again the $\mathbf{p}$ on the RHS of the equation by the price equation itself. We get

$$\begin{aligned}\mathbf{p} = {} & (1+r)\mathbf{A}^0 w\mathbf{l} + (1+r)^2\mathbf{A}^1 w\mathbf{l} + (1+r)^3\mathbf{A}^2 w\mathbf{l} \\ & + \ldots + (1+r)^t\mathbf{A}^{t-1} w\mathbf{l} + (1+r)^t\mathbf{A}^t\mathbf{p},\end{aligned} \tag{10.7}$$

where $\mathbf{A}^1 = \mathbf{A}$ and $\mathbf{A}^0 = \mathbf{I}$. With production as a circular flow—the production of commodities by means of commodities—the series is infinite, whereas with production as a unidirectional or linear process, it is finite. As was mentioned already, Ricardo frequently assumed unidirectional processes in order to simplify matters. Since Ricardo was interested first and foremost in relative prices, we may divide $\mathbf{p}$ by w and arrive at what Adam Smith had called "labour commanded prices", $\mathbf{p}^\circ = \mathbf{p}/w$. This renders clear that prices depend generally on (i) the technique used and (ii) the general rate of profits (or, because of Eq. (10.4), on the real wage rate). With $r = 0$, Eq. (10.6) tells us that labour commanded prices equal labour embodied values.

Ricardo and modern classical economics. Stigler published his 1958 paper two years before Piero Sraffa's 1960 book came out. He therefore did not know Sraffa's analysis, which once and for all clarified the analytical issues at hand and had the potential of putting long-standing controversies concerning Ricardo's doctrine (but also Marx's) to rest.[42] However, Stigler knew Sraffa's introduction in volume I of *The Works and Correspondence of David Ricardo*, which had been published in 1951. There Sraffa had laid out the foundation of his surplus-based interpretation of Ricardo, which stood in marked contrast to received interpretations, especially the one of Alfred Marshall, who portrayed Ricardo essentially as a precursor of the marginalist or demand-and-supply theory, with the demand side still in its infancy. In view of this, the concluding paragraph of Stigler's essay comes as a surprise. He writes:

> The basic reason Ricardo's theory is often misinterpreted is that it was often misinterpreted in the past. If a theory once acquires an established meaning, each generation of economists bequeaths this meaning to the next, and it is almost impossible for a famous theory to get a fresh hearing. Perhaps one hearing is all that a theory is entitled to, but one may plead that Ricardo deserves at least a rehearing – *his theory is relatively more widely misunderstood today than it was in his lifetime*. One can build a strong case that the modern economist need not be acquainted with Ricardo's work, but there is no case for his being acquainted with an imposter. (Stigler 1958: 367; emphasis added)

This passage elicits the following observations. First, as has already been said, it can be presumed that a correct interpretation presupposes the availability of a fully coherent version of the theory Ricardo was intent on elaborating, as a measuring rod. Only against the background of such a theory is it possible to see clearly how far Ricardo had got in his endeavour, where he went wrong, what his intuition told him and the like. This is in turn a precondition for judging adequately the hits and misses of Ricardo's interpreters. This theory is now available to us. Therefore it can safely be assumed that today the situation is in principle a great deal better than it was a few decades ago and grave misunderstandings of Ricardo can (and ought to) be avoided.[43] Secondly, as Sraffa's revival of the classical theory of value and distribution also showed, marginalist theory—whether the partial equilibrium version in the tradition of Marshall or the general equilibrium one in the tradition of Walras—are difficult to sustain because of serious capital theoretic difficulties. Hence, the modern economist is well advised to get acquainted with the modern reformulation of Ricardo's theory.

Concluding Remarks

The paper scrutinized George Stigler's numerous papers and remarks in books on David Ricardo's Political Economy. The emphasis is on the theory of value and distribution. It is shown that Stigler, like many other marginalist scholars, assesses Ricardo's contribution in terms of marginalist theory, based on the twin concepts of marginal productivity and marginal utility. This confirms Sraffa's observation that by the end of the nineteenth century the analytical structure, content and genuine significance of the classical theory had been "submerged and forgotten". Smith and Ricardo were then largely seen through neoclassical lenses. This applies also to Stigler in the papers he published in the 1950s. However, different from many others, his historical acuteness and concern with textual evidence from Ricardo's writings in support of the interpretation advocated, made him see that important elements of Ricardo's analysis resist the marginalist point of view. He noted with great care the incompatibilities, but was not able to disentangle Ricardo's genuine approach from the marginalist clasp.

His irritation can only have been increased by Sraffa's edition of the works and correspondence of David Ricardo and especially his introduction in volume I. In it Sraffa laid out the analytical core of Ricardo's surplus-based theory of the rate of profits. This flew in the face of the marginalist interpretation, which takes profits to reflect the marginal productivity of capital. Interestingly, Stigler praises Sraffa's editorial work, introductions and notes beyond all measure. However, he refrains from entering into a detailed discussion of Sraffa's novel point of view. Things do not change after Sraffa in 1960 published his book in which he put forward a logically consistent formulation of the classical theory of value and distribution. We do not know the reasons for Stigler's reticence. Was it because he would have to take a position and possibly distance himself from his earlier work on Ricardo?[44] Was it because he held Sraffa's work, including the latter's article of 1926, in such high esteem that he saw no need, or possibility, to enter into a critical discussion of it? Was it because he did not feel yet in a position to form a judgement on the issue? It can safely be said that ideologically Stigler and Sraffa did not belong to the same flock. Hence one would have expected Stigler to enter the intellectual battlefield once the essence of Sraffa's view of Ricardo and the logical implications of the latter's theory had become clear. Alas, this did not happen.

Stigler has variously been accused of being an ideologue.[45] Against this he always insisted to be a scientist. In this regard, Schumpeter's following observation is perhaps of some use. Schumpeter argued that economists are confronted with an explanandum, the economy, which is extremely complex. They cannot approach the subject without pre-scientific views, which might be called ideologies. No economist can escape this. The question is, whether and to what extent an author in the course of his scientific work is able to correct his preconceptions, transcend them or establish them firmly. Some fare better in this regard than others. Seen from this perspective, Stigler was right to see himself as a scientist, but an ideologue he still was. The fact that he unswervingly stuck to the ideal of *perfect* competition despite Sraffa's devastating criticism of Marshall's analysis of it might be seen as reflecting a strong ideological bias in his thinking. In conditions of universally perfect competition, by construction there is no economic power whatsoever. In the real world, however, there is no perfect competition, as a matter of fact. To mistake the real world for an ideal, one implicitly favours those that actually do have economic power vis-à-vis those that don't. In contradistinction, the classical concept of *free* competition, which must not be confounded with that of perfect competition, does not refer to a powerless situation and therefore lacks the ideological bias of the view that the real world is best understood in terms of a reference to perfect competition.[46]

I conclude by paraphrasing Sigler's final remark in his review article (1953: 599). We are still to receive from Stigler his view of Sraffa's interpretation and reformulation of Ricardo's theory of value and distribution. We would be prepared to wait for it with the patience which he munificently rewards, but we know that we would wait in vain.

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Notes

1. The reference is to the Royal Economic Society edition of *The Works and Correspondence of David Ricardo* in eleven volumes (Ricardo 1951–1973), edited by Piero Sraffa with the collaboration by Maurice H. Dobb, abbreviated as *Works*, followed by volume and page number.
2. The book grew out of Stigler's Ph.D. thesis of 1938.
3. In a footnote appended here, Stigler directs the reader to Knight (1935). There is next to nothing to be found in Stigler's book that would support his harsh strictures of the classical economists. In fact, setting aside a few remarks, neither Adam Smith nor Ricardo is dealt with at all. I wonder how well Stigler was acquainted with the doctrines of the classical economists and especially Ricardo at the time when he composed his thesis.
4. As is well known, because of Euler's Theorem the production technology must exhibit constant returns to scale for the product to be entirely distributed, neither more nor less of it, in terms of marginal productivities of the various factors of production. It deserves to be mentioned that Smith's concept of the division of labour (which, as can be shown, Ricardo endorsed) involves dynamically increasing returns. Also for this reason marginalist (or neoclassical) interpretations of Smith are difficult to sustain.
5. Craig Freedman informed me that Stigler had borrowed from Frank Knight the habit of sneering at mathematical formalization. Therefore, the form of Ricardo's theory can hardly have been the reason for Stigler's assessment. However, if we were to follow Jürg Niehans (1990: 105), Stigler's devoted student, it was "Ricardo's frequent inability to make his meaning clear", to which Niehans added: "However, [Ricardo] went about as far as an analytical genius could go without scientific training. Further progress was attainable only with the aid of mathematics".
6. As far as I can see, Stigler throughout his academic career stuck firmly to methodological individualism and advocated the market form of perfect competition as approximating near enough real-world conditions. With perfect competition, no economic agent has any power whatsoever. Market results do not reflect any distortions caused by economic power or control and may therefore be seen to be "just". Stigler defended this position also with regard to the literature on monopolistic competition, championed by Edward Chamberlin and Joan Robinson, and thus denied a significant and lasting influence of monopolies on income

distribution. On the treatment (and neglect) of power in economics, see Kurz (2018). Stigler's unwillingness to admit the impact of economic power on the properties of the economic system reflects a remarkable ideological bias in his analysis. On important differences—methodological and substantive—between the classical and the marginalist viewpoint, see Kurz (2016a: Chaps. 2 and 4). Here it suffices to stress that the classical economists took the existing society as it was, stratified in social classes (workers, capitalists and land owners), and did not seek to reconstruct the economy by starting from the needy individual. Methodological individualism was not a classical concept.

7. In an autobiographical talk Stigler gave, he traced his interest in Ricardo back to his studies in Chicago: "We were taught [by Knight and Simons] by example that Ricardo's errors and Marshall's foibles deserved more careful and thorough attention than the nonsense or froth of the day" (Stigler in Breit and Spencer 1988: 96).
8. He criticises, for example, Alfred Marshall for having read into Ricardo an early allusion to the notion of marginal utility; see Stigler (1965a: 75–78). Stigler also rightly insists that Ricardo "was not a Benthamite" and "did not apply the utility calculus to economics" (1965a: 75).
9. In his autobiography, Stigler (1988: 214) reported that "Jacob Viner, whose vast and honest erudition has long been my despair, once told me that the average modern reference to the classical economists is so vulgarly ignorant as not to deserve notice, let alone refutation… [F]amous economists have made breathtaking misrepresentations of Malthus on population, Ricardo on value, and so on".
10. It suffices to mention contributions by Mark Blaug, John Hicks, Samuel Hollander and Paul Samuelson on the side of the critics and Tony Aspromourgos, Krishna Bharadwaj, John Eatwell, Pierangelo Garegnani, Christian Gehrke, Gary Mongiovi, Neri Salvadori and Giancarlo de Vivo on the side of the supporters of Sraffa's interpretation.
11. Stigler has, however, very useful things to say in his methodological pronouncements on how to do the history of economic thought; see Stigler (1965a, b). In his autobiography, Stigler provides compelling arguments in favour of doing and teaching the history of economic thought; see Stigler (1988: Chap. 4). Actually, people who despise the history of economic thought ought to be reminded of the fact that publications today are history tomorrow. The cult of modernity is simply an expression of provincialism in time, as one sage observer insisted.

12. Alas, Malthus time and again found means and ways to escape the argument of the "stern logician and powerful debater" that was Ricardo (Stigler 1952: 206) by bringing in new problems or shifting the argument to some new field. Yet the validity of Ricardo's surplus explanation of profits does not presuppose a particular level of wages, but is compatible with any level from the feasible range of wage rates; see on this section 'Stigler on Ricardo's "93% Labor Theory of Value"' of the text.
13. Ricardo's theory of economic growth and development is clearly an *endogenous* theory that explains the phenomena under consideration from within the economic system and not as a response to factors given from the outside, as the models put forward by Gustav Cassel or Robert Solow more recently. Modern theories of endogenous growth seem to be unaware of this fact and are therefore bound to recapture lost territory little by little. See on this Kurz and Salvadori (2003). Not remembering important chapters of the history of economics evidently comes at a high cost.
14. Tony Aspromourgos informed me that Adam Smith appears to have had a similar idea, which is supported by his notion of "emulation" in the *Theory of Moral Sentiments*. While it is somewhat difficult to textually support this in Smith, James Steuart is very explicit about such a ratchet effect.
15. In his autobiography, Stigler (1988: 217) comes close to what has been said in the above. He writes, "that Ricardo's text is often ambiguous: Page X takes or implies one position and page Y another. That sort of ambiguity is not due simply to carelessness, for at one point he may have been thinking of the short run and at another of the long run, or at one point the focus is on another topic so the wage question is simplified to get it out of the way".
16. This argument is reminiscent of Joseph A. Schumpeter's (1912) concept of *innovations* as "new combinations" of known pieces of knowledge. Even if none of the building blocks of Ricardo's analysis had been his own invention, the combination of them represents an original novelty.
17. Ricardo stated: "I shall greatly regret that considerations for any particular class, are allowed to check the progress of the wealth and population of the country" (*Works* IV: 41). And in the same context he drew an analogy between the Corn Laws and the prevention of "all improvements in agriculture, and in the implements of husbandry". Clearly, suppressing innovations would entail a tendency towards economic stagnation.

18. Ricardo's claims have since then been rigorously shown to be correct; see Sraffa (1960) and, among others, Kurz and Salvadori (1995).
19. This shows anew the basic solidity of Ricardo's analysis. Given the limited tools at his disposal, he was remarkably successful in "elucidating economic principles" in the face of a "labyrinth of difficulties"; see Kurz (2011, 2015). The fact that the way he presented his findings was frequently less than optimal should not be received with surprise, given the intrinsic complexities of the issues at hand.
20. According to Stigler (1982: 109), Cannan "was an acute analyst as well as an erudite student of the English classical school", who however "simply could not understand a man like Ricardo". By "examining each sentence, phrase, and word with scrupulous care", he failed to see the forest for the trees. Interestingly, Stigler asks the reader to compare Cannan's (misleading) interpretation with that of Sraffa (1951).
21. The marginalist concept of perfect competition, fully flexible "factor prices" and ample opportunities of substitution between the various factors of production leads to the supposition that *all* factors, including the various qualities of land, will *always* tend to be fully utilized. No such view is to be found in Ricardo and the classical authors more generally.
22. As regards Smith's view in this regard, see the valid observations in Stigler (1952: 192). Interestingly, he calls agriculture "the basic industry", because in Smith it was the only industry whose product ("corn") was needed as an input (via wages) in the production of each and every commodity. (Sraffa [1960], as is well known, defined "basic products" as products needed directly or indirectly in the production of all products.) Different from Smith, Ricardo counted also certain manufacturing goods (coats, hats and other necessaries, but also tools, machines, etc.) as basics. But in his numerical examples he did not do so consistently, as Stigler perceptively remarks.
23. Gross income in Ricardo's system of national accounting equals net income (profits and rents) plus wages, and in his labour value reckoning equals the value added by a year's labour of a given society.
24. Marx translated Ricardo's particular case into a rising "organic composition" of capital—the ratio between dead labour incorporated in capital goods and living labour actually performed—and interpreted it as *the* form of technical progress induced by the capitalist mode of production. The labour displacing effect is taken to give rise to an "industrial reserve army of the unemployed". Marx contended that this particular form of

technical progress is also responsible for a falling tendency of the general rate of profits, whereas Ricardo had argued that technical progress counters the effect of diminishing returns in agriculture on profitability. Anwar Shaikh (2016: esp. 650–651) entertains instead the view, which he formalizes, that the tendency towards a reserve army is only exacerbated by a rising organic composition, but exists also independently of it.

25. Incidentally it deserves to be mentioned that Samuelson (1989) maintained that "Ricardo was right!" in the machinery chapter. There is no need to enter into a critical discussion of Samuelson's view on the matter; see, therefore, Garegnani (2007) and the short remark in the second part of the following section. On the machinery issue, see also, for example, Neisser ([1932] 1990) and Hagemann (2008).
26. Stigler's review article was one among several others. There were only a few authors who came close to seeing immediately the gist of Sraffa's novel interpretation of Ricardo. See, in particular, Hutchinson (1952).
27. As we have heard, already in Stigler (1952: 187) he spoke of Sraffa's "magnificent edition". Stigler's high esteem of Sraffa apparently dates back to Sraffa's criticism of Marshall's partial equilibrium analysis in Sraffa (1926); see the references to the paper in Stigler (1941: 71–73). Stigler is fully aware of the devastating implications of the criticism for Marshall's theory: "As Sraffa has pointed out, partial equilibrium analysis is completely applicable only to those economies external to the firm but internal to the industry. Here, he says, 'nothing, or virtually nothing', is to be found" (1941: 72). As regards possible escapes from the "apparent impasse" (ibid.), the first one lies in the abandonment of partial equilibrium theory and thus of Marshall's technique of analysis in favour of general equilibrium theory. Stigler warns, however, that the "notorious difficulties of application of general equilibrium theory should undermine overly sanguine hopes of thus securing useful conclusions quickly or easily" (1941: 73). "The second escape", he adds, "lies in restricting partial equilibrium analysis to those economies which are external to the firm and internal to the industry, recognizing the restricted scope of economies of this type" (ibid.). In short, Stigler basically accepts Sraffa's argument about the very limited applicability of partial equilibrium analysis. This raises, of course, the question why Stigler in much of his work, including his work on economic policy, relies almost exclusively on competitive partial equilibrium analysis without ever justifying it by entering into a critical discussion of Sraffa's analysis.

28. Stigler (1953: 586) quotes approvingly John Maynard Keynes's famous quip that Sraffa is a man "from whom nothing is hid". The perhaps crassest form of twisting the facts came from Samuel Hollander, who argued that whenever Ricardo did not explicitly use a particular (marginalist) concept, we can be sure that it was at the back of his mind and so obvious to him that he did not feel the need to even mention it. This is wishful thinking in the extreme. We have already heard how Stigler characterized Hollander's attitude.
29. William Stanley Jevons and Walras anticipated the objection that Ricardo's system was underdetermined—the number of equations was smaller than the number of unknowns to be ascertained; see Kurz (2016b, 2017).
30. I side with Stigler (1982: 4) when he stresses that preaching "is almost non-existent in Ricardo's *Principles*".
31. In his treatment of the machinery question in Ricardo, Samuelson (1989) assumed that wages are fixed at a rigid subsistence level. Unemployment therefore results in a decline of population—even in the short run. In this way labour supply is assumed to adjust quickly to labour demand. Samuelson speaks nevertheless of a Pareto optimal sequence of situations despite the fact that some people are destined to premature death. In my view Samuelson's interpretation does not reflect adequately Ricardo's position on the matter, but is caught in what might be called a Malthusian trap of the mind.
32. The main purpose of Ricardo's numerical example is to insist that the effects of a change in the methods of production and thus technical progress on relative prices are more important than those associated with a change in income distribution, given the methods of production employed. Technical change tends to reduce the quantities of labour needed in the production of the various commodities. The effects of what Ricardo calls "the other great cause [!] of the variation in the value of commodities, namely, the increase or diminution in the quantity of labour necessary to produce them" (*Works* I: 36), are "not so" comparatively slight.
33. Since we are here concerned with comparative statics, a "fall" or "rise" in price should be read as a lower or higher level of the respective price compared with what it was in the initial situation.
34. This is typically the case with regard to Marx's "transformation" of labour values in production prices, which is based on the assumption that the positive and negative deviations cancel out in the aggregate.

35. This does not mean, of course, that the labour theory of value can never be a fairly good empirical predictor of prices. Indeed, Ricardo was convinced that relative prices corresponded closely to relative quantities of labour embodied in commodities and developed his argument on the simplifying assumption of their equality, even though he knew very well that this was not strictly true. For the most recent work on empirics and the labour theory of value, see Shaikh (2016: Chap. 9).
36. Prior to Sraffa, authors such as Dmitriev ([1898] 1974) and von Bortkiewicz (1906–1907) had made attempts to re-interpret (parts of) Ricardo's theory in mathematical terms. See the entries on their contributions in volume I of Faccarello and Kurz (2016).
37. All commodities are assumed to be "basics" (see Sraffa 1960: 9–10); matrix **A** is indecomposable. For a discussion of the case with basics and non-basics, see Kurz and Salvadori (1995: Chap. 4).
38. As a vector, **d** may be interpreted as defining the elementary wage goods basket, a multiple of which gives the ruling real wage.
39. Here we assume a given real wage rate. The reader is invited to formalize the case with given proportional wages (that is, a given share of wages).
40. It deserves to be mentioned that Ricardo had formulated his fundamental law with regard to a system of production in which all means of production could, in a finite number of steps, be reduced to, or resolved in, wages. This is not true with regard to circular systems of production, as Marx was the first to point out. However, there can be no doubt that Ricardo was interested especially in circular production, but felt that the simpler case of unidirectional or linear production would lead him a long way towards a proper understanding also of the former case; for an analytical comparison of the two, see Kurz and Salvadori (1995: Chap. 6).
41. And not just on one, wages, as Ricardo typically assumed in much of his analysis of profits, and for which Marx had rightly criticized him.
42. The fact that it did not quite succeed in the second respect had many reasons. These included inter alia: the unwillingness of those that had for some time advocated different interpretations to admit that they were wrong; the need for achievement and originality of those that came up with entirely new interpretations; ideological reservations with regard to the implications of the fully worked out Ricardian theory of value and distribution; and, of course, basic misunderstandings of its

analytical complexities. Incidentally, I am not aware of any evidence showing that Stigler ever seriously read Sraffa's 1960 book.

43. It may of course be argued that people living in the same period of time are capable of understanding each other better than later generations, because they have been brought up in the same intellectual climate, had perhaps had the same teachers and have read the same books and so on. The case of James Mill, Robert Torrens and others illustrates this. There is obviously some truth in this, but it is also not uncommon that all people have tremendous difficulties in understanding each other. The discussions between Ricardo and Malthus are a case in point. The communication between two scholars who are on the lookout for different things need not exactly be easy and smooth.
44. Stigler was apparently known for typically not responding to criticisms.
45. On this issues, see also McCann and Perlman (1993).
46. Harry Bloch has insisted in correspondence with me that in fairness to Stigler it ought to be mentioned that in his work on price theory he refers to the distinction between free and perfect competition and in his applied work makes more use of the former than the latter. See, in particular, the survivor principle for determining economies of scale or the effect of concentration on price through the probabilistic approach to ascertaining cheating on tacit price coordination in his theory of oligopoly. Nonetheless, ideology is certainly important, not least in Stigler's choice of topics to research in his applied work.

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George Stigler: Marshall's Loyal but Faithless Follower

Neil Hart

Introduction

> Alfred Marshall ranks so high among the greatest figures in Anglo-Saxon economics that it is still almost presumptuous to praise his accomplishments, and indeed there is little need for doing so.… Marshall was almost incomparably superior to his immediate predecessors and his early contemporaries in the profundity and originality of his thought, and in the breadth of his vision. (Stigler 1941: 61)

The above passage is to be found in George Stigler's *Production and Distribution Theories*, the published outcome of his doctoral dissertation completed in 1937, supervised by Frank Knight at the University of Chicago. The dissertation was a critical study of the theories of distribution that emerged between 1870 and 1895, the period in which 'economic theory was transformed from an art, in many respects literary, to a science' (Stigler 1941: 1). The high esteem in which Stigler held

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Alfred Marshall as an economist did not fade with the passage of time, judging Marshall's *Principles*[1] to be the second greatest work in the history of economics, surpassed only by Adam Smith's *Wealth of Nations*. His assessment of Marshall's contribution was founded on a close familiarity with Marshall's published writings. This, for example, can be observed in Stigler's (1962) review of C.W. Guillebaud's (1961) ninth variorum edition of Marshall's *Principles*, which is somewhat critical of the way the revisions to the eight editions of the *Principles* were being documented and analysed.[2]

The context in which Stigler's interest in Marshall's economics was established and sustained is captured succinctly in Harold Demsetz's reflections on his Chicago colleague:

> Stigler, it must be remembered, belonged to the last group of economists whose training included considerable exposure to Alfred Marshall's *Principles of Economics*, and he maintained an active interest in Marshall as well as in other major economists of the past through his work on the history of thought. His private library contained many early editions of their works. I believe, but cannot document directly, that he felt strongly that the neoclassical theoretical apparatus was, on the whole, quite powerful and, after a half century's investment in its development, that it should not be abandoned lightly. Rather, he sought to strengthen it further by extending the theory and subjecting it to empirical verification. (Demsetz 1993: 794)

Stigler's highly influential expositions and assessments of Marshall's writings had the effect of according to Marshall a leading role in what Stigler perceived to be the advancement of economic analysis. However, it is argued here that Stigler should be included among a group of Marshall's followers that Denis Robertson (1952: 73) aptly termed 'loyal but faithless Marshallians', a term initially coined by Robertson to describe much of the Marshallian economics of the 1920s (emanating from A.C. Pigou in particular), where, despite loyalty to Marshall's hallowed tradition, very little of Marshall's economics in fact remained intact. The nature of Stigler's 'loyalty' to Marshall is discussed in section "Marshall's 'Loyal Follower'", through a consideration of his assessments of Marshall's role in the development of our discipline. Section "Marshall's 'Faithless Follower'" focuses attention directly on Stigler's representation of Marshall's theory of value, placed by Stigler within a competitive equilibrium setting. It is here that the 'faithless' Marshall follower can be detected. In section "Industrial Analysis: Stigler's Agenda", Stigler's depiction of these aspects of Marshall's writings is placed in the more general context of his defence of traditional price theory, along with the tactics used by Stigler to persuade and cajole the profession to embrace his line of thinking.

Marshall's 'Loyal Follower'

The enduring nature of Stigler's acclaim for Marshall can be observed directly in the concluding comments to his centenary appreciation of Marshall's work, written just over fifty years after his doctoral dissertation:

> At the time of Marshall's death, Keynes wrote that "As a scientist he was, within his own field, the greatest in the world for a hundred year" (Keynes, 1924, p. 12). Viewing Marshall with the increased objectivity that comes from the passage of 65 years and the absence of filial obligations, I find this judgement as valid today as it was in 1924. (Stigler 1990: 12)

Stigler's positive assessment of Marshall's work was not shared by some of his well-known contemporaries. Joseph Schumpeter (1954: 837), for example, argued that Marshall's theoretical structure, 'barring its technological superiority and various developments of detail', was 'fundamentally the same as that of Jevons, Menger, and especially Walras'. Paul Samuelson's judgement was that that no one could understand the history of the subject 'if it was not realized that much of the work from 1920 to 1933 was merely the negative task of getting Marshall out of the way' (Samuelson 1967: 111).[3] It is of interest, therefore, to consider those aspects of Marshall's *Principles* that led Stigler to hold Marshall in such high esteem.

In his centenary essay on Marshall, Stigler posited four major influences Marshall had on the advancement of economic doctrines. These took the form of subjects which probably would have found their way into economic analysis considerably later, and in a different form. Significantly, these four influences were selected by Stigler on the basis of the role they were interpreted to have played in the development and application of the traditional partial equilibrium theories of price determination and production, within the setting of competitive markets.

The first of the influences listed by Stigler related to Marshall's recognition of the role of time in price determination. Marshall had cautioned that the element of time is a chief cause of the difficulties in economics, making it necessary for man with his limited powers to go step by step; breaking up a complex question, studying one bit at a time (Marshall 1920: 366). This issue was resolved by Marshall, in part, by the now familiar separability of time periods. Stigler saw the significance of Marshall's time period apparatus as enabling the establishment of an 'elementary dynamics' that did not require the complex methodology necessary to deal with interdependent periods. Importantly, this permitted the construction of theories within a comparative static framework based on logical time. The one interesting qualification Stigler noted to the applicability of the time period concept was that this separability 'was possible only under conditions of competition' as a monopolist 'could not sensibly ignore the effect of present decisions upon future demands' (Stigler 1990: 6). Whether this qualification was applicable to all nature of departures from 'competitive conditions' was not addressed by Stigler. More importantly, while Stigler recognised the ingenuity in Marshall's response to the difficulties associated with the continuous nature of time, the implications confronted by Marshall arising from the irreversibility of time largely escaped Stigler's attention. As is noted below, it was the latter consideration which Marshall contended created the greatest challenge to the applicability of the equilibrium analysis of price determination, particularly when the analysis ventured beyond the short period.

The second major area of influence emphasised by Stigler was Marshall's doctrine of internal and external economies, likened by Stigler to the role played by adverbs and logarithms. These doctrines were acclaimed for the part they played in enriching theories of production and for the major role they performed in welfare economics. Marshall's formulation of the laws of return had been discussed in some detail in Stigler's earlier 1941 assessment of Marshall's work, where some of the logical peculiarities in Marshall's treatment of the laws of return are closely scrutinised. More significant is what Stigler viewed as limitations in the applicability of these concepts. In the case of Marshall's 'ambitious' treatment of internal economies, Stigler's concern was that these are emphasised so strongly that 'one finds difficulty in explaining the very existence of competition' (Stigler 1941: 76). With respect to Marshall's doctrine of external economies, Stigler found it difficult to pass final judgement, arguing that their chief role in Marshall's analysis was to explain why the great historical reduction in production costs associated with increases in output and size of the firm had not, to a large extent, been accompanied by monopolisation. However, Stigler (1941: 76) complained that the concept of external economies 'as a device for the elucidation of relative prices, it seems to have a very restricted scope'.

The interpretation Stigler placed on Marshall's treatment on the laws of return will be discussed in some detail in the following section. However, it can be observed here that Stigler is seeking to place Marshall's analysis of economies of scale in a setting that is not at odds with the objective of explaining relative prices in competitive markets. The question that needs to be addressed is whether or not this amounts to a credible depiction of the role Marshall intended the laws of return to play in his theoretical apparatus.

The third area noted by Stigler where Marshall had a significant influence is in the prominence Marshall gave to the theory of the firm; not the individual firm, but the representative firm concept, introduced 'to get around complications which arose when one studies variety of individual firms' (Stigler 1990: 7). Marshall's introduction of the representative firm is seen not to be directed to the individual firm's individual differentia, but to the role it plays as a decision unit in decentralised industries (Stigler 1990: 8). This implied that the properties of an industry could be deduced from the study of the behaviour of the representative firm, which came to be transformed into an equilibrium firm in the hands of Lionel Robbins (1928) and A.C. Pigou (1927, 1928). Importantly, this perspective led Stigler to defend what he saw as being the Marshallian tradition. He did so from the strategic aspects of theory that gained prominence with the emergence of theories of imperfect competition and oligopoly. This issue will be considered further in section “Industrial Analysis: Stigler’s Agenda” below.

The last of the four major influences attributed to Marshall was the introduction of, and emphasis upon, consumer surplus. Stigler observed that, while Marshall’s role in the refinement of the concept of consumer surplus was greeted with much controversy and thereafter much neglect, it constituted a significant step in the development of welfare economics in the form that was revived in the later work of J.R. Hicks during the 1940s.[4] Marshall’s refinement of the concept of consumer surplus was acclaimed largely because of its role in enabling the evaluation of market equilibria against welfare criteria, which had obvious appeal for those seeking fertile grounds for the application of traditional demand and supply theories of price determination. Marshall had expressed much disappointment in the usefulness of the concept he had originally referred to as ‘consumer rent’, as it appeared to be devoid of practical application because it was not capable of being quantified in a meaningful way (Guillebaud 1971: 6). Moreover, Marshall had warned that important elements of collective wealth are apt to be overlooked in estimating the dependence of well-being on material wealth, with a person’s happiness often depending more on his physical, mental and moral health than on his external conditions (Marshall 1920: 133–4). In this context, there is much merit in Steven Medema’s (2006: 246) conclusion that the unsettled nature of welfare economics over the last three quarters of a century in some ways reflects Marshall’s hesitancy about his own contribution, and there is little doubt that his concern about distributional issues would have led him to view pessimistically the efficiency—orientated turn of the field to which Stigler refers.[5]

In the concluding section of his 1990 essay on Marshall, Stigler added a further achievement that should be attributed to Marshall, in this case, credit for what he prevented:

> I believe that Marshall by his towering prestige delayed the coming of age of abstract formalism of the Lausanne tradition by at least a generation, and with the aid of his premier student, Keynes, by possibly two generations. Marshall insisted that the primary task of economics was the explanation of observable economic phenomena, and displayed impatience with theorizing which was not closely related to that task. (Stigler 1990: 12)[6]

Indeed, Stigler suggests that it is the denial of the wisdom of Marshall's position that contributed to the disappearance of his *Principles* from the contemporary literature.

Despite his high regard for Marshall, Stigler (1941: 9) nevertheless confessed to being vexed by what he described as 'a certain evasiveness in Marshall's scientific character'.[7] This was in part due to what Stigler saw as unresolved ambiguities and frustrating qualifications in Marshall's writings, many of which, as is highlighted in the following section, made more difficult the task of transferring Marshall's handy tools to what became the domain of traditional price theory.

Marshall's 'Faithless Follower'

Discussion in this section focuses on Stigler's interpretation of what is sometimes referred to as Marshall's 'reconciliation problem', characterised as the attempt to construct supply schedules under conditions of pure competition in the presence of increasing returns to scale. A solution to this dilemma was thought necessary in order to legitimise the partial equilibrium analysis of competitive markets founded on demand and supply functions. It was also the topic of much controversy during the 1920s, leading up to the symposium on increasing returns and the representative firm published in the *Economic Journal* in 1930.

According to Stigler's account, Marshall's 'resolution' to the 'reconciliation problem' took the following form:

> Of the many concepts which Marshall has contributed to economic analysis, none is in more urgent need of re-examination than the celebrated

> distinction between external and internal economies. For it is the existence of external economies, and not, as Robertson has suggested, that of the representative firm, which permits reconciliation of competition and decreasing long-run average costs. (Stigler 1941: 68)

> The doctrine of external and internal economies was a major Marshallian contribution. This classification permitted an analytical reconciliation of competition and increasing returns, and thus repaired a major gap in classical price theory. (Stigler 1990: 6)

However, it is argued here that Stigler's account amounts to a fundamental misrepresentation of the nature of the logical and methodological difficulties Marshall had been attempting to resolve in his *Principles*. Moreover, it was an interpretation conveniently crafted to align Marshall's economics with the traditional approach to pricing theory being proclaimed by Stigler. First, reference to Marshall's own writings indicates clearly that he was not attempting to protect a theory of competitive equilibrium from the damaging implications flowing from the existence of economies of scale. There were much more fundamental issues relating to the role and limitations of equilibrium analysis which Marshall struggled to come to terms with. Second, Marshall could not accept as legitimate any analysis of price and industrial organisation which explained decreasing long-run average costs purely in terms of external economies. In fact, it was Robertson who was correct in stating that it was Marshall's representative firm theory which was the *intended* vehicle by which economies of scale could be incorporated into long-run industry supply curves, irrespective of market structure.

From the onset it must be emphasised that it was never Marshall's objective to restrict his analysis to market structures that resembled later notions of pure or perfect competition. Indeed, despite the claims reported above, Stigler himself was well aware of this:

> We must remember that he [Marshall] discussed the "fear of spoiling the market" and the firms with negatively sloping demand curves in the main chapters on competition and that the only time perfect competition was mentioned was when it was expressly spurned. (Stigler 1957: 10)[8]

Indeed, Marshall was, somewhat uncharacteristically it could be said, very clear on not wanting his study of 'normal values' to be associated with 'competitive' markets:

> Another misunderstanding to be guarded against arises from the notion that only those economic results are normal, which are due to the undisturbed action of free competition. But the term has often to be applied to conditions in which perfectly free competition does not exist, and can hardly even be supposed to exist; and even where free competition is most dominant, the normal conditions of every facet and tendency will include vital elements that are not a part of competition nor even akin to it. (Marshall 1920: 35)[9]

More pointedly, from his lengthy discussion of the concept of competition in the first chapter of the *Principles*, it is clear that Marshall analysed competition from the perspective of a behavioural activity, in contrast with later definitions of competition specified in terms of market structures. Competition was a form of organisation that evolved through time, with the endless process of adaptation and re-organisation associated with economic change ensuring that market structure itself was a transitory configuration. Subsequent reconstructions of Marshall's economics based on notions of 'perfect competition' are a fabrication, lacking any validation in terms of Marshall's own writings.

Stigler's contention that Marshall assigned the key role to external economies in his attempts to reconcile increasing returns and 'competitive' equilibrium emerges directly from Pigou (1927, 1928) and Sraffa's (1926) consequential contribution to the cost controversies of the late 1920s. However, this stands in stark contrast to the conclusion presented by Marshall to Book IV of the *Principles*:

> The general argument of the present Book shows that an increase in the aggregate volume of production of anything will generally increase the size, and therefore the internal economies possessed by such a representative firm; that it will always increase the external economies to which the firm has access; and thus will enable it to manufacture at a less proportionate cost of labour and sacrifice than before. (Marshall 1920: 318)

Clearly Marshall does not wish to imply that the existence of external economies limits firm size or the extent to which internal economies may be realised. Nor does the 'external economy solution' emerge from Marshall's later discussion of returns to scale in *Industry and Trade*, where it is observed:

> But with the growth of capital, the development of machinery, and the improvement of the means of communication, *the importance of internal economies has increased steadily and fast*, while some of the old external economies have declined in importance; and many of those which have risen in their place are national, or even cosmopolitan, rather than local. (Marshall 1919: 167, *emphasis added*)

Rather, this passage clearly illustrates that Marshall, like Adam Smith before him, *intended* his investigation of increasing returns to proceed within the context of an analysis of economic progress. Marshall's depictions of economic evolution that shaped industrial organisation would have been incomplete if either category of economies was excluded from his theoretical structure. Indeed, Stigler was aware of Marshall's intentions in this regard:

> At the outset it should be emphasized that Marshall's external economies form an essentially historical category. The development of knowledge and invention, cross-fertilization, the emergence of subsidiary firms to exploit by-products and to supply equipment, the accumulation of skilled labor, all are characterized by growth. Indeed the notion of external economies may be a useful interpretive tool in economic history. For the purpose of modern theoretical analysis, however, the question must be raised; Do external economies have any importance in a stationary economy? (Stigler 1941: 71–2)

Marshall's answer to Stigler's question is plain to see in Marshall's description of the 'monotonous world' of a stationary economy:

> But nothing of this is true in the world in which we live. Here every economic force is constantly changing its action, under the influence of other forces which are acting around it. Here changes in the volume of

> production, in its methods, and in its cost are ever mutually modifying one another; they are always affecting and being affected by the character and the extent of demand. Further all these mutual influences take time to work themselves out, and, as a rule, no two influences move at equal pace. In this world therefore every plain and simple doctrine as to the relations between cost of production, demand and value is necessarily false: and the greater the appearance of lucidity which is given to it by skilful exposition, the more mischievous it is. A man is likely to be a better economist if he trusts to his common sense, and practical instincts, than if he professes to study the theory of value and is resolved to find it easy. (Marshall 1920: 368)

Contrary to Stigler, it was, as Denis Robertson had maintained, Marshall's representative firm theory which was intended to assist in the process of incorporating economies of scale within a theory of relative prices. Marshall's explanation as to why monopolisation was not the inevitable outcome of internal economies, and why large and small businesses could co-exist under such circumstances, is to be found in his well-known biological analogy that likened the growth process of firms to the growth of trees in a forest. Limits to business size arose not because of technical limits to internal economies associated with expansion, but rather because of the eventual decline in the capacity of businesses to exploit the opportunities that arose from returns to scale.[10] It was in this setting that Marshall introduced his representative firm concept, intended to depict 'in miniature' the supply curve of the industry.[11] To investigate how the industry may respond to a given change in demand, we simply analyse how the hypothetical representative firm is likely to react (Marshall 1920: 317).

However, Marshall was well aware of the limitations inherent in his long-period analysis derived from his representative firm theory:

> The Statical theory of equilibrium is only an introduction to economic studies; and it is barely even an introduction to the study of progress and development of industries which show a tendency to increasing return. Its limitations are so constantly overlooked, especially by those who approach it from an abstract point of view, that there is a danger in throwing it into definite form at all. (Marshall 1920: 461)

> It must however be admitted that this theory is out of touch with real conditions of life, in so far as it assumes that, if the normal production of a commodity increases and afterwards again diminishes to its old amount, the demand price and the supply price will return to their old positions for that amount … For, when any casual disturbance has caused a great increase in the production of any commodity, and thereby has led to the introduction of extensive economies, these economies are not readily lost. Developments of mechanical appliances, of division of labour and of the means of transport, and improved organisation of all kinds, when they have been once obtained are not readily abandoned. (Marshall 1920: 807–8)

This meant that shifts in demand schedules could not be coupled with movements along the long-period supply schedule, as it would imply that economies, once introduced, can be reversed or reconstructed. This could not be reconciled with Marshall's dynamic and evolutionary depictions of industrial organisation assembled in Book IV of the *Principles*. Consequently, while Marshall's long-period analysis could conceivably indicate equilibrium positions ex-post, it could not describe the process by which the equilibrium positions may be attained or sustained, and therefore could not explain in a meaningful way the determination of long-period values. Marshall had discovered the limitations of the static equilibrium analysis of price determination when confronted with the realities of the forces which shape industrial organisation.[12] Significantly, these difficulties are not dependent on the degree of competition in a market, and occur irrespective of whether economies of scale are internal or external to the firms within the industry.

The precise nature of Marshall's reconciliation problem was therefore associated with the difficulties in representing at a point in time the outcomes of economic events within an equilibrium framework which were irreversible and evolutionary in nature, and not, as Stigler had argued, in reconciling the existence of increasing returns with competitive equilibrium in the context of a 'stationary economy'.[13] For Marshall to be a hero for Stigler, 'economic history' had to be purged from the analysis, rendering Stigler a worthy recipient of the 'loyal but faithless' friend of Marshall's verdict. This faithlessness can be understood in the context of his fervent defence of traditional pricing theory, and the manner in which he reacted to any rival accounts of industrial analysis which were considered heretical to this tradition.

Industrial Analysis: Stigler's Agenda

Notably absent from Stigler's evaluation of Marshall's contributions to economics is any direct discussion of Marshall's *Industry and Trade*, denoted by Marshall in the introduction to the final edition of the *Principles* as 'a continuation of the present volume' (Marshall 1920: xii). Unfortunately, *Industry and Trade* did not appear in print until just prior to the eighth and final edition of the *Principles*, leading it to be largely neglected by many historians of economic analysis. However, it was undoubtedly the case that Marshall viewed *Industry and Trade* as an indispensable companion to his *Principles*, aligned with his contention that economic theorising could not be divorced from empirical observation and historical studies. The historically specific industry case studies reflected the enormous mass of knowledge of industry that Marshall had accumulated through time. However, despite receiving generally acclamatory reviews from the press and in academic publications,[14] Marshall's vision of detailed industry studies informing theoretical reasoning was not widely adopted by his followers. This is reflected in the comments by one of Marshall's loyal and faithful followers, P.W.S. Andrews:

> The abandonment of the concept of static equilibrium for the individual business… would mean some improvement since it is certainly one of the major elements in this thought-pattern which prevents an academic economist from understanding what is happening in business life… We need some different patterns of analysis. But these will have to be built on empirical grounds, just as Marshallian concepts were largely informed by their founder's studies of historical processes. No amount of spinning-out of logical chains of analysis based upon static concepts will help in this task. The need is for more empirical studies, and for the cooperation of business men and academics in their making. Before such cooperation

> can be fruitful, however, economic theory must not be positively wrong in its approach, and I would conclude this paper by suggesting that that was the negative effect of the inter-war rejection of Marshall. (Andrews 1951: 172)[15]

Clearly, the ideal of applied industrial analysis informing theoretical analysis of price and production that may have been attributed to Marshall had, in the main, faded far into the distance by the time Stigler had entered into his postgraduate studies. In his defence of the 'empty economic boxes' rumblings of the 1920s, Pigou (1922, 1924) had insisted on the need for a clear distinction to be made between the analytical requirements of value theory and the more 'practical questions' concerned with relating theoretical concepts with actual industrial conditions. This methodological position had also been emphasised in Frank Knight's (1921: 14) critique of Marshall's method, where it was argued that 'a sharper separation of the theoretical portion of economics from the empirical portion is required'. Within mainstream economics, Marshall's methodological perspective had been forsaken, and the way appeared to have been cleared for the domination of pricing theories based on competitive markets divorced from the 'complications' that applied industry studies would seek to present.

The major challenge to competitive price theory emerging from the Marshallian value theory debates of the 1920s was the subsequent emergence of theories of imperfect/monopolistic competition, implicit in the writings of Marshall, and made explicit in the constructive suggestions contained in Sraffa's famous 1926 *Economic Journal* article. Sraffa (1926: 542) had called on the profession to 'abandon the path of free competition and turn instead in the opposite direction, namely, towards monopoly', arguing that 'many of the obstacles which break up that unity of the market which is the essential condition of competition are not of the nature of "frictions," but are themselves active forces which produce permanent and even cumulative effects'. These theories, developed from the initial contributions made by Joan Robinson and Edward Chamberlin, threatened the foundations of competitive pricing theory and substituted notions of market power. Importantly, in the place of the clearly defined policy prescriptions associated with competitive price theories, the results associated with the emerging theories were contingent upon specific circumstances, and appeared to open the door for more interventionist collective and government policies that, it was feared, would 'distort' market incentives and signals.

In this setting, rather than follow the signposts provided by Marshall, and Sraffa's (1926) suggestion, Stigler staunchly defended and acclaimed the usage of the concept of perfect competition in pricing theory:

> The concept of perfect competition has defeated its newer rivals in the decisive area: the day-to-day work of the economic theorist. Since the 1930s, when the rival doctrines of imperfect and monopolistic competition were in their heyday, economists have increasingly reverted to the use of the concept of perfect competition as their standard model for analysis. Today the concept of perfect competition is being used more widely by the profession in its theoretical work than at any time in the past. The vitality of the concept is strongly spoken for by its triumph. (Stigler 1957: 17)

As observed by Demsetz, Stigler was particularly antagonistic to theoretical approaches which challenged notions of price flexibility and which emphasised the complications arising from market concentration:

> Evidence of Stigler's attachment to neoclassical price theory is also given by that part of his work mainly critical of the work of others. Price rigidity, administered price inflation, the theory of monopolistic competition, and X-efficiency were prominent targets, and each of them denied the efficacy of the neoclassical analytical framework. (Demsetz 1993: 800)[16]

In terms of the construction of Joan Robinson's model of imperfect competition, Stigler (1949: 12–3) was able to conclude that her 'message was in no sense revolutionary, although at times her language was rebellious', and that 'her volume marks no break with the tradition of neoclassical economics'.[17] Stigler's perspective here is not surprising, given that a prominent feature of the Cambridge approach was the widespread usage of Pigou's equilibrium firm construct endowed with 'U'- shaped long-run average cost curves. Stigler's (1942) highly influential *Theory of Competitive Price* provided a synthesis of much of the work of the 1930s in a form which enabled partial equilibrium analysis to arrive at deterministic results for both the individual firm and the industry.[18] Importantly, in the setting formalised by Stigler, perfectly and imperfectly competitive industries could be analysed with the same set of analytical tools. The extent to which these models had departed from Marshall's approach was clearly enunciated in Joan Robinson's reconsideration' of the *Theory of Imperfect Competition*:

> It seems to me now that I took a wrong turning. Professor Pigou had long since worked the hard core of Marshall's analysis into a logical system of static theory. Instead of abandoning the static analysis and trying to come to terms with Marshall's theory of development, I followed Pigou and worked out the *Economics of Imperfect Competition* on static assumptions. (Robinson 1951: vii–viii)

Chamberlin's model of monopolistic competition represented a much more serious threat to the supporters of traditional price theory apparatus, as indicated by the '*A Re-orientation of the Theory of Value*' subtitle attached to Chamberlin's volume. Rather than treating perfect competition as an ideal, Chamberlin combined elements of monopoly with competition, where price, quality and advertising were all strategic weapons and choice and diversity were components of economic welfare. Stigler (1949: 13) viewed Chamberlin's contribution as 'revolutionary' and as a call to 'throw off our theoretical heritage', and launched a concerted and at times vitriolic attack on the monopolistic competition theory. The thrust of Stigler's opposition to Chamberlin's theory was that it added things that were 'realistic', but that these elements were 'part of a theory that didn't tell us anything that was seriously interesting, different, or more insightful that the abstract theory of perfect competition' (Stigler 1988: 10).[19] His dismissal of Chamberlin's approach was stated perhaps most acrimoniously in the sardonically titled 'monopolistic competition in retrospect', comments 'reinforced' through some rather disapproving references to Marshall's method:

> Professor Chamberlin's failure to construct an analytical system capable of dealing informatively with his picture of reality is not hard to explain. The fundamental fact is that, although Chamberlin could throw off the

> shackles of Marshall's view of economic life, he could not throw of the shackles of Marshall's view of economic analysis. Marshall's technique was appropriate to the problem set to it: it deals informatively and with tolerable logic with the world of competitive industries and monopolies. But it is lost in the sea of diversity and unsystematism, and Chamberlin is lost with it. (Stigler 1949: 22)[20]

As has already been established, Marshall had rejected the usefulness of static equilibrium analysis based on 'perfect competition', while pure monopoly was considered to be a special case. Chamberlin had indeed ventured into Marshall's territory, with downward sloping demand curves facing individual firms, and where 'diversity' was at the forefront of industrial organisation and consumer behaviour.

Stigler's defence of traditional pricing theory from the monopolistic competition challenge was accompanied by a similar attack on the relevance of the implications arising from the work of Hall and Hitch (1939) and Sweezy (1939). Importantly, these contributions not only appeared to challenge the relevance of conventional pricing theory, based on profit-maximising behaviour on the part of individual firms, but also posed questions relating to the applicability of the equilibrium concept used in traditional pricing theory.[21]

However, it is Stigler's (1947) 'kinky' reformulation of these theories that now characterises the textbook renditions, where the kinked demand curve model has simply been relegated to the role of describing price rigidity in oligopolistic markets. The kinked demand curve has been transformed in such a way that its role and usefulness can be evaluated in terms of its connections with more orthodox theories of pricing behaviour:

> There is then no particular need for a special theory for so limited an explanation: literally no scientific function is now performed by the kinked demand curve theory that would not equally be supplied by the simple argument that price changes cannot be made without cost. (Stigler 1978: 200)

In this setting, the Hall and Hitch, and Sweezy's accounts are largely superfluous, given that 'price rigidity' can be 'explained' in terms of temporary frictions or externally imposed constraints that hinder what would otherwise be the operation of the forces of demand and supply in freely operating markets. In such a setting, a general theory of pricing can be 'preserved' without recourse to case study deliberations. Consequently, the complications arising from the treatment of history and uncertainty inherent in the original Hall and Hitch, and Sweezy's analyses are conveniently avoided. These theories, along with other contending theories were essentially dismissed as being unnecessary. They were rejected as being incapable of challenging the relevance of traditional theories of price determination:

> The economic writings in the earlier muckraking period and in the period following Gardiner Means's influential studies were most economical in their use of economic theory. The 'theory' of price rigidity, for example, was primarily an assertion of an empirical fact, not a practice explicable by ordinary profit-maximizing theory. (Stigler 1992: 456)

Likewise, the emergence of Game Theory as a rival analytical structure to describe interdependent pricing strategies among firms was dismissed as being barren, as it was incapable of yielding generalisations and empirical applicability:

> The more recent development in industrial organization is the emergence of game theory, which has dominated the writings of the younger economists in the major eastern schools and Stanford. This literature is closely related in spirit to Chamberlinian economics: It is much more rigorous (as well it should be, fifty years later) but has not shown equal gains in empirical motivation or empirical applicability. (Stigler 1988: 168)

As Demsetz (1993: 800) observed, Stigler could not abandon the traditional set of tools which economists had created 'for an untested set that elevated negotiations between individuals to the level of a central paradigm of thought.' Stigler's references to 'empirical motivation' and 'empirical applicability' are again of interest, given that he had claimed that his 1947 appraisal of the alternative theories was stimulated more by a growing interest in the empirical testing of theories than by the intrinsic interest of the kinked demand curve. However, as Craig Freedman (1995: 205) observed in his critique of Stigler's 'mythology', the method of 'testing' of economic theories, as described by Stigler at least, appears anything other than 'scientific':

> The computer has made it easy to fish for results. If the statistical analysis doesn't come out "right" the first or the twentieth time, one can drop a year from the data, add a new variable to explain contradictions, take the logarithm of another variable, and so on until, lo, the desired answer appears-all in just a few minutes. (Stigler 1988: 116)

Stigler's views on 'empirical motivation' stood in stark contrast to Marshall's careful empiricism, founded on a profound knowledge of facts about industry that could breath life into otherwise barren economic theorising.[22] Stigler was at the forefront in the development of a theory of the firm described in the following terms in Denis O'Brien's insightful commentary on Marshall's approach:

> It is apparent in their approach to the theory of the firm after Marshall economists, especially in Britain, were seeking a Philosopher's Stone – the gold of clear cut and indisputable welfare conclusions derived from an analysis devoid of empirical content … Marshall's warning about the danger of trying to find a value theory to be simple was disregarded; the search was on for a universal model which would make empirical enquiry completely irrelevant. Even the vital role of information was discarded – firms, their conduct entirely determined by their situation, were assumed to enjoy costless omniscience. (O'Brien 2006: 631)

Clearly, the approach to industrial analysis being advocated, marketed and defended by Stigler bore scant resemblance to that which had been advocated by Marshall.

Concluding Comments

Despite his stated loyalty to Marshall's hallowed tradition, Stigler's Marshall bore very little resemblance to the author of the 'second greatest work in the history of economics'. For Marshall to be a hero to Stigler, some significant elements of Marshall's economics had to be recreated by Stigler in such a way as to concur with his own vision of what constituted positive developments in economics as a discipline. Consequently, Stigler's Marshall became a very marketable commodity among those working within the confines of traditional theories of industrial activity, those founded on notions of the coordinating role of competitive market forces.[23] However, as highlighted in section "Marshall's 'Faithless Follower'", Stigler's misrepresentation of Marshall's value theory involved a distortion of Marshall's perspective on key issues such as the nature of competition, the role of returns to scale, and the dynamic or evolutionary dimensions of market structures. Stigler's fervent defence of traditional price theory and industrial organisation clearly embraced a methodological approach to economic analysis that was fundamentally opposed to that advocated by Marshall. This was made abundantly clear in Stigler's introductory comments to his 1941 evaluation of Marshall's theory of price and distribution:

> It may be well to discuss briefly two important characteristics of Marshall's work which, from the present view point, serve to diminish his contribution to theoretical economics. A first generalization is that Marshall was so concerned with historical economic developments that he had relatively small patience with the theoretical economics of a stationary state. Almost every important subject in the *Principles* receives its exposition in terms of evolutionary change…No one can question the importance of historical studies, nor is it easy to deny that Marshall's treatment of difficult historical problems is masterful - vastly superior to the "analysis" of the typical economic historian. But the question of expediency is basic. Was it expedient to attempt to achieve (as Marshall did) a high degree of realism, without first establishing the very much simpler theory of stationary economics? And was it expedient to mix inextricably historical and stationary analysis in a work which was path-breaking, especially in the latter field? The writer is convinced that both questions should be answered in the negative. (Stigler 1941: 62–3)

Stigler was well aware that Marshall's 'economic biology Mecca' was incompatible with his own agenda to promote, market and defend the traditional theory of competitive price theory. Despite being a staunch defender of Marshall's heritage, Stigler epitomised the faithless follower of Marshall's tradition.

Notes

1. Unless otherwise noted, all references to Marshall's *Principles of Economics* refer to the eighth edition as published by Macmillan in 1920. References to C.W. Guillebaud's *Notes to the Ninth Variorum Edition* (vol. 2) are cited as Guillebaud (1961).
2. Stigler concluded that Guillebaud's work on the variorum edition of the *Principles* was 'disappointing':

   > It is difficult to judge how well Guillebaud performed this task, short of an equally laborious reproduction of large parts of it. My impression is that the performance is tolerable but not first class, and it is based upon the fact that, in a fair proportion of the (few) instances I have checked, the performance is defective. (Stigler 1962: 282)

   His familiarity with Marshall's *Principles* is further witnessed, for example, with his reference to a passage in the third edition of the *Principles* in his correspondence with Friedman (Letter from Stigler to Friedman, dated December 5, 1949, in Hammond and Hammond [2006, 104]).
3. In his correspondence with Stigler, Samuelson made it very clear that he did not share Stigler's assessment of the significance of Marshall's role in the advancement of economic analysis:

   > I recently reread all of Marshall for the 1890 Centennial… I did not begin with a high opinion of Marshall (he was overpraised by my teachers and I rebelled). But I ended with a diminished opinion. He shows no development, and this from a very unsatisfactory 1880 state. He never got partial equilibrium right: not only did he fail ever to work out the demand functions for independently-additive utilities, he never shows that he knew how to do so. He simply stopped working at his mathematical economics (Letter from Paul Samuelson to George Stigler: April 11, 1990)
4. On welfare economics, Stigler (1990: 6, n9) was critical of Pigou's failure to acknowledge the inspiration provided by Marshall. By way of contrast, Hicks (1941/1981: 101) called consumer surplus 'the most striking novelty of the *Principles*'.

5. Stigler went on to list and describe briefly some additional areas where Marshall was deemed to have made significant contributions, including in what has become known as human capital theory, and in international trade analysis. Marshall's work on 'monetary economics' was also noted, particularly in the area of the demand for money. Here Stigler describes the significance of Marshall with reference to the 'strong influence' it had on his disciples, and on Keynes in particular. Stigler also notes some 'mistaken claims' made on Marshall's behalf, arguing, for example, that 'If Marshall had never lived, the roles of supply and demand … the mutual determination of values would have been generally acknowledged in early-twentieth-century economics' (Stigler 1990: 3). He is also critical of aspects of Schumpeter's 'curious and even elusive' appraisal of Marshall, including the 'extravagant claim' for Marshall 'as the father of econometrics'.
6. Marshall expressed his distaste for 'pure theory' on a number of occasions, including in the following correspondence with William Hewins (Director of the London School of Economics from 1895 to 1903)

   > It seems strange to me to be asked my views as to the study of pure economic theory; as tho' that were a subject on wh[ich] I were fit to speak. For indeed I was never a partisan of it; and for more than a quarter of a century I have set my face away from it. As early as 1873 (I think it was that year) Walras pressed me to write something about it; & I declined with emphasis. The fact is I am the dull mean man, who holds Economics to be an organic whole, & has as little respect for pure theory (otherwise than as a branch of mathematics or the science of numbers), as for that crude collection & interpretation of facts without the aid of high analysis which sometimes claims to be a part of economic history. (Marshall to Hewins, 1899, in Whitaker 1996: II, 256)

7. Stigler's early mentor, Frank Knight, had made similar complaints regarding Marshall:

   > But Marshall himself has adopted a cautious, almost anti-theoretical attitude toward fundamentals; he refuses to lay down and follow rigidly defined hypotheses, but insists on sticking as closely as possible to concrete reality and discussing "representative" conditions as opposed to limiting tendencies. The gain in concreteness and realism is in our opinion much more than offset by the obscurity, vagueness, and unsystematic character of the discussion, the inevitable consequence of burying fundamentals in a mass of qualification and detail. (Knight 1921: 15)

8. Indeed, when manufacturing industries are being discussed by Marshall, we enter the territory later to be termed imperfect or monopolistic competition, with Marshall drawing a distinction between 'general' and 'particular' markets, with 'general' markets being portrayed as the sum total of the 'particular' (or 'special') markets of the firms in the industry:

> This may be expressed by saying that when we are considering an individual producer, we must couple his supply curve – not with the general demand curve for his commodity in a wide market, but – with the particular demand curve of his own special market. And this particular demand curve will generally be very steep; perhaps as steep as his own supply curve is likely to be, even when an increased output will give him an important increase of internal economies. (Marshall 1920: 458n)

9. Similar cautions can be found frequently in Marshall's *Principles*, most notably in chapter III of Book V, where it is explicitly stated that 'of course Normal does not mean Competitive', with both market and normal prices 'brought about by a multitude of influences, of which some rest on a moral and some on a physical; of which some are competitive and some are not' (*Principles*: 347–8). In a note found in an 1886 printing of Alfred and Mary Paley Marshall's *Economics of Industry* (Marshall and Marshall 1881) annotated for an envisaged revision, Marshall commented: 'Be careful to strike out everything wh. implies that normal value = competitive value' (Whitaker 1975: 73; Becattini and Dardi 2006: 55–6).
10. Marshall became increasingly aware that the biological analogy required qualification when confronted with the existence of large-scale joint stock companies that were growing in significance as the nineteenth century progressed. The significance of this development was somewhat reluctantly acknowledged from the 6th edition of *Principles* published in 1910, where it is conceded that the 'general rule' of eventual stagnation 'is far from universal', given the 'great recent development of vast joint stock companies'. Further qualifications and alternative explanations can be found in Marshall's (1919) *Industry and Trade.*
11. The representative firm was defined in the following terms by Marshall:

> But our representative firm must be one which has had a fairly long life, and fair success, which is managed with normal ability, and which has normal access to the economies, external and internal, which belong to that aggregate volume of production; account being taken of the class of goods produced, the conditions of marketing them and the economic environment generally. (Marshall 1920: 317)

Pigou protested that the situation analysed by Marshall, where the industry as a whole was in equilibrium without individual firms necessarily being so, was 'highly complicated'. His attempts to make the analysis 'more tractable' saw the 'representative firm' transformed into an 'equilibrium firm', defined as follows:

> It implies that there can exist some one firm, which, whenever the industry as a whole is in equilibrium, in the sense that it is producing a regular output y in response to a normal supply price p, will itself also individually be in equilibrium with a regular output x_r. (Pigou 1928: 239–40)

12. It is interesting to compare Marshall's ideas here with those of Frank Knight (1921: 17):

> It is in the nature of every change in the universe known to science to have "final" results under any given conditions, and the description of the change is incomplete if it stops short of a statement of these ultimate tendencies. Every movement in the world is and can be clearly seen to be a progress towards an equilibrium.

13. Among those who participated in the Marshallian value theory debates of the late 1920s, the nature of these difficulties was most clearly recognised by Allyn Young (1928), while Shove's (1942) later evaluation is also significant. The evolutionary dimension of Marshall's economics is most clearly depicted in Raffaelli (2003), while the nature of Marshall's 'reconciliation problem' is discussed further in this context by writers such as Groenewegen (1982), Loasby (2006), Blankenburg and Harcourt (2007), Metcalfe (2007), and Hart (2012).
14. A general impression of the character of the reviews of Marshall's *Industry and Trade* can be gathered from the accounts referred to in Groenewegen (2007: 158–9). Pigou (1919: 443), for example, praised the author's 'unrivalled mastery of his subject', where 'the comparative history of many countries, the detailed technique of many industries, elaborate realistic analysis, are all welded together into an ordered whole'.
15. The rejection of Marshall's perspective on the role of industry economics has been highlighted by writers such as O'Brien (1990) and Raffaelli (2004). The essence of Marshall's approach to industrial organisation survived in the teaching and published work of a group of his pupils and closest disciples, including in particular, David MacGregor and Dennis Robertson, and later by George Richardson, another 'faithful' follower of Marshall whose work was strongly influenced by Andrews. Details on the content of these contributions can be found in Raffaelli (2004), Groenewegen (2011), Hart (2013: 114–34) and the relevant chapters in the Raffaelli et al. (2011) edited volume. The contribution by Langlois (2011) presents a particularly interesting interpretation of Marshall's work in the context of 'present-day' industrial economics, which stands in stark contrast to Stigler's vision.

16. For a detailed account of content and context of the anti-monopolistic competition (and subsequent developments) attacks led by Stigler and the Chicago School, the reader is referred to Freedman (2016).
17. Stigler's disinterest in Robinson's theoretical contribution is observable in a letter he wrote to Robert Solow in April 1961:

    > Joan Robinson is here for a visit, I had my first view of her today. You're a fool to leave Bobby and go to Corfu to see her. (quoted in Freedman [2016: 197])

18. Some aspects of the pathway towards Stigler's (1942) synthesis are outlined in Aslanbeigui and Naples (1997: 522–7). Interestingly, J.R. Hicks (1935: 10), in his survey of the work on 'monopoly', concluded that, in relation to the Robinson—Chamberlin theories 'it is probable that it does correspond with a certain region of reality. But I cannot help feeling that the application of the apparatus is implicitly much exaggerated'.
19. Chamberlin's views on Stigler's dismissal of the significance of his work on monopolistic competition are stated directly in his review of Stigler's (1946) textbook on the theory of prices, where monopolistic competition was largely ignored:

    > All this is not very different from what could be found in the textbooks twenty-five years ago. I can only conclude that the student who takes these chapters of Professor Stigler's as a guide will be off to a bad start in understanding what writers on "imperfect" and monopolistic competition have been talking about in recent years. (Chamberlin 1947: 418)

20. Stigler's views were communicated directly through his correspondence with Chamberlin, contending, for example, that Chamberlin's theory was 'indeterminate', 'not useful (often) in realistic analysis', and 'without a single application of it to a real problem' (Letter from Stigler to Chamberlin, August 1947, in Hammond and Hammond [2006: 8–9]).
21. Hall and Hitch (1939: 31) had concluded that the absence of profit maximisation and adoption instead of 'full-cost' pricing arises directly from the consequences of the existence of uncertainty: 'Only where oligopoly elements are present is the demand curve "in-determinate" in the economist's sense, but in the other cases it is unknown to the entrepreneur, and this seems to be the essential point'. Consequently, there is usually some element in the prices ruling at any time which can only be explained in the light of the history of the industry. This observation, together with its implications, is also emphasised in the concluding paragraphs to Sweezy's contribution:

> Generally speaking, there may be any number of price-output combinations which constitute equilibriums in the sense that, ceteris paribus, there is no tendency for the oligopolistic to move away from them. But which of these combinations will be actually established in practice depends upon the previous history of the case. Looking at the problem in this way the theorist should attempt to develop an analysis which will enable him to understand the processes of change which characterize the real world rather than waste his time in chasing the will-o'-the-wisp of equilibrium. (Sweezy 1939: 573)

22. Marshall's perspective here is described in the following manner by his favourite student, J.M Keynes:

    > Marshall… arrived very early at the point of view that the bare bones of economic theory are not worth much in themselves and do not carry one far in the direction of useful, practical conclusions. This requires a profound knowledge of the actual facts in industry and trade… he was naturally reluctant to publish the isolated apparatus of economics, divorced from its appropriate applications. (Keynes 1924: 34–5)

    In this context, it is interesting to observe Milton Friedman's defence of Marshall's usage of 'empirical evidence', an observation made during a lengthy correspondence between Friedman and Stigler on Marshallian pricing theory and Marshall's treatment of the laws of return:

    > You may ask, why all this fuss, when Stigler accepts the law on other grounds, namely, technological experiments. The reason is that economic empirical evidence of the kind given by Marshall is intellectually far more satisfying to an economist than technological evidence. In addition, part of my purpose is to show that Marshall here as elsewhere, was proceeding on a truly scientific basis, not on the tautological, formal basis that enervates so much of modern theory. (Letter from Friedman to Stigler, dated August 10, 1946, Friedman's emphasis, in Hammond and Hammond [2006: 24–5])

23. Similarly, Stigler (1976: 1201) acclaimed the work of Adam Smith in the following manner:

    > Adam Smith had one overwhelmingly important triumph: he put into the center of economics the systematic analysis of the behavior of individuals pursuing their self-interest under conditions of competition. This theory of the crown jewel of *The Wealth of Nations*, and it became, and remains to this day, the foundation of the theory of the allocation of resources.

    In promoting this aspect of Smith's contribution, Stigler at the same time dismissed the economic relevance of the content of Smith's *Theory of Moral Sentiments*:

    > In fact Smith's professional work on psychology (in the Theory of Moral Sentiments) bears scarcely any relationship to his economics, and this

> tradition of independence of economics from psychology has persisted despite continued efforts from Jennings (1855) to George Katona to destroy it. (Stigler 1965: 28)

For further discussion, of Stigler's' evaluation of Smith's contributions, see Freedman (2007). Young's (1928) discussion of the implications flowing from Smith's treatment of economic progress are related directly to Marshall's treatment of increasing returns and competition.

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Shattering Hope and Building Empire: Economics the Imperial Science at Chicago, George Stigler and Aaron Director

Edward Nik-Khah and Robert Van Horn

Introduction

Of the many legacies of the Chicago School of Economics, none has been more distinctive, controversial, and, frankly, misunderstood than its use of economic language to address topics traditionally within the province of noneconomists. Among economists, Chicago economists were the first in using economic models to explain the formation of families or the formation of regulation, to explain why the law assigns liability in the way it does, and much else. The accepted term for such activities is "economics imperialism" (Fig. 14.1).

Imperialistic forays in other disciplines are often understood to be seeking explanatory unification across the social sciences, and thereby

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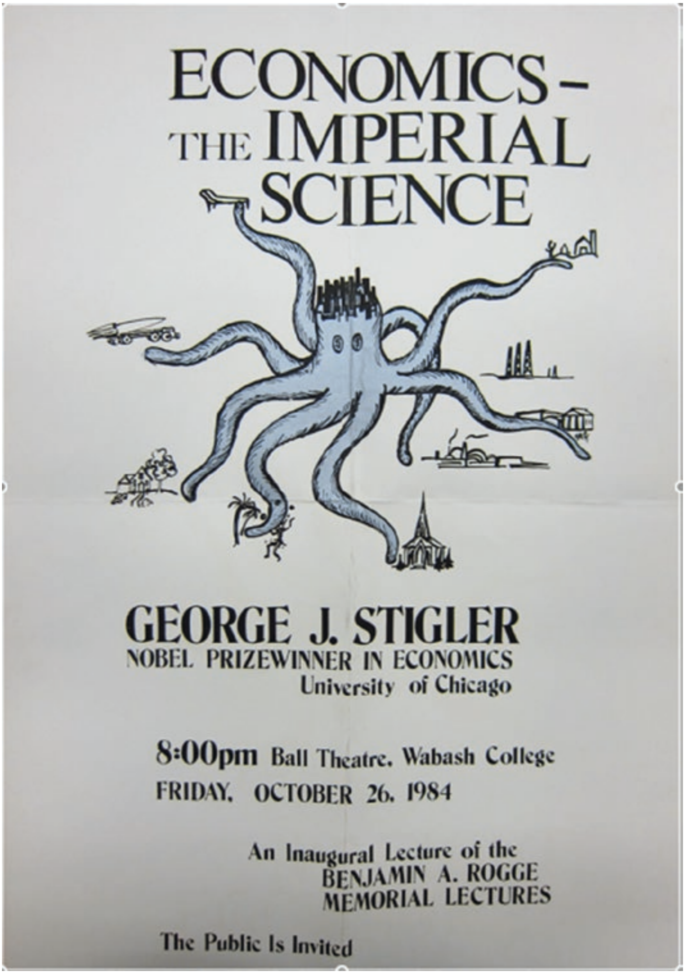

Fig. 14.1 Poster announcing George Stigler's lecture at Wabash College ("Economics—The Imperial Science?" GSRL Box 19. Reproduced with permission from the Department of Special Collections, University of Chicago)

evaluated in such a light. However, this benign gloss misses the deeper significance of economics imperialism at Chicago: it *intentionally* sought to undermine the political standing of other disciplines. As the term implies, and as this chapter argues, for Chicago imperialists, "imperialism" has the connotation of conquering: they do not make disinterested and welcomed arguments to advance academic discourse, but assert their position to conquer another discipline, and sometimes even physically take the property of another discipline so their position may prevail—we recount one such, "bloodbath," below.

It is interesting to observe that many *economists* have used the term "economics imperialism," and sometimes self referentially—especially members of the Chicago School. George Stigler wrote: "So economics is an imperial science: it has been aggressive in addressing central problems in other disciplines and without any invitations" (1984: 311). As we will see, he was no mere observer of these "aggressions." Other economists have recurrently called for "expeditionary forces," "invasion," and even "hegemony." This lexicon of imperialism used by economists begs the question: Why would anyone want to be *known* as an imperialist, especially economists who claim to value "positive economics"?

To address this question, it is necessary to turn to the genesis and rise of Chicago neoliberalism. The history of Chicago neoliberalism shows that the most bellicose among imperial economists were not primarily seeking explanatory unification. Their primary aim was not to claim the pages of the *American Journal of Sociology* or the *American Political Science Review*. Rather they were committed to a collective political and intellectual project to forge a reconstituted liberalism, a "neo-liberalism," and then to undermine any large scale opposition to it; this collective endeavor motivated their hegemonic interventions in other disciplines. They believed other disciples: (1) held views inimical to their own vision of economic and political freedom and (2) gave intellectual aid and capital to proponents of socialism and other anti-neoliberal "-isms." They agreed with Friedrich Hayek, who famously argued in the *Road to Serfdom*:

> Though for the time being the different ideals are represented by hostile nations fighting for their existence, we must not forget that this conflict has grown out of a struggle of ideas within what, not so long ago, was a common European civilization…. Though the first task must now be to win the war, to win it will only gain us another opportunity to face the basic problems and to find a way of averting the fate which has overtaken kindred civilizations. (Hayek [1944] 2007: 66)

In short, Chicago neoliberals repeatedly resorted to the language of war, because they believed themselves to be in one.

To associate the Chicago School of Economics with "laissez-faire liberalism" is wrong, and to label the Chicago School "classical liberal" is inaccurate. Once there *was* a group of economists at the University of Chicago aptly labeled such. In the interwar period, luminaries such as Jacob Viner, Frank Knight, and Henry Simons espoused classical liberalism. In the spirit of classical liberalism, Knight, Viner, and Simons eschewed intentionally creating a unified "school of thought," let alone conquering other disciplines (Van Horn and Mirowski 2009). In contrast, Chicago neoliberals sought to create a unified school of thought and displace other disciplines.

According to George Stigler, there was no "Chicago School" prior to the first meeting of the Mont Pèlerin Society (MPS), and Jacob Viner only spoke of the emergence of a "Chicago School" after WWII. Following the first MPS meeting, MPS members Stigler, Friedman, Aaron Director, and others worked to construct the Chicago School—it did not spontaneously emerge and free market forces did not dictate its configuration. Indeed, the Chicago School departed from the previous generation of Chicago scholars and reached conclusions strongly opposed to tenets of classical liberalism.

One of the reasons there has been so much confusion about the orientation of the Chicago School is that scholars and commentators have focused mostly on its members' published work. But it is necessary to look beyond the published work of individuals to understand economics imperialism—and, for that matter, to understand the Chicago School. This chapter draws from and synthesizes previous research that has made extensive use of such archival evidence, including private correspondences and unpublished manuscripts, to bring to focus often-overlooked dimensions of the Chicago School, and hence reveals the imperialistic motivations and forays of Chicago neoliberals.

Notably, the exclusive use of the published work of members of Chicago School by scholars and commentators has led to a bias toward Milton Friedman's work and ultimately to the view that Friedman was the central architect of the Chicago School. This is unfortunate because, not only is it inaccurate to label Friedman as the central architect, but also when it comes to the School's imperialistic efforts, Director and Stigler were far more important. For these reasons, we focus on their work.

What Is "Neoliberalism?"

There is confusion about the term "neoliberalism." For example, sometimes neoliberalism is equated with laissez-faire liberalism, libertarianism, and neoclassical economics; sometimes it is viewed as a new stage in capitalism; and sometimes it is seen as an insult to the Right. But none of these positions are correct.

Neoliberalism was born at a specific time, under specific circumstances, and through specific persons. The times were the 1940s. The circumstances were the perceived failure of laissez-faire classical liberalism in the face of an ascendant "collectivism." And the persons were members of the MPS.

Organized in 1947 by the Austrian economist Friedrich Hayek, the MPS was a transnational project that sought to reinvent a liberalism that had some prospect of challenging "collectivism." By "collectivism," MPS members meant not only socialism, but also social welfare liberalism. The MPS was a closed society, which offered members a forum to debate and to find mutual support.

Participants in the MPS *rejected* laissez-faire classical liberalism. They rejected it because they believed it failed to mount an effective challenge to collectivism. They were wounded by the Great Depression and its aftermath, culminating the rise of the welfare state. They believed that the ideas of socialism and social welfare liberalism bolstered the welfare state and the existence of socialism. In their view, laissez-faire liberalism failed to adequately challenge the prevalent ideologies.

Departing from classical liberals such as Henry Simons, *neo*liberals concluded that markets required political organization to be brought into existence, and required protection from the public to be maintained. To achieve these tasks, they became activists, which took multifarious forms during the ascendancy of neoliberalism. Unlike classical liberals, they denied the strict separation of economics and politics.

They sought to take over the state for the purpose of bringing into existence their version of an ideal market society and protecting it from the public. And neoliberals required a strong state to advance their program.

Moreover, neoliberals revised their understanding of what markets accomplished. They praised markets not because they gave people what they wanted—that is, for their allocative properties—but instead for their purported epistemic virtues. They reconceived markets as information processors, which produced and conveyed knowledge. Indeed, they insisted markets were the most powerful information processors ever known to humankind (Mirowski 2011; Nik-Khah 2014; Tyfield 2016).

In sum, neoliberalism was a political and intellectual project opposed to laissez-faire liberalism and distinct from neoclassical economics that sought to rethink the foundations of liberalism. It's important to grasp these features because the economists celebrating "economics imperialism" were neoliberals, and they were either members of the MPS or their comrades-in-arms. The point of economics imperialism, then, was to advance neoliberal goals, and it did so through the use of economic language.

The epicenter of such activity was the Chicago School of Economics. Why was this the case?

The Chicago Imperialistic Imperative

Economics imperialism, neoliberalism, and the Chicago School of Economics are directly connected to one another. To understand how and why, we must review the history of the genesis and rise of the Chicago School.

It is important to realize that the Chicago School and the MPS were conjoined from birth. In *The Road to Serfdom*, Hayek positioned himself as an opponent of laissez-faire liberalism. He instead championed the creation of an institutional framework, what Hayek later called a "competitive order," so that effective competition would flourish. This required a well-crafted legal framework. Hayek suggested that the task for the future would be to succeed where nineteenth-century liberals failed. Twentieth-century liberals would need to thoroughly investigate and understand the competitive order.[1]

In the United States, the Chicago based Free Market Study, or the "Hayek Project," as it was known at Chicago, carried out this objective of Hayek's (Van Horn 2014: 528). Notably, the Free Market Study was primarily the product of the persistent efforts of Friedrich Hayek. In April 1945, when on tour in the United States promoting his recently published *The Road to Serfdom*, Hayek met with Harold Luhnow, head of the Volker Fund. Luhnow wanted Hayek to write an American version of *The Road to Serfdom* and offered him Volker money to do so. The two men agreed that the Volker Fund would finance an investigation of the legal foundations of capitalism and that a product of this investigation would be an American *Road to Serfdom*. The two also agreed that Hayek could outsource the investigation.

By May 1946, Hayek successfully arranged for what came to be called the Free Market Study to be housed at—note well—the Chicago *Law School*, and its members included Milton Friedman and Edward Levi (Van Horn and Klaes 2011). He also arranged for the economist Aaron Director to head the project. Once the Study got underway in the fall of 1946, its members convened regularly in order to debate how to reconstitute liberalism and create a competitive order. During the Free Market Study, Director and other members departed from classical liberalism. For example, whereas classical liberals expressed concern about concentrations of business power, the Free Market Study relinquished such concerns. Director claimed that large corporations no longer should be considered a threat to competition. Instead, they should be considered another feature of a competitive market. Hence, the Free Market Study served as an incubator for a new form of liberalism, "Chicago neoliberalism."[2]

Notably, the rise of neoliberalism occurred when Director viewed collectivism as a real and dangerous threat (1951). Director held the same position when he agreed to head the Free Market Study. For example, in his review of Hayek's *Road to Serfdom*, Director extolled Hayek and Hayek's portrayal of the ideas that gave rise to socialism: "There is no economist writing in English more eminently qualified to do this job. In addition to his unique personal experience…and his great repute as an economist, Professor Hayek is our most accomplished historian of the development of economic ideas" (1945: 174). Like Hayek, Director believed that it was essential to win the war of ideas, and he believed that pernicious government intervention would eventually lead to totalitarianism. Director saw arguments that portrayed the harmless actions of businesses as illegal to be deleterious because they supported government intervention that undermined the competitive order, leading to more collectivist policies.

On the heels of the Free Market Study, Director and Levi organized and led the Antitrust Project. The funding came from corporations and the Volker Fund. The Antitrust Project focused on issues of monopoly and select areas of antitrust law. It investigated these topics in the light of the conclusions of the Free Market Study. And the Antitrust Project extensively trained noneconomists (as well as economists) in economic thinking. Imperialism would now become a characteristic activity of the Chicago School.

The Antitrust Project attacked the conventional wisdom of the legal profession in a number of ways. To take a couple of examples: Robert Bork claimed that vertical mergers did not enhance monopoly power; he therefore suggested that they should always be legal. Ward Bowman examined other practices (such as tying arrangements) that had conventionally been viewed as engendering monopolistic exploitation. He argued that the conventional legal wisdom grossly exaggerated such effects.

It is important to emphasize that the Antitrust Project served to train and educate the next generation of Chicago neoliberals. Director, with the support of Levi and private funding, educated the first generation of Chicago neoliberal lawyers, including Bowman and Bork. Not only did they publish in the spirit of Director's teaching, but since they acquired jobs in other law schools, they also obtained the financial resources that would have otherwise gone to a traditionally educated law professor. In the case of Bork and Bowman, they both found positions at the Yale Law School. Along with Richard Posner and other Chicago lawyer-economists, they helped Chicago law and economics become one of the dominant schools of jurisprudence in the 1980s.[3]

The Free Market Study and Antitrust Project also served to inspire empire builders of Chicago law and economics—for example, Henry Manne. Manne graduated from the Chicago Law School in 1952, and learned about how to apply economic reasoning to legal issues from Director. He received an LLM and J.S.D. from Yale in 1953 and 1966 respectively. His research focused on the economics of corporate law, and contained key premises advanced during the Free Market Study and Antitrust Project. His research culminated in his book, *Insider Trading and the Stock Market* (1967). Echoing the work of Director, Manne wrote:

> The market for corporate control serves an extraordinarily important purpose in the functioning of the corporate system. Unless a publicly traded company is efficiently managed, the price of its shares on the open market will decline, thus lowering the price at which an outsider can take over control of the corporation…It conditions managers to a specific point of view perfectly consistent with the shareholder's interest, to wit, keeping the price of the company's shares as high as possible. (1967: 265–266)

Notably, Manne stated that the legal profession derided him and his work. He retrospectively reported:

> In 1966, shortly after my book, *Insider Trading and the Stock Market*, was published, I was introduced, for the first time, to Harvard Professor Louis Loss, then the reigning guru of corporation and securities law. Without even acknowledging the introduction, he simply said: 'We didn't need a book on insider trading. I know it's bad' and walked away. (2005: 327, fn 3)

Manne claimed that at this time law schools primarily housed hostile opponents of Chicago law and economics. He wrote: "The mandarins of academic law all knew very well that if the economics came out of Chicago, it must be ideology masquerading as scholarship" (2005: 310). The historical context of the late 1950s and the early 1960s suggests there is validity to Manne's observation. For example, the legal scholar Herbert Packard deemed Director and Levi's 1956 article the "manifesto" of the "Chicago school of antitrust thought" (1963: 55–56). Frustrated by the legal profession's reception of his Chicago neoliberal ideas, Manne turned his attention primarily to empire building from this point onward, which Manne suggests was necessary to make Chicago law and economics acceptable in the field of law.

In 1968, Manne went to the University of Rochester where Allen Wallis, MPS member, and an important architect of the postwar Chicago School, was president.[4] Among other administrative duties, Manne focused on correcting "the economic illiteracy of law professors" and changing the perception of the law professors that Chicago law and economics was ideology (Manne 2005: 312). Manne designed and orchestrated a summer program in economics to educate law professors—Manne reported that he would stridently insist he taught "microeconomics," not "ideology." In practice, the program served the following purpose: "I also had a hidden agenda. In time, the bar, the bench, and perhaps even (one can dream) the legislature, might grasp the simple but powerful lessons of open markets, free contracts, and private property if we could reach their teachers" (2005: 313). To fill the first class, Manne paid each attendant and provided some sumptuous meals—he said a form of bribery was necessary to ensure attendance during the early years of the summer program. For the five years the program ran at Rochester, Manne carefully selected the students and depended on corporate sponsors for funding. Besides training law professors, the summer program had another impact:

> Large corporations usually had professional economists on their staffs, though they were rarely used in legal matters. But as corporate lawyers and economists became more aware of the academic development discussed here, and of each other, their relationship began to change dramatically. It is a little-known aspect of the whole law and economics movement that it greatly encouraged broad ranging cooperation between professional economists and lawyers, both inside and outside the corporations. (2005: 315)

Notably, Manne continued work in the spirit of his Rochester summer program after he left; he implemented similar programs while at Emory and Miami and broadened his focus to include a seminar for judges and the creation of centers for law and economics, to name just a couple of his other empire building accomplishments. In the mid-1980s, Manne's empire building culminated in a "bloodbath":

> Of course, I did not tell them – and apparently they did not figure out – that I planned not to renew the contracts of any of the untenured professors. And I would use the money that [President George] Johnson promised – and whatever other means I could dream up – to terminate the contracts of as many tenured professors as I could. There was no other way to start the new kind of law school that I wanted. …By the end of my first academic year at George Mason, 14 members of the old faculty were gone and several more were starting the process of leaving. The faculty was not happy with this turn of events; a 'bloodbath' I believe they called it. As Johnson said later, 'I might have thought that it was possible to get rid of 14 professors in one year, but not without a lawsuit.' (2005: 325)[5]

In sum, the permutations of his empire building served to bring about the welcome reception of the ideas of Chicago law and economics by many lawyers and jurists and hence to facilitate economics imperialism in the field of law. One lesson of the history of Chicago neoliberalism is that just because law professors and jurists welcomed Chicago law and economics does not indicate that no form of imperialism occurred. Hence Robert Bork's *The Antitrust Paradox* (1978)—to name just one scholarly work linked to the Antitrust Project—became widely accepted by many in the legal profession and ultimately impacted Supreme Court decisions partly because Manne created institutions and scholarly networks to bring about its acceptance.

To conclude this section, we turn to some of the distinguishing characteristics of Chicago neoliberalism in the field of antitrust law. While the work of the Antitrust Project shares some superficial similarities to work on antitrust law completed at Chicago prior to 1946, there are key differences. First, the Antitrust Project was consciously organized. Before 1946, economists and law professors at Chicago had not undertaken the work on antitrust law as part of a project. Second, the Antitrust Project extensively trained noneconomists (as well as economists) in economic thinking and these noneconomists obtained positions in other law schools, where they conducted research in the spirit of the Antitrust Project. No such extensive training of noneconomists occurred prior to 1946 and, therefore, Chicago scholars could not exert direct influence on legal studies at other schools. Third, the desire to countervail collectivism provided a central motivating factor for the work on antitrust law at Chicago, leading to the publication of a number of journal articles, most of which were in law journals and many of which forcefully attacked the conventional wisdom of antitrust law. Prior to 1946, no such concentrated assault on the conventional wisdom of law by economists and law professors at Chicago occurred.

Science, Democracy, and Chicago

In 1958, Chicago launched a second major imperialistic offensive. Allen Wallis (with the support of another MPS member Leonard Read) arranged with the University of Chicago for George Stigler to assume an unusually powerful position: the Walgreen Chair, which came with control of a substantial research budget. Stigler was located in the Graduate School of Business, thereby extending the neoliberal project to the business school, and establishing a second staging area for imperialistic excursions.[6]

Shortly after his arrival at Chicago, Stigler announced his intention to devote the Walgreen resources to a study of what he called the “causes and effects of governmental control over economic life.” Stigler himself contributed studies of the regulation of electricity and securities and of the enforcement of antitrust laws and financed through Walgreen the work of several others. He recruited to Chicago the leading neoliberal economists Gary Becker, Sam Peltzman, and Robert Lucas and financed short stays for other sympathetic economists.[7]

Like Director, Stigler motivated the “governmental control” project by appealing to the need to counteract collectivism. One can find representative products of this project in the 1988 volume *Chicago Studies in Political Economy*, which collected some of the most celebrated pieces from this self-described “Chicago” approach to the study of politics. Topics ranged from the uses of primitive “law” as an instrument for wealth maximization to an evaluation of the efficacy of mattress flammability standards, and the volume contained digressions to consider such questions as the circumstances under which monarchy is more "efficient" than democracy. Relative to their classical liberal mentors, the terrain that Chicago now covered was vast.

The topic of democracy was of utmost concern to economics imperialists. Time and again, Chicago neoliberals identified the democratic public as a threat to their objectives to establish a "free" market. But they felt that professors in other disciplines (and, for that matter, economists in non-neoliberal economics departments—which is to say, most economists) posed a similar threat. According to Director, intellectuals—particularly legal scholars like Alexander Meiklejohn—were too optimistic about the prospects for democratic deliberation (Director [1953] 1964; Van Horn and Emmett 2014). Director argued that people should have *less* confidence in democratic discussion, and *more* in economic exchange. He suggested instead that the majority rule of democracy was a necessary evil that needed to be minimized. He expressed deep skepticism of economic policies based on consensus, where the voices of all perspectives had been taken into account. Hence, Director suggested that the legal framework necessary for effective competition, such as antitrust policy, should not be based on a mature consensus through democratic discussion, but rather on economic analysis, particularly of the Chicago neoliberal hue.

Stigler shared Director's skepticism toward the judgment of the public and the professors. His project sought to find determinants of the growth of government and regulation, and it developed pressure group models. One of the most important contributions of such studies pertained to the notion of "capture": politics was treated as a market, and hence was an expression of private interest. This being so, groups with intense interests in policies would tend to prevail, often to society's detriment. For example, the material interests of producers would usually triumph over those of consumers. In Stigler's view, most professors did not appreciate such problems with democracy. But even if they did come to appreciate them, they would fail to arrive at a correct appraisal of markets. Quoting Stigler:

> The intellectual has never felt kindly toward the marketplace. Whether this intellectual be an ancient Greek philosopher…or whether this intellectual be a modern man…the basic similarity of view has been pronounced. (1963: 85)

Stigler elaborated:

> I cannot believe that any amount of economic training would wholly eliminate the instinctive dislike of a system of organizing economic life through the search for profits. It will still appear to many intellectuals that a system in which men were driven by a reasonably selfless devotion to the welfare of other men would be superior to one in which they sought to their own preferment. This ethic is deeply imbedded in the major religions. (1963: 94–95)

Opposition to markets was a holdover from earlier times, embedded in peoples' most cherished beliefs, and therefore very difficult to eradicate. Even the most learned were not immune. One could find it in the work of political scientists and economists, which presented an unrealistic view of what democracy could accomplish, and uncritically adhered to the belief that regulation was sought for the public interest. This tainted existing studies of democracy and regulation.

Hence, Stigler, like Director, believed that the professors, too, supported collectivism. Stigler, Director, and other Chicago neoliberals felt *themselves* to be a besieged minority. In an MPS address, Stigler spelt out the consequences of this view:

> Affairs of science, and intellectual life generally, are not to be conducted on democratic procedures. One cannot establish a mathematical theorem by a vote, even a vote of mathematicians. [Therefore] an elite must emerge and instill higher standards than the public or the profession instinctively desire.
>
> The best econ[omics] in the US is not the one the public would elect: a science must impose the standards of an elite upon a profession.[8]

Economics imperialists did not intend to privilege economics as such. They were trained as economists, but they were committed to the neoliberal project. And from the latter perspective, economists could be just as troublesome as political scientists or legal scholars. Instead, the imperialists wished to privilege only a *specific form economic analysis*—one informed by Chicago neoliberal ideals.

The goal was to undermine the ideal of the state governed by democratic consensus. In an unpublished memo, Stigler proposed that one way to address the democracy problem would be to contribute skeptical studies of democracy: such work would, in his words, "shatter the fond hopes of the scholarly professions."[9] Many studies that fell within the "governmental control" project—studies of the determinants of the size of government, pressure group models, economic theories of political agency, historical studies of the origins of popular regulatory policies, to name but a few—served this end.

But to "shatter fond hopes" would, at best, happen only in the very long run; moreover, it provided no guidance concerning what actions the state should take in the interim. And so, Stigler launched a complementary offensive. He called for a formal program that would enlist Chicago expertise in the service of addressing current economic regulation. At first, Stigler himself produced studies of regulation that seemed to demonstrate that it had no effect—a conclusion that served the "shattering fond hopes" goal well enough. But he and his colleagues quickly relinquished this idea, and began to portray regulation as *improvable*. Improvable, that is, via the guidance of Chicago neoliberal analysis. He sought to develop for regulators a set of what he called "intelligent guides," and subject regulators to performance audits (Stigler 1973). He called for the development of new techniques of quantitative measurement of the costs and benefits of alternative arrangements of regulation, and set members of his project to the task.

Here, the goal was to undermine the standing of the disciplines informing science-based regulatory policy. The best example of this type of study concerned the performance of the US Food and Drug Administration (FDA; see Nik-Khah 2014, 2016). In 1972, Stigler and his cohort brought together Chicago neoliberals, clinical scientists, and members of pharmaceutical corporations to challenge the status quo regulatory view held by the FDA. These corporations included Pfizer, Smith Kline & French, Searle, Merck, and Upjohn. The most famous product of this conference was Sam Peltzman's study of the "costs and benefits" of drug regulation (Peltzman 1973). He concluded that drug regulation, which had been tightened in the wake of the Thalidomide disaster, had reduced the public's knowledge about (and hence appetite for) new drugs. The solution, Peltzman insisted, was to allow the marketplace to determine which drugs should be used. The solution was actually more Thalidomides: "we don't have enough thalidomide tragedies in the United States today" (Peltzman, quoted in US Senate Subcommittee on Monopoly 1973: 9831).

Although the conclusions of this study supported the "shattering hopes" goal, it also served an additional—and novel—purpose: to undermine the standing of clinical science in pharmaceuticals regulation. Relying on randomized controlled trials to judge the efficacy of medicines was far more "costly" than relying upon the market. These escalated costs served as evidence of the dangers inherent in the regulator supplanting the market's rightful role in generating and promulgating knowledge. But this did not cash out as a call to eliminate the FDA, but instead to *control* it, by forcing the regulator to account for such costs in its decision-making (Carpenter 2010: 374 et seq.). *This* marked it as neoliberal.

Both the "shattering hopes" and providing "intelligent guides" forms of economics imperialism were pursued not only at academic institutions, such as Chicago's Graduate School of Business, but also academic centers that lay outside traditional departmental structures (notably the Stigler Center for the Economy and the State), as well as non-academic institutions (such as the American Enterprise Institute). In some noteworthy cases, such work came to be "applied" to, and by, regulatory bodies—one unheralded reason for the immense influence of the Chicago School of Economics.

Conclusion

In this chapter, we have sought to correct misconceptions about economics imperialism and the Chicago School of Economics. In light of the preceding history of the genesis and rise of Chicago neoliberalism, it is possible to appreciate characteristics of Chicago imperialism. First, through deliberate organized efforts, Chicago imperialists attempted to shape other disciplines, displace rival views, exert political pressure, and influence the policy approach of the state. In doing so, they substantially departed from the (heterogeneous) tradition of Chicago economics of the 1930s, which eschewed all forms of self-conscious organization. Second, it is inaccurate to solely understand the activities of the Antitrust Project as merely an exercise in Chicago price theory. During the Antitrust Project, Director sought to counter the conventional legal wisdom because he believed it supported the growth of collectivism. Stigler's program to study "governmental control" constituted a new and distinct approach, rather than mere application of a core Chicago approach to a new domain. He sought to conceptualize politics and information as subject to the market, in direct contradiction to the views held by his teacher Frank Knight, who was deeply skeptical of discovering the principles governing political life.[10] Notably, it is only through an investigation of the archival record that Director's and Stigler's motivations come into focus. Without examining the archival record, one could plausibly argue that Director simply applied price theory to the law to improve the standing of the economics discipline and that Stigler's call for "new theories and methods" in studying politics reflected a wish to benefit from open communication with political science and other disciplines. Although it would be an exaggeration to say that Director and Stigler eschewed academic persuasion, they sought far more than that, and acted accordingly.

Economics imperialism served the neoliberal end of combatting collectivism to remarkable effect. To give but a couple of examples, the Reagan Administration's appointees to the Antitrust Division of the Department of Justice echoed the arguments in Bowman's *Patent and Antitrust Law*, including his analysis of patent tie-ins. And Robert Bork's *The Antitrust Paradox* (1978), which is strongly linked to the Antitrust Project, became widely accepted by many in the legal profession and ultimately impacted Supreme Court decisions. Peltzman's argument was buttressed by complementary ones, propounded by a network of interlinked think tanks with shared memberships and interlocking directorates—all the better to deploy the infamous "echo chamber" tactic pioneered by tobacco companies (Oreskes and Conway 2010). Eventually, the enabling legislation of the FDA was amended to force the agency to explicitly account for the "costs" of regulation—in the first instance with the Prescription Drug User Fee Act of 1992, in the second instance with the FDA Modernization Act of 1997 (Ceccoli 2003). As the FDA adopted a stance of cooperation with industry in bringing drugs to market more quickly, "more Thalidomides" has become a reality. The United States has witnessed an epidemic of high-profile drug withdrawals and adverse effects from taking drugs have skyrocketed. Both Director and Stigler traced the previously flawed policies to an overreliance on democratic discussion, and blamed misdirected policies on the idiot savants—"educated" policymakers, regulators, and jurists who lacked essential training in Chicago neoliberal economic thinking. The implicit message of the Antitrust Project was that jurists untrained or uninformed by neoliberal Chicago economic analysis would steer antitrust law in the wrong direction and undermine the competitive market. Manne's later work sought to "educate" judges and lawyers so a proper policy course could be established and maintained. Stigler's work implied that regulation should be guided by neoliberal cost-benefit analysis; democratic discussion undertaken by an ignorant public would, if heeded, obstruct the acquisition of knowledge. In sum, to use the apt phrase of Will Davies, Director and Stigler ultimately sought "the disenchantment of politics by economics" (2014). Clearly, achieving such a goal would involve much more than engaging rival academic work within the confines of the conference presentation and the academic journal.

How, then, did they accomplish such momentous feats? Here, the importance of institution building and practice development assumed crucial importance. Chicago imperialists belonged to a larger epistemic community that not only spanned national and disciplinary boundaries, but also included non-academics, often located at think tanks. Think tanks operate between politics, academia, and the media. Their location within this "hybrid and interstitial position" gave them their capacity for effective public intervention (Eyal and Buchholz 2010; Medvetz 2010). For example, Chicago neoliberals staffed think tanks such as the American Enterprise Institute; in several important cases, they also founded think tank-type institutions on Chicago's campus, such as the Stigler Center for the Study of the Economy and the State and the Becker Friedman Institute (Nik-Khah 2011a, b). When compared with its rivals, the features of Chicago's epistemic community offered its scholars more opportunity to leverage ideas for world-changing action. These networked think tanks became formidable intellectual actors in their own right. They were interdisciplinary, poised for the targeted political strike, vigilant, and remarkably successful in attracting financial support from funders, styled as "philanthropies" (such as the Volker Foundation, the Bradley Foundation, and the Olin Foundation), along with numerous corporations (such as Pfizer, Proctor and Gamble, and Getty).

To neoliberals, this feature was pleasing. That people were willing to fund their activities meant that their work had received the sanction of the ultimate arbiter of truth: the marketplace. To those lacking familiarity with neoliberal epistemology, submitting ideas to a competition within the "marketplace of ideas" seemed to convey a sense of judicious pragmatism, an even intellectual playing field, a respect for diverse voices, a defense of unpopular speech, even the foundation for a mature democratic order. Neoliberals would sometimes encourage this interpretation, particularly to placate faculty who complained about the establishment of privately funded neoliberal institutes on their campuses. But behind closed doors, the true nature of this "marketplace of ideas" revealed itself. The "marketplace" they lauded was one *they* sought to construct or *they* approved; neither the populace nor the polity acting in the interest of the general welfare should be heeded. Recall the words of Stigler, delivered to a meeting of the MPS: "an elite must emerge and instill higher standards than the public or the profession instinctively desire."

Hence, Chicago scholars' understanding of the "marketplace of ideas" had nothing to do with seeking the favorable judgment of the scientific community, or the democratic public. Instead, it provided a justification for advancing on other disciplines in order to *exclude* certain perspectives. And, to neoliberals like Stigler, who understood that the advancement of a market society depended on subordinating science to the market, the "marketplace of ideas" provided the justification and ideological inspiration to do so.

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Notes

1. For more on Hayek's position described in this paragraph, see Van Horn (2013).
2. For more on the significance of the Free Market Study in this regard, see Van Horn (2009, 2011) as well as Van Horn and Klaes (2011b).
3. For a detailed look at the rise of Chicago law and economics, see Teles (2008) and Van Horn (2009, 2018).
4. Wallis's efforts were crucial in establishing Chicago's Graduate School of Business as one of the most important sites for imperialistic excursions. See Nik-Khah (2011a, 2014).
5. On the history of neoliberals' attitudes toward tenure and academic freedom, see Nik-Khah (2018).
6. One should not underestimate the boldness of Wallis's decision to hire Stigler for what was widely deemed as one of the most important positions in a leading business school. Staff at the Ford Foundation, which had awarded a large grant to advance interdisciplinary behavioral science at the Chicago GSB, expressed concern about the prospects for interdisciplinarity if Stigler accepted the position and, later, disappointment with the subsequent dominance of economics there (Fourcade and Khurana 2013: 145–146). For this reason and others (e.g., his support of Manne at Rochester, discussed above), Wallis should also be regarded as a crucial figure in the history of Chicago imperialism.
7. For a more comprehensive discussion of this project, see Nik-Khah (2011a).
8. "Comments on Rogge's 'Financing Higher Education in the United States'," GSRL Box 26, File: Mont Pèlerin Society 10th Anniversary Meeting.
9. "A Research Institute in Economics," GSRL Box 21.
10. Stigler also explicitly departed from Knight's position on markets and information: "My teacher, Frank H. Knight, used to say that in order to choose the best physician, a person would have to know how much medicine every physician knew, and if he knew that much, he would have sense enough to treat himself" (Stigler 1975: 12).

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George Stigler, the First Apostle of the "Coase Theorem"

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Introduction

Counterintuitive it may be, but we owe the "Coase theorem" not to Ronald Coase himself but to George Stigler.

In a passage in his article "The Federal Communications Commission", Coase (1959) argued that, like any other private good, and despite the risk of interference, radio frequencies could be allocated through prices, thus questioning the Pigovian tradition of externality analysis. Judging this point to be erroneous, Aaron Director, editor of the *Journal of Law and Economics*, asked Coase to abandon it, which he refused to do. He explained his point of view during an evening discussion with, among others, Director, Milton Friedman, and George Stigler. Coase's arguments convinced the participants, and he wrote "The Problem of Social Cost" (Coase 1960) to develop them further. There, he asserted that in the

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presence of externalities, if transaction costs are nil and if property rights are well defined and allocated, then agents will negotiate and the result will be optimal and independent of the initial allocation of rights. Stigler, who was converted[1] during this legendary night, assigned the name "the Coase theorem" to this assertion in the third edition of his *Theory of Price*. Yet, he stated it differently, as follows: "under perfect competition private and social costs will be equal" (Stigler 1966: 113).

This name was inappropriate for two reasons. First, Coase's (1960) article did not state a theorem but only used examples, exactly as Stigler's textbook did. Second, the main part of Coase's article explores the consequences of the introduction of transaction costs. When they are not nil, the result may no longer be optimal or independent from the initial allocation of rights, which means that other solutions (such as governmental intervention) may be less costly and that law exerts an influence on economic output. This is why Coase distanced himself from this "theorem": "It took a whole evening of all these economists to get it right. But then in the end they didn't get it right, because they amended something called the Coase Theorem, which I don't like" (Coase 2012).

The name coined by Stigler was, however, taken up by economists to examine the consequences of this idea. Beyond its criticism of the traditional analysis of externalities, the "Coase theorem" is important in economics. It calls into question the theory of market failures and reasserts the efficiency of the market; and it puts the emphasis on the necessity of introducing property rights. The "theorem" and the article which originated it became imprinted on the infant discipline of environmental economics, and contributed to the transformation of Law and Economics in the 1970s (Medema 1998, 2014). Stigler (1992: 456) wrote in this regard: "In the field of law and/or economics, B.C. means Before Coase. B.C., the economists paid little attention to most branches of law. A.C., 'The Problem of Social Cost' became the most cited article in the literature of the field, perhaps in the entire literature of economics. Law, like other social institutions, came to be viewed by economists as an instrument for the organization of social life".

The name "the Coase theorem" had appeared in a textbook, and was inappropriate; but it remained nonetheless. It remained because it transformed an intuition—newly discussed in the research literature and far from being accepted—into a theorem, giving it some persuasive power. It remained because it focused the economists' attention on the zero-transaction-costs world, a world with which they were already familiar, and which was easier to handle.

There was no demonstration in Coase's original article, and there have been multiple post-Coasean "Coase theorems" as interpretations of Coase's idea and examples, Stigler's being the most prominent. Coase made clear that the "Coase theorem" was just a first step in the analysis, and that economists should study the world of positive transaction costs. But Stigler, albeit conscious of this, helped shift the focus to the zero-transaction-costs world of the "theorem". Then the "Coase theorem" took on a life of its own, autonomous from Stigler and Coase. This story is now well known (Medema 1994; Bertrand 2010), yet it has rarely been asked what Stigler's "Coase theorem" actually was. Steven Medema (2011) has examined the evolution of Stigler's attitude toward externalities and the "Coase theorem", and compared it with Coase's own. He explains what he perceives as Stigler's fascination with the "Coase theorem" (whose unrealism is entirely recognized) by reference to the facts that it is an argument for negotiated solutions, and that it puts the stress on the necessity of studying transaction costs and the law. Alain Marciano (2018) argues that Stigler transformed the "Problem of Social Cost" into a general statement, making it consistent with his own methodology, and conception of economics as a science. I will answer these questions more specifically: since the "Coase theorem" is an unidentified object in economic theory, which theorem is Stigler's "Coase theorem"? What are its assumptions and how is it argued for? What are its consequences?

The Story of Stigler's Conversion

The Ancient Faith

Pigou was interested in divergences between the private and the social products of a good or service. In "simple competition", a divergence occurs when "a part of the product of a unit of resources consists of something, which, instead of coming in the first instance to the person who invests the unit, comes instead, in the first instance (i.e. prior to sale if sale takes place), as a positive or negative item, to other people" (Pigou 1932: 174). In the case that interests us here (what would come to be known as technological externalities), "one person A, in the course of rendering some service, for which payment is made, to a second person B, incidentally also renders services or disservices to other persons (not producers of like services), of such a sort that payment cannot be exacted from benefited parties or compensation enforced on behalf of the injured parties" (Pigou 1932: 183).[2] The government can suppress this divergence by "extraordinary encouragements" or "extraordinary restraints", whose "most obvious forms" are "bounties and taxes" (Pigou 1932: 192). It can also intervene by direct regulation (Pigou 1932: 194).

This is this "Pigovian tradition" that Coase would incidentally call into question in his article on the allocation of radio frequencies (Coase 1959), and then in "The Problem of Social Cost" (Coase 1960).[3] Coase later made clear that his "target (or targets) were the modern economists who had adopted Pigou's approach", referring to Samuelson (1947) and Stigler (1952) (Coase 1996: 117). In the second edition of his *Theory of Price*, Stigler's approach was indeed close to Pigou's (see Medema 2011: 14). He was writing: "Some disharmonies between private and social products are large and important, and they are dealt with by a variety of techniques such as taxes and subsidies, dissemination of information, and the police power (for example, zoning)" (Stigler 1952: 105). This sentence is quoted by Coase (1996: 117) as an example of "pure Pigou".

Coase 1959: The First Exposition of the New Faith

While at the London School of Economics, Ronald Coase had become interested in the economics of public utilities, broadcasting in particular. When he migrated to the United States in 1951, he pursued his interest on this subject and studied American television and radio broadcasting institutions. In "The Federal Communications Commission" (Coase 1959) he argues that the definition of rights over radio frequencies is sufficient for the price mechanism to operate. His argument proceeds as follows.

First, for any good, once the property right is distributed, its final allocation is determined by the market transactions. Coase suggests this idea with one example. "Whether a newly discovered cave belongs to the man who discovered it, the man on whose land the entrance to the cave is located, or the man who owns the surface under which the cave is situated is no doubt dependent on the law of property. But the law merely determines the person with whom it is necessary to make a contract to obtain the use of the cave. Whether the cave is used for storing bank records, as a natural gas reservoir, or for growing mushrooms depends, not on the law of property, but on whether the bank, the natural gas corporation, or the mushroom concern will pay the most in order to be able to use the cave" (Coase 1959: 25). Coase here asserts that the legal system is neutral with regard to final resources allocation (the use of the cave). He also suggests that this allocation maximizes the value of production since the person who acquires the right is the one who offers the highest payment; that is, whoever values it the most.

Second, Coase broadens these ideas to the rights whose use implies effects on others, with the example of the Sturges v. Bridgman case (1879), which concerned a doctor whose practice was made difficult because of the noise generated by his neighbor, a confectioner. In the same manner, whatever the Court decision is, the right to work in silence or to make noise will end up in the hands of the person who values it the most. "[T]he delimitation of rights is an essential prelude to market transactions; but the ultimate result (which maximizes the value of production) is independent of the legal decision" (Coase 1959: 27). Retrospectively, Coase (1988: 158) will see in this assertion the "essence of the Coase theorem".

Director and others at Chicago were not convinced by this rebuttal of the Pigovian analysis and asked Coase to delete this passage; he refused. Instead, he insisted on discussing this argument with the members of the Chicago faculty who objected to it, a discussion which took place one evening at Director's home, where George Stigler was present.

The Night of Conversion

The discussion took place with around twenty leading Chicago figures (among which, besides Coase, Director, and Stigler: Reuben Kessel, Milton Friedman, Arnold Harberger). There are several accountings of this famous night: by Coase himself (mainly in Coase 1993: 249–50), by Stigler (1988), and a collective one (in Kitch 1983: 220–1). Stigler recounts: "At the beginning of the evening we took a vote and there were twenty votes for Pigou and one for Ronald … The discussion began. … My recollection is that Ronald didn't persuade us. But he refused to yield to all our erroneous arguments. Milton would hit him from one side, then from another, then from another. Then to our horror, Milton missed him and hit us. At the end of that evening the vote had changed. There were twenty-one votes for Ronald and no votes for Pigou" (in Kitch 1983: 221).

Stigler's *Memoirs* make clear that the group agreed to assume zero transaction costs and, in this world, was not troubled by Coase's conclusion of efficiency in the presence of externalities. They were much more troubled by his conclusion that the result would be the same whatever the initial distribution of rights. They "strongly objected to this heresy" (Stigler 1988: 76). And it was on this element that they struggled, in particular Stigler—who finally submitted and eventually popularized this idea.

The conclusion of this night was a request by Director that Coase develop more fully his argument in the *Journal of Law and Economics* without reference to radio frequencies (Coase 1993: 250).

The New Testament: "The Problem of Social Cost"

"The Problem of Social Cost" (Coase 1960) thus developed the argument asserted in "The Federal Communications Commission", and put forward the claim that it depended on the assumption of zero transaction costs (Coase 1988: 158).[4]

Coase's (1960) article starts from the "case of straying cattle which destroy crops growing on neighbouring land" (Coase 1960: 2). It first makes clear that the problem under discussion is reciprocal: if there were no cattle, there would be no destruction of crops; but if there were no crops, there would not be any destruction either.

The first part of the paper assumes that "the pricing system works smoothly (strictly this means that the operation of a pricing system is without cost)" (Coase 1960: 2). In the first case studied by Coase, the rancher has to pay for all damage caused by his herd: he does not own the right to harm the farmer. Obviously, he therefore takes into account the damage inflicted, hence the social cost of his actions. His choice of output is then socially optimal.

To suggest that the result is independent of the initial allocation of property rights, Coase now assumes that the rancher owns the right to cause damage to his neighbor. In his example, the structure of the costs entailed by the rancher is the same as before since cost is opportunity cost. For example, Coase assumes that the value of the marginal damage on the corn caused by a third steer is \$3. Either the rancher has to pay for all damage and he adds these \$3 to his marginal production cost, or he does not have to pay compensation and the farmer would pay him \$3 so that he diminishes his herd to two steers. "Whether the \$3 is a payment which the cattle-raiser has to make if he adds the third steer to his herd (which it would be if the cattle-raiser was liable to the farmer for damage caused to the crop) or whether it is a sum of money which he would have received if he did not keep a third steer (which it would be if the cattle-raiser was not liable to the farmer for damage caused to the crop) does not affect the final result. In both cases \$3 is part of the cost of adding a third steer, to be included along with the other costs" (Coase 1960: 7).

Note that there is a slight shift in the amount here (which will be reproduced by Stigler). Why should the farmer pay \$3 if the amount of the marginal profit of the third steer would have been sufficient? In fact, the amount of the payment from the farmer to the rancher is indeterminate. It has to be superior or equal to the marginal profit of the rancher (unknown here) and inferior or equal to the marginal damage (\$3). The opportunity cost of the third steer is therefore also indeterminate. But Coase assumes that the farmer would pay the total marginal damage. In other words, the rancher would obtain the entire surplus. This assumption is implicit and appears as follows: "the farmer would be willing to *pay up to $3* if the cattle-raiser would reduce his herd to 2 steers … The cattle-raiser would therefore *receive $3* from the farmer if he kept 2 steers instead of 3" (Coase 1960: 6–7, my emphases).

This shift allows Coase to conclude that in both initial allocations of rights, the rancher faces the same marginal production costs, so the physical result—the level of the externality—is the same as before. It is therefore optimal *and* invariable. "The size of the herd will be the same whether the cattle-raiser is liable for damage caused to the crop or not" (Coase 1960: 7). Coase then asserts what most resembles a Coasean "Coase theorem": "It is necessary to know whether the damaging business is liable or not for damage caused since without the establishment of this initial delimitation of rights there can be no market transactions to transfer and recombine them. But the ultimate result (which maximises the value of production) is independent of the legal position if the pricing system is assumed to work without cost" (Coase 1960: 8). We here have two assumptions—zero transaction costs and defined property rights—and two conclusions—efficiency and independence.

However, the "very unrealistic" assumption of zero transaction costs concerns only one-third of this article, and Coase quickly and explicitly pushes the reader beyond this world. "In order to carry out a market transaction it is necessary to discover who it is that one wishes to deal with, to inform people that one wishes to deal and on what terms, to conduct negotiations leading up to a bargain, to draw up the contract, to undertake the inspection needed to make sure that the terms of the contract are being observed, and so on. These operations are often extremely costly, sufficiently costly at any rate to prevent many transactions that would be carried out in a world in which the pricing system worked without cost" (Coase 1960: 15). Consequently, a bargain occurs only if its gain is higher than the cost involved. If all the exchanges of rights necessary to bring the optimal result do not take place, two consequences follow.

First, the optimal result will not be achieved and solutions other than the negotiation may lead to better results. Coase here provides an innovative discussion on policy. Since "[a]ll solutions have costs" (Coase 1960: 18), it is necessary to compare different institutional arrangements (integration in a unique firm, public regulation, status quo) in terms of their net output values. In opposition to the comparison with an "ideal world", the Coasean method of policy design entails examining the initial actual situation and comparing the net values yielded by alternative arrangements (Coase 1960: 43).

Second, the independence of the result is equally called into question since the final allocation now depends on the initial distribution of rights. Therefore, "the courts directly influence economic activity" (Coase 1960: 19). It is his final conclusion on the influence of the law and not its neutrality that in Coase's view was important.

The Baptism

As noted by Medema (2011: 12), Coase's conclusion in the zero-transaction-costs world was already under discussion in the literature (Buchanan and Stubblebine 1962; Turvey 1963; Demsetz 1964; Calabresi 1965) when Stigler (1966) gave it a name, changed its status to a theorem, and inscribed it in a textbook. This was in the third edition of his *Theory of Price*.

In the chapter "Costs and Production", after having argued that true cost is opportunity cost, Stigler deals with the difference between private cost and social cost: "One of the most tendencious questions in economics has been: when social and private costs diverge appreciably, will competition lead to correct amounts (and prices) of goods?" (Stigler 1966: 110). He uses the same example as Coase, of a rancher and a farmer, and first approaches the problem in the traditional way. If the rancher is not liable, the result is not optimal: "This is in fact an instance of a general theorem: consumers will be best off (on the highest indifference curves) when the relative prices of goods are equal to their relative (marginal) social costs. Where private costs differ from social costs, obviously this optimum position will not be reached, because producers will gear output to their private costs" (Stigler 1966: 111). Private and social costs will be equal only if the rancher was liable.

However, the problem can be seen in a reverse way. "[S]uppose … that the area had originally been devoted to cattle raising and now a wheat farmer enters. The argument is completely analogous, but this time we reach the conclusion that the wheat farmer should pay for the fencing! It is his arrival which creates the problem of wandering cattle, and therefore to get the true (social) cost of his wheat we should take account of the damage he inflicts on cattle raisers if they should for example have to erect fences" (Stigler 1966: 112). Here Stigler recognizes the reciprocity of the problem, but he calls it symmetry: "The fundamental symmetry in the relations of cattle and grain farmers, no matter where the law places the liability for damages, deserves elaboration" (Stigler 1966: 112). Then, instead of elaborating on reciprocity, this is the independence result that he develops (together with efficiency).

If the rancher is liable, he will take into account the harm he inflicts and the social cost will be minimized—in his example for 12 steers. (Stigler will later say that "[s]ocial returns are maximized" Stigler 1987: 119.) In the case where the farmer is liable, the result is the same since the structure of the rancher's costs is identical—for the same reason as in Coase (opportunity cost), and with the same shift. The farmers would pay the total damage because they would pay *up* to the total damage. Stigler writes: "For now the grain growers will offer him [the rancher] sums equal to the marginal damage if he does not increase the herd. If the herd is 12, for example, they will offer up to $4 if he will not add a thirteenth animal. Since he foregoes this receipt by adding the 13th animal this is the cost (for costs are foregone alternatives). The manner in which the law assigns liability will not affect the relative private marginal costs of production of cattle and grain" (Stigler 1966: 113). This invariant result is moreover socially optimal: "But this procedure obviously leads to the correct social results— the results which would arise if the cattle and grain farms were owned by the same man" (Stigler 1966: 113).

Finally comes the baptism of Coase's idea. "The Coase theorem thus asserts that under perfect competition private and social costs will be equal" (Stigler 1966: 113). Stigler insists on the novelty of this proposition, and certainly remembers the doubts with which he had first received it: "It is a more remarkable proposition to us older economists who have believed the opposite for a generation, than it will appear to the young reader who was never wrong, here" (Stigler 1966: 113).[5] It is the independence conclusion that in his view is the most surprising:

"The proposition that the composition of output will not be affected by the manner in which the law assigns liability for damage seems astonishing. But it should not be. … The assignment of responsibility for damages … can be ignored: assume that the same farmer grows grain and cattle, and it is obvious that his determination of output will be independent of the assignment" (Stigler 1966: 113). The implicit argument is therefore that the social optimum is unique, and that two agents will act as one.

The assumption of zero (or low) transaction costs is made explicit when Stigler details the consequences of the impossibility of negotiation. "The proposition, he writes, must, to be sure, be qualified by an important fact. When a factory spews smoke on a thousand homes, the ideal solution is to arrange a compensation system whereby the homeowners pay the factory to install smoke reduction devices up to the point where the marginal cost of smoke reduction equals the sum of the marginal gains to the homeowners. But the costs of this transaction may be prohibitive—of getting the people together, of assessing damages, and so on—so only a statutory intervention may be feasible" (Stigler 1966: 113–4). This is certainly the reason why the solutions to many externalities are public. In his view, "[t]he differences between private and social costs or returns have provided a fertile field for public control of economic activity. In fact one can attribute most limitations on private ownership or control of property to this source. These controls are of every degree of perspicacity, ranging from traffic controls (where private contracts between rapidly converging drivers would be difficult to arrange) to petroleum import restrictions (designed to conserve the supply of domestic petroleum!)" (Stigler 1966: 114).

The "Coase theorem" was important to Stigler, as illustrated by the fact that he returns to it several times. We have already mentioned his own recollections (Stigler 1988; Kitch 1983), and the fourth edition of *The Theory of Price* (Stigler 1987). In 1972, Stigler published a short article on the complementary roles of law and economics to formulate economic policies, in which a few lines were devoted to the "theorem". In his Nobel address (Stigler 1982), the Coase v. Pigou debate is examined as a particular case of how new economic ideas are accepted in the market for ideas.[6] Stigler's thesis is that the choice between Coase and

Pigou was not decided by critical empirical tests but by a comparison of their heuristic power. In another reflexive article on economics, Stigler (1984) examines how economics is mobilized in different domains (law, history, sociology, politics), and of course the "Coase theorem" is discussed as an example, in the case of law, of the role economists can play. He writes a specific commentary on the "theorem" in 1989, actually two commentaries—on testing the "theorem" and on income effects. The first one is repeated in his 1992 article on law and economics, which adds a discussion on what "efficient law" could mean.[7]

Coase's Reactions to This Baptism

How did Coase react? He accepted the fatherhood of the notion Stigler had baptized. This was first made explicit in his "Notes on the problem of social cost", a new chapter introduced in his 1988 collection, in which he wrote: "I did not originate the phrase, the 'Coase Theorem', nor its precise formulation, both of which we owe to Stigler. However, it is true that his statement of the theorem is based on work of mine in which the same thought is found, although expressed rather differently" (Coase 1988: 157).

Indeed, the first difference is in the vocabulary. Stigler uses Pigovian language in terms of equalization of private and social costs, while Coase reasons in terms of maximization of the value of production. "There is, however, no inconsistency" (Coase 1988: 158). Stigler's formulation of the "Coase theorem" is appropriate in the sense that if private cost equals social cost, the minimization of the former by each agent entails the minimization of the latter and the total value of production is thus maximized. Coase explains as follows why Stigler's formulation, although using another vocabulary, accurately translates what he meant in "The Problem of Social Cost": "Social cost represents the greatest value that factors of production would yield in an alternative use. Producers, however, who are normally only interested in maximizing their own incomes, are not concerned with social cost and will only undertake an activity if the value of the product of the factors employed is greater than their private cost (the amount these factors would *earn* in their best alternative employment). But if private cost is equal to social cost, it follows that producers will only engage in an activity if the value of the product of the factors employed is greater than the value which they would yield in their best alternative use. That is to say, with zero transaction costs, the value of production would be maximized" (Coase 1988: 158, his emphasis).

The second difference between Stigler's and Coase's "Coase theorems" lies in their explicit assumptions. In the same "Notes", Coase rejects the necessity of Stigler's assumption of perfect competition, the assumption of zero transaction costs being sufficient in his view. He makes clear that "it would seem that even the qualifying phrase 'under perfect competition' can be omitted" (Coase 1988: 175). Stigler's implicit assumptions are detailed and showed to be identical to Coase's ones in the following section.

The third apparent difference lies in what Coase and Stigler do with this "theorem". Coase repeated in his Nobel address that he did not reject the "theorem", which he formulates as follows. "What I showed in that article, as I thought, was that in a regime of zero transaction costs, an assumption of standard economic theory, negotiations between the parties would lead to those arrangements being made which would maximize wealth and this irrespective of the initial assignment of rights. This is the infamous Coase theorem, named and formulated by George Stigler, although it is based on work of mine. Stigler argues that the Coase theorem follows from the standard assumptions of economic theory. Its logic cannot be questioned, only its domain (Stigler 1989: 631–3) [see below]. I do not disagree with Stigler" (Coase 1992: 717). That being said, Coase insists that what interested him was the study of a world with positive transaction costs: "However, I tend to regard the Coase theorem as a stepping stone on the way to an analysis of an economy with positive transaction costs" (Coase 1992: 717). Coase has regularly been far more critical of the "Coase theorem" and its unrealism, claiming for example that "[w]e do not do well to devote ourselves to a detailed study of the world of zero transaction costs, like augurs divining the future by the minute inspection of the entrails of a goose" (Coase 1981: 187).

Reconciling Coase's recognition of paternity with his dismissal of the "Coase theorem" requires identifying the roles that the assumption of zero transaction costs plays in his argument. Coase's "Coase theorem" had three roles in Coase's works (Bertrand 2010): critical (of the Pigovian tradition, its analysis and policy proposals), heuristic (bringing to light the influence of transaction costs), and normative (in the derivation of policy prescriptions—namely lower transaction costs and allocate the property right to whoever values it the most).

What about Stigler? As Marciano (2018) argued, he transformed Coase's argument in a logical and general theorem—unrealistic assumption being not a problem at all, in line with Friedman's credo. But he also insisted on transaction costs and the role of law. So what was the meaning of his "Coase theorem"? And what did he do with it?

The Significance of Stigler's "Coase Theorem"

Even if its statement takes the form of a theorem, it is not one. Stigler's (1966) argument contains no demonstration, but is led by numerical examples. The assumptions are not explicit; and what Stigler calls "symmetry" mixes reciprocity and independence. This section will explore the meaning of Stigler's argument. I will focus on the assumption about exchange, then on the conclusions of efficiency and independence (and on their consequences).

The Meaning of the Assumption of Perfect Competition

Let me first recall the nature of Coase's argument, which is also based on a numerical example. Coase mentions the assumption of perfect competition (1960: 5 and 6) for the markets for corn and cattle (the prices of corn and cattle are given). In his first example of bilateral negotiation, the rancher, liable for the damage caused by his cattle, signs an agreement with the farmer by which the latter abandons his culture in exchange for a payment falling between the farmer's net profit and the amount of the damage, since "… [t]here is clearly room for a mutually satisfactory bargain" (Coase 1960: 4). Although Coase sets this bargaining process within a framework of perfect competition—since the agents *take* the price of the commodities as given (meat and wheat)—they *make* the price of the externality, and the price is thus indeterminate. "What payment would in fact be made would depend on the shrewdness of the farmer and the cattle-raiser as bargainers" (Coase 1960: 5). The final result is optimal since all the mutually satisfactory bargains are assumed to be struck: "… if such market transactions are costless, such a rearrangement of rights will always take place if it would lead to an increase in the value of production" (Coase 1960: 15). What Coase envisages is therefore an island of bilateral bargaining (price-making) in an ocean of perfect competition (price-taking). And Stigler will respect this framework.

Stigler (1966) does not explicitly mention the assumptions of zero transaction costs and defined property rights (although they are implicit in his examples), but only that of perfect competition. Perfect competition means that prices are parametric for the individuals. This is confirmed in Stigler's Chapter 5, whose first sections deal with "the market" and "the competition". A market is characterized by a unique price for the product.[8] Stigler makes clear that a perfect market is centralized: "in a perfect market no buyer ever pays more than any seller will accept, and no seller accepts less than any buyer will pay. These conditions can be met only in a completely centralized market, which is approximated by a few exchanges such as the New York Stock Exchange" (Stigler 1966: 87). Among perfect markets, a competitive one is a market where prices are parametric, "in which the individual buyer or seller does not influence the price by his purchases or sales" (Stigler 1966: 87). Since in Stigler's example the farmer and the rancher negotiate the amount of the money transfer, this is not perfect competition. Stigler's detailing of the conditions under which a "perfectly competitive market" arises removes any doubt that the rancher–farmer situation is not perfectly competitive: perfect knowledge (otherwise, there is "scope for haggling, and to this extent a situation termed bilateral monopoly arises"); large numbers ("there must be many buyers or sellers if each is to have no appreciable influence upon the price, and they act independently"); product homogeneity; and divisibility of the product (Stigler 1966: 88).

Externalities are by definition external to the market, they have no price. This definition is made explicit in the 1987 edition of *The Theory* *of Price*. After his section on private costs and social costs, which almost exactly replicates that of 1966, Stigler adds a new subsection on "The discovery of externalities" (121) in which he states that "by definition, explicit transactions do not take place in externalities and therefore no explicit prices are quoted for (e.g.) smoke damage". Or again, "[e]xternalities are not traded in, by definition: people who are affected are not parties to transactions such as producing smoke" (Stigler 1987: 121). And externalities are a common problem, since "[t]here can be no doubt of the existence of these external effects of an individual's behavior. In fact, in strictest logic there are very few actions whose entire consequences accrue to the actor" (Stigler 1966: 110).

Since they have no price, a solution could be to give them a price, as each commodity has in a perfectly competitive market. This is not the solution proposed by Stigler. Perfect competition cannot solve the problem, since two agents will haggle over the price of the externality. The solution envisaged in the section on "Private and social costs" is bilateral monopoly, i.e. bilateral bargaining.[9] Like Coase, Stigler mentions the assumption of perfect competition for the markets for corn and cattle, but between the farmer and the rancher this is in fact a bilateral negotiation (price-making). The perfect competition assumption does not concern the exchange between the rancher and the farmer.

Efficiency: Conclusion or Assumption?

In Stigler's view, the efficiency of these bilateral negotiations is obvious. There is no demonstration in his 1966 textbook. Stigler will make explicit in the 1980s some implicit assumptions that make this thesis self-evident. His writings underline the extent to which efficiency is a consequence of economists' usual assumptions: economic (rational) behavior, complete information and, of course, zero transaction costs.

As early as 1982, Stigler suggests that Coase was only drawing the consequences of the usual assumptions on behavior: "The Pigovian theory of external economies was challenged directly by Ronald Coase, who in effect argued that the Pigovian theory had assumed *non-economic* behavior on the part of the economic actors in a wide class of phenomena" (Stigler 1982: 68, my emphasis). It is (or should be) self-evident for economists that transaction costs are the only obstacle to a mutually beneficial exchange: "Ronald Coase taught us, what of course we should already have known, that when it is to the benefit of people to reach an agreement, they will seek to reach it. Reaching agreement can be costly in time and other resources, so many potential agreements will not be achieved, but these unachieved agreements will have been inhibited by the smallness of the benefits or the largeness of the costs of agreement" (Stigler 1989: 631). The "proof" is that if there was a profit opportunity, it would have already been taken. "Does the proposition require proof? One would think not. It is similar to a proposition in international trade: The prices of internationally traded goods in two national markets will differ by no more than the cost of movement of the goods between the markets. Suppose I started to test the proposition and found that a pair of prices differed by more than the costs of movement. I would immediately abandon the test and embark on lucrative arbitrage transactions. Similarly, if I found that Coase's famous grain farmer and cattle rancher were making foolish decisions with respect to the damage to grain from wandering cattle, I would buy the two enterprises and reap a capital gain from an efficient reorganization" (Stigler 1989: 631). Stigler expressed the same ideas in the 1992 text: "Coase *reminded* economists and taught lawyers that, in a world of exchange by agreement rather than by coercion, the costs and benefits of agreement determine its scope" (Stigler 1992: 456, my emphasis).

Consequently, besides transaction costs, the only reason why a mutually beneficial exchange would not be struck is that agents can demonstrate noneconomic behavior. "There are people who do not care for wealth, more who do not reason well, and vastly more who are incompletely informed. These people will not necessarily achieve optimal agreements, and especially is this true in new circumstances" (Stigler 1989: 631). Stigler thus assumes that mutually beneficial exchanges are realized if information is complete and agents are rational. Empirical studies of the "theorem" should therefore be directed to transaction costs and "to the determination of the efficiency of small markets with special attention to short-run reactions to altered circumstances ('shocks')" (Stigler 1989: 631).[10] None of them "is directed to the logic of the Coase Theorem but instead to its domain" (Stigler 1989: 632).[11] The logic of the "theorem" is indisputable in Stigler's view; this is why it cannot be tested.

However, this "theorem", if it assumes individual rationality, complete information, and zero transaction costs, is in fact disputable, since these assumptions are not sufficient to establish efficiency.

One must assume, in addition, the efficiency of bilateral bargaining because bargaining does not exclude, even in these conditions, an inefficient result. The fundamental criticism leveled at the resolution of externalities by bargaining rests on the problem of the distribution of the exchange surplus. If the agents, who maximize their own part of the surplus, do not agree on its distribution, the mutually beneficial bargain is not struck and the result is then suboptimal, even if transaction costs are nil. Paul Samuelson writes, referring to Coase (1960):

> Let us be clear, though, that the rational self-interest of each of two free wills does not necessitate that there will emerge, even in the most idealized game-theoretic situation, a Pareto-optimal solution that maximizes the sum of the two opponents' profits, *in advance of and without regard to how that maximized profit is to be divided up among them*. Except by fiat of the economic analyst or by his tautologically redefining what constitutes "nonrational" behavior, we cannot rule out a non-Pareto-optimal outcome. We can rule it out only by Humpty-Dumptyism. (Samuelson 1967: 35, his emphasis)

This criticism was also leveled at the "Coase theorem" by, in particular, Regan (1972), Arrow (1979), Cooter (1982), Veljanovski (1982), and Coleman (1984). William Samuelson clearly poses the problem, distinguishes the possible strategic behaviors, and stresses that the conflict over distribution is not solely due to an undetermined result or to incomplete information:

> The presumption is simply that such agreements [preferred by both sides] can and will be reached since it is in the joint interest of the parties to do so. Although this conclusion is appealing, it is considerably stronger than the economist's customary hypothesis of *individual* rationality.

> Even under perfect information, this presumption is far from obvious. Each individual seeks only to maximize his individual utility and does not seek a point on the utility-possibility frontier per se. Moreover, in any interesting problem, there will be a multiplicity (in fact, an infinity) of efficient agreements – such that an improvement in one agent's welfare necessitates a sacrifice in the other's. Thus, one would expect the agents to negotiate (or haggle) over these candidates – a process that may in turn expend resources and delay agreement. Other types of strategic behavior are also possible. In pursuit of a preferred agreement, one party may threaten the other and, for credibility's sake, bind himself to carry out the threat some portion of the time. When he does, efficiency fails. Alternatively, the parties may adopt the standard negotiation bluff, insisting on ultrafavorable (and incompatible) terms of agreement. If agents persist in these demands, a mutually beneficial agreement may be lost. (Samuelson 1985: 322, his emphasis)

Stigler's "Coase theorem" is indisputable because it adds to usual assumptions an assumption of efficiency of bargaining. More clearly, Stigler mixes the assumption and conclusion of efficiency, as for example when he says: "Ronald Coase taught us, what of course we should already have known, that when it is to the benefit of people to reach an agreement, they will seek to reach it" (Stigler 1989: 631). What Stigler says that Coase "taught" us is in fact Coase's assumption, and not his result. But Coase's insight, according to Stigler, is that he brings to light the consequences of this efficiency assumption.

This thesis had in Stigler's view an empirical counterpart, as illustrated by his argument about the actual impossibility of unexploited mutual gains. Stigler seemed to think that externalities are for the most part already internalized or on the verge of being internalized. As recounted by Claire Friedland, "[h]e described externalities as that for which there are no transactions *at the present time*" (1993: 781, her emphasis). In other words, externalities are a problem that markets can handle in practice. Applying his methodology to this specific case, we understand that Stigler was not so much interested in the details of the theorem, or in the modelization of bargaining, than in its application to the real world (see Freedman's [2016] interview with Peltzman).

Mixing assumption and conclusion, Stigler thus makes the *assumption* that mutually beneficial negotiations take place into the true *result* of the "Coase theorem". This is the first heuristic role of the "theorem" in Stigler's view.

The Consequences of the Efficiency Thesis

Its second heuristic role is that it is a means to examine the world with zero transaction costs, in order to stress their influence in the real economic system and the necessity of introducing them in the analysis, even if Stigler neither engaged himself on these paths.

In his 1988 memoirs, Stigler insists on how revolutionary it was to think about the world of zero transaction costs. Following Coase's invitation to assume zero transaction costs "seemed reasonable because economic theorists, like all theorists, are accustomed (nay, compelled) to deal with simplified and therefore unrealistic 'models' and problems. Still, zero transaction costs are a bold theoretical construct. It implies, for example, that in buying an automobile one knows the prices all dealers charge (with no cost to anyone in time or money), that one is completely certain what all warranties for replacement of defective parts or provision of services mean and has complete confidence that they will be fulfilled (without controversy), and so on. Zero transaction costs mean that the economic world has no friction or ambiguity" (Stigler 1988: 75).

Like Coase, Stigler draws the consequences of the zero transaction cost assumption, absurd as they may be, in order to stress the need for introducing positive transaction costs in the analysis: "The same amount of smoke would be released from the factory's chimney whether the factory owner or the householder was legally responsible for the smoke damage. If this proposition strikes you as incredible on first hearing, join the club. The world of zero transaction costs turns out to be as strange as the physical world would be with zero friction. Monopolies would be compensated to act like competitors, and insurance companies and banks would not exist. The Coase analysis has emphasized the urgent need in economics for a general theory of transaction costs" (Stigler 1972: 11–2).

That this assumption of zero transaction costs is unrealistic is also often acknowledged, as here: "The Coase theorem is not realistically applicable when many parties are concerned—for example, when one seeks to control a factory whose chimney spews noxious pollutants on five thousand households—because it is too costly for the factory owner and the thousands of households to contract with each other" (Stigler 1984: 305).

It is a consequence of transaction costs that, in terms of economic policy, it seems that the "Coase theorem" does not really change Stigler's treatment of externalities in his textbook. In the 1987 edition of his *Theory of Price*, at the end of the section on private costs and social costs (p. 120), Stigler refers to a new Chapter 20. In this chapter, there is a section on "The detection of externalities" that takes into account the consequences of "The discovery of externalities", the new subsection added in the chapter on costs and production. He writes here that "[e]xternal effects are by definition not part of the contracts of purchase and sale in which most economic transactions are effected—if they were negotiated, they would not be external to the parties" (Stigler 1987: 327). This means that if transaction costs are so low that a negotiation occurs, then the externality as such disappears. But if this is not the case because of transaction costs, then the externality remains and will have to be corrected by the traditional Pigovian solutions. Indeed, the following section on "The correction of externalities" resembles the traditional analysis of externalities. First, the definition of externality is Pigovian: "The existence of externalities can be presented as a difference between the marginal private and marginal social products of a resource, where the private product accrues to the person utilizing the resource and the social product includes all effects of the use of the resource upon others" (Stigler 1987: 328). Then the problem is posed in traditional terms: "The allocation of privately owned resources is governed by their private return, and to maximize his return, the owner of the resource will allocate it so its marginal private product is equal in all uses. But to maximize total income of the society, it is the marginal social product of a resource that should be equalized in all uses. How is this to be achieved?" (Stigler 1987: 328). Finally, the solutions are also traditional: "The direct 'solution' is simply to tax activities yielding external diseconomies and subsidize activities yielding external economies" (Stigler 1987: 328). But because this taxation is not always feasible, there is a variety of other policies: (1) "The need for contracting may be removed by an assignment of rights" (such as zoning); (2) direct regulations; and (3) direct provision of reparations for negative effects (Stigler 1987: 329–30).

The introduction of transaction costs into the analysis also implies that economists should pay attention to aspects of the law. This is clear as early as 1972, when Stigler explains that the law is not only a condition for contracts but also an alternative to them when they are impossible: "The development of a theory of [transaction] costs is a task for economists, but an integral part of that task is the understanding of the legal processes which may be employed. Economic life requires reliable commitments by the transactors, and economic disagreements call for methods of resolution. The civil law and private arbitration are the peaceable methods by which a society achieves commitments and conciliation. It comes as more of a surprise to the theoretical economist, I am sure, than to his legal brethren that economic order has deep relationships to legal order" (Stigler 1972: 12). If transaction costs impede exchange then the role of law is to reconcile opposite interests: "Law, like other social institutions, came to be viewed by economists as an instrument for the organization of social life" (Stigler 1992: 456).

The part of Stigler's "Coase theorem" that draws the conclusion of efficiency is circular because it assumes efficiency in bilateral bargaining. It has, however, a heuristic role: to bring the conclusions of economic theory to completion, which leads then to a study of the positive-transaction-costs world. Its third heuristic power is to pose the question of the independence of the result in a specified theoretical framework. Independence was indeed not self-evident at all.

Independence: Argument and Consequences

There is a third heuristic role for the assumption of bargaining efficiency: the independence thesis. While the efficiency conclusion is considered by Stigler as self-evident, the independence thesis is not, and to him this is Coase's important result. In his first statement of a "Coase theorem" in 1966, Stigler puts the stress on what would be Coase's main insight: the independence of the result. And in his later commentaries on the "theorem", Stigler will each time insist on the novelty of this thesis (as opposed to the not-so-new efficiency thesis). For example: "In 1961, my colleague Ronald Coase demonstrated in a powerful article that this analysis of external effects was wholly superficial. If transaction costs were zero, all parties to any economic activity would contract with respect to all benefits or detriments—there would be no external effects. This is obvious enough, but what was extraordinarily unobvious was the Coase theorem that the manner in which legal rights were assigned would have no effect whatever upon the methods of production" (Stigler 1972: 11).

In 1966 examples, independence is based on the use of the concept of opportunity cost, as in Coase. It is certainly not by mere chance that the section on "Private costs and social costs" is preceded by a section entitled "The nature of costs", which makes clear that real cost is not historical cost but opportunity cost. Indeed "the cost of any productive service to use *A* is the maximum amount it could produce elsewhere. The foregone alternative is the cost" (Stigler 1966: 105). Coase's argument, both in "The Problem of Social Cost" and during the night of discussion where Stigler was present, was indeed based on use of the opportunity cost concept.[12] During that night it was the independence result that posed problems for the other participants, and it was Coase's argument in terms of opportunity costs that convinced them.

Remember that in his cattle and crops example, Coase asserts that in both allocations of rights the rancher faces the same marginal production costs, since "a receipt foregone of a given amount is the equivalent of a payment of the same amount" (Coase 1960: 7). In Coase's recollection, this was the very idea that convinced the participants of the Chicago seminar who first had rejected the "Coase theorem" idea: "I remember at one stage, Harberger saying, 'Well, if you can't say that the marginal cost schedule changes when there's a change in liability, he can run right through'. What he meant was that, if this was so, there was no way of stopping me from reaching my conclusions. And of course that was right. I said, 'What is the cost schedule if a person is liable, and what is the cost schedule if he isn't liable for damage?' It's the same. The opportunity cost doesn't shift. There were a lot of other points too, but the decisive thing was that this schedule didn't change. They thought if someone was liable it would be different than if he weren't" (Coase 1997). This is the same argument in Stigler's (1966) example described above.

When Stigler generalizes his example, the independence of the final allocation is due to the efficiency thesis and the uniqueness of the optimal result. The optimal allocation is the allocation that would be chosen by a single owner, or a planning authority, who maximizes the sum of profits. If it is assumed that there is only one optimal allocation of crops and cattle and that this optimum is reached whatever the initial distribution of rights is, then of course the same result is reached. It was already the case in the 1966 textbook, and it is repeated here: "Coase's conclusion can be reached by the following argument. It is clearly desirable that the sum total of the produce of the two farming enterprises be as large as possible, for then *each* farmer can receive more than when there is a smaller pie to be divided. If the rancher is responsible for the damage, he will erect the fence or reduce his herd or pay for the grain damage, or pay the farmer to grow less grain, whichever is cheaper. If the farmer is responsible, exactly the same action will be chosen, except that now he compensates the rancher. In short, with either legal rule the same farming practices will be used as if the two farms were jointly owned" (Stigler 1984: 304–5, his emphasis).

In the 1987 edition of his *Theory of Price*, Stigler makes one concession to the independence thesis. More accurately, while accepting the major criticism of the independence thesis in terms of income effects,[13] he is defending that this thesis nevertheless holds in the long run (when there is some stability of the rights). "Such [income] effects are clearly present when legal rights are suddenly changed: the new holder of rights has gained and the new holder of liabilities has lost. The effect comes from the unexpectedness of the reassignment of rights, and such income effects are not likely to be important if the legal rules have long been unchanged" (Stigler 1987: 120).

The argument is developed in his second 1989 note on the "Coase theorem". If there is perfect competition on the corn and meat markets, and on the markets for lands for growing cattle and raise corn, the usual equality theorems will apply. An anticipated change in the law will therefore not change the distribution of incomes, nor by consequence the allocation of resources. "The Coase Theorem is of course consistent with the fundamental theorem of competitive markets, that homogeneous resources receive equal returns in all uses.[14] The state may shift rights from one party to another and confer short-run gains and losses … In the long run, however, the various parties will continue to earn only competitive rates of return, and even in the short run the Coase theorem allows the allocation of resources to be unaffected. A stable property or tort law, to repeat, would not affect the distribution of income" (Stigler 1989: 632–3).

The invariance thesis has consequences for understanding the necessity and the effect of law, even in the zero-transaction-cost world. This is why this thesis is so important: "An immensely influential article by Ronald Coase, in which the law played a role that oddly enough was both indispensable and incidental, is the example I choose to portray the economist's role. Coase asked the question: does it matter where legal rights and responsibilities are placed, so long as they are placed on definite persons? … His astonishing answer was, no, it will make no difference. The assignment will not affect the number of cattle or the amount of grain or the precautions taken to reduce damage. For lawyers in particular, this conclusion went, I may say, against the grain" (Stigler 1984: 304).

Even if this positive thesis may be useful for normative purposes,[15] Stigler favors a "positive theory of law", "a general theory of the determinants of the laws of torts, property, contract, and other branches of the law. … [L]egal scholarship would seek to explain, not just what the law should be, but why it is what it is" (Stigler 1984: 305). It is in this explanation that the economist has a part to play: "If the overwhelmingly normative orientation of legal writing should be redirected to the explanation of legal institution and their evolution, there will be a place for economic analysis in legal scholarship" (Stigler 1984: 312). The legal goals should indeed be left to lawyers.

This is why Stigler departs from Posnerian economic analysis of the law (Stigler 1992: 459). The goal of the law is justice, and the economist cannot discuss this role, since "efficiency is to be judged only with respect to the goals one seeks" (Stigler 1992: 459). The economist can only evaluate the efficiency of the means to achieve this goal, and help to explain rational behavior. "The difference between a discipline that seeks to explain economic life (and, indeed, all rational behavior) and a discipline that seeks to achieve justice in regulating all aspects of human behavior is profound. This difference means that, basically, the economist and the lawyer live in different worlds and speak different languages" (Stigler 1992: 463).

Therefore, in Stigler's view, on the one hand economics should take into account law because law is necessary for exchanges and may be alternative to them, and on the other hand economics could contribute to the positive analysis of the law.

Conclusion

A few years after having been converted to Coase's argument on the efficiency of costless negotiations over externalities, Stigler invented the "Coase theorem".

Stigler's understanding of "The Problem of Social Cost" was close to Coase's. Like him, Stigler posed the problem in terms of bilateral bargaining in an ocean of perfect competition. As with Coase, the efficiency thesis permitted drawing the consequences of the economists' assumptions and studying the independence result. As with Coase, it was important because it brought to light the role of transaction costs and of the law.[16] All this explains Coase's ambiguous attitude toward Stigler's naming. He did not deny his paternity but he rejected the economists' focus on the "theorem".

Nevertheless, Stigler's statement of the "Coase theorem" was inappropriate on two counts. First, it was neither Coase's language nor his message. Second, it was not demonstrated as a theorem. We can now add a third count, the same as Coase's. Beginning with the zero-transaction-costs world in order to show the importance of transaction costs did not prove to be an efficient heuristic device, since economists focused on that world and stayed in it. Since these mistakes had been made in a textbook, the "Coase theorem" gained fame and economists persisted with the name. The "Coase theorem" seemed to demonstrate the efficiency of bargaining without the weighty assumptions of perfect competition (even while it was assuming it). Stigler's name and his statement of it certainly contributed to giving the "theorem" its importance. Stigler claimed the status of a theorem for an assertion that was not demonstrated, and was at best circular. This gave a demonstrative power to a statement that in fact possessed none. Moreover he did this in a textbook, as if it were already an established result whereas it was addressed in only a very few academic articles, mainly in a critical tone. As Medema wrote (2011: 12): "Stigler's remaking of Coase's idea into a 'theorem' had significant rhetorical power, which, combined with the challenge that it posed to received thinking about externality problems, both lent credibility to the idea and made it a force to be reckoned with".

At an epistemological level, the drive to label as a "theorem" an assertion that was not formally proven or was at best circular is not simply testament to a failure of awareness or excessive enthusiasm. It also shows the peculiar nature of many of the assertions that economists find meaningful—assertions which cannot be reduced to demonstrable results, but draw their power from preexisting beliefs, or single examples.

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Notes

1. The term "conversion" is also used by Medema (2011: 24).
2. The two other cases are land tenancy, and economies external to the firm but internal to the industry.
3. But note that Medema (2019) calls into question the actual existence of such a Pigovian tradition before Coase's article, and also that Coase misinterprets Pigou (see Bertrand 2010 and the references therein).
4. The assumption of zero transaction costs is not absent from the 1959 article, nor are the consequences of high transaction costs (Coase 1959: 27, fn 54).

5. This sentence disappears in the next (and last) edition of *The Theory of Price* (Stigler 1987).
6. It is interesting to note that Coase tackled the same topic in a 1981 conference published in 1982.
7. We will analyze these articles later; for a different and sometimes more detailed exposition of most of them, see Medema (2011).
8. "A market, according to the masters, is the area within which the price of a commodity tends to uniformity, allowance being made for transportation costs" (Stigler 1966: 85).
9. The solution in terms of perfectly competitive markets would have to wait for Arrow (1969). See Berta and Bertrand (2014). The bargaining version of the "Coase theorem" is adopted by, for example, Turvey (1963), Calabresi (1968: 68), and Dahlman (1979: 142) (see Bertrand 2019).
10. Here again, Stigler repeats these ideas in his 1992 article (457–8).
11. The delimitation of the domain of applicability of a theory is important in Stigler's methodology (Marciano 2018).
12. On the centrality of the concept of opportunity cost in Coase's argument, see Bertrand (2015a, b).
13. If liability changes, revenue changes, and then demand for commodities including the commodity at the origin of the external effect. This criticism was for example leveled at Coase's argument by Turvey (1963: 310), Dolbear (1967: 91), and Mishan (1967).
14. This formulation confirms Medema's (2011) conjecture that Stigler formulated the "Coase theorem" in the manner of the first theorem of welfare economics.
15. "This result, now called the Coase theorem, raises a host of questions about the purpose of legal rules and the criteria by which they are chosen, and for the reformer, the criteria by which they should be chosen" (Stigler 1984: 305).
16. For the differences with Coase's exact message, see Medema (2011).

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Roads Not Taken: The Coase Conundrum

Craig Freedman

Robert Frost (1916) reminds us of the importance.
Of Roads Not Taken.
For George Stigler and Ronald Coase,
These remain as important as choices actually made.

The story of the eventful dinner at the home of Aaron Director (Chicago) remains well documented by George Stigler (1988),[1] as well as too many others. That revelatory moment of triumph, when the scales of dissimulation and confusion fell from the assembled eyes of a conclave of doubting academics, has been rendered dramatically by one of the more notorious participants. In his autobiography, George Stigler conjures up the nearly mythical story of that now famous gathering, where Coase single-handedly challenged and changed the minds of a cohort of Chicago's finest economists. Drama though does not necessarily underwrite verisimilitude.[2] In Coase's understanding, his argument was primarily focused on transactions costs using a direct and unambiguous application of the marginal cost curve.[3] The prevailing

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Pigouvian model, at least as understood within the profession, failed to recognize such costs, but then implicitly analyzed the problem as if transactions costs were simply absent.[4] Coase persistently argued that if one assumed the total absence of transaction costs, then one should expect any externality to be effectively handled by inter-agent agreements. Consequently, if there are no transactions costs, then there is no relevant need for government intervention.[5] Externality problems would be simply internalized. That this runs counter to Coase's intention appears superfluous when run through the methodological millstones favored by the Chicago School.[6] However, if his work is read carefully, by first removing the Chicago installed goggles, Coase instead favors an open-minded, skeptical approach to such questions. Directly eschewed is any legal or moral notion of assigning blame by identifying a transgressor or formulating anything resembling a universal policy.[7] Rather, the driving rationale is simply a pragmatic one. The operative imperative is to discover, through careful empirical investigation, the most efficient response to a given (and rather specific) situation. No a priori judgments need apply.

> *Interviewer*: The place the Coase Theorem comes into play most often is when talking about pollution. The pollution problem has been seen in a very different light because of the Coase Theorem.
>
> *Coase*: It should be seen in a different light, but I don't see why you needed the Coase Theorem to do it. The pollution problem is always seen as someone who was doing something bad that has to be stopped. To me, pollution is doing something bad and good. People don't pollute because they like polluting. They do it because it's a cheaper way of producing something else. The cheaper way of producing something else is the good; the loss in value that you get from the pollution is the bad. You've got to compare the two. That's the way to look at it. It isn't the way that people look at it. They think zero pollution is the best situation. (Coase 1997: 2–3)

The use of the Coase theorem to support a narrow market approach to economics serves as a useful demonstration of the argumentation style that formed an essential part of the Chicago School. The vision implemented is a policy-based agenda structurally anchored by an unimpeachable and universal system of markets and decision-making. (Actors are defined as rational agents performing within a competitive market topography.) The emphasis remains firmly on the tactical advantage to be gained, rather than on any potential explanatory benefits afforded by such conceptualized responses. The Coase Theorem, as stipulated by George Stigler, clearly deviates from the intentions of its assumed creator. Instead it represents Stigler's attempt to buttress his own particular critical vision.[8] Consequently, the way in which the Coase theorem subsequently evolved into a key building block in policy debates is instructive in reflecting the manner and extent to which the Chicago School managed to sharply deviate from Classical Liberalism, which simultaneously highlights an alternative largely forsaken.[9]

> Similarly ideological is the way he [George Stigler] lit on Ron Coase and read the Coase theorem incorrectly, much to Coase's own amazement.[10] Coase never realized there was a theorem there. That's all a wonderful example of ideologically inspired criticism and also a perception of the subtle weakness of economics. This, from an economist who otherwise would, of course, have denied that ideology had any role to play in advancing the role of economics. (Conversation with Mark Blaug, April 1998)

The difference here is far more subtle than may appear initially. Stigler, like Coase, did not believe that transaction costs were ever strictly zero. The issue instead rested on the verisimilitude markets possessed in resembling those more theoretical versions underpinning the model of perfect competition. This construct formed the basis for Chicago-style price theory. Coase professed to be an agnostic in this matter, a 'Doubting Thomas' who needed to examine the specifics of the world on a case by case basis. He preferred feeling the actual and specific wounds rather than depending on any strict act of faith. In contrast, Chicago, and particularly Stigler, saw their surroundings as approximating, or running closely parallel to, the world limned by perfect competition. Markets in this perspective acted to resolve externalities since inherently ingrained incentives provided sufficient rewards for discovering solutions to any quandary.[11] In essence, the Coase Theorem indicated that transaction costs did not create serious barriers to the efficient working of markets. In which case, ignoring them would not traduce either economics or policies based on that bedrock.[12]

> Well that's a very strong externality. Because if you don't get vaccinated you endanger the others. I think that this is one of the reasons that Coase's work was so important to George. Coase's work indicated that an awful lot of things which *were* externalities or had the *potential* to be externalities might be handled in a non-governmental way. I know there is a problem with Coase. I've read a lot about the literature based on Coase for Stigler's "Law or Economics" article because that deals specifically with Coase. And it was very interesting. Many people interpreted Coase as saying it only works with *zero* transaction costs, and that wasn't how *I* interpreted Coase. Not that I'm anybody, but I thought that Coase was saying that to the *extent* that transaction costs are important, markets will be *less* effective, but not that you had to have zero. A lot depends upon how you interpret that one item. If you have to have 'zero', we don't have any 'zeros'. Now the Coase approach worked to some degree. You see that once again we have some empirical issues to test and measure. How much are the transaction costs that we do have? What mechanisms are at work to get rid of them? The way George saw the economy was as one in which the market *constantly* was adapting to all the non-market deficiencies that so much of the profession were concerned with. He knew that they were out there, that externalities were out there. However, he said, "Look, the market's rushing in every moment to take care of them. Here's Coase opening this big door for the market to rush in". And *that* was what George was focused on, starting perhaps in 1960 or '61, whenever it was that Coase gave his famous talk. George described in his memoirs that wonderful talk in which he said that everybody in Chicago who was there was wrong and Coase was actually right. George was focused on the way the market marches into *eliminate* the externalities, to work *around* them to make them a market problem instead of a non-market problem. I think I've quoted him in my memoir as saying something like, 'externalities are what the market has not *yet* eliminated'. That's not an exact quote but in my memoir I do have the exact quote. You see he saw the market as *the* force. He was looking at the other side of the market, at how the market may provide an appropriate solution.

> He said he saw this arbitrage going on all around him. Whenever there was a situation that somebody could take advantage of to make money he would. That was what solved a lot of these problems. (Conversation with Claire Friedland, October 1997)

Knight's Disciple

> When you say it is un-Chicago, you mean that it is an unmodern Chicago View. Because Frank Knight was at Chicago, and I was brought up more on Knight than I was on any of the others. And my views were quite consistent with what he says. They're not consistent with what George Stigler, Gary Backer and Richard Posner say. Posner condemns me because I don't think people maximize utility (Coase 1997: 3).

In 1964, Ronald Coase migrated to Chicago from the University of Virginia. He was designated, and acted as a not inappropriate successor, to the law school position previously developed under the watchful eye of Aaron Director, after an initial, and brief, appearance by Henry Simons. Coase was (and remained) a Classical Liberal, writing and working consistently in that tradition. Inherent in that approach, was his critical conclusion confirming the failure of the entire Pigovian framework in capturing the nuances that composed the arguments constructed to attack or defend the nature of markets.[13] He proceeded to expose those conceptual flaws in an article that seemed to combine basic notions of marginal cost with the insight that in any operative market economy, transaction costs shaped outcomes. The strategy of conjuring up a world lacking any shred or remnant of a transaction cost exists as an extended thought experiment. The intention is to direct a conceptual spotlight on the issue of transaction costs, rather than on the more Stiglerian obsession that insisted upon the long-run ability of the market mechanism to resolve any and all conceivable problems or seeming conflicts of interest.[14] Maintaining a Coasian practical and productive economic analysis of specific markets would consequently need initially to determine the relevant transaction costs operative under a given environment and set of circumstances. Policies, under this more pragmatic imprimatur, could only proceed from what, arguably, is the state of the world rather than from what any analyst might insist that state should be. Coase in this case was once again guided by Knight's Classical Liberalism (and good sense).[15]

> There are several reasons why the approximate character of theoretical economic laws and their inapplicability without empirical correction to real situations should be especially emphasized as compared, for instance, with those of mechanics. … The limitations of the results have not always been clear, and theorists themselves as well as writers in practical economics and statecraft have carelessly used them without regard for the corrections necessary to make them fit concrete facts. Policies must fail, and fail disastrously, which are based on perpetual motion reasoning *without the recognition that it is such*. (Knight 1971: 11)

The basis for Coase's seminal work finds its immediate inspiration in a paper published a mere one year before. In "The Federal Communications Commission" (1959), he attempted to examine the tacit agreement within the profession that radio and television channels were public goods to be allocated by the federal government rather than through a market price mechanism. Coase instituted a skeptical examination of this particular instance of regulation and the reason why it existed.[16] He did so by placing the history of such legislation leading up to the current state of regulation (as of 1959) under his critical microscope. The result of such a focused investigation failed to find a compelling case for regulation despite standard claims to the contrary.[17]

> It was indeed in the shadows cast by a mysterious technology that our views on broadcasting policy were formed. It has been the burden of this article to show that the problems posed by the broadcasting industry do not call for any fundamental changes in the legal and economic arrangements which serve other industries. But the belief that broadcasting industry is unique and requires regulation of a kind which would be unthinkable in the other media of communication is now so firmly held as perhaps to be beyond critical examination. The history of regulation in broadcasting demonstrates the crucial importance of events in the early days of a new development in determining long-run governmental

> policy. It also suggests that lawyers and economists should not be so overwhelmed by the emergence of new technologies as to change the existing legal and economic without first making quite certain that this is required. (Coase 1959: 41)

The fruition of this concrete and specifically targeted line of thought and approach, clearly evident in the FCC paper, reached its natural fruition the year after, following that fateful dinner at Aaron Director's house. Like that previous article, the full working out of Coase's argument also appeared in the *Journal of Law and Economics*, as "The Problem of Social Cost" (1960). The article to a healthy extent incorporates a clear application of Knight's unvarnished skepticism, entailing a refusal to accept any tenet of economics, no matter how firmly established. Such assertions, (economic truths that everyone knew) viewed from this entrenched, arms-length perspective, were assumed to be guilty and highly suspect until carefully examined and completely exonerated.

> Knight was a man of formidable character as well as intelligence. He was fiercely independent, and insisted upon a critical and searching examination of all matters intellectual. I suspect that he approached even the multiplication table with initial scepticism. (Stigler 1991: 1)

Coase specifically refused to interpret his social cost argument in a fashion implying that government intervention could never be required, independent of all and any circumstances. Such an extreme version of *laissez-faire* policy would have directly violated his style of economic thinking, a stance sustained consistently throughout his work. He fully recognized that when faced with a total absence of transactions costs, not only does such an environment prohibit the necessity for government intervention, but it equally fails to induce any serious substantiation requiring either markets or the mechanisms they are able to offer.[18] Completely specified inter-agent agreements could perfectly substitute for the role performed by market exchanges. However, the inherent existence of transaction costs negates the usefulness of applying any such fanciful model to serve as a practical basis for forming economic policy. Instead, as Coase stubbornly insisted, policy should preferably be negotiated on a case by case investigation of the relevant specifics of each and every instance under examination. Coase's argument, developed over his lengthy academic career, took aim directly at the broadly maintained acceptance of the Pigovian stylized economic welfare framework. His papers were not intended primarily to mount a persuasive argument either for or against government intervention. The goal instead was a concerted attempt to rethink the basic methodology used to buttress regulation, rather than to knit together a working condemnation of all such external interference.[19]

For Coase, economic theory had nothing directly to say about whether a government should or should not disrupt market mechanisms. Instead, he supported an approach essentially incorporating a fundamental aspect of Classical Liberal thinking, at least as filtered through such economists as Frank Knight. Coase's argument, sustained by case studies, examples and critical logic contended that assuming away transaction costs, excluded any model, no matter how intricately constructed, from being relevant to actual policy discussions. Such thinking remained incapable of resolving whether government intervention would make a positive difference given a specified set of circumstances. (Market problems persistently fail to exist in the abstract. Government administrators must face irritatingly specific issues not average or general ones.) Decision-making consequently had to be evidence, rather than theoretically, based.

> I wrote that "direct government regulation will not necessarily give better results than leaving the problem to be solved by the market or the firm. But equally there is no reason why, on occasion, such governmental administrative regulation should not lead to an improvement in economic efficiency." (Coase 1994a: 62)

Coase's approach, left room for too much as well as too little government. A priori judgements were not let in either through the front or back door, disallowing the attempt to mold observations to align with preordained conclusions. Both logic and evidence insisted on such stipulations. This perspective closely followed the Classical tradition—theory and models were used as aids to judgment, not definitive guides to policy. Coase clearly exhibited such open-minded intentions. His dislike of jumping from theory to policy was announced explicitly and repeatedly.[20]

> What is studied is a system which lives in the minds of economists but not on earth. I have called the result "blackboard economics," The firm and the market appear by name but they lack any substance. The firm in mainstream economic theory has often been described as a "black box." And so it is. This is very extraordinary given that most resources in a modern economic system are employed within firms, with how these resources are used dependent on administrative decisions and not directly on the operation of a market. (Coase 1994d: 5–6)

Stigler, however, seized upon this Coasian argument which persuasively undercut the stark Pigovian approach of moving directly from theory to policy. Stigler in fact proved to be as agile in basing conclusions on theoretical constructs as any of his East Coast opponents (perhaps even more skilled at times). He chose instead to encapsulate a lengthy article, bulging with abbreviated case studies, into a slogan length dictum. His newly hatched catechism of faith transformed a methodologically alternative argument into one that followed the modernist instinct to move inevitably from theory to policy. The striking difference with the rejected Pigovian stylized method was that Stigler's reformulation, which he neatly labeled the Coase theorem,[21] effectively buttressed a policy that commanded his a priori support—leave the market alone, since the market unhindered must achieve (almost by definition) an efficient outcome.[22] Thus, instead of interpreting the argument as Coase desired, focusing on the need to incorporate transaction costs when formulating policies, Stigler chose to interpret the article's arguments as underwriting a framework that would effectively demolish the problem posed by externalities.[23] For Stigler, his reframing of Coase demonstrated the preordained conclusion that markets were capable of taking care of such troublesome issues. The market mechanism was essentially self-correcting since from Stigler's perspective, it simply had to be.[24]

Using his carefully tailored version of Coase's work, Stigler adapted this purpose constructed argument to serve as a bulwark against government intervention.[25] Coase's insight, or strategic ploy, now transformed into the irrefutable Coase's Theorem, sported an implacable insistence that in the absence of transactions costs, externalities should never be conceived as a problem awaiting a solution. The Pigovian externality analysis, hitherto a thorn in the side of market fundamentalism, could now be effectively extricated. Price theory could provide both a necessary and sufficient reason for supporting an unconstrained laissez-faire policy. While technically (and nearly by definition) true, the Coase Theorem was simultaneously irrelevant for any direct policy application.[26] The theorem unfortunately implied that in the absence of those same transactions costs, there would be no need for markets—people would just freely negotiate all agreements. But for opposing mainstream economists to challenge Stigler's creation and its tacit policies, they would have to be willing to backtrack on their own foundations. Essentially these academics would need to confess the errors of their proverbial transgressions. Critics would be obliged to admit that the Pigovian framework, the construction that they had so carelessly embraced as a model and way of thinking, failed equally to propose any direct policy applications, or at least any legitimate options. To effectively defeat the Chicago position they had to be willing to surrender and discard their own predisposition by turning their back on the policy methodology they chose to employ.[27]

Stigler's invention of a Coasian theorem inevitably became highly contentious, given its role as a rhetorical ploy. Stigler habitually courted such controversy. However, when viewed from a more practical perspective, both market and government solutions inevitably involve a set of specific attributes and conditions attached to them. Unfortunately, in this case theory persistently triumphed over available evidence. Instead of promoting the more reasonable Coasian position, that real-world decisions need to be made on a case by case basis, this 'through the looking glass' transformation offered the seemingly tantalizing gift of delivering policy absolutes. Under Stigler's skillful stage crafting, Coase's Classical Liberal position was forced to transcend any imposed constraints, offering instead a rhetorical basis that proved capable of supporting an array of market mechanisms. Mainstream models, faced with the attack constructed under the aegis of Stigler's Coase Theorem, could only hope to counter this torrent of logic by cannibalizing its own underpinnings.[28] Unfortunately, would be opponents to the Stigler steamroller proved reluctant to surrender the comfort of their own scientifically bolstered version of modernism. Consequently, they almost deliberately failed to place what would seem to be an effective spoke in the wheels of the opposing ideological express.

Laissez Faire Uber Alles: Or at Least When Appropriate

> Of course governmental action, if effective, limits freedom, and few of us are anarchists. It should not be necessary to argue either for or against laissez faire in principle. The issue lies in the amount of freedom, or of control, and the kinds, which depend on circumstances. (Knight 1967: 782)

Coase's consistent message in his written and printed work views transaction costs as fundamental to analyzing any market economy. Because of such inherent frictions, universal principles, those obtained by the science of economics, cannot be applied directly to each and every market. Policy conclusions based on this brand of 'blackboard economics' are bound to mislead and are in a crucial sense absurd. The only fruitful response to the unfortunate inconvenience of transaction costs is to examine each situation on a case by case basis before suggesting possible policy responses. Such a response inevitably entails judgment and thus solicits differences of opinion even when assisted, if not aided and abetted, by truckloads of statistical evidence. Thus Coase saw the necessity for getting his hands dirty, something that Knight welcomed without necessarily indulging in the practice himself.[29]

Though trained in the 1930s, not unlike Stigler or Friedman, Coase never saw the same allure to the type of modernism that arose during that decade. Though he was hardly averse to the employment of statistics, utilizing them during a period when the available tools made such strategic employment a genuine challenge, he never quite bought into the waltz that took the Chicago School by storm: formulate a hypothesis, test the hypothesis and formulate policy from the theoretical model. Thus Chicago, with George Stigler performing as the engineer driving that academic locomotive, campaigned relentlessly for the employment of statistics and quantitative methods as the path to scientific certainty.

> I was sitting with Aaron Director at the time when he [George Stigler] gave his Presidential address and we did look at one another at the time to try to see what each one thought about all of this. (Conversation with Ronald Coase, October 1997)

Coase remained an outlier in the profession despite his growing reputation during the latter part of his career. He became best known for a theorem he neither created nor promoted. Equally he stood his distance from the theorem's implied objective. He was a loyalist to the Marshallian tradition as inculcated in him by his LSE teachers, Edwin Canaan and especially Arnold Plant. Thus he remained in his own odd way methodologically impervious to the furor that seemed to overturn economics, beginning in the 1930s and sweeping away Classical Liberalism in the postwar period. Certainly, he dug in his heels when presented with the sort of strategic methodology formulated by Friedman and Stigler. Such a construct, in his view, was simply unworkable.[30]

> If all economists followed Friedman's principles in choosing theories, no economist could be found who believed in a theory until it had been tested, which would have the paradoxical result that no tests would be carried out. This is what I meant when I said that acceptance of Friedman's methodology would result in the paralysis of scientific activity. Work could certainly continue but no new theories would emerge. (Coase 1994b: 24)

However, in the 1930s at least, he was not quite the oddity he would become. Classical Liberalism still largely dominated the discipline, especially among the older academics. Many in the profession continued to follow Marshall in taking a cautious approach to theory, wary of imposing a physics-like agenda on the discipline. In his presidential speech to the American Economic Association, a future colleague of Stigler's (John Maurice Clark) could sum up the past and present by reminding his audience of crucial limitations inherent in theoretical approaches. (Contrasting Clark's speech with that of Stigler, almost thirty years later, reveals to a considerable degree the transformation of the profession's methods and approaches.)

> … while a picture of perfect equilibrium deals in its way with forces which are at work in the actual world, the form in which it presents these forces will almost inevitably need to be modified when we move on to the task of studying them as they actually operate. (Clark 1936: 4)

For Coase, empirical work embraced more than simply the sort of statistical practices that seems to define modern-day economics.[31] From his perspective, humble facts and observations also played a significant role. In sum, sheer elegance and difficulty in model construction, and testing, did not equate with suitability for application. Rigor could be rightfully redefined, from this perspective, as containing the desired degree of persuasion using the least amount of resources. His reliance on empirical detail appears to have been largely overlooked by those who seemingly embraced his work (or claimed to do so).

> *He doesn't seem to have ever been very interested in Coase's '37 work on the firm and transaction costs. Is that correct, and why? Why is that of no concern?*
> Milton Friedman: I don't really …
> Aaron Director: I never thought there was anything in it. But George always said, that well, you've got to start somewhere and that was as good a place as any.
> *Because I know, in none of his work, is there any interest in that aspect. I mean it seems to me that Ronald Coase has always stressed going out and looking at firms.*
> Milton Friedman: And where do you see him doing that? The industry work that Ronnie did is a study on the British Broadcasting industry (1954) and …

> Aaron Director: The post office (1961).
> Milton Friedman: Yes, the post office … but none of that had to do with the issue of what determine the boundaries of a firm. His '37 article said essentially that market transactions involve transactions costs. If you have fewer transaction costs by doing it within a firm then you do it within a firm. If you have more, etc. But to the best of my knowledge, none of his later work really answered that question. (Conversation with Milton Friedman, Rose Friedman, Aaron Director, August 1997)

What is interesting here is the narrow interpretation of Coase's method and his work, particularly the role that transaction costs plays. Like their friend and colleague, George Stigler, they simply don't appear to comprehend his purpose. As previously mentioned, their respect seems largely dependent on a group misunderstanding of his *Social Cost* (1960) paper. His other work was not really contiguous with their own methodology or understanding of economics.[32] Common ground was only shared in the realm of ideology, although unlike the Chicago contingent, Coase did not seem to claim such a firm grasp on how the world worked. Coase departed company with his Chicago colleagues in this sense, since his Classical Liberal approach shunned such absolutes. Consequently, he was unable to wholeheartedly subscribe to a core faith in rational decision-making, which seemed to sustain much of the work done at Chicago.[33]

> I don't say people are wholly irrational. I have said that almost the only thing we can say about consumer behavior is, if you raise the price of something, people will demand less. And that we know, but it doesn't follow that because a person does less foolishness when the price is high for foolishness that you don't have foolishness. The foolishness follows the universal law of demand. The greater the price you have to pay for being foolish, the less you do. (Coase 1997: 3)

In some ways, though perhaps straining for a literary correspondence, Coase is not unlike another English compatriot, one who over the centuries since her death was often misunderstood, especially by male authors. Jane Austen sketched her novels in miniature, unlike the grand historical romances that made her contemporary literary figure, Sir Walter Scott, that era's most popular novelist. Chicago hunted the Snark of Lewis Carroll, the universal that opened all locked passages and resolved all matters of importance. In contrast, Coase seemed uninterested in how markets should operate or people should decide, but remained endlessly fascinated with delving into the specifics of the actual world, trying to sift through the available evidence for what might be the case. He proved ever reluctant to accept a theory based claim, or a policy deduction dependent on logic alone.

Perhaps Austen was not much of an enthusiastic fit for the ensuing Romantic era, which gained momentum soon after her death (1817), just as Coase was swamped by a postwar modernist methodology. Though more perspicacious authors, such as Trollope or Bronte, certainly had no trouble recognizing her specific and unusual talents.

> I have likewise read one of Miss Austen's works, *Emma* – read it with interest and with just the degree of admiration which Miss Austen herself would have thought sensible and suitable – anything like warmth or enthusiasm, anything energetic, poignant, or heartfelt, is utterly out of place in commending these works: all such demonstrations the authoress would have met with a well-bred sneer, would have calmly scorned as outré and extravagant. She does her business of delineating the surface of the lives of genteel English people curiously well; there is a Chinese fidelity, a miniature delicacy in the painting: she ruffles her reader by nothing vehement, disturbs him by nothing profound: the Passions are perfectly unknown to her; she rejects even a speaking acquaintance with that stormy Sisterhood; even to the Feelings she vouchsafes no more than an occasional graceful but distant recognition; too frequent converse with them would ruffle the smooth elegance of her progress. (Bronte 1850)

However, unlike many of the major literary figures of her time, Scott did recognize Austen's worth. Unfortunately, the Chicago School never quite fathomed Coase's objectives.

> Also read again, and for the third time at least, Miss Austen's very finely written novel of _Pride and Prejudice_. That young lady had a talent for describing the involvements and feelings and characters of ordinary life, which is to me the most wonderful I ever met with. The Big Bow-wow

> strain I can do myself like any now going; but the exquisite touch, which renders ordinary commonplace things and characters interesting, from the truth of the description and the sentiment, is denied to me. (Scott 1826)

One Example Does Not a Universe Create: Classical Liberalism in Practice

> Now take an economics textbook. It's more or less the same. Exposition improves, techniques improve. There is a lot more illustrative material that didn't exist before, but anyway that's my view so the empirical work doesn't seem to change a vision, or hasn't in economics, but I don't know why. It's very tricky, this whole business of how ideas emerge and subjects change, and so on. (Conversation with Ronald Coase, October 1997)

An all too common mistake, based on this example of an alternative to the Chicago forged path, would be to conclude that the specific case of Ronald Coase somehow represents an incontrovertible and identifiable result of applying a Classical Liberal approach to economic policy formulation. The consequent error would be to confuse, if not emphatically muddle, a specifically defined ideology with a broad-based methodological approach. The deductive logic would identify the similarities in the topological reach of Chicago and Virginia based policies with the type of liberalism originating largely in the nineteenth century.[34] Doing so would fall abruptly into the fallacy of universalizing based on a very limited number of examples. Thus the conclusion would be that methodologies fail to exert any effective leverage, since in either case, the end result is a very similar conservative or right-wing policy perspective. To do so however is simply to misunderstand the issue under discussion.

The focus in this particular analysis is deliberately not on ideology, which often drives the formation, and particularly, the targeted marketing of specific policies. Instead, the relevant concern is attached to the relation between economic theories and policy formation. Theory in this analysis has nothing definitive to say about devising an appropriate policy, namely one that would achieve a very concrete objective in a specific environment. The science of economics is then not to be considered as some sort of mechanical automaton capable of spitting out an array of all-purpose policies. Consequently, applied analysts are never excused from carefully investigating the specifics of individual markets and firms, from getting their hands dirty pawing through details.[35] Observations and closely considered empirical work are indispensable. Undertaking such activities is not merely busy work and drudgery only suited to those with mediocre abilities and aspirations. As a consequence, rejecting Classical Liberalism for perceived ideological reasons turns out to be as equally wrong-headed as accepting it for counter-composed ideological positioning.

As pointed out previously, his published work is spangled with often detailed investigations of specific items, whether they be pigs, post offices, the BBC, or the Federal Communications Commission. In line with John Stuart Mill, but unlike his colleague, George Stigler, he declined to view broad-based or universal economic rationality as encompassing the boundaries of economic inquiry.

> Political economists generally and English political economists above others, have been accustomed to lay almost exclusive stress upon the first of these agencies, to exaggerate the effect of competition, and to take into little account the other and conflicting principle (custom). They are apt to express themselves as if they thought that competition actually does, in all cases, whatever it can be shown to be the tendency of competition to do. This is partly intelligible, if we consider that only through the principle of competition has political economy any pretense to the character of a science. (Mill 1965: 242)

Coase modestly accepted any evidence he deemed legitimate, validating historical as well as psychological evidence.[36] Unlike George Stigler, who adeptly shaped statistical evidence to fit into his preordained jigsaw puzzle, Coase pawed among the details of historical records in the attempt to determine what occurred in the past, rather than assuming that the world worked in line with his own modeled realities.[37] While Stigler considered such details of industries and firms to be superfluous, Coase focused on these intrinsic particulars rather than relay on abstract theorizing.[38] By embracing this Classical Liberal perspective, policies become dependent on operative specifics, rather than theoretical conclusions.[39] Ignoring the dominant institutional constraints of a given era would consequently insure that policy recommendations would lack any compelling logical or practical basis for subsequent application.

> In my long life I have known some great economists but I have never counted myself among their number nor walked in their company. I have made no innovations in high theory. My contribution to economics has been to urge the inclusion in our analysis of features of the economic system so obvious that, like the postman in G.K. Chesterton's Father Brown tale, "The Invisible Man," they have tended to be overlooked. Nonetheless, once included in the analysis, they will, I believe, bring about a complete change in the structure of economic theory, at least in what is called price theory or microeconomics. What I have done is to show the importance for the working of the economic system of what may be termed the institutional structure of production. (Coase 1992: 713)

One way to comprehend the approach Coase adopted, and its relation to Classical Liberalism, is to look at two of his works which are strictly empirical in content. Both efforts, however, remain devoid of the standard statistical testing that represents, almost in its entirety, what economists are wont to categorize as empirical analysis. According to the Chicago School, theoretical hypotheses are proposed and then submitted to a test by statistical ordeal. If deemed verified, conclusions, including policy recommendations, are then allowed to flow. Coase instead, often sought to verify commonplace ideas that were virtually accepted as being logical necessities, given the way in which economists insist that the world must operate. Coase, in essence, carefully exhumed evidence that definitely undermined seemingly unarguable verities. This predilection remains a significant trait for those papers previously discussed, including his most well-known efforts (1937a, 1960). In many cases, he unsurprisingly targeted Pigovian ideas of private versus social welfare functions, including conceptions of public goods and the implied policies attached to such notions.

In his 1974 examination of lighthouses, Coase directly sheds an empirical floodlight on the problem of public goods. Given that these are non-rival and non-exclusionary goods or services, governments, according to textbook theory, are obliged to unquestionably insure their existence. The private sector is accordingly excluded from lines of production given the nature of the output required. Such a definitive public policy conclusion can be directly approved while remaining comfortably ensconced in one's office armchair. Conveniently, deductive logic takes over to an extent that excludes the necessity for checking the historical record. Coase quotes Samuelson's textbook as a convenient reflection of the discipline's unquestioned faith in unverified theory.

> … in the lighthouse example one thing should be noticed: The fact that the lighthouse operators cannot appropriate in the form of a purchase price a fee from those it benefits certainly helps to make it a suitable social or public good. But even if the operators were able – say, by radar reconnaissance – to claim a toll from every nearby user, that fact would not necessarily make it socially optimal for this service to be provided like a private good at a market-determined individual price. Why not? Because it costs society *zero extra cost*. To let one extra ship use the service; hence any ships discouraged from those waters by the requirement to pay a positive price will represent a social economic loss – even if the price changed to all is no more than enough to pay the long-run expenses of the light-house. If the lighthouse is socially worth building and operating – and it need not be – a more advanced treatise can show how this social good is worth being made optimally available to all. (Samuelson quoted in Coase 1974: 359)

Notice the common preoccupations displayed in this 1974 piece. Previously (1946) Coase had explored what he came to consider as the myth of marginal cost pricing. But in this specific example of a public good, economic wisdom appeared willing to claim, despite lacking any historical, or verifiable, facts that the private sector and lighthouses maintained no contiguous borders. In the lighthouse example, readers are rigidly instructed by economists to deny the wisdom of private enterprise since users cannot be excluded (property rights not preserved). Even more tellingly, at least according to standard theoretical analysis, the marketplace, in this case, would be unwilling to supply any additional beams of light at a zero price despite the marginal cost of doing so remaining resolutely at an indicative price of zero. (By definition, non-rival goods must be priced accordingly.) Consequently, only government funded activity could claim the prize of theoretical efficiency. Coase refuses to simply accept this reality despite the overwhelming evidence that to economists these conclusions are indisputable.[40] He proceeds to poke an inquisitive finger into the solidity of such claims (governments must build, own and operate these edifices) by examining the historical record. Within the coterie defining the academic realm, grubbing through piles of moldering paper for the facts of a matter lies beneath the dignity of most members of the profession. They deliberately view themselves as inhabiting and exploring a higher sphere of knowledge. Careful examination, unfortunately for the self-assured textbook-based analysis, indicates that in Britain at least, private ownership and operation of lighthouses did exist at one time. This indisputable fact would seem to entirely undercut the basis for the textbook snippet of theory dictated policy. Notice that Coase consistently failed to claim that such evidence supported any conclusion regarding the best option for constructing and maintaining such edifices. Again, his Classical Liberal ethos would not permit such a leap of unfounded faith. But it laid to rest the claim that only governments can supply such a service simply by labeling lighthouses as being an indisputable public good.[41] As Coase implies, the real policy challenge lies in discovering the best method for providing this particular service given the specifics of the case.[42]

> The system apparently favoured by Samuelson, finance by government out of general taxation has never been tried in Britain. Such a government-financed system does not necessarily exclude the participation of private enterprise in the building or operation of lighthouses, except in a very attenuated form and would certainly be quite different from the system in Britain which came to an end in the 1830s. Of course, government finance would be very likely to involve both government

> operation and government ownership of lighthouses. How such governmental systems actually operate I do not know. Bierce's definition of an American lighthouse – "A tall building on the seashore in which government maintains a lamp and the friend of a politician" – presumably does not tell the whole story.
> We may conclude that economists should not use the lighthouse as an example of a service which could only be provided by the government. But this paper is not intended to settle the question of how lighthouse service ought to be organized and financed. This must await more detailed studies. In the meantime, economists wishing to point to a service which is best provided by the government should use an example which has a more solid backing. (Coase 1974: 376)

The second related case takes aim at the predilection of economists for backing up their theories with 'just so' stories. Thus a long running predilection within the profession is for ersatz examples that vivify and seemingly prove their theoretical constructs. These often repeated fables appear to put meat on what would otherwise remain as theoretical bones. In this category, if the described incidents failed to occur or lacked the grace to manifest themselves in the fashion demanded, the economist's implicit response is to simply claim that 'it should have happened'. We are immersed then, just as in the realm of policy, with accepting that the theoretical tail in this case should continue to wag the historical or evidentiary dog. In much the same way, films often advertise, or preface their screening by claiming that they are based on a true story. Such an assertion supposedly provides a gloss of verisimilitude to the cinematic fiction that the film would otherwise lack. A false degree of credence becomes associated with what is no more than a dramatic construction. Unfortunately, this carries no more weight in economics than it does within cinematic endeavors. In the case Coase tackled, with his usual skeptical vision, he re-examined a specific chunk of business history that had managed to become something approaching gospel in the repertoire of standard economic illustrations.

Historical fact, meaning an event that is difficult or even foolish to dispute, states that in 1926 General Motors merged with Fisher Body at a time when closed bodies on cars were overtaking the demand for open roadsters. This takeover occurred as the demand for closed bodies grew. The open issue is why this historical fact unfolded at this time. In 1978, Klein, Crawford and Alchian, used this undeniable occurrence to illustrate their theory of 'hold-up' as the prime motivating force behind vertical integration. Their fashioning and explanation of the story behind such a corporate marriage was seemingly molded to form a useful illustration highlighting the express needs of the theory presented. Thus the validity of the explanation lay in the manner in which it so snugly accorded with their suggested hypothesis. In a perverse fashion, the theory justified or explained the events rather than having the historical record supporting the theoretical construct. As with many such tales, subsequent repetitions of this verity, in print and in the classroom, transformed this story (this jury-rigged explanation) into unalloyed (and unquestioned) fact. The story could be said to worm itself into the discipline's oral tradition. Yet because of this noncontroversial facade, for a number of decades, no economist bothered to actually examine the historical record to distinguish mythmaking from business history. When others, including Coase, did so, questions were raised and the explanation began to crumble. The previously untarnished story seemed to suddenly edge distinctly over into the realm of historical fiction. From the Classical Liberal perspective, history matters and theoretical imperatives consistently fail to shape the contours created by the past.

> It is commonly said that in 1926 General Motors was led to acquire its supplier of automobile bodies, Fisher Body, because Fisher Body held up General Motors. It is claimed that Fisher Body did this by locating its body plants far away from the General Motors assembly plants and by adapting inefficient methods of production, thus increasing both the cost of producing bodies and the profits of Fisher Body under its cost-plus contract. This tale is factually incorrect. What General Motors acquired in 1926 was the 40 percent of the shares of Fisher Body that it did not already own. Furthermore, Fisher Body did not locate its plants far away from the General Motors assembly plants. It is also most implausible, for many reasons, that the Fisher brothers would have used inefficient methods of production. There is no evidence that a holdup occurred. (Coase 2006: 16)

Respecting Coase's methods and approaches to economics entails no equal necessity to support his ideological leanings or his evaluation of available evidence. However, Classical Liberalism does require that his results be taken seriously and not dismissed simply because they are not congenial to some conflicting set of a priori values. Instead, the obligation of those who dissent is to attempt to seriously understand the basis of the argument presented and only then to investigate any weak links and the possibility of misinterpreted evidence. In the spirit of James Stuart Mill or Albert Hirschman,[43] choosing voice, instead of either a categorical loyalty or exit option, and engaging in mutually respectful argument serves as the best path for broadening understanding. Gladiatorial displays within academic arenas are intended merely to decimate and bury opposing theories and ideas. These modes of debate seek instead to simply substantiate, without either deepening or certainly improving, any given set of ideological perspectives.

I know in that same piece, 'How do Economists Choose', you start off by referring to Warren Nutter and then you quote the line from Frank Knight about how in order to achieve objectivity you need competence, integrity and humility.[44] *Do you think George Stigler had humility?*

Ronald Coase: He didn't show it. He may have had it, but it wasn't that apparent. He always appeared confident, sure of himself, but in a way that suggests to me a sense of insecurity. I don't know whether others felt that, but I did. He was always very nice and kind and helpful in many ways. Always. But I often wondered how far he agreed with what I was saying. I think he thought I was all right, but a little odd. (Conversation with Ronald Coase, October 1997)

Notes

1. George Stigler presents the evening as a triumph of market logic over poorly examined textbook verities. But a key to understanding Coase's insight was widely missed. The largely unrecognized impact of that evening's debate remained buried by having the subsequent focus of inquiry shifted quite sharply away from the article's original direction.

Initially unremarked in the ensuing debate was Stigler's willingness to jump, from his formulation of a fundamentally ersatz Coase's theorem, directly to a set of implied policy recommendations. Instead, Stigler in his strategically remembered account presents a more romantic and historical snapshot where truth inevitably triumphs over false belief.

> We strongly objected to this heresy. Milton Friedman did most of the talking, as usual. He also did much of the thinking, as usual. In the course of two hours of argument the vote went from twenty against and one for Coase to twenty-one for Coase. What an exhilarating event! (Stigler 1988: 76)

This description fits rather snugly with Stigler's almost sweet, but nearly ingenuous belief in Edwin Canaan's dictum that in the long run truth wins out. "However lucky Error may be for a time, Truth keeps the bank and wins in the long run" (Cannan 1903: 392).

2. Even the scientific yearnings of economics often surrender to the demands of professional mythmaking. In such instances the ruling dictum coincides with the sentiments expressed in John Ford's classic western, *The Man Who Shot Liberty Valence*. "No, sir. This is the West, sir. When the legend becomes fact, print the legend."
3. In contrast, Coase has recollected that during that fateful dinner, only Arnold Harberger managed to actually understand what he was attempting to do (Conversation with Ronald Coase, October 1997).

> I remember at one stage, Harberger saying, "Well, if you can't say that the marginal cost schedule changes when there's a change in liability, he can run right through." What he meant was that, if this was so, there was no way of stopping me from reaching my conclusions. And of course that was right. I said, "What is the cost schedule if a person is liable, and what is the cost schedule if he isn't liable for damage?" It's the same. The opportunity cost doesn't shift. (Coase 1997: 2)

The habit of thinking in terms of transaction costs while employing a straight-forward application of the marginal cost concept is not original with his 1960 paper. Certainly a similar approach appears in his earlier, but equally famous paper on the firm (1937a). Coase in his insights and methodology seemed to remain an unabashed hedgehog throughout his lengthy career. Though in fact, transaction costs were not the one big thing he knew. They served more practically, only as a set of useful goggles helping to understand a number of very specific things about given economic phenomena. Perhaps then, Coase was more of a fox (knowing many little things) who successfully masqueraded as a hedgehog.

4. Pigou was careful to note the limitations of his framework. Later users of his welfare framework, such as Lerner, or even Samuelson, were less finicky. See Pigou (1920).

5. Given Coase's world of positive, and often significant transaction costs, the importance of property rights and the role of government immediately come to the fore.

   If we move from a regime of zero transaction costs to one of positive transaction costs, what becomes immediately clear is the crucial importance of the legal system in this new world. (Coase 1994d: 11)

6. The confused relation between the Stigler formulated 'Coase Theorem' and Coase's intentions reflects a level of muddle, which in the realm of chemistry would confuse a reaction, with the catalyst essential to producing a particular chemical outcome. More precisely, what textbooks inevitably attach to Coase's name, perhaps being the sole knowledge many economist have about the Nobel-awarded economist, exists as no more than a Hitchcock-style McGuffin in the actual article itself. (How carefully such articles are read, or if they are perused at all, is yet another question perhaps best left unsolved.) The formulated Coase Theorem then is simply employed as a plot device, a starting point that initiates the story that Coase wants to tell. The terminology itself (a McGuffin) was devised and employed by Alfred Hitchcock to describe his own plot devices. Essentially they existed as a means to an end, necessary but in and of themselves only of negligible importance. For instance, in Hitchcock's film *Psycho*, the money that Janet Leigh steals at the commencement of the film serves merely as a convenient mechanism to deliver her to the dubious environs of the Bates Motel. But if viewed from the perspective that Stigler uses to analyze that seminal 1960 article by Coase, the film could only be understood as a story about a trusted employee who came to a bad end. Such an unfortunate result ensued due to her inability to resist temptation, proving once again that crime does not pay. However, such an analysis manages to somehow ignore approximately two-thirds of the actual film, which consequently could logically be dismissed as no more than an extravagant waste of time and effort. In much the same fashion, Stigler's formulation of Coase's intentions deftly ignores the bulk of the article, reducing it to nothing more than unremarkable space filler. Stigler, whether deliberately or not, allows the McGuffin of the article to become the fulcrum point, and the very essence, of Coase's thought. The stark differences in Stigler's and Coase's methodological approaches to economics could not be laid out with greater surgical precision. The differentiated responses to the same material act as equivalent to a

strategic autopsy that succeeds in revealing the economics practiced by these two famed, but quite dissimilar, economists.

7. An irreducible tendency persisted within these Chicago-style ramparts, one which served to create a persistent resistance to Coase's basic methodology. (Coase's approach harked back to the classical liberalism of Frank Knight, rather than the post war reconstruction of liberalism engineered by Stigler, Friedman and Director.) The lack of universal dictums offered within the Coasean literature represented a failure according to the standards formulated by their scientific analysis, rather than a possible path to practical policy applications. In sharp contrast, work such as Coase's (1974) detailed analysis of lighthouses in Britain is a distinct initial exercise in understanding the possible economic roles government might play in a world of positive transaction costs. This investigation deliberately eschews the pure world of theoretic models, whether proffered by those perched on either side of the political spectrum.
8. Care must be exercised at this point to emphasize the total absence of any malevolent objective on Stigler's part. Machiavellian manipulators and creators of spidery conspiracies have no role to play in the unfolding of these described events. No evidence of dishonorable intentions appear ready to be unearthed. Rather, this is the way George Stigler comprehended the world. Given his underlying understanding of Coase's (1960) work, if anything, Stigler was being aboveboard and generous by recognizing Coase as the originator of this soon to be famous (if not at times infamous) theory.

   > I christened the proposition the "Coase Theorem" and that is how it is known today. Scientific theories are hardly ever named after their first discoverers (more on this later), so this is a rare example of correct attribution of a priority. (Stigler 1988: 77)

9. Another Chicago trained economist who remained much more closely aligned to the Classical Liberal tradition, especially as envisioned by Frank Knight, was one of his few Ph.D. students. James Buchanan created what might best be labeled as a Constitutional approach to constructing economic theory. Buchanan eschewed the temptation of arguing for markets by marshaling standard economic models to perform the dirty work. Instead he fashioned broader arguments that supported the notion of free markets and laissez-faire. But precisely because he chose to move in that direction, one that ran counter to the period's *Geist*, his arguments failed to exercise the degree of influence

wielded by his erstwhile Chicago School counterparts. His public choice work (especially as influenced by Gordon Tullock [1962]), insight that was reducible to a mathematical economic analysis of politics (and thus more tractable to desired objectives), consequently managed to gain a noticeably greater level of support and associated impact.

10. To underline this point, the textbook formulation familiar to students is not explicitly stated anywhere within Coase's 1960 article. What became widely known as Coase's Theorem is more correctly the creation of George Stigler in his subsequent interpretation of the paper. Some economists have objected that such a formulation conveys neither the content of the article nor Coase's intention. Coase himself has raised doubts as to whether Stigler understood his work or the objectives of that work.

    > But you know our relationship was very cordial, very friendly, but I didn't know, half of the time, what he thought about me. You know it was respect, but how far? (Conversation with Ronald Coase, October 1997)

11. The shorthand for this notion is that 'no fifty dollar bill is left on the sidewalk'. People are on the lookout for, and eager to exploit, any opportunity which would leave them better off. Markets are structured to provide rewards for those removing those blockages which are tenaciously restricting exchange. Consequently, externalities in George Stigler's parlance are merely problems that markets have yet to solve.

12. Coase clearly delineated his differences with Stigler in his Nobel Prize speech. He never assumed that it was his role to enter into any confrontation with his colleague over the validity of Stigler's formulation of something he insisted on labeling the Coase Theorem. Given the hedged assumptions forming the foundation of that proposition, little of interest can be said about the free flowing exchanges occurring in such a world. But the point of Coase's work is to see the necessity of vacating a world where transaction costs cease to exist.

    > What I showed in that article, as I thought, was that in a regime of zero transaction costs – an assumption of standard economic theory – negotiations between the parties would lead to those arrangements being made which would maximize wealth, and this irrespective of the initial assignment of rights. This is the infamous Coase Theorem, named and formulated by George Stigler, although it is based on work of mine. Stigler argues that the Coase Theorem follows from the standard assumptions of economic theory. Its logic cannot be question, only its domain. I do not disagree with Stigler. However, I tend to regard the Coase Theorem as a stepping stone on the way to an analysis of an economy with positive transaction costs. The significance to me of the Coase Theorem is that it undermines the Pigouvian system. Since standard economic theory assumes

> transaction costs to be zero, the Coase Theorem demonstrates that the Pigouvian solutions are unnecessary in these circumstances. Of course, it does not imply, when transaction costs are positive, that government actions (such as government operation, regulation or taxation), including subsidies could not produce a better result than relying on negotiations between individuals in the market. Whether this would be so could be discovered not by studying imaginary governments but what real governments actually do. My conclusion: Let us study the world of positive transaction costs. (Coase 1994d: 10–11)

13. In many ways, Ronald Coase was inspired, if only negatively, by Pigou's efforts. As early as 1946 (and as will be demonstrated, even before), Coase took aim at what he saw as being basic missteps in understanding the tools economics could provide. Here it might prove convenient to remember Coase's practical turn of mind, which proved not to be overly amenable to purely theoretical efforts. In particular, the modernist spirit in the 1940s had allowed young economists, such as Lerner (1944), to deduce the need for government intervention in markets where firms enjoyed economies of scale. The science was impeccable. Efficient pricing (and production) demanded marginal cost settings. Economies of scale implied falling average costs and therefore marginal costs lying below average unit ones. Since firms would find efficient pricing to be antithetical to survival (the need to cover opportunity costs), governments were scientifically obliged to uphold market requirements (to duplicate optimum resource results). "The amount by which total costs exceed total receipts (the loss, as it is sometimes termed) should be a charge on the Government and should be borne out of taxation" (Coase 1946: 169). The interesting problem raised by this early example of model-based policy is the lack of any concrete examination of what firms actually do (or could do) when faced with exactly this challenge. Even worse, no indication in either Lerner's work or that of Hotelling (1938) hints at the need to consider other potential pricing choices or the response of consumers within the relevant market. Policy formation is instead stripped down to the construction of an economic model with policy directly derived from that model's conclusions. But for Coase the very logic underpinning this broadly accepted analysis is faulty.

    > Any actual economic situation is complex and a single economic problem does not exist in isolation. Consequently, confusion is liable to result because economists dealing with an actual situation are attempting to solve several problems at once. (Coase 1946: 170)

    For Coase, the methodology, and unsubstantiated certainty, attached to such policy recommendations, have an otherworldly quality attached to them. Historical results become unnecessary when markets are expected to align themselves to the constraints imposed by economic science. These cleverly stitched together policies carry with them more than a definite whiff of the methodology deployed by Swift's tailors of Laputa. Imbued with Knight's skepticism of any such all-embracing plans of improving the world, Coase would have little faith that government of any stripe or ability would be capable of implementing such immaculately conceived schemes.

> This, he [Hotelling] says, "is an interesting historical question". And he adds later: "When the question arises of building new railroads or new major industries of any kind or of scrapping the old, we shall face, not a historical, but a mathematical and economic problem". Nowhere in Professor Hotelling's article does one find recognition of the fact that it will be more difficult to discover whether to build new railroads or new industries if one does not know whether the creation of past railroads or industries was wise social policy. And it is certainly not absurd to take into account the fact that decisions are likely to be better made if afterwards there is some test of whether such decisions were wise social policy than if such an enquiry is never made. (Coase 1946: 175–176)

14. Stigler sometimes verges on the boundaries of formulating a mirror version (though implacably defined by the market) of Marx' historical materialism. Individuals passively respond to market necessities having surrendered economic power or influence in a Hobbesian type arrangement. In a throwback to the Old Norse Gods, within Stigler's framework, 'Fate is inexorable'.

> I believe, on the contrary, that if Cobden had spoken only Yiddish, and with a stammer, and Peel had been a narrow, stupid man, England would have moved toward free trade in grain as its agricultural classes declined and its manufacturing and commercial classes grew. Perhaps a few years later, but not many. (Stigler 1976: 352)

15. Unquestionably the key engineers of the postwar Chicago School (Stigler, Friedman and Director) broke with Frank Knight (their most influential teacher) on a variety of levels, especially in rejecting the methodology of Classical Liberalism. But what may be surprising is the implicit attempt, at times, to rescue Knight (fitting him, somewhat strenuously, back within the fold) by reinterpreting his views on crucial conceptions (knowledge and uncertainty) such that it aligned with their own perspectives.

> *The other thing is on uncertainty, the role of uncertainty. Knight of course writes that volume on Risk and Uncertainty. Yet if you read George Stigler, time and time again he wants to marginalize the effects of uncertainty, which is understandable, I think, given his work and what he is trying to do.*
>
> Milton Friedman: It's understandable from a different point of view. See I'm a great admirer of Knight, but I think his distinction between risk and uncertainty is untenable.
>
> *In what aspect?*

Milton Friedman: I believe that it uses a false theory of probability. I believe that the only theory of probability that can hold water is personal probability, the kind of thing that Jimmy Savage helped develop. If you take that approach, you can't distinguish uncertainty from risk. There's no break point. But also, you see, it means that Knight implicitly was working on a definition of probability as a relative frequency. And that misleads people into thinking that there are objective probabilities that you can know. Therefore it leads to a distinction between risk and uncertainty in terms of costs. Knight assumes you know some probabilities and that there's no way you can know others. In a personal probability sense, nobody really knows any probability. There are no objective probabilities.

But is there a continuum of, for instance, how much you'd be willing to bet on one.

Milton Friedman: Well, if I can experiment with your willingness to bet, I can determine your probabilities. There's going to be a war next year. Knight would say that's uncertainty. But in principle, if I can experiment with you, I can find out at what odds you are willing to take a bet that there will be a war next year. And thus I can extract your subjective probability of there being a war and in that sense there's no distinction between risk and uncertainty.

In the sense of subjective probability

Milton Friedman: At any moment of time, you will in principle have subjective probabilities of any strategic event.

Yes, potentially

Milton Friedman: And I think George was influenced by that approach to probability as well.

So, he basically saw that distinction as a dead end.

Milton Friedman: That's right.

Taking you nowhere.

Milton Friedman: I think it is a dead end. It's received a lot of attention and a lot of people talk about it. But I think it is very, very hard to make a logical distinction. Where does uncertainty begin and where does probability end, risk end? … How happy do you think Knight was about that distinction?

Aaron Director: I don't know. I thought he drew his distinction from the fact that you can insure one and you couldn't insure the other. Period. I'm only establishing his belief.

Milton Friedman: That was his belief. You can insure any of them in principle.

Aaron Director: Really?

Milton Friedman: And do.

Aaron Director: I understand that. But we are only talking about what Knight thought about it.

Milton Friedman: And Knight thought there were problems …

Aaron Director: You couldn't.

Milton Friedman: You couldn't?

That's right. That was his distinction. Uninsurable risk.

Aaron Director: Yep.

Milton Friedman: But if you say all risk is insurable, of course it means nothing.

Aaron Director: No distinction.

If you've got a zero set of uninsurable risks, then it's of no use.

> Milton Friedman: If you ask somebody 'I want to make a bet with you' you'll find somebody who'll take your bet, if you advertise widely enough.
> *If you give them the appropriate odds.*
> Milton Friedman: Right. And at that point, you've insured the risk. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

16. Following Frank Knight, Coase proved reluctant to accept a priori, professional truths deemed too obvious to be questioned. Theoretical conclusions needed perforce to be compared with observed economic outcomes. Theory itself, starting from different premises, was productive of alternative paths and solutions.
17. Such work predates Stigler's pioneering efforts examining whether regulation actually achieves its objectives (1962) and the reason for the existence of such regulations (1971). Notice however, the difference in approaches. Stigler abstracts and makes wide ranging, universal claims. Coase works in miniature, examining a specific case at hand. Significantly, Coase's articles tend to shy away from staking out any extravagant claims.
18. Here it might be useful to distinguish between structural economic models and the dangers of employing policies derived from such idealized constructions. Perfectly competitive markets devoid of any transaction costs eliminate the fundamental reason for the formation of such markets. An economy could, under such circumstances, operate effectively in the guise of one multi-divisional, integrated firm. In contrast, it is useful to remember that Coase took his inspiration partly from Knight who happily pointed out the absurdities of such a world where transactions or 'frictions' ceased to exist. "… if competition worked without let or hindrance, pure business profit would be annihilated as fast as it could be created" (Clark quoted by Knight 1971: 34). But Knight then drives home the difference between the realm of economic models and an operative economy by pushing this idea of an absence of 'frictions' (or for that matter transaction costs) to a logical extreme.

    > This is fallacious even under the assumptions, since the profits of change come largely in the form of readjustments of capital values. The difficulty is, of course, avoided if "friction" be so broadly defined that "perfect mobility" means the absence of all resistance to the human will. But in a world where a breath could transform a brick factory building into a railway yard or an ocean greyhound there would be no need for economic activity or economic science. (Knight 1971: 34 ftn. 4)

19. Clearly the same approach could be equally employed to limit the extent of unconstrained markets. The Classical Liberal methodology

that Coase advanced was, in its untainted version, not associated or defined by any specific ideology.

20. Coase never denigrated theory, nor did the economists he admired, such as Frank Knight. The problem however was to meticulously maintain the boundaries separating economic science, or theory, from the practical realm of policy. In the later kingdom, the necessity of working in practice is far more important than working in theory.

    Blackboard economics is undoubtedly an exercise requiring great intellectual ability and it may have a role in developing the skills of an economist, but it misdirects our attention when thinking about economic policy. (Coase 1988: 19)

21. The theorem might be better understood if it came supplied with a cinematic tagline: 'a theorem inspired by an article written by Ronald Coase.'
22. That Stigler chose to title that chapter of his autobiography 'Eureka!', easily reveals a not so well-hidden agenda. Samuelson also saw a bit of sleight of hand as being performed by the Chicago contingent.

    But Stigler and Friedman jumped on to Ronald Coase and felt that the Coase doctrines about transaction costs and property rights—just get the property rights right then laissez-faire could be relied upon—was the lifeline that they sought. Now, all that I know about this part of the story is what's called the Coase Theorem. And that's a coinage of Stigler's. I don't think Coase knew what his theorem was. There's great argumentation as to whether there is a theorem. (Conversation with Paul Samuelson, October 1997)

23. Stigler's long time researcher, Claire Friedland best sums up Stigler's views on externalities.

    George was focused on the way the market marches into *eliminate* the externalities, to work *around* them to make them a market problem instead of a non-market problem. I think I've quoted him in my memoir as saying something like, 'externalities are what the market has not *yet* eliminated.' (Conversation with Claire Friedland November 1997)

    He described externalities as that for which there are no transactions *at the present time*. (Friedland 1993: 781)

24. From Stigler's chosen perspective, perfect competition represented not a possible theoretical model, but 'the model' that accurately mirrored working, concrete markets.
25. It would be foolish to simply dismiss the rhetorical value of the 'Coase Theorem'. The formulation served as an effective way to dismiss the idea of market failure (and to focus by default on government failure), or at least the need to consider such a possibility seriously. Not to denigrate the scientific value of his work, but Stigler was able to simultaneously consider the value of his efforts in terms of their effectiveness as a

marketing platform forwarding his a priori vision of how the economy works. In a similar sense, although he studiously championed the value of quantitative methods, he often used his own empirical research more as a rhetorical device than an engine of discovery.

> The interesting thing is he was a great enthusiast for quantitative methods. So, it doesn't seem altogether consistent. But he certainly was. On the other hand, he knew what the answer was going to be. He just regarded it, as I say, as a way of persuading other people. (Conversation with Ronald Coase, October 1997)

26. The validity of the Coase Theorem as a scientific construct is inevitably debatable. In one sense it is no more than a tautology shedding only limited light. (In a seemingly unlimited wedge of literature on its scientific validity, a useful place to start is with Cooter [1982, 1987].) But the formulated theory has been challenged periodically. In discussing it, Coase's erstwhile colleague (though admittedly residing in different Chicago locales), Lester Telser, dismissed the scientific formulation by referencing the theory of the core (Conversation with Lester Telser, October 1997). However, while a debate over Stigler's theoretical conception is legitimate, becoming immersed in such a discussion distracts from Coase's original intention in the actual cited article.
27. The almost instinctive reaction was simply to attack its scientific validity, a strategy also readily employed by counterparts at Chicago when faced with policies objectionable from their own perspective. However, except by committing to a voluntary leap of faith, undercutting a scientific formulation provided no conclusive direction for pragmatic policy construction. What did, or did not, occur in an alternative universe of zero transaction costs need not necessarily constrain or even channel the interactions within actual market environments.
28. In a private conversation (October 1997), Coase suggested that at that famous dinner it was only Arnold Harberger who had seriously grasped both his intentions and the associated employment of marginal analysis. When asked about the 'Coase Theorem' he agreed that Stigler had transformed his argument into something that wasn't exactly there. Coase stated "He was always very nice and kind and helpful in many ways. Always. But I often wondered how far he agreed with what I was saying. I think he thought I was all right, but a little odd" (Conversation with Ronald Coase, October 1997).
29. If one is willing to assume a sufficiently cynical perspective, then a distinct temptation exists to contrast the difficulty, and associated labor, involved in adopting a more Coase-like approach to policy

construction. Compare this alternative to simply embracing theoretical conclusions as unequivocal guides for all aspects of economics. Given a need for career advancement, amid increased competition for even a base level of recognition, time constraints alone favor the theoretic (or scientific) path to policy formulation.

30. The reference is to Friedman's (1953) solitary attempt, though an influential one, to analyse economic methodology. Though even in these times, many economists would remain familiar, in some vague fashion, with this work, relatively few would be aware of Stigler's (1949) essential, if not equal, contribution in formulating this methodological roadmap.
31. Certainly he wasn't reluctant to use statistics when and if he deemed it to be appropriate to do so. At the very start of his career, he investigated the expectations-driven pricing model first formulated by Nicholas Kaldor (1934). Using actual pricing statistics, instead of assuming the way in which pig farmers (in this case) should operate, Coase and Fowler (1935a) were able to raise serious doubts about the efficacy of such a model and consequently any agricultural policy based on such claims.

    In those days, there was something called a cobweb theorem. The name was invented by Nicky Kaldor (1934). It is based on the idea that people assume that existing prices and costs are going to continue as is. So Fowler [a fellow student with Ronald Coase at the London School of Economics] and I took the pig industry and we showed that it wasn't true [1935a, 1935b, 1937a]. We showed that it wasn't true, because what you had was a market for two types of pigs. You had breeders and feeders. I think that in a lot of these sorts of industries, you still do. Anyway, there was a market for young pigs. Now, one knew in a general sort of way what the costs were of feeding. Well, the price that they paid for young pigs would reflect what they thought the price of the pigs, once fed, would fetch. So you could work out what people's expectations were. We showed that when prices were exceptionally high, they thought they would fall, and when they were low, they thought they would rise, and so on. (Conversation with Ronald Coase, October 1997)

32. The difficulty such luminaries as Stigler and Friedman seem to have had with Coase's methods might be a simple misunderstanding of intentions. They were apt to try to fit work into predetermined configurations. But Coase provided the proverbial square pegs for their round theoretical holes. This mismatch led to Coase's erstwhile colleagues and friends glossing over the actual contours of his efforts. Their imperative was to abstract from observed phenomena in order to generalize, an unalloyed obsession with the generation and application of universal

laws. In a throwback to the myth of Procrustes, and his formidable bed, in Chicago, one size was reflexively made to fit all. Coase, in contrast, insisted on the importance of specifics, of working in miniature while honoring the divergence of specific cases and observations. However, all of them, including Coase did hold similar political views and supported aligned policies. Perhaps this led these Chicago compatriots to implicitly transform Coase's work into something a bit more palatable to their cultivated taste. Consequently, they would naturally find it difficult to swallow some of his output or to give the work its complete due.

Ronald Coase: George didn't like my piece that I wrote on 'Economics and The Contiguous Disciplines'.
Yes, I've read it. It's a good work.
Ronald Coase: He didn't think so.
What was his problem?
Ronald Coase: Well, I didn't actually ask him. He indicated his displeasure by saying—you know, it had been written for originally, and then given at, a conference in Germany. He said it would have been better if it had been written in German.
I suppose he didn't care for it.
Ronald Coase: He didn't care for it. This must mean, I think, and this may be of interest to you, that he didn't understand it. (Conversation with Ronald Coase, October 1997)

33. As an extreme Chicago instance, George Stigler became more and more convinced that rational decision-making not only supplied a necessary, but also sufficient explanation for any observation.

It's getting more and more, more and more part of him as he got older actually, this whole view. He insists it's rational. He would tell you, "There is some rational explanation for it. It's just that you haven't looked completely into it and found it." (Conversation with Sam Peltzman October 1997)

34. The Virginia School of Economics, now not widely recognized as such within the profession, flourished at the University of Virginia from the mid-1950s through most of the 1960s. Pioneering in areas like Public Choice, Law and Economics, as well as Constitutional Economics, much of the work done would only flourish later at other institutions. The Department included such luminaries as James Buchanan, Gordon Tullock, Ronald Coase, G. Warren Nutter and Robert Tollison among others. These economists saw themselves, more or less, as operating within the spirit of Classical Liberal Economics, or at least that spirit as defined by Frank Knight. (Buchanan, of course, was one of Knight's few students.) The Department largely lost the support of the University Administration who viewed it as old fashioned and out of

sync with current economic thought. Coase left for Chicago in 1964, with Buchanan decamping for Virginia Tech by 1968. Boettke (1987) provides some interesting insights into this once and future school.

35. Perhaps, rather than forcing observations and events to be wedged tightly into preconceived categories, anything resembling a general or even universal statement should be teased out of many detailed examinations of specific cases and even then only employed as a tentative rule of thumb.

36. Though in some aspects an extreme example, it might prove useful to examine the contrasting understandings of Smith's view of humanity. George Stigler almost carelessly dismissed any psychological factors as extraneous to economic analysis and to its directly derived policies. Given his fixed, almost Kantian schemata (his corresponding vision of how markets operated and individuals chose), such considerations could only be ruled peremptorily out of court.

    > If one were to seek a major economic theory whose existence depended directly and essentially upon prior work in another field, he would find few likely candidates. Putting aside for a moment the methodological fields of statistics and mathematics, there is in fact no important candidate. A theory of behaviour, such as our profit maximizing assumption implies could have come from psychology, but of course it did not. In fact Smith's professional work on psychology (in the *Theory of Moral Sentiments*) bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted despite continued efforts from Jennings (1855) to Herbert Simon and George Katona to destroy it. (Stigler 1960: 44)

    Coase sharply parts company with his erstwhile colleague in regard to psychology and the role Smith's two great works play in formulating a workable conception of human behavior. "Well, you know my argument [regarding *The Wealth of* Nations and *The Moral Sentiments*] is the opposite" (Conversation with Ronald Coase, October 1997).

    > It is sometimes said that Smith assumes that human beings are motivated solely by self-interest. Self-interest is certainly, in Smith's view, a powerful motive in human behavior, but it is by no means the only motive. I think his analysis does not weaken but rather strengthens his argument for the use of the market and the limitation of governmental action in economic affairs. (Coase 1994c: 95)

37. Evidence that failed to assume required topological arrangements, and to fit Stigler's orthodox price models, could be summarily dismissed as unreliable. Such anomalies automatically commandeered further investigation with existing evidence subsequently interrogated, or more precisely water-tortured, until these errant observations surrendered to deduced logical outcomes.

> He was absolutely convinced that prices were flexible from Day 1. That was clearly his a priori intention. It was more than an intention. It was his belief. In the true believer sense. He really believed that. The very first interview we did was with a big firm. We have to keep this all anonymous, but it was a large firm in the Chicago area. That doesn't give very much away. Yes, perhaps there were more then than there are now. Anyway, the essence of it was that the people there absolutely denied anything about price flexibility, hidden discounts or things like that. They put out a price book and that was that. Everybody paid the same price. There were quantity discounts that were clearly stated in this price book. Anyone who bought large quantities got the large quantity prices, and small quantities got small quantities prices. George Stigler was clearly disappointed in that. He came back and he told me, 'Well, we'll get to the bottom of this.' (Conversation with James Kindahl, October 1997)

38. Any significant overlap in the methodologies employed by Coase and the Chicago School were not in evidence. Stigler, for instance, sought to isolate commonalities within markets by abstracting away from any specifics. But for Coase, crucial understanding could often lie hidden in such details and specifics.

> He was not really interested in what firms did or really what the political parties did, although he writes about it, what their effects were on registration and so on. He sort of studied them from the outside. But why political parties were organised the way they were, which is an interesting question in itself, he never investigated. (Conversation with Ronald Coase, October 1997)

39. A famous essay by Isaiah Berlin (1953), *The Hedgehog and the Fox* draws its title from a fragment attributed to the Ancient Greek poet Archilochus: πόλλ' οἶδ' ἀλώπηξ, ἀλλ' ἐχῖνος ἓν μέγα ("a fox knows many things, but a hedgehog one important thing"). In the case of George Stigler and Ronald Coase appearances mostly deceive. Stigler, who superficially seems to have jumped from one topic of research to the next, upon examination has obsessively reduced all observations to reflections of perfectly competitive markets. Coase, on the other hand, appears transfixed by the single idea of transaction costs, but in practice his work depends upon his knowing many specific things, rather than one all-encompassing concept. George Stigler in action becomes the dominating hedgehog pretending to be a fox while Coase remains the eternal vixen disguised as the indefatigable hedgehog.
40. That economists could talk so confidently about lighthouses, which they had never investigated, nor even perhaps viewed, is reminiscent of Frank Knight's favourite quote by Josh Billings, a nineteenth-century humourist, who during his time rivalled Mark Twain in popularity.

> The trouble with people is not that they don't know but that they know so much that ain't so.

41. Apropos of nothing in particular, the family of the author Robert Louis Stevenson (commencing with his grandfather) was famous as being the foremost lighthouse engineers in Scotland. They designed and constructed many of the more famous lighthouses in the nineteenth century and largely succeeded in lighting up the Scottish coast. An interesting bit of lighthouse liturgy, though not particularly economically decisive as regards either Coase or Stigler.
42. The problem, once again, lies in the confidence with which economists expound on subjects of which they actually know very little. "Most economists would say "How do you spell 'gold'? And then they'll tell you what we should be doing about gold or anything else that you can imagine" (Conversation with Paul Samuelson October 1997). In the same vein as Coase's 1974 paper is an article produced by Steven Cheung (1973). Again, a textbook example employed to demonstrate an unarguable positive externality involved the pollination performed by bees for at least some primary crops. Economists logically deduced that lacking enforceable property rights, the contribution provided by these ever buzzing bees was simply unpriced. However, Cheung found that upon examination, beekeepers and farmers had come to mutually beneficial arrangements effectively negating the assumed positive externality aspect of such pollination. Theory in such cases can underestimate the ability of actual individuals to resolve difficulties projected by assured abstract constructions.
43. The reference here is to Hirschman's (1970) book *Exit, Voice, and Loyalty*. The Chicago School of Friedman, Stigler and Director seemingly recognized only the bivariate market options of either accepting or rejecting an exchange, or for that matter remaining in a given organization (loyalty) or departing from it (exit). Opposing positions were to be decimated and overrun rather than analyzed and discussed. Hirschman in his work puts forward the often neglected option of trying to change the existing status quo by voicing one's dissent and arguing one's view. The idea is to persuade with reasoned argument and evidence rather than to bully opponents into submission.
44. The exact quote is "The presuppositions of objectivity are integrity, competence and humility" (Knight quoted in Coase 1994b: 15).

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George J. Stigler's Relationship to the Virginia School of Political Economy

Gordon L. Brady and Francesco Forte

Introduction

This paper contrasts George J. Stigler's credentials in the Chicago School of Economics to his relationship with the Virginia School of Political Economy. To be consistent, we use Stigler's Chicago School to refer to the Chicago School of Economics associated with the work of Stigler, Gary Becker, and Sam Peltzman and use Virginia School in reference to the Virginia School of Political Economy, associated with the work of James M. Buchanan, Gordon Tullock, and Ronald H. Coase. We draw out similarities and highlight differences between Stigler and the Virginia School, although in many ways they

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share a common philosophical core (methodological individualism and Smith's invisible hand). As a major contributor to the Chicago School of Political Economy, Stigler was a neoclassical economist. Buchanan, on the other hand, more of an Austrian style economist, founded Constitutional Political Economy and co-founded Public Choice, both sub-disciplines, as part of the broader enterprise known as the Virginia School of Political Economy. Buchanan's co-founder of Public Choice, Gordon Tullock (also a product of Chicago, though the Law School rather than economics) may be more aptly described as *homo economicus.* Ronald H. Coase, although at Chicago for much of his career, was a major contributor to the Charlottesville epoch of the Virginia School. Like Stigler and Buchanan, Coase was to win a Nobel Prize in Economic Science. Later he became a leading figure in the New Institutional School. Our paper is divided as follows:

The Chicago school of political economy, its scholars, and students.

1. Stigler as the focal point for discussion
   a. Biographical information,
   b. Stigler's background relevant to the Chicago school.
2. What the Chicago school of economics stands for, its principal scholars, and foundations (Frank H. Knight [1885–1972], Henry C. Simons [1899–1946], Jacob Viner [1892–1970], Aaron Director [1901–2004], Harold Demsetz [1930–2019], and others).
3. The foundations of the Virginia School of Political Economy, its seminal contributors, and the influence of Chicago "spores" on the Virginia School of Political Economy. This designation encompasses students of Knight (1885–1972), Viner, and Simons and others such as: Buchanan (1919–2013), G. Warren Nutter [1923–1979], Gordon Tullock [1922–2014], Ronald H. Coase [1910–2013], and D. Rutledge Vining [1908–2000].
4. Stigler's break with Knight and Buchanan.

Stigler and Virginia School: Theoretical overlap despite the fact that Chicago and Virginia largely ignored the other.

1. Theories of voting and redistribution, as well as of regulation and organized interest groups as proposed by Stigler and Virginia Public Choice School.
2. Stigler's Chicago School theory of regulatory capture by pressure groups, rent seeking, and the bureaucracy theory of Virginia Public Choice.
3. Stigler's "De gustibus non est disputandum" principle for dependence goods of choices, and economics of information, industrial economics versus Coasean transaction costs and the theory of firm.
4. Stigler on the economics of information, on industrial economics, and on Coasean transactions cost theorem.

Concluding comments and concluding thoughts.

The Chicago School(s), Its Scholars, and Spores

During the 1950s and early 1960s, the Chicago school of economics became widely known for the work of Milton Friedman[1] in monetary economics and Stigler in the microeconomics of individual choice, price theory, industrial organization, and government regulation. Both Friedman and Stigler were instrumental in establishing what came to be known as the Chicago School of Economics, but for much different reasons. Stigler's enrollment at Chicago in 1933 brought him into a select circle of future Nobel laureates including Friedman and Paul A. Samuelson [1915–2009] (later transferred to Harvard and ultimately to MIT). In addition to his lifelong friend W. Allen Wallace [1912–1998], Stigler, and Friedman remained close friends and colleagues throughout their careers.[2]

The main characteristics of the Chicago school are twofold:

1. The belief in the power of neoclassical price theory to explain observed economic behavior.
2. The belief in the efficacy of free markets to coordinate individual actions, and to allocate resources and distribute income.

The ideas associated with the Chicago School include Adam Smith's "invisible hand" postulate, opposition to government intervention (in general and to Keynesianism in particular), monetarism, and, as a later development, the economic analysis of the law. The "invisible hand" describes the phenomenon by which desirable outcomes are achieved without human design. It is used by both Chicago and Virginia to explain some of the consequences of individual behavior and the evolution of institutions. In essence there are several schools of economic thought which are loosely known as part of "the" Chicago school—such traditions include the work of Friedman, Coase, Arnold Harberger, and Robert E. Lucas, Jr. The Virginia School has a similar mix of traditions which emerged in the 1960s–1990s. These include work done at the Virginia Polytechnic Institute and the State University (now Virginia Tech). For simplicity, we shall refer to this period as the Blacksburg epoch (1969–83).[3] Like the Chicago School, the Virginia School has a number of components developed in different venues, but are dependent upon Chicago–Virginia roots, spores, and common ancestry. In addition to Buchanan and Tullock, the Blacksburg group includes Charles J. Goetz, Robert D. Tollison, Richard D. Wagner, Robert Mackay, Robert J. Staaf, Melvin J. Hinich, and other associates.

It is useful to begin with Stigler, his enterprise, and self-description.

Stigler's Career and Biographical Information

George Joseph Stigler (1911–1991) was born in Seattle, Washington, and educated at the University of Washington (B.B.A. 1931), Northwestern University (M.B.A. 1932), and University of Chicago (Ph.D. 1938). In 1933, he enrolled in the University of Chicago to study economics and began the associations that would have a long-lasting effect on the direction of his career. These would ultimately win him the Nobel Prize in Economic Science in 1982. In his acceptance speech for the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel in 1982, Stigler characterized his career as follows:

> Early in my professional life, I found that many areas of economics attracted me. I started working and publishing in price theory by 1938. In 1946, I published an early work on linear programming (*The Cost*

of Subsistence) which solved the problem only approximately; George Dantzig soon presented the exact solution. In the 1940s, I began empirical work on price theory, starting with a test of the kinked oligopoly demand curve theory of rigid prices. In the 1950s, I proposed the survivor method of determining the efficient sizes of enterprises, and worked on delivered price systems, vertical integration, and similar topics. It was in the 1960s that I began the detailed study of public regulation. My interests were aroused, and my faith in the clichés of the subject destroyed, as so often with other subjects, by the discussions with my friend, Aaron Director. This wonderful man is that rarest of scholars: a clear-headed, imaginative, erudite man who enjoys the task of constructing luminous and original theories but does not even write them down! (Stigler 1982: 56)

Stigler made seminal contributions in the economics of information, Stigler-Director's law of redistribution, on regulatory capture by the regulated, the innovative theory of dependence goods based on invariance of tastes, and industrial organization and structure in a competitive market setting. Stigler and several of his Chicago colleagues refer to their enterprise as "Chicago Political Economy." To better relate Stigler and his Virginia colleagues, it becomes useful to define the salient features of Chicago Political Economy (CPE). Most of the arguments discussed in this section are found in Stigler's (1971) article "The theory of economic regulation."

Stigler's background relevant to the Chicago School

The focus of Stigler's enterprise is on the analysis of government through the lens of price theory and positive economics. Robert D. Tollison (Buchanan's student in the late 1960s) referred to Stigler's enterprise as the "Chicago version of the modern development of public choice theory" (Tollison 1989: 293–297). In Stigler's model the state serves as a legitimated collective mechanism by which rational economic agents seek to redistribute income. Wealth transfers are the primary output of government regulation and servicing clienteles through tailored and often thinly disguised wealth enhancing programs and policies. Although government may produce some real goods and services, these are merely by-products of the system to facilitate wealth transfers. In this sense, Stigler's Chicago School may be viewed as an extension of Director's Law (1970) of wealth transfers.

Much of Stigler's basic Chicago application was introduced in his 1971 paper on economic regulation which was extended by his Chicago colleague Sam Peltzman in his 1976 paper. Peltzman viewed regulation as a set of off-budget government programs having objectives largely engineered by the regulated rather than the regulators. Fellow Chicagoan Gary S. Becker generalized this off-budget insight to government programs on the basis that they created massive wealth transfers, but not always to the intended recipients. William Landes and Richard Posner (1975) (later appointed as federal judge) further generalized the wealth transfer into a theory of government which became known as the interest group theory of government.

Stigler's Chicago School has an equilibrium assumption in that politically motivated transfers by politicians and government regulators (many end up in the private sector) are subject to discipline by voters, interest groups, and political managers. Unlike the Virginia School of Political Economy to be discussed next, Stigler's CPE paid less attention to specific political institutions. Becker modeled lobbying as a mechanism to minimize the deadweight loss to the economy of government intervention. There are no voting rules, legislative constraints, or purposefully behaving bureaucratic agents in carrying out programs to their benefit as is characterized in the Virginia School. Tullock modeled lobbying and logrolling as efficiency-enhancing features to promote preference revelation. Stigler's Chicago school had few competitors when his 1971 paper was published with the prominent exception of what was underway in Charlottesville and later Blacksburg. By the early 1960s, the Pigovian public interest theory of government was already under assault by Buchanan, Tullock, and other public choice practitioners. Not all were Chicago trained, but all held similar core values to those then prominent in Chicago. Consequently, it is useful to discuss the foundations of the Virginia School at this point in our paper.

In his discussion of the ways in which the Virginia School differs from mainstream economics, Congleton (1999) points out that there is no textbook treatment of the Virginia School. Underlying the Virginia approach is the idea that self-regarding individuals learn to use the rules of a political system in order to achieve their personal or ideological objectives. Virginia economists recognize that both good and bad economic policies are the outcomes of rational individuals making choices within specific institutional arrangements. Undesirable public policies such as protective tariffs, costly and inefficient regulations that may cartelize industries, distortionary taxes, wasteful expenditures, and ill-conceived transfer programs may result. However, they note that such apparent adverse outcomes are not accidents or mistakes waiting to be corrected, but the consequences of self-interested individuals choosing within particular institutional settings. Improving the collective outcomes requires that rules be changed. Major revisions in rules (constitutions) are required—not simply to change the players or to give them "better" morals or policy advice, etc. Further to this point, institutional arrangements should be compared to each other and relative to what is feasible. Finally, in so far as people's goals are considered as "givens," the role of the Virginia School economist is to find the most efficacious means to achieve these goals.

The essentials of the Virginia School may be summarized by four key points. First, the focus is on institutional arrangements as the reason for either economic success or failure. Second, it recognizes that the only way to turn economic failure into success is by changing the institutional arrangements. Third, institutional arrangements are compared by reference to the outcomes they yield—and not in comparison to an unfeasible ideal. Finally, the Virginia School analysis is constrained to provide positive rather than normative recommendations.

We now take a close look at Stigler and the Chicago School, which characterizes Stigler's work. Stigler wrote his dissertation under Frank Knight and thereby shared an intellectual lineage with Buchanan who founded the Virginia School of Political Economy. Knight's underlying philosophy of examination was crucial in the shaping of Public Choice to utilize a more interdisciplinary approach. In contrast, to the broader Virginia School of Political Economy, the practitioners of Public Choice trace its core to two elements of the organization of the state. In *Social choice and individual values* (1951), Arrow showed mathematically that the state cannot be explained as an aggregation of individual preferences into a collective ordering. Arrow's work was heralded as the justification for a social planning approach—an anathema to both Virginia and Chicago. However, it was Virginia scholars who spearheaded this challenge. Nutter's work in the 1960s was largely accomplished at the University of Virginia's Thomas Jefferson Center for Political Economy (founded by Nutter and Buchanan). Nutter mounted a direct attack on the assumptions of central planning. However, the theoretical attack on central planning came much earlier than Nutter's applied work. The Virginia School's challenge to central planning came via Buchanan's 1949 and 1954 papers which emphasized the importance of individual exchange in nonmarket decision-making.

The Virginia School, Public Choice, and Stigler's Chicago School (CPE) share a number of foundational features. All three are grounded in methodological individualism and an anthropological view of skepticism about the ability of nonmarket entities to produce efficient results. Methodological individualism lies as a core attribute of the economic way of thinking. The Virginia School and Stigler's Chicago School view people as self-regarding, behaving rationally (rational actors and rational agents), and are inclined to behaving in the same way in private choice as they do in the broader context of collective choice through voting and participation in political markets.

A major tenet of the Virginia School is Knight's principle that institutions matter and different institutions imply a different behavior among individuals.[4] On the other hand, Stigler's focus is on the preferences of the individual, the firm, and the organization. While the primary focus of the Virginia School is on the public sector and institutions affecting participants in nonmarket decisions as public sector choices, the focus of Stigler's Chicago School is on the market and its organization. It is only peripherally focused on the individuals involved.

Stigler's methodology rests upon two pillars: empirical research and abstract theorems with a strong tendency to what might be called "economic imperialism." Virginia Public Choice had a much different orientation as its foundation. Unlike Stigler's "economic imperialism," the Virginia School is characterized by an interdisciplinary approach which melds law, politics, and sociology (Tullock's work was inclusive of sociobiology). Those associated with the Virginia School have been critical of abstract mathematical modeling and high-powered econometrics, which does not take into account the complex institutional arrangements of advanced economies.

The leaders of the Virginia School were pioneers in the identification of an institutional research agenda which applied economic analysis to new topics like bureaucracy, voting, constitutions, central planning, and law and economics. The scattered shots of Tullock and his editorship of *Public Choice* provide insight into the breadth of the Virginia School and its practitioners. Our focus in this paper is restricted to differences in Stigler's Chicago School and Virginia's School of Political Economy.

It is undeniable that Stigler's Chicago School advanced the ball in many areas but tended to focus narrowly on regulatory capture as a key component of the government control equation. Buchanan's early work guided by methodological individualism was important in challenging the equilibrium focus of mainstream economics. Imbued with subjectivist insights, Buchanan's Virginia School recognized that individual behavioral responses are guided by individual perceptions of cost and benefits. It is from this individualist subjectivist foundation that meaningful Virginia School analysis flows. Its basis derives from self-regarding politicians who are not necessarily promoters of the public interest broadly conceived, as both Buchanan and Stigler certainly recognized.

It is not surprising that the non-benevolent view of politicians (politics devoid of romance), which became associated with the Virginia school, gave rise to charges that the school embraced an ideological point of view. This perception has served as the basis for criticism of the Virginia School then and now.[5] While the founders of the Virginia School are recognized as major innovators in understanding the economic underpinnings of democratic institutions and political economy, the acceptance of their work has come slowly. Stigler tended to escape charges that his Chicago School was biased and outside mainstream economics. The Virginia spores of the broader Chicago School were not so lucky, although both Buchanan and Coase were to win Nobel recognition. It is now useful (although perhaps too broad a diversion) to provide an overview of the intellectual influences at Chicago which shaped Stigler, Chicago economics, Buchanan, and others among the Chicago spores of the Virginia school.

What the Chicago Schools Stands for, Its Principal Scholars, and Foundations: Crusaders, Puzzlers, and Rigorous Systematizers

The Chicago tradition encompassed by Viner, Knight, Simons, and Director was a crucial element in the various strains of the Virginia School, as constructed by Buchanan, Nutter, Tullock, Vining, and Coase. Among the founders of the Chicago School we may wish to distinguish those who sought to develop a system (Viner's rigorous analytical framework).with those intent on sustaining a crusade or mission (Henry Simons' crusade for what he believed was freedom and equality), or those with a passion and a problematic vision (Knight and Director). The approach and contributions of some of the founders and their successors fit more than one category.[6] Simons' mission was to advance political freedom and some measure of income equalization, which in his view, could be obtained by a progressive tax structure and restraints on monopolies.[7] He feared that government would expand dramatically during the 1930s and that this development would be difficult, if not impossible, to reverse once set in motion.

Chicago and Virginia economists have a narrow focus. Chicago economists in general have been problem-oriented and advocates of the use of simplified theoretical tools to accomplish the task. The causes determining a particular economic event are numerous and complex, but exponents of both the Chicago School and their Virginia colleagues believe that economists should focus on the role played by utilitarian incentives (guided by price theory) in explaining human behavior.

General equilibrium theorists, on the other hand, seek comprehensive and rigorous mathematical theories to show how the economic system can attain equilibrium under a set of rarefied assumptions. Although the principal members of the Chicago School had similar leanings toward key elements of economic theory and policy, their personal approaches differed greatly and these differences affected the development and dissemination of their ideas, and hence the development of the Chicago School.

Chicago and Virginia as Social Interrogators

Both Knight and Simons emphasized the need to be critical in evaluating scientific work. They also insisted that the age or high office of the researcher was of no consequence when evaluating that work. Along with Director, their inquiry left no stone unturned and considered no doctrine sacrosanct no matter the identity of its originator. This approach was a criterion of the Chicago approach and its spores. The framework for questioning consistency differed among practitioners by can be categorized

Jacob Viner (1892–1970)

Viner was a "systematizer" who held fewer of the ideas associated with the Chicago school than did Knight. However, he may be credited with the emphasis of the Chicago school on microeconomics and his view of the economy as a complex system which could be modeled. He was both economic theorist and historian of economic thought, and he possessed a strong empirical orientation. Viner worked primarily on problems in international trade and related issues in monetary theory. Viner not only had an analytical mind that held many original ideas but he combined this with an expansive understanding of the humanities and social sciences (Spiegel 1997: 813).

Although Viner did consulting and other work for the federal government, he was foremost an academician. In 1950 Viner published *The Customs Union Issue*, which, by carefully distinguishing between trade creation and trade diversion, had a lasting influence on the policy debate in international trade and opened new insights to the Chicago scholars. Viner jointly edited the *Journal of Political Economy* from 1929 to 1946. Much of Viner's editorship was with Knight, although they were known to differ on many issues.

Viner and Knight shared an interest in the development of economic thought. They were both devotees of neoclassical price theory, and resistant to the theoretical innovations of the 1930s, including the theories of E. H. Chamberlin and Joan Robinson on imperfect competition and J. M. Keynes' *General Theory*. Further, they opposed the interventionist aspects of the New Deal and the full employment policies of the latter part of the New Deal. Like Knight later in his career, Viner sought to explore the relationship between religious and economic thought. Viner was a major influence on Chicago, its faculty, students, and later the Chicago "spores" who were to establish new outposts in which to practice Chicago economics.

Viner's emphasis on rigorous analysis influenced Buchanan and other students in both positive and negative ways. According to Buchanan, Viner was "the classically erudite scholar whose self-appointed task in life seemed to be that of destroying confidence in students," and "he along with others were not the persons who encouraged students to believe that they too might eventually have ideas worthy of merit" (Breit and Spencer 1995). No doubt, Viner's overbearing personal style explained his lack of acolytes (Brady 1994; Breit and Spencer 1995: 75). Nor did his students constitute a club but were dispersed in time and intellectual interest with little in common except their early contact with Viner (Reder 1982). However, despite his teaching style, Viner established successful research programs, enlisted graduate students as participants in his work, and supervised far more doctoral dissertations than Knight.

Having said that, Viner's influence on the broader Chicago school was not as great as that of Knight or Simons (or later Stigler). This is perhaps owing to the personal charisma of Knight and Simons and their unique intellectual ability to convert students to their way of thinking. According to Coase, although Director took courses from Paul H. Douglas, Knight, Theodore Yntema and Henry Schultz, the course which changed his way of looking at the world was one from Viner on Marshallian economics (Coase 1998: 603). Buchanan and Knight's students wrote well, networked widely, and became effective advocates of the ideas they held. But in that they displayed rigorous analysis, Buchanan's methodological views were, ironically, more similar to Viner's approach than to Knight's.

Like Knight, Viner urged deficit spending during the Great Depression, and he went so far as to call the plea for an annual balanced budget a "moldy fallacy" He was critical of Hayek's libertarianism (Viner 1961: 232). Moreover, in common with Knight, Viner denied that perfect competition was both a norm and normal. He further argued that monopoly was so prevalent in modern western economies that competition seemed to him "academic in the only pejorative sense of that adjective" (Viner 1960: 66). Like Knight and Simons, Viner was skeptical about received doctrine. His skeptical orientation was certainly consistent with Buchanan, Stigler, and Chicago in general.

Stigler's Break with Knight and Buchanan

Frank H. Knight (1885–1972): The debunking of myths is a characteristic shared by Stigler and Buchanan. Knight's work focused on the conceptual underpinnings of neoclassical price theory, and his main concerns were to clarify and improve its logical structure. Buchanan described Knight's qualities of mind as his willingness to question anything and anybody on any subject at any time. He categorically refused to accept anything as sacred and had a genuine openness to all ideas. Although Knight was sympathetic to the aspirations of those seeking to quantify economics, he was outspoken about his skepticism of their prospects for success. Buchanan describes Knight as having a basic conviction that most ideas peddled are nonsense or worse when examined critically (Buchanan 1995: 95). According to Buchanan, Knight (like Viner noted above) recognized that the model of perfect competition is an idealization of reality rather than a description (Buchanan 2008).

Buchanan argued that "lesser theorists who followed Knight overlooked this essential point and erroneously expected real-world institutions to match up descriptively with the idealized model" (Buchanan 2008). It was their overly simplistic comparisons of theory and observed reality that are responsible for allowing the critics of a competitive economic order to undermine effectively much of its general social support, especially when comparisons failed to consider the flaws of alternative arrangements (Buchanan 2008). Buchanan acknowledges Vining's emphasis on the importance of rules in economic analysis.

Knight, Stigler, and Buchanan

According to Stigler (Breit and Spencer 1995), and Buchanan (Breit and Spencer 1995), Knight's teaching style made him difficult to follow and his refusal to accept anything uncritically made him the source of endless ideas for student discussion and research. Stigler described Knight as alternating between a great teacher and an absurd teacher, but communicating beyond any possible confusion the message that intellectual inquiry was a sacred calling. This posed a mission excruciatingly difficult for even the best of scholars to pursue with complete fidelity to truth and evidence (Breit and Spencer 1995: 96).

Stigler had personal contact with Knight early on as evidenced by his involvement with later Chicago students Milton Friedman, Homer Jones, and W. Allen Wallis to publish a selection of these papers as *The Ethics of Competition* (1935). Their objective was to make available to students of the social sciences some of Knight's essays (primarily on social control and its implications) which they believed were particularly relevant to the social problems of the day. They noted that "had the selection been made by the author, not only might the contents have been different, but some revisions might have been made" (Knight 1935). Our point is that the influences on Stigler go deep to the heart of the Chicago school.

Buchanan further described Knight as a teacher "who gave us, who bothered to listen, the abiding notion that all is up for intellectual grabs, that much of what paraded as truth was highly questionable, and that the hallmark of a scholar was his or her courage in cutting through the intellectual haze. The willingness to deny all gods, the courage to hold nothing as sacrosanct—these were the qualities of mind and character that best describe Frank Knight."

> But Knight's thought process probed depths that the scholars about him could not realize even existed. To Knight, things were never as simple as they seemed, and he remained, at base, tolerant in the extreme because he sensed the elements of truth in all principles. Knight left us with the awful realization that if we did not have the simple courage to work out our own answers, we were vulnerable to victimization by false gods. (Buchanan 1992: 5)

Nutter, on the other hand, was said to have taken immediately to Knight's teaching style and found his ideas easy to absorb (Tullock 2001). The debates among students spurred by Knight's lectures became a very important vehicle in the development and transfer of the Chicago tradition.

Stigler and Virginia Public Choice: Theoretical Overlap Despite the Fact that Chicago and Virginia Largely Ignored the Other

Stigler-Director's Law of Public Sector Income Redistribution and the Virginia School on Redistribution as Exploitation

At first reading, Stigler's (1970) paper might appear as containing only a superficial insight and not one that is particularly unique. But this is not the case. In "Director's Law of public income redistribution," Stigler argued that redistribution of some government programs was from the rich and the poor to the middle class. While this finding might appear to be little more than a corollary of the median voter theorem, it is not true. Stigler's explanation of the law implies that even if the middle class did not constitute the majority, the redistribution policies would work in its favor and to the detriment of the poor. This outcome occurs due to the greater political influence of the middle-income voters in comparison with the lower income classes. This occurs despite the greater number of low-income voters.

Stigler presents Director's Law of redistribution as follows: "Many have long believed that big government helps the poor at the expense of the wealthy. But, back in the glory days of economics at the University of Chicago, Aaron Director noticed a recurring theme in the way democracies cut the pie. Given the power to tax and redistribute, you might expect those on the bottom (plus one to gain a majority) would form a coalition to snatch away the wealth of the top 49 percent. But this doesn't happen. Director teased out why: forming a winning coalition requires intelligence, effort, organization, endurance, resources, indignation as capabilities the poor simply do not have. This is part of the reason why they're poor, after all. For the very same reasons they struggle in a free market, they fail in the political market too. By Director's logic and contrary to popular belief, the big jackpot winners in democracy are the members of the motivated middle class. They take from the rich and the poor" (Stigler 1970).

Stigler supports his redistributional insights and the validity of Director's law by showing the state of California's expenditure on higher education. The principle benefits of these statewide programs were found skewed in favor of the middle class and the upper class. On the other hand, the data on distribution of state and local government taxes, which funds the public expenditure on the state universities shows that the largest share falls on the lower class. Stigler infers that the children of middle income and high-income earners which comprise the vast majority of the students of the state colleges receive an income redistribution at the expense of the poor. Stigler's paper stimulated much empirical research that evidenced the truth of the Director-Stigler law of income redistribution in the United States.

More recent research which focuses on the US challenges this view. It shows that the redistribution takes place from the middle class to the lower class and to the rich, even if the middle-class voters constitute about 60% of the total. Still the Director-Stigler law remains relevant for the section of the poor upon which it focuses. Stigler's explanation for the redistribution is the difficulty of organizing political coalitions which results in a bias against low-income recipients. Stigler argued their lack of resources also explains their lack of success in the market economy.

The Virginia public choice approach to the distributive issue is more generalized. It is based on Wicksell's theorem of nonmarket decision-making, in which only the unanimity rule prevents exploitation of some politics of voters by some other groups of voters. In the Wicksellian Public Choice model, the main issue is the exploitation exercised by organized groups of voters over less organized voters both in the elections and in the assemblies of elected politicians. According to the Virginia public choice approach, a fiscal constitution agreed by quasi-unanimity rules may minimize the risk of exploitation of the minority by the majority.

Much of Buchanan's work focuses on the institutional structure of governments through such mechanisms as revenue which is earmarked for some specific function based on the benefit principle. Buchanan employs his model of club governments as a means of analysis. In Buchanan's club model, governments supply specific public goods without any redistributive function in a competitive setting in which there is always the possibility to opt out. If a business entity or a person feels exploited by the club, some or all of them may move to another club. Similarly, at the economic unions level, governments of countries which feel exploited by other members of the club may opt out of any such association. They may do so either by moving to another economic union, by remaining independent, or by remaining associated to the original club without still being member of it.

Tullock's interpretation of Director's Law focused on the expenditure side of nonmarket decision-making. He provides extensive research on the various federal, state, and local programs targeting the low-income voters by means of charity, old age pensions, medicine, or education. Unlike Stigler in his seminal paper (though consistent with Buchanan's methodology of examining in kind aid versus transfers), Tullock's public choice analysis examines programs with specific beneficiaries and universal coverage. Stigler's focus was on the end state rather than the process by which the redistribution takes place (Tullock 1983). In addition to frequent references to Buchanan's fiscal illusion, Tullock added cognitive dissonance, in a paper which he published in 1970. It is interesting to note that neither Buchanan nor Tullock mentions Stigler in the works cited above. Nor does Stigler cite or discuss previous public choice contributions in his 1970 paper. Stigler's (1972) paper entitled "Economic competition and political competition" appeared in *Public Choice* (Stigler 1972). While it cited the work of Arrow's Harvard Ph.D. student Anthony Downs and Carnegie Mellon graduate students Melvin J. Hinich and Peter Ordeshook, it failed to cite the relevant work of Buchanan and Tullock.

Stigler's Chicago School Theory of Regulatory Capture by Pressure Groups, Rent Seeking, and the Bureaucracy Theory of Virginia's Public Choice

Stigler's main contribution which is consistent with Virginia Public Choice is his 1971 paper the theory of economic regulation in which he introduces a capture theory of regulators by the regulated (Stigler 1971). Stigler (1975) complemented his seminal paper by an Appendix on the Posner contribution to that theme, when republished in a book of essays on regulation, entitled *The Citizen and the State (1975)* however, no mention is made about the parallel work by Buchanan and Tullock on the rent seeking society or on Tullock's then recent insights detailing inefficient versus efficient rent seeking.

The central thesis of Stigler's (1971) paper is that "as a rule, regulation is acquired by the industry and designed and operated primarily for its benefit … we may call it 'acquired' regulation." According to Stigler's 1971 paper, "political process defies rational explanation: 'politics' is an imponderable, a constantly and unpredictably shifting mixture of forces of the most diverse nature, comprehending acts of great moral virtue (the emancipation of slaves) and of the most vulgar venality (the congressman feathering his own nest)."

Despite the negative tone of Stigler's description of politics, the author appears content to build his analysis on the presumption "that political systems are rationally devised and rationally employed, which is to say that they are appropriate instruments far the fulfillment of desires of members of the society. We do not want to argue that at the state will serve any person's concept of the public interest: indeed, the problem of regulation is the problem of discovering when and why an industry (or other group of like-minded people) is able to use the state for its purposes or is singled out by the state to be used for alien purposes" (Stigler 1970).

Firms stand to gain from government regulation of their industrial sector if it is tailored or structured to their characteristics. Firms profit differentially but they stand to gain from direct regulation by government bodies through coercion and the power to tax. Examples include government intervention over prices and quality of their products, monetary subsidies from government coffers, and the development of policies to control new entrants and rivals. Finally, state regulatory powers may be used to discourage or increase the price of substitutes or more positively by subsidizing complements. This is not to mention the use of price controls administered by a body with coercive powers. According to Stigler, businesses with power to obtain public sector favors do not ask for certain types of interventions because they must be coupled with limitations on the list of beneficiaries in order to prevent dissipation of rents among a growing number of rivals.

Stigler further argues that rational behavior implies that every industry or occupation that has enough political influence to utilize the power of the state will seek to control entry, often by regulatory barriers and/or by protective tariffs. A third set of regulatory measures to benefit existing firms involves state regulations to limit competition. A fourth category of public sector regulation sought by an industry is price-fixing. If the number of firms in the regulated industry is even moderately large, price discrimination will be difficult to maintain in the absence of government support. Where there are no diseconomies of large scale for individual firms (such as in the motor trucking industry) price controls become essential to achieve rates of return higher than those gained in more competitive markets.

Stigler describes the process of acquiring favorable regulations as also involving the ability to effectively transmit policy needs. If everyone has a negligible preference for policy A over B, the preference reflecting this interest is not transmitted. The system will implement all strongly felt preferences of majorities and many of the strong preferences evidenced by minorities while disregarding the less strongly held preferences by majorities and minorities. The filtering will fade given any discernable reduction in the cost of acquiring information and transmitting desires. Finally, such incentives will alter given any changes which increase the perception that his/her vote affects the probability that his vote will influence policy. This of course is very similar to Tullock's views on the rationality of individual voting choices.

The final political equilibrium shall not be such that one group wins because it is more powerful, but a compromise solution in which marginal opposing forces are equal. Notice that Stigler does not introduce a Nash equilibrium nor any other game theoretic equilibrium model in describing the interactive relationship among pressure groups. The result is a monopoly model in which the regulated industries maximize their profits subject to the constraint of the political and resource costs attached to obtaining favorable regulations tailored to their industry characteristics (as described above). The other side of the regulatory equation is that the purchasers behave to maximize their utility subject to the constraint imposed by the regulated supply side. However, Stigler does not formalize his model, choosing instead to provide an impressive set of empirical evidence.

However, as noted above, Stigler does not refer to the relevant Virginia School research on rent seeking, including Tullock's 1967 paper on the "Welfare costs of tariff, monopoly and theft." Tullock's pathbreaking insight specifically deals with Stigler's regulatory world in which the effects of regulations are tailored to protect domestic firms from foreign competition and from monopoly control (Tullock 1967). Tullock's diagram describing the cost and benefits of rent seeking would have been helpful in integrating Stigler analysis of the topic. No mention of Tullock, nor does this appear to be an oversight?

A formalization of regulatory capture is provided in two papers that Stigler devotes to the theory of the agents in charge of enforcing regulations. Stigler and Becker analyze their compensation and the disincentive for misbehavior (see Becker and Stigler 1974). This paper is preceded by Stigler's 1973 paper on regulatory agencies which examines qualifications for agents, their incentives to be efficient and various controls by management to reduce shirking and misbehavior. He also offers proposals to improve the prevailing incentive structure (Stigler 1973) as well as discussing the optimal enforcement of law (Stigler 1970).

Given the limits of our paper, we cannot provide Stigler's full analysis of the incentives and disincentives provided to the agents enforcing a given set of regulations. Nor can we fully discuss the theory detailing the constructed deterrence that prevents the violation of these regulations by a given business sector. This is described in Stigler's seminal three papers. Stigler's thesis is that the agents should not be praised for the maximization of their activity as measured by the number of cases examined and decided, but evaluation should be based on the relevance of their individual cases. Evaluation of the relevance and results was not the accepted wisdom at that time.

However, one could complain that Stigler did not consider what attributes/emoluments the regulatory bureaucrats may seek to maximize. During this same period, other relevant work was published, such as the seminal work of fellow Chicagoan William Niskanen (1933–2011), whose work had been encouraged by his colleague Tullock. Niskanen's book was published in 1968, followed by additional later work on bureaucracy in representative governments (1971). Buchanan and Tullock reviewed the book positively in *Public Choice* in 1971. Despite similarities between Tullock's work on bureaucracy and that of Niskanen, plus the widespread, favorable reviews it received, (not to mention the well-established Chicago connection) Stigler apparently chose to ignore it. Stigler's oversight in this case is difficult to explain unless one chooses to argue, somewhat perversely, that Stigler and his colleagues chose to provide status to such work by granting them neither approval nor acknowledgment.

Stigler's "De gustibus non est disputandum"—Algorithm for Dependence Goods and the "Opportunity Cost of Choices" Perspective of Buchanan and Coase

Stigler's seminal contributions to the microeconomic approach to public policy is a key component in his joint 1977 paper with Becker ("De gustibus non est disputandum"). This paper examines the consumption choices surrounding dependent goods displaying either positive or negative effects on an individual's subjective utility. More precisely the paper focuses on the effects caused by marginal exposures to such goods (Becker and Stigler 1977). This joint paper with Becker, unlike most of Stigler's other work, is limited to a theoretical construction lacking any supporting empirical evidence. It is based on a hypothesis on the state of the world relating to the subjective structure of the preferences of the consumers, namely that they do not need to change through time, to generate a dependence on these "dependence goods." Since the mental phenomenon is unobservable by the economist, it is hard to test the validity of the hypothesis, except through the employment of experimental economics, an approach to economics not yet developed in the late 1970s when the paper appeared.

The core of Stigler's model consists of its application to positive and negative dependence goods. Becker presents a consumer choice paradigm from one of his earlier papers in which to the price of the good purchased, the purchaser adds the internal costs of the consumption choices. They consist of non-priced inputs, such as the time spent in its consumption, as well as in the value of human capital that allows for an appreciation of that good, plus there are other inputs not considered in the basic choice algorithm.

Consequently, the utility (*U*) of music (a good with a positive dependence) in the Becker/Stigler paper depends on the utility of (*M*) and (*Z*) the utility of alternative goods that one can buy with the same amount of purchasing power. In turn, (*M*) is measured by the amount of appreciation of the Music produced by internal inputs t_m (time spent in its consumption) and S_m (the human capital that allows an appreciation of (*M*)).

As the human capital S_m increases in proportion to the amount of (*M*) obtained in the previous period, the relation between the successive units of S_m and their value is positive. The level of human capital increases as the utility gained from music rises, $\alpha S_m/\alpha M > 0$ by a sort of learning by doing. Specifically, as the utility of the units of time dedicated to music increases over time, a positive dependence develops without the need to assume that tastes have changed. We view this as being analogous to the Socrates theorem which assumes that any addition to Socrates' knowledge, due to his previous philosophical speculation, serves to broaden his knowledge horizon. The greater the elasticity of the price of music to the quantity consumed, the more its consumption in terms of quantity of time or quality of units of music will be expected to increase through time. However, in old age, people may be less interested in increasing or maintaining their stock of S_m. Further to this point, the present authors of this paper assume it is likely that the consumption of music will decrease in old age ceteris paribus due to the income effects on the elasticity of consumption of music, which Stigler's simplified paradigm overlooks. This small point is not unlike Stigler's precision in criticizing Coase's theorem on social cost (later discussed in this paper).

An opposite effect occurs when the dependence good has primarily negative effects, as in the case of the consumption of alcohol, tobacco, narcotics, or other deleterious goods. In this case, the human capital Sm decreases with the amount of (*M*) obtained in the previous period and, consequently, the relation between the successive units of S_m and their value is negative $\alpha S_m/\alpha M < 0$, reflecting a sort of "unlearning by doing."

Tastes remain constant (according to the Stigler Becket assumption) but the amount of additional pleasure gained per additional unit of consumption (of a given quantity and quality) diminishes.

Becker and Stigler (1977) argue that one cannot expect a decrease in total consumption because demand is inelastic. For a perfectly inelastic demand curve (one that is parallel to the vertical axis) the demand will increase. To obtain the same quantity (S_m) of a given quality, one must increase the quantity of consumption. This implies that while a tax on alcoholic drinks and tobacco may not decrease the quantity consumed. Moreover, the prohibition of drugs backed by severe punishment may not change their consumption. It is however undeniable that penalties will reduce the consumption of goods with an income elasticities. Several unanswered questions remain concerning Becker and Stigler's algorithm for dependence goods based on the postulate stable consumer preferences, as expressed with the *De gustibus non est disputandum* dictum. For example, why are cultural goods likely to have a positive income and price elasticity while alcoholic drinks, tobacco, and drugs (one may add gambling) have an inelastic demand?

Further insights on dependence goods are provided by Buchanan's concept "opportunity cost." This entails a subjective approach to individual choice analogous to Coase's approach to a firm's input decisions. Since this involves opportunity cost, one must consider the human capital S_M relating to Music (M) as well as the Human (H) capital S_h relating to the consumption of those goods having a negative dependence. Both, however, exist as components of the human capital S_n relating to all choices.

The choice of Dependence Good *M* or *H* and their associated foregone choices have an opportunity cost which objectively affects S_n. The consumption of a cultural good such as M does not generally negatively affect the total human capital (S_n). There is no reason to believe that the subjective consideration of such opportunity costs differ greatly from those that are more objective. Thus the consumption of these goods has a normal positive income elasticity and negative price elasticity.

On the contrary, the consumption of goods having negative dependence characteristics may negatively affect, to varying degrees S_n. Consumption of such goods is consistent with both physiological and intellectual deterioration. Hence, an eventual reduction of income available for non-*H* goods goes together with a reduction in the propensity for their consumption. However, the inelastic nature of the demand curve for *H* increases, the more human capital (S_n) is linked to S_h.

Stigler on the Economics of Information, on Industrial Economics, and on Coasean Transaction Costs

Here again we find an example of a mutual failure to recognize the parallel work accomplished by Chicago and Virginia. Both the seminal paper by Stigler on the economics of information and Coase's work on the problem of social costs (in which transaction costs play a central role) were published in 1961. Yet that neither cites the other is difficult to explain (Stigler 1961; Coase 1960). Particularly since Coase's views on transaction costs extend back to his much earlier 1937 paper investigating the theory of the firm.

Stigler did not relate information costs to the category of transaction costs, in which they may rightly belong. These can be viewed, within the preliminary stage of contracting, as a search cost to discover the relevant, appropriate prices. Information costs are also involved in the execution of the contract, namely to assess the contractual compliance. Alternatively, transaction costs may be rival of the information costs, when firms are vertically integrated.

It is likely that Stigler did not view information costs in this way, because he was considering transaction costs from the perspective of the contract as it emerges from a process of bilateral negotiations and not from the Coasean contract seen as a bargaining process (Stigler 1961). The objective of Stigler's seminal paper is to analyze the quantitative information variables interacting on prices for a given sets of goods. These quantitative variables explain the supply of information by the relevant suppliers as they search for purchasers.

Empirical research found a large dispersion in prices, even for consumption goods offered by a large number of firms. Stigler argues that it is worthwhile to determine the reasons for the dispersion, particularly what means might be used to reduce the price dispersion in order to increase the efficiency of the competitive enterprise system.

Consumers search for the lowest prices of a given good of constant quality. Their search costs are primarily the non-pecuniary costs consisting of time and effort devoted to search. On the other hand, the seller searches for buyers that react to the price reductions by increasing purchases that result in additional revenue net of costs.

Search costs must include the information cost in addition to the cost of bargaining. Increased search by the potential consumers will lead to increased advertising by low price sellers and reduced advertising by high-price sellers. It is important to understand the conditions in which their interaction may take place and the objectives of consumers at this stage of contracting. Their objective is to get low prices while the incentive of suppliers is to maximize profits by setting prices sufficiently low to increase sales volume.

The size and shape of the distribution curve of the supply prices condition the cost-benefits of the search while in general benefits diminish with the length of search. For a variety of non-standard goods, the efforts of search by the consumers do not appreciably reduce the dispersion of prices. According to Stigler, a typical household buys "several hundred" different items each month and receives information from a number of sellers. Consequently, there is a range of sellers of these items and frequently enough unsold items to remind buyers of prices that might be too expensive.

Firms that operate under increasing returns to scale may more easily be able to cover any increase attached to the cost of obtaining price information than firms which operate under decreasing returns. On the other hand, in a previous paper (Stigler 1958) which applies the survivors' methodology to this issue, Stigler demonstrated that there may be a broad range in the optimal size of firms, such that one cannot argue that there is a univocal trend to large firms in response to an imperative to minimize costs. For instance, to analyze the profitability of advertising by the suppliers, Stigler (1964) presents an elegant algorithm:

Marginal cost of production = price p $(1 + 1/\eta_{qp} + \eta_{kp})$

where η_{qp} is the elasticity of the demand curve of a purchaser and η_{kp} is the elasticity of demand for the fraction of purchasers which buy at the price offered.

Since advertising prices will focus on products for which the marginal value of search is high, price dispersion will be reduced for most commodities requiring large aggregate expenditures. New forms of economic organization may develop to reduce search cost for consumers and to increase standardized information by the suppliers such as supermarkets.

In a later paper (1962), Stigler applied his economic theory of information relating to the search conducted within the labor market. He argues that informational issues remain an important cause of persistent unemployment, even when there is an unsatisfied demand for labor. A solution then would be to stimulate organizational approaches to cope with the information lag. However, according to Stigler, the information lags may also be beneficial to the competitive functioning of the market. Indeed, in Stigler's (1964) paper on oligopoly, he demonstrates that given the high information costs inherent to enforcement of collusion among oligopolies, we can expect that agreements about prices, quality, and market shares are likely to be breached and difficult to enforce. Under such circumstances, competition among participants is likely to emerge. It is interesting to note that Stigler is dealing with high information costs as a component of enforceable collusive transactions. This relates closely to the final section of our paper.

Conclusion

The Chicago/Virginia Connection and Interchange; Stigler, Coase and Buchanan on the "Coase Theorem" (Disagreement Rather than Controversy)

Stigler's price theory focused on those externalities which affect transaction costs in two ways:

1. Externalities and transactions cost of Coase who joined the Chicago faculty in 1962. From Coase, Stigler incorporated Coasean transaction costs in his work.
2. Externalities from the point of view of Buchanan and Tullock who consider external costs as minimally relevant.

For his analysis of externalities, Stigler adopts competition and increasing costs. In the long-run costs may be decreasing. He viewed the average costs in the long run as mimicking short-run costs. Stigler's interchange with Coase's concept of transaction costs provides a useful opportunity to explore the competitive equilibrium sought in the conversation between Chicago and Virginia.

It is well known that the "Coase theorem" describing negative externalities as social costs was not "coined" by Coase, but rather by Stigler. In the third edition (1966) of his *Price Theory*, this concept is presented as a general law of economics stated in plain words to read as follows:

> In a world without transaction costs, bargaining among the party shall solve the problem of external dis-economies in the way most efficient for the parties, independently from the legal allocation of the right of polluting to the polluters or the polluted.

It is equally well known that Coase was often unhappy about the nuances of the interpretations of the message he intended to convey in his paper on the "The Problem of Social Cost." Coase argued that Stigler's clarification of his work was prompted "based on perhaps four pages" while the focus of the paper was on the existence of transaction costs in the real world. Coase's paper, indeed, intended to criticize the "Pigouvian approach" that the government should remove market failures due to the existence of transaction costs, while ignoring the concomitant need to consider the problems associated with transaction costs. In Stigler's interpretation, the deepest meaning of Coase's theorem is reached by assuming that transaction costs are zero. In Coase's *ex post* view, the deepest and most policy relevant meaning of his theorem emerges when the transaction costs are positive and significant.

It is less well known that not only Stigler, but also Buchanan, commented on the "irrelevance" of Coasean transaction costs in his paper "Rights Efficiency and Exchange" (Buchanan 1986: 278). Here Buchanan parts company with Stigler by reformulating the Coase Theorem in a different way from Stigler. Buchanan's bottom line is that "voluntary exchanges in well-defined rights provides a sufficient condition for allocative efficiency" eliminating the clause of "zero transaction costs."

Buchanan argues from the objectivist perspective that outcomes may not achieve maximum efficiency. Eliminating uncertainty about property rights relating to an externality such as pollution may not be sufficient for bargainers to reach the Pareto frontier representing an allocative optimum outcome. Buchanan argues that transaction costs may be an obstacle to achieving the allocative optimum outcome, but that although not obtaining optimal efficiency, the parties may still reach a point that to them produces satisfactory results. Buchanan argues "a Pareto subjective optimality" may occur even if the bargainers are on a transformation curve below the Pareto allocative frontier. This would allow one to argue that transaction costs are irrelevant in this particular setting because transaction costs do not limit the ability of the parties to trade—thus making them irrelevant.

However, in a setting of well-defined property rights, transaction costs will become relevant in changing the institutional setting in order to find common ground for agreement among the law making parties. In this way, Stigler's interpretation establishes an alternative meaning for the Coase theorem. Nevertheless, these points remain open to misinterpretation. For example, if transaction costs are considered in the contractarian stage as Coase insisted in telling his story, there is an inseparable component of the market economy which is based on voluntary exchange. Transactions costs are irrelevant in this setting not because they may be removed, but because they are an unavoidable component of potential exchanges. While one may choose to use a synonym of some sort, transactions costs are also an unavoidable component of public choice in this context.

Concluding Thought

The three Nobel Laureates (Stigler, Coase, and Buchanan) were not always in agreement among themselves, but implicitly held a common view grounded in methodological individualism reflecting a basic Chicago-style axiom. However, there is no proof that they debated their views among themselves, as one might have desired. Apparently, in the market of ideas, as for any other commodity, transaction costs are important and will serve to limit trade. The Chicago and Virginia Schools benefitted greatly from such scrutiny. Indeed, one might refer to the scrutiny by Coase, Stigler, and Buchanan as a form of laser tag in which nothing is glossed over and all points (some more difficult and contentious to identify than others) are fair game for the sort of open, honest discussion which characterized Knight's influence on the development of the parallel enterprises (Stigler's Chicago School and Buchanan's Virginia School). Society benefitted greatly from both schools of economics and their associated brand of political economy. Both yielded valuable perspectives and insights on the public sector. Inevitable conflicts and challenges sharpened their laser like insights and improved the precision of their respective objectives. Although the Schools did not harbor identical methods or goals, both Schools were firmly grounded in a realistic subjectivist approach to understanding the public sector.

Notes

1. Milton Friedman (1912–2006) was born in New York City and educated at: Rutgers (B.A. 1932), University of Chicago (M.A. 1933), and Columbia University (Ph.D. 1946). He was awarded a Nobel Prize in Economic Science (1976).
2. See W. Breit and R.W. Spencer (1995).
3. Of the three main periods of the Virginia School of Political Economy, the Charlottesville epoch (University of Virginia) was 1957–1969, the Blacksburg epoch (Virginia Polytechnic Institute and State University (now "Virginia Tech") was 1968–1983, and the Fairfax epoch (George Mason University) was 1983–2013.
4. It is likely that the emphasis on the importance of institutions was first brought to the University of Virginia in the mid-1940s by fellow Chicago economist Daniel Rutledge Vining.
5. See e.g., N. MacLean (2017).
6. Friedman and Stigler had a mission to empirically test economic theories. As noted by Stigler, Friedman (and this was true of others as well), refused to "accommodate nice dichotomies, for he was a masterful

peddler. He is above all, however, a great theoretician, the discoverer of powerful systematization of reality" (Brady 2007).

7. While Simons advocated steeply progressive income taxation, the tradition of the Chicago School is associated with a proportional income tax or "flat tax." See analyses by Chicago scholars such as Blum and Kalven (1953), Hayek (1960) and Friedman (1962).

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The Pervasive Lightness of the Chicago Price Theory

The Dulcinea Complex: Defending the Unobtainable

Craig Freedman

> Hark you, heretic, have I not repeated it a thousand times, that I never saw the peerless Dulcinea, nor ever entered the portals of her palace; but that I am in love with her purely by hearsay, and upon the great fame of her beauty and rare accomplishments? (Cervantes 1993: 411)

Don Quixote worshipped his Dulcinea. Jay Gatsby attempted to escape his past to claim his Daisy Buchanan. For George Stigler, Chicago Price Theory played a highly similar role. Like Gatsby or Quixote,[1] Stigler attempted to transform himself from the gauche provincial boy into a transformative figure in economics. Like the fictional characters, he was driven by an almost romantic idealism. In a sense, the theory he helped

Dulcinea del Toboso is the necessary high born beauty to whom Don Quixote dedicates all his adventures and quests. She is as much an unarguable requirement of his imagined knighthood as is his squire, Sancho Panza, or his steed, Roccinante. For the Knight of the Woeful Countenance, some things simply needed to be believed. The actual truth of the matter is of lesser importance.

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to create reflected deep-seated needs. Chicago Price Theory in this context provided a unique key, one that was capable of rescuing the Dulcinea of liberty, individual responsibility and freedom from the malignant necromancers willing to advocate collectivist solutions.[2] To do so, the theory based on rational choice had to be universally applicable not only to decisions reflecting obvious economic concerns, but to all choices whether of a political, sociological or anthropological nature. Namely, in Chicago Price Theory, Stigler found the one true theory, the one theory to rule all others. Thus when challenged, the theory had to be defended at all costs.

> Evidence of Stigler's attachment to neoclassical price theory is also given by that part of his work mainly critical of the work of others. Price rigidity, administered price inflation, the theory of monopolistic competition, and X-efficiency were prominent targets, and each of them denied the efficacy of the neoclassical analytical framework. (Demsetz 1993: 800)

Each of these opposing theories threatened the Chicago version of price theory. That is why Stigler in attacking these threats, was utterly ruthless in his approach.[3] Poking holes was insufficient. By definition such alternatives had to be intrinsically wrong as they explicitly contradicted the way the world worked, at least as far as George Stigler comprehended those specific mechanics.

> Oh, he was a true believer. He wouldn't like that term. But put that in because he thought in that sense. He was absolutely convinced that he was right. It wasn't a doctrinal battle. It was a battle of facts. I almost said good and evil.
> *And those facts?*
> He was sure. But then some people are sure that God is on their side.
> *Yes.*
> He was absolutely sure the economy was on his side and if research was properly done it would show this. He really believed that he understood how the world works. And the way the world works had been shown to him by the theory of price. (Conversation with James Kindahl, October 1997)

Thus what was at stake in these battles was not limited to some mere theoretical issue or disagreement. At risk was the very basis of freedom and liberty, namely the defence and preservation of markets and individual choice as the organising principle of society. In essence, a Hobbesian style agreement insured the sanctity of such freedom. Individuals and organisations surrendered all economic power to the marketplace. In the absence of such power, individuals (and such organisations as firms) simply responded to market signals in the form of prices. By surrendering private power, individuals were left free to choose.

But what underscored the value of such an arrangement, from Stigler's perspective, was not only the guaranteed efficiency of outcomes but equally important, the intrinsic equity involved. Not only were resources guided efficiently to their best uses, but in a distributional sense, income received reflected individual effort. Fairness was accordingly inherent in market outcomes, if one followed the received wisdom of Stigler's beloved price theory. Under such conditions, government intervention into the workings of the economy (constraining markets in some way) could only lead to less efficient and less equitable outcomes. By redistributing income, governments inevitably changed incentives in a detrimental fashion, at least as viewed from George Stigler's perspective. In the post-war fundamental conflict (one almost characterised by some ideologies as a battle between good and evil) freely operating markets provided the bulwark against creeping collectivism. This opposing view sapped moral fibre while encouraging essentially authoritarian governments to flourish.

> Why, then, did the classical economists display such great and persistent concern with policies that maximize output? Their concern was with the maximising, not with the output. The struggle of men for larger incomes was good because in the process they learned independence, self-reliance, self-discipline – because, in short, they became better men. (Stigler 1949: 4)

As with much of his research and writings, there is always a subtext despite the undeniably scientific nature of the work in question. For Stigler, and these other self-appointed bulwarks against tyranny, in the immediate post-war period, after struggling with the fascism of Nazi Germany, the threat to individual freedom came directly from the growing army of collectivists on the left. Markets, and thus individual freedom, were under serious and undeniable threats from these sources. Thus at all costs, Chicago style price theory, which unquestionably championed markets, must be made ready to fight off the depredations posed by government intervention.

> But, the only thing I can remember him (George Stigler) saying or writing, he wrote it somewhere but I can't remember where, was that he favoured a capitalistic oriented system. He favoured it because it created the kind of person that he'd like better to live with. And that kind of person was somebody who felt responsibility for himself, and not one who thought that others were responsible for him. (Conversation with Harold Demsetz, October 1997)

Notes

1. One can imagine Milton Friedman, short, voluble and down to earth, as playing Sancho Panza to Stigler's tall and lean Don Quixote. However, it is doubtful that Stigler ever imitated Jay Gatsby by referring to friends and acquaintances as 'old sport'.
2. The theory, to stretch an analogy to the breaking point, represented a sort of philosopher's stone for Stigler. During the Middle-Ages, alchemists strove to discover the philosopher's stone with a single-minded determination. Besides having the ability to transform any base metal into gold, it could be used to create the Elixir of Life, which would conveniently provide the drinker with searched for immortality. Stigler believed he had discovered something metaphorically similar in the Chicago version of neoclassical price theory.
3. Note should be taken that Stigler largely limited his formal attacks (articles in leading journals) to those theories that seemed to be gaining some traction within the profession. In other words, such alternatives appeared to be gaining a sufficient number of adherents that they might be seen as posing a valid threat to neoclassical price theory (as revealed to the Chicago School of Economics).

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Between Old and New: George Stigler's Chicago Price Theory

J. Daniel Hammond

Natura non facit saltum
Alfred Marshall, *Principles of Economics*, 8th ed. 1920, epigram.

"Nature does not move in jumps", says the proverb.
George Stigler, Nobel Lecture, "The Process and Progress of Economics" (1983)

Historical questions about the identity and distinctiveness of Chicago economics, about a Chicago School of Economics, raise historiographic questions akin to methodological questions encountered with economic theory—how does one negotiate between the particular and general. Take for example the theory of the firm. A theory of this or that particular business firm is of little use for understanding how markets work. But neither is a theory built a priori and abstractly without connections to actual firms. If every firm and every product is viewed as unique, each firm is an industry unto itself, and theory devolves into

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description with no analytical power. But if differences between firms and products are not taken account of, and "firms" are reduced to mathematical symbols outside space and time, the theory is purified of that which it ostensibly is meant to explain. Tension in finding a balance between extremes of the overly particular and overly general runs through the twentieth-century history of economics at the University of Chicago. It is manifested for instance in the rise of and reactions to Institutionalism, itself a reaction to neoclassical economics, and the rise and reactions to mathematical economics.[1] Chicago economists settled in a middle ground between the overly particular and overly general in economic theory. This is the Chicago School of Economics, as it was developed by Jacob Viner and Frank Knight and passed on to their students Henry Simons, Aaron Director, Milton Friedman, and George Stigler. The labels often used for the prototypical Chicago way of doing economics are Chicago price theory or, with reference to its roots, Marshallian price theory.

These labels of course are in themselves generalizations, and carry the danger of exaggerating the uniformity in approach and content of price theory within cohorts of Chicago economists, across cohorts, or through the career of any individual Chicago economist. So, in writing about "Chicago economics" attention to details of economists' lives and scholarship is necessary so as to avoid labels that are neater than the economics that is being labeled. Summary depictions of Chicago economics should not be allowed to obscure the textures across Chicago economists and over their careers. I have made an attempt to generalize from biographical and bibliographical details in a sketch of the early connections and interactions between Milton Friedman and George Stigler (Hammond 2016). That piece focused on Friedman's and Stigler's careers from graduate school in the early 1930s into the early 1950s, with attention to their ideas on market structure and on methodology, topics that were intertwined for Stigler and Friedman and on which the ideas of one man were intertwined with those of the other.

In this essay I focus on George Stigler, looking for continuities and changes in his conception and practice of price theory. The essay is structured as a chronological account of selected details from Stigler's education and writings, from the beginning of his career through the

1960s. The approach is to assemble facts of Stigler's scholarship without reference to interpretations that have been made by his students, colleagues, or historians of economics.[2] The essay concludes with three retrospective accounts that Stigler himself made of the history of economics over his career and his role in this history. Thus we see in Stigler's writings from when he was a young scholar in 1930s to when he was a mature scholar in the 1960s his developing views of the problems and prospects of economics. And then we see in his retrospective pieces some of what he thought he had learned over the course of his career about economics and economists. There are threads that run from Stigler's earliest work into his work as a mature scholar. Some threads are the same throughout; others change their twist over the course of his career. We see that to understand George Stigler's work as a mature scholar it helps to know his early work, including his training as an economist. This is because, like nature in the Marshall epigram and the proverb quoted in his Nobel Lecture, George Stigler did not move in jumps.

Stigler's Doctoral Training

George Stigler entered the Chicago doctoral program in autumn quarter 1933, having completed his B.B.A. at the University of Washington and M.B.A. at Northwestern. His study of economics actually began at Northwestern where he wrote papers on "A Theory of Distribution" (1932a) and "The Theory of Value from Adam Smith to Stanley Jevons" (1931–1932) for Professor F.S. Deibler.[3] His master's thesis on "Some Economic Aspects of Municipal Land Policies of American Cities" (1932b) was supervised by Coleman Woodbury, who was a student of Richard T. Ely, and who became recognized as a leader in urban planning. In the same year that Stigler wrote his thesis (1931–1932) Woodbury became Executive Secretary of the Illinois Housing Commission.

Among the courses that Stigler took in the Chicago program were Economics 301, "Price and Distribution Theory" from Jacob Viner; 302, "History of Economic Thought" from Frank Knight; 303, "Modern Economic Tendencies" from Viner; 311, "Correlation and

Curve Fitting" and 405, "Theory and Measurement of Demand" from Henry Schultz; 330, "Graduate Study in Money and Banking" from Lloyd Mints; 340 "Trade Unions" from Harry Millis; 360 "Government Finance" from Henry Simons; 362 "State and Local Taxation" from Simeon Leland; and 370, "International Trade and Finance" from Viner. His final course, in the spring quarter 1936, was Knight's Economics 404, "Seminar in History and Institutional Economics." Having arrived at Chicago without especially strong preparation in mathematics, Stigler took "Elementary Mathematical Analysis I," "Calculus I and II," "Differential Equations," and "Introduction to Higher Algebra." He completed his dissertation under Knight after joining the faculty of Iowa State University in 1936. The title was "Studies in the History of Production and Distribution Theories," a revised version of which was published by Macmillan in 1941 as *Production and Distribution Theories: The Formative Period* (1941/1994).

In the introduction to the Transaction Press edition of *Production and Distribution Theories: The Formative Period*, Douglas Irwin (1994) notes that Stigler's survey of the development of neoclassical distribution theory bears marks of Frank Knight's critical approach to the history of economic theory, but that one could overemphasize Knight's influence. Stigler arrived at Chicago with an interest in the history of economics, having written the two history of economics papers at Northwestern. Whether by prior inclination or influence, Stigler shared several attributes with Knight. Both were of a sharply critical mind and pen along with their shared interest in the history of economic theory. Indeed, both Stigler and Knight lived their intellectual lives in the "extended present," as Craufurd Goodwin (2002) said of Stigler. They shared a whiggish view of economics in that they believed economics had a progressive history and that progress consisted in part in discovering and learning from the mistakes of the past. But both believed that the best economics of the past was superior to much of present economics.

The central question for Stigler in *Production and Distribution Theories* was why there was a lag between the development of subjective value theory based on the concept of marginal utility and development of neoclassical distribution theory based on the analogous concept of marginal product. As he expressed the mystery:

> However, the simultaneity with which the marginal productivity theory was finally formulated by so many economists is less astonishing than is the fact that it had not been clearly formulated at the same time as the theory of subjective value and become at once an integral part of the general body of doctrine. (1941/1994: 4–5)

Value theory was developed in the 1870s by Jevons, Marshall, and Walras, but Stigler thought the economics of distribution remained description without analysis until the 1890s. Though Stigler would later claim that scientific economics began with Adam Smith, he regarded the last quarter of the nineteenth century as the watershed period when economics was transformed from an art, as it was in Smith's day, into a science. Once the theory of production and distribution, i.e., marginal productivity theory, was paired with the theory of value of products, economics had a unified analytical system. The transition from the art of classical economics to the science of neoclassical economics was completed. We will see that while other twentieth-century economists regarded neoclassical economic theory as inadequate for understanding twentieth-century economies, so inadequate that it should be razed and rebuilt, Stigler thought it was structurally sound.

Stigler's treatment of the development of production and distribution theory is organized into twelve chapters, each except the last devoted to the work of an individual. The economists are William Stanley Jevons, Philip H. Wicksteed, Alfred Marshall, Francis Y. Edgeworth, Carl Menger, Friederich von Wieser, Eugen Böhm-Bawerk, Leon Walras, Knut Wicksell, and John Bates Clark. The final chapter is on the application of Euler's Theorem in marginal productivity theory. Stigler's standard for evaluating theory was the same as his teacher Knight. He looked for logical consistency within a theory connecting its assumptions and conclusions. He viewed pure economic theory as a field of logic like mathematics. Some economic relationships, such as the law of diminishing returns, could be known in a rudimentary form without training in economics. Farmers acknowledge the law of diminishing returns in the same way that they acknowledge the law of gravity. But they may not understand either. Stigler's first criterion for evaluating an economist's insight into an economic relationship such as diminishing returns was: "Does the observation [of a relationship] fit into the economist's general theoretical system, or is it an obiter dictum, either inconsistent with or irrelevant to his structure as a whole?" (1941/1994: 9). But he thought economics was more than logic, and "loses its interest and importance if the assumptions do not correspond to the 'facts'" (1941/1994: 7). So he also evaluated economists' contributions to production and distribution theory on the basis of the empirical validity of their theory—"have the 'right' assumptions been chosen" (1941/1994: 7). "Economics is, after all, political economy, and social policy is, as it has always been, its central problem" (1941/1994: 8).

The question of how to judge the empirical validity of a theory, by the realism of its assumptions as Stigler suggests here, or by the realism of its implications, as Milton Friedman was to suggest later, would loom large in Chicago economics shortly after Stigler's book came out. Also looming large was the economist who Stigler took as the exemplar of neoclassical economics, Alfred Marshall. Although he thought Marshall's fellow Englishman Philip Wicksteed had pride of place on the exhaustion of total product in payments to factors of production, Marshall set the standard for overall contribution to neoclassical theory. "Marshall was almost incomparably superior to his immediate predecessors and his early contemporaries in the profundity and originality of his thought, in his consistency, and in the breadth of his vision" (1941/1994: 61).

Historian and Critic of Economic Theory

Of the ten economists whose contributions to marginal productivity theory Stigler covered in the dissertation and book, he chose Carl Menger as the subject of his first journal article, "The Economics of Carl Menger" (1937a). He put his facility with the German language to use, introducing Menger to English-speaking economists. None of Menger's work had been translated and his most important work, *Grundsatze der Volkwirtschaftslehre* (1871) had been out of print until it was published in an LSE series of "Scarce Tracts." Stigler surveyed Menger's value theory, leaving aside his contribution to the

Methodenstreit between Austrians and the German Historical School and his work on currency. His admiration for Menger's accomplishments comes through in the article. He considered Menger's value theory superior to that of Stanley Jevons, whose *Theory of Political Economy* (1871) was contemporaneous with the *Grundsatze*. Menger was more profound and systematic, avoided unnecessary and clumsy mathematics, and included a sound general theory of distribution. "Menger's treatment was lucid, systematic, and comprehensive; and, to mention a factor of ambiguous importance, his was good economic theory" (1937a: 231).[4] Because of Menger's superiority to Jevons, plus his advantage in the absence of a strong classical school of economics among German-language economists, Menger founded a school—the Austrian School. Jevons left no followers.

Stigler's purpose was both to introduce readers to Menger's theory and to evaluate it. In addition to the general merits already mentioned, Stigler singled out Menger's expansion of the concept of economic goods to include nonmaterial goods, such as monopoly privileges, good-will and patents; his insight that productive resources (goods of higher order) should be amenable to the same theory as consumer goods (goods of the first order); his insight into the "germ" of imputed value of resources from the value of consumer goods; and his use of static analysis with fixed stocks of productive resources and flows of services. Thus Menger set up what Stigler referred to as the basic economic question: "how should the given quantities be distributed to secure the greatest satisfaction of needs?" (1937a: 235). He regarded Menger's demand theory more fully developed than his theory of supply, the latter lacking a theory of alternative cost. But Menger "lays the groundwork for a correct theory of productive organization—i.e., for the determination of the allocation of resources" (1937a: 242).

Menger's primary advance in the theory of production was in recognizing that as a general rule, resources could be combined in varying proportions to produce a given product rather than, as in classical theory, varying proportions only of capital and labor. He was instrumental in the move away from the classical conception of qualitatively different types of productive resources—land, labor, and capital—and the distribution of product to separate social classes—landlords, laborers, and owners of capital. Among the gaps in Menger's theory, according to Stigler, was his failure to distinguish physical resources from the stream of resource services. This prohibited development of an adequate theory of capital. On the whole Stigler judged the *Grundsatze* to be "in fundamental respects unexcelled by any other between the *Wealth of Nations* and Marshall's *Principles*" (1937a: 250).

Stigler presented a paper at the 1937 Mid-West Economic Association meeting on the theory of imperfect competition, a version of which was published in the *Journal of Farm Economics*. This was the first of many works by Stigler on market structure. "A Generalization of the Theory of Imperfect Competition" (1937b) may be misnamed, because Stigler did not generalize a theory of imperfect competition. What he did was to make a methodological argument against the proliferation of theoretical work on imperfect competition published over the previous decade. His "generalization" was to suggest that economists return to the theory of perfect competition, and give attention to which of the theory's assumptions were violated in real-world instances of less than perfect competition. His critique of the literature on imperfect competition was general rather than directed at specific works.[5] It consists of three propositions: (1) progress on the theory of imperfect competition cannot be made until the theory of perfect competition is more fully developed, (2) mathematical technique has been more important than economic policy in giving shape to the theory of imperfect competition, and (3) tools of investigation rather than economic problems in need of solution have guided development of the theory.

These propositions display what were to become characteristic features of Chicago economics in the mid-twentieth century. First is the use and refinement of existing theories rather than developing replacements for neoclassical value theory. Here the existing theory is the theory of perfect competition and the new is the theory of imperfect competition. In Chicago "macroeconomics" the old theory was the quantity theory of money and the new was Keynesian theory. The second and third propositions make essentially the same point that would also be a basic premise at Chicago—that the purpose of economic theory is to solve problems in the real-world economy. The value of economics is in its practical usefulness, in the use of analytical tools rather than in the tools themselves. Theories should not be abandoned and new theories and tools of analysis developed unless the economic problem under study requires new tools because the old tools are not up to the job. This was the point of Chicago critiques of mathematical and general equilibrium economics.

This brings us to Stigler's "generalization" of the theory of imperfect competition. Competition in industries does not fit the model of perfect competition, so the question is not whether there is imperfect competition. The question is what its causes are, and how to identify and analyze the causes. Stigler's approach was to generalize the theory of perfect competition—"stating rigorously the assumptions of perfect competition, and then modifying each assumption" (1937b: 711). He illustrated this with three of the theory's assumptions: perfect knowledge, the "economic man," and complete divisibility of inputs and products. Any one of these assumed conditions, suggested Stigler, may by their violation be a source of imperfect competition. He thought imperfect information, i.e., ignorance, was the primary steady-state basis of imperfect competition. Here we see the germ of the problem dealt with in what became perhaps Stigler's most important contribution to economics, the economics of information.[6] He suggested the primary departure was consumer ignorance of prices and qualities of goods, and to a lesser extent resource owners' ignorance and entrepreneurs' ignorance on various dimensions. Entrepreneurs' ignorance paradoxically provides the basis for entrepreneurship.[7]

Stigler thought the use of "economic man" as an ideal type in economic theory was misunderstood by those who would abandon the theory of perfect competition for what they consider a more realistic theory. Flesh and blood people are not "economic men." They base decisions on economic and noneconomic considerations, such as love of land and home, racial prejudice, charity components in wages, and "personality" quirks. Stigler regarded this as an area in want of research by economists, sociologists, and psychologists.[8] He suggested that indivisibilities of products and especially of resources was another area ripe for research. "It is demonstrable that all economies, external or internal, must rise out of indivisibilities of productive resources. But no one has paid much attention, either theoretical or empirical, to the question of indivisible resources" (1937b: 714). This was, he thought, one of the pressing empirical questions of the day.[9] He quoted his teacher Frank Knight. Although Knight looked askance at empirical work, he claimed that "no fallacy is more pernicious with reference to intelligent economic policy than the popular illusion that large-scale business is in general more economical than small-scale" (1937b: 714). If Knight was right about this, then why, asked Stigler, is there a tendency toward larger economic units. He suggested that a noneconomic factor was responsible—the drive for business power facilitated by imprudent economic policies.

Stigler's Chicago training is in evidence in this article, as in *Production and Distribution Theories*. He cites Knight's *Risk, Uncertainty, and Profit* (1921a) twice, in addition to Knight's "Cost of Production and Price" (1921b); Henry Schultz's "Marginal Productivity and the Pricing Process" (1929); and Jacob Viner's class lectures on price leadership by a dominant firm. The classical liberalism of mid-century Chicago economics also comes through in this article. Stigler cites Henry Simons's *Positive Program for Laissez-Faire* (1934), and endorses breakup of monopolies at a time when economists were coming to view monopoly as an inevitable part of industrial evolution. Stigler concluded the piece: "And finally, of course, there are those who still adhere to the belief that a liberal economic society is the object of all good social policy. To them it is important to compare economic policy under perfect and imperfect competition, to understand the causes of imperfect competition, and to reformulate remedies to restore a competitive regime" (1937b: 717).

Along the same lines Stigler included among his references Frank A. Fetter's article-length review (1937) of Arthur R. Burns, "The Decline of Competition: A Study of the Evolution of American Industry" (1936). Fetter's concluding paragraph is notable for its resemblance to the perspective on politics, policy, and economic scholarship that Stigler would later develop. Fetter wrote:

> Accordingly, this review has not attempted to pass judgment on the author's views from the standpoint of a different social philosophy, and merely to express a different personal preference as to the kind of society

> we might like to live in. That is a matter of taste about which, as we are frequently reminded, "there is no disputing." If enough other citizens of our democracy come to yearn for the autocratic monopolistic state idealized by the author, the nation will some day give it a trial, come what may – logic or no logic. But if a case is to be made for universal monopoly that will impress rational minds, such a book remains to be written. We have simply sought dispassionately to measure the thesis of this book by its own internal evidence and to test the consistency of the author's reasoning on his own assumptions. (1937: 110)

Stigler's first article dealing directly with economic policy was "Social Welfare and Differential Prices" (1938). This provides another look at his early ideas on economic theory, methodology, and social philosophy that would run through Stigler's later work. It also is our first look at Stigler's use of economic analysis for policy. The policy matter in the article was government-sponsored marketing programs that used price discrimination to enhance social welfare. One such proposal was analyzed and advocated by Frederick V. Waugh, an economist with the Bureau of Agricultural Economics (Waugh 1938). The plan was for price discrimination on basic agricultural products, charging higher prices to high-income consumers and lower prices to low-income consumers, intending to enhance the welfare of both low-income consumers and farmers. Stigler's critique was in three parts. First, he criticized the hedonistic economics and ethics in cardinal utility that was the basis of Waugh's analysis. Second, Stigler claimed that apart from questionable utility analysis the plan was unworkable given what were likely to be demand elasticities and administrative costs. And third, Stigler made a political economy critique of price discrimination programs with insights that would become prominent in his later work in the economics of regulation.[10]

We have seen already in his survey of early neoclassical production and distribution theory the importance of the scientific status of economics for Stigler. He believed that in the later nineteenth-century development of value theory economics was transformed from an art into a science. Here we see details of Stigler's vision of the nature of scientific economics. It includes measurement as a critical component.

But he was critical of the hedonistic calculus of cardinal utility. He thought using cardinal utility as the basis of market value was not wrong in intention, but was a dead end in two ways. First, utility is not measurable. And second, if it was measurable, utility is not comparable across individuals. "No one has ever offered a real defence [sic] for the practice of comparing utilities: the argument has always been that such a comparison *must* be permitted because it is necessary to certain conclusions regarding economic policy" (1938: 576). He cited Henry Simons's *Personal Income Taxation* (1938) in questioning the normative case for utilitarian ethics brought into economics along with utility-based value theory. Personally, Stigler regarded a poor consumer's dollar of additional income as more important than a dollar of income to a producer. But as expert advisers, economists had no way to make this comparison. This was a matter for the public to decide democratically.

> Granting that these propositions are questions of policy rather than of economic analysis, they may still be valid, that is, compatible with the ethical notions of the public. Progressive personal income taxation, which almost everyone endorses, is popular because it is "democratic." This is certainly an adequate basis, and one to which the writer fully subscribes. (1938: 577)

We see in this discussion three elements of Stigler's social ethics. One is from Adam Smith—that economic policy should serve the interests of consumers rather than producers. Another, illustrated with a quotation from Henry Simons, is that human behavior and choice involve more than "satisfactions."[11] And the third, likely from Knight, is that "democracy" is the appropriate means of making social choices. Stigler does not indicate in any detail what democracy means to him, but it is clearly not decision-making by an elite for the common people. Of Waugh's recommendation that marketing charges for poor consumers be set below those for the rich, Stigler wrote:

> This argument is, to the writer, completely inacceptable. Marketing costs to poor consumers may admittedly be reduced either by reducing

> the services rendered or by reducing the cost of rendering such services. But why a differential pricing system is necessary to secure these economies is not clear. If poor consumers would rather have lower prices and fewer services, they can certainly secure these objectives unless retail trade is monopolized to an extent not generally recognized. On the other hand, should poor consumers actually prefer delivery service and fancy containers to mere calories, the economist must accept this situation as a datum. (1938: 583)

Another statement that shows Stigler's regard for common people and democracy is:

> The proposal that certain products be subsidized for the poor consumer, because they are good for him, seems a bit out of place for an economist per se. It is just possible that on equally valid grounds some people may prefer beer and others pork chops. In a democracy, if such utopian construction may be permitted, it is fundamental that desirable changes in consumer's tastes (desirable, that is, in the light, say of medical knowledge) be effected by education, not by manipulation of relative prices. The obstacle raised by insufficient income is better overcome by appropriate taxation and social service expenditure policies. (1938: 584–85)

He suggested in a footnote that the first step might be to make the income tax truly progressive.

To judge whether the price discrimination scheme might work as intended Stigler used a technique that would become a signature mark of Chicago price theory. This was applying partial equilibrium demand and supply theory with what he considered reasonable assumptions about quantitative relationships. He suggested that demand elasticities of low-income and high-income consumers, and proportions of output purchased by each group, were unlikely to be in the ranges necessary for transfer of value from high-income to low-income consumers while also increasing total sales revenue of farmers. He suggested that:

> only if (1) rich consumers are of great importance quantitatively, (2) poor consumers have highly elastic demands, (3) rich consumers have relatively inelastic demands, and (4) the marginal costs of agricultural products

> increase slowly as outputs expand, – then it is possible that poor consumers will secure a reduction in price by a policy of differential pricing designed to maximize farmers' incomes. The first two assumptions are simply contrary to all our experience. (1938: 582)

Stigler's political economy expectation of the marketing proposal was that Waugh's "unquestionably benevolent objectives … would find small acceptance from politically articulate groups about to embark on a differential pricing policy for their product. Differential prices would most probably be used to increase the revenue of special producer groups, not to lower prices to poor consumers" (1938: 583). In addition, as proposed, the pricing scheme was for "basic" agricultural products. Stigler predicted that if economists compiled a list of basic products "the length of the list, however, would be somewhat lengthened before it passed through Congress, if our knowledge of special producer groups (vide tariff history) has any validity. 'Basic' products, the writer submits, would eventually include watermelons as well as wheat" (1938: 584).

Stigler on Empirical Economics

That Stigler highlighted the elasticity, market share, and cost requirements for the differential pricing scheme to work as Waugh expected shows that Stigler was more balanced between theory and empirical evidence than his mentor Frank Knight. Stigler thought quantification was an important part of scientific economics, and in the early 1940s began a thirty-five-year relationship with the National Bureau of Economic Research, where quantification was emphasized. The first two of his empirical studies were National Bureau projects, *Domestic Servants in the United States* (1946a) and *Trends in Output and Employment* (1947a). These and other National Bureau empirical projects on labor markets were followed by empirical work with Claire Friedland on the effects of regulation and on citation practices in the economics literature. Shortly before he became a National Bureau research associate Stigler published a critique of empirical derivation of demand curves, aimed in part at the National Bureau's Wesley C. Mitchell and his teacher Henry Schultz. In "The Limitations of Statistical Demand Curves" (1939) we see Stigler's early view of the relationship between theory and estimation. He directed the critique at empiricists who formed the "cult of correlation."[12] He acknowledged that progress in both statistical methods and economic theory may allow discovery of "some of the constants of the equations of economics," though none had yet been produced. And he averred that further progress would depend on reformulations on both sides of the work, economic theory, and estimation techniques. Stigler warned that "the price theorist … must entertain more scruples, questioning in particular the relation between these statistical demand curves and the demand curves of economic theory. It is the purpose of the present paper to analyze the discrepancy between these two types of demand curves and to suggest certain advances necessary to the elimination of the hiatus between them" (1939: 469–70).

Stigler saw little linkage between estimates of demand functions and economic theory. Theory did not even pin down qualitative relationships to be expected in estimates, because theory was both too general and too narrow. On one hand general equilibrium theory was too general, with "solutions stated in terms of a determinant with n^2 elements, of which $(n - 1)^2$ elements are quantitatively unknown. … The general equilibrium method is not fertile: we sacrifice content to formal generality until we achieve the state of the perfect dilettante, and know nothing about everything" (1939: 471). Stigler proposed setting studies at an intermediate scope between full general equilibrium and a single market—general equilibrium for a small set of related markets.[13] On the other hand, theory was overly narrow, omitting factors that are important for empirical analysis of demand. Key among these was time. Theoretical studies dealt with stationary conditions, but empirical studies used data in time. This was true in two senses: adjustments occur over time and buyers anticipate future prices.[14] Also, theoretical studies dealt only with relative prices, and empirical analysts had not come up with satisfactory ways of controlling for changes in the value of money. Still another gap in empirical estimation was accounting for demographic heterogeneity of the population across location and time. Reduction of quantity figures to per capita measures "seems, to the economist, to be equally arbitrary and meaningless" (1939: 473). Furthermore, in theory all transactions occur at equilibrium, but transactions from which data are derived occur on and off demand and supply curves. "It is not possible to estimate a priori the error arising from the use of average prices and total quantities. If the price is stable, the error is not likely to be serious. But if the price is changing, then the demand curve secured will depend on the time units chosen. … It is impossible to select the 'correct' time periods, –there are none!" (1939: 475).

We highlight one final problem that Stigler discussed, because of its connection with his future work with Gary Becker.[15] He suggested that attempts to estimate long-term demand functions were futile because tastes change over the long term and "such changes are fatal to any demand curve" (1939: 475). Moreover, substantial changes in income are likely to have effects on preferences.

> It seems reasonable to assume that the indifference system of an individual is independent of small income fluctuations, but anything like significant income variations will lead to a changed "standard of living" and a new indifference system. Until this problem is handled theoretically, I can see no significance in income elasticities based on anything but small income variations. (1939: 476)

After identifying several other problems in measurement Stigler concluded:

> If the preceding discussion be accepted, the conclusion follows that statistical demand curves are still remote from the demand curves of the economic theorist. In the writer's opinion, the gap between the two types of demand curves will never be completely bridged, and in fact that rapprochement is likely to be slow. (1939: 481)

Theory and Evidence of Market Power

In "Notes on the Theory of Duopoly" (1940) Stigler continued the practice from previous articles of critically evaluating the literature in an area of theory. He had claimed in "A Generalization of the Theory of Imperfect Competition" that this very active area of research was not fruitful, as he also did of empirically derived demand curves. In these cases Stigler's lack of enthusiasm for the research programs was methodological. He thought the theory of perfect competition a better framework than monopolistic competition for studying departures from competition, and he thought grounding in good theory was crucial for estimation of demand functions. In the case of duopoly he was "half-heartedly" favorable to the research program. "For once empirical evidence joins a priori considerations in suggesting that explicit agreement is a typical 'solution' of duopoly relationships" (1940: 521). He suggested that in some industries with few firms there were gaps in collusion—advertising in cigarettes, labor policy in steel. Personalities, such as the case of Henry Ford, could be important in whether or not to "play the game" of collusion. Also, with a number of competitive dimensions administrative costs hampered efforts to collude. "For these and other reasons the theoretical duopoly problem merits the attention that economists have lavished on it" (1940: 522).

The question was whether theory could provide a definite solution for the duopoly problem. He began with the case of a dominant firm that sets price and takes as its demand the quantity remaining from the smaller firms. For this relatively straightforward case he used diagrams from Jacob Viner's Economics 301 class to draw four generalizations about the output of the dominant firm in relation to its proportion of total sales and to elasticities of supply and demand. He then moved to the Cournot and Bertrand models, which he judged inadequate on the basis of their unrealistic assumption that firms retain anticipations of how rivals will react no matter how their rivals actually do react. "In both cases [Cournot and Bertrand models] a degree of stupidity is gratuitously attributed to duopolists that surely inhibits any rational treatment of the duopoly problem" (1940: 527). This led Stigler into the thicket of knowledge, ignorance, and learning. "Since the rival's anticipated reaction is the crux of the duopoly problem, this assumption of quasi-perfect knowledge requires that we deal only with special cases – there can be no general theory of duopoly" (1940: 525).[16] Thereafter Stigler dealt with special cases of rivals' original expectations about price and quantity. He considered cases of correct original expectations either that prices cuts would be met or that market share would be protected. He acknowledged a potential set of cases where expectations were incorrect but the market moved quickly to equilibrium. But he could not think of any reasonable general illustrations of this. He suggested that work was needed on rival firms' revisions of expectations.

Then Stigler extended the theory to differentiated products. His analysis suggested that duopolists' price would be near the monopoly price, even considering the potential for new entry. Rivalry was more likely on competitive dimensions other than price, such as product quality, advertising, and development of new products.

> Especially when uncertainty is present, and this is usually an easily conceded assumption, there is considerable support for the view that duopolists will engage in many forms of rivalry. Such rivalry has frequently been pointed out in the field of advertising, and the evidence is also fairly clear in research in product improvement, cost reductions, expansion of investment in an expanding industry, and similar variables. To these and other forms of rivalry the theory of duopoly is completely appropriate. (1940: 538–39)

Stigler suggested that there was no general analytical device for dealing with rivalry in product quality. The numerous dimensions of quality made it possible for duopolists to pursue two apparently incompatible goals—making their product simultaneously a close substitute to attract the rival's customers, and differentiated to retain their own customers. Stigler raised a question that would come to the fore later in questions of markets and the public interest. This was whether or not competition in product quality would result in real quality improvement. He answered that it depends. "It may be remarked, finally, that these competitive pressures will necessarily lead to product improvement (defined in commonsense terms) only in a relatively informed market. Salesmanship and real quality improvement (e.g., durability) are very good substitutes for one another in uninformed markets" (1940: 540). This consideration of consumer knowledge and his comment on the need for work on revisions of expectations of duopolists were another prelude to Stigler's work two decades later on the economics of information.

In June 1942 the *American Economic Review* published a supplemental issue devoted to the Temporary National Economic Committee, established by Congress in June 1938 to study concentration of economic power in American industry. The presumption behind TNEC was that American industry had become increasingly concentrated. Stigler's "The Extent and Bases of Monopoly" (1942a) was one of four review articles included in an AER supplement along with documents relating to the committee. His concern was with two matters under study by the committee and consultants in the 33,000 pages of hearing transcripts and reports: (1) the extent of competition or monopoly in the American economy, and (2) the leading sources of monopoly. Stigler opened with the matter of appropriate frame of reference for competitive or monopolistic industry. It could not be perfect competition or perfect monopoly. To suggest such would be to betray ignorance of the way these limiting concepts were used by theorists. According the frame of reference would vary depending on the purpose of the investigation. And the sole purpose of a study of competition and monopoly, in Stigler's view, was to improve economic policy. He endorsed what he claimed was the historical preference of Americans, and the majority of witnesses before the committee, for a "competitive enterprise economy." But lacking precision in the working definition of competition, studies of various industries were not comparable. He suggested as a standard of *workable competition*: (1) a considerable number of firms selling closely related products in each important market area, (2) no collusion, (3) long-run average cost for new entrants not appreciably higher than that of established firms. Anticipating again his work on the economics of information, he remarked that in retail trade workable competition would require easy access to information on the technical qualities of products for consumers.[17]

Much of his attention was on the studies' deficiencies in measuring market power. Lacking quantitative measures of departure of industries from workable competition, analysts relied on anecdotal evidence of monopolistic practices that biased the picture toward monopoly. He was critical of studies that used concentration ratios for domestic production but excluded foreign producers of imports or that defined products too narrowly, such as *new* cars. These omissions biased conclusions toward market power. But on the other side national concentration ratios introduced a bias by hiding local and regional collusion.

Textbook Author

Less than a decade after entering the Chicago graduate program Stigler wrote a textbook. *The Theory of Competitive Price*, was published by Macmillan in 1942. Following a plan he had from the beginning, he added material on imperfect competition in a 1946 edition, abbreviating the title to *The Theory of Price*.[18] *The Theory of Competitive Price* has an introductory section in four chapters. These are on methodology; the meaning of basic concepts used in analysis such as competition and equilibrium; the function of an economic system; and fundamental relationships—total and marginal product and cost, demand functions, and elasticity. The theory of price under competitive conditions is covered in six chapters. Two of these are on demand, two on costs, and one each on pricing of products and of productive services.

Stigler claimed that economics is a science and explained what this meant. Although the scientific status of economics was important, Stigler did not believe that science was the only source of knowledge. "The fundamental characteristic of a science is the establishment of generalizations with respect to the relationship between various distinguishable phenomena. If one can say, if A, then B, one has a scientific law" (1942b: 3). In addition, "generalizations should be interrelated and, if possible, reducible to one comprehensive generalization; and the field of study should have fairly distinct boundaries" (1942b: 3). Scientific laws allow prediction (if-then statements) and control. Although sciences have boundaries, the more general are scientific laws the greater is the scope and usefulness of the law for prediction. The first step in testing and applying scientific hypotheses is to make sure a conclusion follows logically from the premises, which may be wholly true (e.g., marginal cost has a smaller slope than marginal revenue), partially true (e.g., entrepreneurs seek maximum profits), or wholly false (e.g., marginal revenue and cost curves are continuous). Economics includes logic and mathematics but is more than this—an economic theory "must explain the behavior of the phenomena in which we are interested; the assumptions must correspond to facts" (1942b: 5–6).[19] So scientific laws cannot be established a priori from introspectively self-evident truths. Science has a necessary empirical component. A better theory is a theory that, given that its conclusions follow logically from its premises, better explains a broader range of facts. No theory can be proved true empirically, but it can be proved erroneous. A body of scientific laws grows when new laws, established logically and supported empirically, are consistent with established laws. Special theories, such as the theory of the burden of an excise tax, and general theories, such as price theory, exist in a mutually supportive but not equal relationship. If the general theory fails, the special theory fails as well. But failure of a special theory need not mean failure of the general theory. For alternative theories of the same phenomena, both of which have empirical support, the theory with simpler assumptions is preferred.

Stigler gave several reasons to study perfect competition even though markets are not perfectly competitive. One was that any theory isolates elements from the multiplicity of particulars. So a theory of markets cannot be a theory of every competitive and anticompetitive dimension. Loss of content is the cost of generalization, but generalization is of the essence of theory. Why generalize with the assumption of competition rather than monopoly? Because experience has shown that the theory of perfect competition isolates important elements of market processes. In addition, Stigler believed that competition was more commonplace than monopoly, even in markets that are considered to be monopolized.

The theory of competition is a simple theory in that there are only three assumptions: (1) no buyer or seller has perceptible influence on price, (2) markets are free of institutional restraints that inhibit movement of prices or resources, (3) all units have complete knowledge.

We have seen that Stigler believed the best way to understand real-world markets is to explore the implications of violations in these assumptions. Twenty years before he published "The Economics of Information" (1961) we find more hints that departures from complete information would be important in his later work. He suggested that many features of market institutions and practices arise from incomplete information. Among these are government functions such as regulating weights and measures, controlling banking and money, and policies concerning education and health. If information was complete there would be no advertising, no reason for the entrepreneurial function, and no price differences for goods within their markets.

Stigler used Lionel Robbins's definition of economics as the study of use of scarce means for alternative ends. He distinguished between economists' and philosophers' roles concerning means and ends. Economists specialize in means, whatever the ends may be, and philosophers in analysis of the ends. That is to say, economics is a positive science; philosophy is normative. "Strictly speaking, words like *ought* and *bad* cannot occur in an economic discussion—at most one may say that an action is not appropriate to the end in view" (1942b: 15). Restricting what economists could say as economists had both a conventional and personal element for Stigler:

> It is the fundamental tenet of those who believe in free discussion that matters of fact and logic can (eventually) be agreed upon by competent men of good will, that matters of taste cannot be reconciled by free discussion. Assuming this to be true, it is apparent that if value judgments were mixed with logic and observation, a science would make but little progress. (1942b: 15–16)

But this did not mean that economists should not express views on what were worthy policy goals, only that they not claim to do so based on economics.

Curiously, his discussion of the role of statistics in economics does not mention use of data and statistics to test theories. He rejected the idea of deriving theory from data, as we have seen in "Limitations of Statistical Demand Curves." Theories are based on a set of assumptions, which need to be factual in the sense that they correspond with essential facts of the problem being analyzed, e.g., scarcity of land and diminishing returns in Ricardo's theory of rent. "If the most general structure of a given type of economic system is in question, there is only a minor need for empirical information. The theory of competition is based fundamentally on about a half a dozen assumptions" (1942b: 19). Application of theory is where empirical evidence is most needed. "No a priori argument from general premises … can tell us what the precise effects of a high excise tax on cigarettes will be, just as no degree of familiarity with mathematical physics will of itself, indicate the necessary strength of the girders for a particular bridge" (1942b: 19). Empirical evidence is also used to modify a theory when an application fails, or to extend the theory to other applications.

Methodology and Use of Price Theory

Stigler modified his restriction of economics "strictly speaking" to the study of only means, not the ends to which means are directed in "The New Welfare Economics (1943), on which he was working as he wrote his textbook. The so-called new welfare economics was an attempt to allow evaluative statements of policy from within economics without traversing the positive-normative divide. Any policy will have distributive effects, with some parties losing income as others gain. Could an economist draw conclusions about the desirability of a policy while maintaining scientific neutrality? The new welfare economists' answer was "yes," so long as the policy increased income for those gaining by a greater amount that the losses incurred by others. This would be the case if the policy led to equality of marginal rates of substitution across individuals and to equality of marginal cost with price. Potential compensation of losers by gainers would obviate the need to compare worthiness of different parties, removing the ethical dimension of the policy. This was a matter of great importance for welfare economics because of the austere limits set by the positive-normative divide between science and ethics. Absent potential compensation, economists could not with their authority as economists recommend any policy.

Stigler's challenge was essentially that potential compensation is an economist's trick. It supposes that maximization of national income in policy analysis is not based on a value judgment, but all other ends of policy are. For purposes of argument he took the compensation assumption seriously and asked rhetorically if anyone would endorse the efficient anti-crime policy of compensating thieves.

> Consider theft; our present policy toward this means of livelihood probably has adverse effects on the national income. Prevention of theft and punishment of thieves involve substantial expenditures for policemen, courts, jails, locks, insurance salesmen, and the like. By compensating thieves for the amount they would otherwise steal, we save these resources and hence secure a net gain. (1943: 356)

Stigler suggested several reasons that neither he nor other economists would approve of this policy. There were practical reasons—how to discover the identity of thieves before they commit theft and how much they would steal, and how to prevent demand for potential thieves from producing its own supply. But most tellingly the policy would violate our moral sensibilities. So "maximum income is never the sole end of policy" (1943: 357).

Stigler suggested loosening the prohibition of value judgments in economics along lines proposed by Talcott Parsons (1937) and J.N. Keynes (1930). There could be two components of economics—economic analysis along conventional lines and ethics applied to economic issues. This would allow economists to do what they have done and will continue to do without the self-deception that they deal only in "facts." Ethical judgments would be brought into the light. Stigler suggested there would still be plenty of work in conventional economics because few policies are ends in themselves. Policies are usually means to ends. So there was no reason to fear that public policy will be left outside the domain of "rational discussion."

Both Stigler's way of doing economics and his ethical presumptions are on view in "The Economics of Minimum Wage Legislation" (1946c). This article was prompted by calls for an increase in the legal minimum wage, which had been reduced in real value by inflation since its creation in the Fair Labor Standards Act of 1938. Stigler posed two questions: (1) does the minimum wage in fact reduce poverty, its purported purpose, and (2) are there efficient alternative ways to reduce poverty. He considered the effects of a minimum wage on the allocation of resources under competitive and monopsonistic labor market conditions, on aggregate employment and family income, and proposed an alternative—the negative income tax. Under competitive conditions, with workers receiving the value of their marginal product, lower efficiency workers would either lose their jobs or become more productive. An increase in their productivity might come about by the workers working harder or by employers adopting higher productivity techniques. Stigler suggested that if poverty was not already a sufficient incentive to work harder a higher wage would not be either. To the argument that the minimum wage would be incentive for lethargic employers, his response was that "this 'shock' theory is at present lacking in empirical evidence but not in popularity" (1946c: 359).

He was not content to make suggestions based on theory that certain effects would happen (loss of employment) or would not happen (adoption of more efficient techniques). He reported data on employment, average earnings, and wages as percent of value added for fourteen low-wage manufacturing industries. From these data he drew two generalizations, that the industries were competitive and that their labor costs were relatively high despite their being low-wage industries. From theory and the data he concluded that there was no need for additional incentive for improvement in efficiency from the minimum wage, i.e., the evidence refuted the applicability of the shock theory.

Then he considered the effects of a minimum wage if employers have monopsonistic power. In this situation the free market wage would be less than the value of marginal product and employment less than the efficient level. A properly calibrated minimum wage could move the wage to the value of marginal product and increase employment to the efficient level. Stigler acknowledged that in theory these effects were possible. But theory alone was not sufficient to support a policy. Quantitative knowledge of demand and supply relationships was necessary. The legal minimum might be set too high, in which case it would decrease employment, and the optimum wage would vary across occupations, firms, and even plants within a firm. Also, keeping the various minima at optimum would require adjustments as market conditions change. With his characteristic sharp humor Stigler suggested that there was no tolerably reliable method of estimating demand and supply schedules and "one is entitled to doubt that a legislative mandate is all that is necessary to bring forth such a method" (1946c: 361).

Stigler's analysis of the effect of the minimum wage on family income focused on the loose relationship between wage rates and family income and needs. The factors that made for looseness between income and wage rates were the unemployment effects on the less productive workers; the loose relationship between hourly and annual earnings; dispersion in the number of workers within families; and nonwage income. He gave data on nonwage income for Minnesota families in various income brackets. With regard to family needs he showed dispersion of family size, and thereby need, for income classes in data from Chicago and Atlanta. Once again pointing out the extreme difficulty of setting minimum wage rates appropriately, he concluded:

> Unless the minimum wage varies with the amount of employment, number of earners, non-wage income, family size, and many other factors, it will be an inept device for combatting poverty even for those who succeed in retaining employment. And if the minimum wage varies with all of these factors, it will be an insane device. (1946c: 363)

There were alternatives to the minimum wage for reduction in employer control over workers and wages and for elimination of poverty. To lessen employer control he proposed public provision of labor market information, vocational training, and loans to cover moving cost of workers. For what he took to be the more important goal of minimum wages, elimination of poverty, subsidies should be based on income and need as indicated by family size, without regard to the beneficiaries' type of work or industry. Stigler's ethical principal was that "those who are equally in need should be helped equally … There may be administrative justification (although I doubt it) for treating the farmer separately from the urban dweller, but there is no defense in equity for helping the one and neglecting the other" (1946c: 364). Stigler's suggested way of providing subsides was the same negative income tax proposal that Milton Friedman was to make years later.

> There is great attractiveness in the proposal that we extend the personal income tax to the lowest income brackets with negative rates in these brackets. Such a scheme could achieve equality of treatment with what appears to be a (large) minimum of administrative machinery. If the negative rates are appropriately graduated, we may still retain some measure of incentive for a family to increase its income. (1946c: 365)

Stigler reminded readers of the hazards posed by the administrative state for liberty in concluding comments on inadequate nourishment among the poor. Once again he highlighted evidence of a loose relationship between income and nutrition. Studies showed that in all income classes a portion of families have adequate diets and a portion do not.

> Either of two inferences may be drawn. The program of increasing income must be supplemented by a program of education – in diet, in housing, in education! Or the assistance to the poor should be given in kind, expertly chosen. The latter approach is administratively very complex, but quicker and in direct expenditure vastly more economical. These factors affect our choice, but a thought should be given also to the two societies to which they lead. (1946c: 365)

Richard Lester criticized Stigler's "strict application of pecuniary marginalism to wage-employment problems" in "Marginalism, Mimimum Wages, and Labor Markets" (1947). Stigler's reply was a defense of neoclassical price theory. This provides additional insight into Stigler's views of the nature and uses of price theory. The salient points were that theoretical analysis has two aspects, the formal, logical relationships and the empirical application of the theory's assumptions and inferences. A theory cannot be refuted by pointing out errors either in logic or empirical content in an application of the theory. To refute the theory it must be shown that after the theory's repair it is internally inconsistent, or inconsistent with empirical facts and thus without ability to forecast economic events.

Progress in Economics?

Stigler's continued interest in historical literature of economics and in the empirical relevance of theory is seen in his comment on the history of references to Giffen goods (1947b). For there to be the paradox of upward sloping demand curves there must be evidence of upward sloping curves. Stigler asked, What has the evidence been? Alfred Marshall had simply stated as a fact that:

> as Mr Giffen has pointed out, a rise in the price of bread makes so large a drain on the resources of the poorer labouring families and raises so much the marginal utility of money to them, that they are forced to curtail their consumption of meat and the more expensive farinaceous foods: and, bread being still the cheapest food which they can get and will take, they consume more, and not less of it. (*Principles*, 3rd ed., 1895: 208; 152 in Stigler 1947b)

Stigler found other references to the paradox as fact in Russell Rea, *Free Trade in Being* (1908) and by Simon Gray shortly after the Napoleonic Wars, a reference for which Stigler credits his teacher Jacob Viner. He reported that his "fairly extensive search has not uncovered any explicit statement of the phenomenon by [Sir Robert] Giffen, or even a hint of it" (1947b: 154). So Stigler looked himself for evidence of Giffen goods in British data from the turn of the century. Lacking ability to account for demand variables other than price and quantity he concluded that from the data "all that we may state is that the evidence does not confirm the paradox" (1947b: 155). Secondly, he used data on family income, expenditures and quantity purchased of bread and flour to check for negative income elasticity (a necessary condition for upward sloping demand curves). There he found evidence both of positive and negative elasticities. Stigler drew the conclusion that:

> We must all agree with Edgeworth [who expressed doubts] that experience and common sense are opposed to the idea of a positively sloping demand curve and that the burden of proof rests on the person who claims to have found a real example. Our investigation does not uncover

> any attempt at a systematic empirical demonstration of the validity of the example of wheat and casts some doubt on the possibility of making such a demonstration. We shall have to find a new example of the positively sloping demand curve or push our discussion of it deeper into footnotes. (1947b: 156)

Stigler's review of Howard Ellis's *A Survey of Contemporary Economics* (1949a) provides his assessment of the state of economics thirteen years into his career and sixteen years after he began graduate study of economics. Ellis's volume included thirteen essays on fields of economics that covered developments over the previous ten to fifteen years. This period matched Stigler's career to that point. He wrote detailed reviews of seven of the book's chapters and an evaluation of the collection as a whole. The five surveys closest to Stigler's own work were by Bernard Haley on value and distribution; two on monopoly by John Kenneth Galbraith and Joe Bain; Paul Samuelson on dynamics; and a survey of labor economics by Lloyd Reynolds. In addition to these he chose to review William Fellner's survey of employment and cycles and a chapter by Arthur Smithies on fiscal policy.

In the survey of value and distribution theory Bernard Haley gave pride of place to oligopoly theory, along with indifference-curve analysis, liquidity preference theory, and bilateral monopoly. Stigler's view was that little was truly original in the contributions Haley surveyed. Moreover, the most original work of the period, Von Neumann and Morgenstern's *Theory of Games*, was not included. Stigler judged that there was not much real progress, that what the literature included were innovations in technique and presentation without evidence of gains in confirmed knowledge of economic relationships. For instance, economists took two approaches to oligopoly. The first was to ask, "what would I do if I were an oligopolist?" The second was to seek a theory consistent with casual empirical evidence. So the kinked demand curve theory was based on the perception that "some oligopolists appear to change their prices infrequently." Innovations in interest theory were similar. Innovations in utility theory and bilateral monopoly were not even based on casual observation of behavior.

In response to William Fellner's praise for advances in the theory of employment based on *The General Theory*, Stigler asked what the content of that advance was, which Fellner had not identified. Neither had he identified empirical regularities discovered with the Keynesian apparatus or shown that it was more effective than the classical apparatus, i.e., the quantity theory. With regard to the business cycle, again Fellner gave no explicit demonstration of an increase in knowledge through the period surveyed.

Stigler found it curious that monopoly was the subject of two surveys and that it was included as if it was a separate field in economics. "Monopoly is not a branch of economics; its relationship to industrial organization is similar to that of runaway inflations to monetary theory" (1949a: 95). But given its prominence in the literature on industrial organization Stigler could understand monopoly being included in the survey (once). He thought Galbraith failed to see the helpfulness of theory for understanding monopoly and antitrust. He zeroed in on the non-systematic character and missing empirical analysis of market structure, behavior, and antitrust in Galbraith's chapter. "He [Galbraith] possess a view of industrial organization that seems to me independent of the empirical studies which he cites; I have used the same studies, and in more detail, to derive the opposite view of our industrial organization" (1949a: 96).[20] Joe Bain agreed with Galbraith that oligopoly structure was prevalent and the theory of imperfect competition not useful. Bain's focus was on the deficiencies of theory and his preference for detailed empirical industry studies. Stigler's response was to question three of Bain's complaints about the theory. These were that anticipated demand and cost, with which theory deals, cannot be observed; that theory deals only with price and quantity; and that a realistic theory would be unworkably complicated.

Stigler's intellectual life in the "extended present," referred to earlier, as well as his wit are front and center in his critique of Arthur Smithies's account of advances in fiscal theory. He set up an imagined conversation in which Henry Thornton questions Arthur Smithies about new knowledge gleaned from use of Keynesian theory that was being applied in policy. Stigler took Smithies's replies from his survey chapter.

Thornton: Does it not make a difference who pays the taxes and who receives the payments?

Smithies: "The statisticians, despite their disagreements on the subject, all seem prepared to relate aggregate consumption to aggregate income without taking into account changes in the income distribution."

Thornton: And is it your position that the extent and nature of public expenditures do not affect private expenditures – for example, that housing subsidies do not affect private expenditures on housing?

Smithies: "The same statistical argument we used above indicates that these effects have not been important so far."

Thornton: What of the effects of public investment on private investment, the effects of relative prices on private investment, the effects of expectations of inflation?

Smithies: "Despite the foregoing qualifications, the propositions we have stated above furnish a guide to the directions policy might take."

Thornton: Which no doubt implies that the statisticians – a very useful group, I must say – have found that the qualifications can be ignored or incorporated into the equations without depriving them of empirical content. Tell me, when you imply that these qualifications are unimportant or manageable, this must mean that the predictions of this theory have been successful, does it not?

Smithies: [silence]

Thornton: Then how do you know that these qualifications and complications – and a hundred more unmentioned – do not decisively vitiate you conclusions and recommendations of policy? (1949a: 97).

On Lloyd Reynolds's survey of labor economics the question of the role to theory was uppermost for Stigler. There was a presumption that economic theory was of limited use for understanding labor markets and issues. To which Stigler made a general methodological point:

> The labor economist can properly reject the received theory without proposing alternative generalizations only if the received theory is useless. In the present case this is wholly untenable. The marginal-productivity theory can explain and predict a large number of important phenomena: why real wages are higher in America than in China; why the average age of carpenters is high; why doctors earn more than dentists; why the salaries of college professors are more equal than the earnings of lawyers;

> ad infinitum. To ask us to abandon this useful theory without giving us something better is a most unreasonable request, and it gains no reasonableness from a demonstration that the theory is incomplete. (1949a: 98)

Samuelson's survey of dynamics was in Stigler's mind a survey of technique rather than substance. Dynamics is, quoting Samuelson, "functional relationships between economic variables and their rates of change, their 'velocities,' 'accelerations,' or higher 'derivatives of derivatives'" (354 in Samuelson, 99 in Stigler 1949a). According to Stigler, this definition of dynamics as mathematical technique rather than economic substance leads to quirky classifications. For example, J.M. Clark's "Business Acceleration and the Law of Demand" (1917) was not dynamics but Ragnar Frisch's comment on this article, "The Interrelation between Capital Production and Consumer Taking" (1931) was dynamics. Stigler's preference was for the conventional definition of dynamics as "the study of economic processes that undergo important change through time" (1949a: 99). By the conventional definition, outstanding work in dynamics since 1930 included Arthur Burns's *Production Trends,* Joseph Schumpeter's *Business Cycles*, and Alfred Hansen's work on stagnation theory. "On Samuelson's definition, I suppose, one writes an essay on mathematics; on the conventional definition, one writes an essay on economics" (1949a: 100).

Stigler pointed out economists whose work was missing from Ellis's volume. These included institutionalists (Veblen, Commons, Hamilton), "all the luminaries of the Chicago school" (Knight, Simons, Viner) and, except for Simon Kuznets, leaders of the school emphasizing empirical work (Mitchell, Arthur F. Burns, Colin Clark). Stigler surmised that the age of contributors to the surveys was reflected in the content of their essays. Interest and knowledge of the history of economics was on the wane so there was little historical benchmark for evaluating contemporary theory, and a bias toward exaggeration of recent advances.

> The plain fact is that the economic theory of 1948 is much the same as that of 1930 – a little better here and a little worse there. If the contributors had taken Ellis' 'main idea' and 'fifteen years' at all literally, the

> volume would have run to fewer pages. Whether more progress should have been made in economic theory in fifteen years, during one-third of which most economists had to abandon scientific work, is a question to which I shall return. (1949a: 101–2)

Related to this was the preponderance of statistics over history for empirical knowledge. Here also Stigler thought the authors exaggerated progress. The shift from history to statistics would be less of a loss if statistical series reached back further into the past, or if statistical analysis was pursued with more vigor.

> The dominant characteristic of the period is the reliance upon casual empirical information. The fact that the *Survey* proved to be a survey of formal theory is ample proof of this. … The period as a whole was distinguished by the ease and rapidity with which matters of fact were decided. …
>
> The tedious and difficult work of testing theories by a comparison of their predictions with evidence not incorporated in the formulation of the theory – that is, by noncircular tests – and the careful interpretation and generalization of inductive studies have been deemed unnecessarily circuitous paths to knowledge and to social welfare. The short cuts, unfortunately, did not take us to where we want to go. That is why each was so short and yet the ensemble so long a detour. (1949a: 103–4)

Stigler compared the *Survey* to Tugwell's *The Trend of Economics* (1924). This earlier survey marked the high point of younger economists dissenting against traditional [neoclassical] economics. Stigler speculated that the *Survey* might mark the high point of abstract theorizing.

> The basic issue in advancing economics is not directly whether we are theoretical or empirical. … Our need is for more reliable results, not for a different type of results. The issue is not whether as individuals we work with large questions or small, with equations or statistics, but whether we shall accept only those results for which the evidence is compelling. (1949a: 105)

Applied Price Theory: Industrial Organization and Antitrust

We have a good look at how Stigler thought he contributed to progress in economic analysis in "A Theory of Delivered Price Systems" (JPE, Dec 1949c). Basing point pricing was practiced in construction supply industries and had been debated by lawyers and economists since the 1920s. The practice was to charge prices for delivered products where the f.o.b. price plus delivery add-on did not match the actual delivery cost. The delivery cost portion of the price was set from a production point that was not where the product being delivered was actually produced. The production point from which delivery charges were based was the pricing base, thus the term basing point pricing. Stigler's article followed by a year the U.S. Supreme Court decision in *Federal Trade Commission v. Cement Institute* (1948) outlawing this practice. Stigler noted that the facts were not in dispute. The dispute was over their interpretation. On one side were economists such as J.M. Clark and Melvin de Chazeau arguing that this pricing was the product of competitive evolution of modern oligopolistic industry. On the other side were economists such as Frank Fetter and Vernon Mund, who interpreted basing point pricing as a technique of collusion.

Stigler sought a theoretical explanation of the pricing practice that was based on maximization of profit and known facts. With a hypothetical set of data for two firms with intersecting market areas he illustrated the logic of basing point pricing. He defined "freight absorption" as pricing such that at the consumption point where a firm and its rivals are selling, the firm's mill-net price, i.e., price received net of actual delivery costs, varied with distance of production point to consumption point. Under competition a firm would not sell in a distant area if the mill-net price was below the mill-net price in a less distant area. So for example, if Firm I and Firm II both have f.o.b. price of \$50 and for a particular consumption point Firm I has delivery cost of \$7 and Firm II of \$3, Firm II will have a break-even delivered price of \$53. Firm I will not try to sell at this consumption point. They would have a loss of \$4 (cost = \$50 + \$7). By the same logic, if a monopolist owned Plant I and Plant II, he would not ship from Plant I to this consumption point.

The assumptions of Stigler's theory were that: (1) transportation costs are substantial relative to f.o.b. price, (2) there are few large firms at each production center, (3) demand for the product is geographically unstable, and (4) the firms wish to collude. The firms' problems are to divide sales among firms at each production center and to divide sales among production centers. If the firms could fully collude they could use a joint sales agency to allocate sales and provide compensation to firms with low sales. But this was illegal. However:

> systematic freight absorption provides a satisfactory solution…. There is a single price at each point in the market (if transportation charges are agreed upon), so price rivalry is eliminated. One production center can sell in the 'natural' territories of other production centers when this is necessary to obtain its share of the industry's sales; these distant sales involve freight absorption, moreover, and are therefore partly self-limiting. The various prices need not change often, so collusion is possible. Given unstable geographical pattern of demand and the antitrust laws, systematic freight absorption permitted efficient collusion. (1949c: 1148–49)

Stigler "tested" his theory with data that in his words "create some presumption for its validity." He also suggested additional tests that others might carry out. The first two data sets showed geographical instability of demand for reinforcing steel bars. This bore on the difficulty of collusion on f.o.b. prices, which the data showed would need frequent adjustment. The next data set compared the relative stability of f.o.b. prices and delivered prices. Delivered prices were more stable. The third test was to show that freight absorption declined in periods of relatively stable demand. He also showed that consumption varied more than production in four states that represented production and consumption centers. The bottom line for Stigler from theory and supporting data was that collusion was required to sustain a system of delivered pricing in the face of geographically unstable demand.

At the 1949 AEA meeting Stigler joined Joe Bain, Tibor Scitovsky, and William Fellner in a session on the theory of oligopoly. Stigler's paper was "Monopoly and Oligopoly by Merger" (1950a). He showed that even under unfavorable conditions of free entry it can be profitable for firms to merge for monopoly. Assuming free but not costless entry, constant cost, stable demand, and indestructible fixed factors of production, merging of firms heretofore in competitive equilibrium leads to monopoly profits. Entry reduces the price and diminishes profits. Because of indestructible fixed factors, i.e., factors that cannot be withdrawn, in long-run equilibrium the merged firm will have permanent losses. Nonetheless even though monopoly will be unsustainable, merger for monopoly may occur. This would be because the present value of the temporary monopoly profit may exceed the present value of the permanent losses.

This paper was in part Stigler's reaction to the presumption that economies of scale were prevalent and were the common motivation for mergers. Stigler argued that the only reliable test for economies of scale was firms' survivability. Studies of per unit cost and rates of return were unreliable due to problems in measuring capital values. If, however, firms of different sizes survived, this was direct evidence that economies of scale were not important within the range of their sizes. He judged that across American industry economies of scale were not important and that entry was relatively free in the sense that long-run cost of newly entered firms was no higher than those of existing firms.

This discussion of prospects of merger for monopoly set up Stigler's survey of the history of mergers in the United States from the 1880s. He viewed the period of merger for monopoly as roughly from 1880 until the Supreme Court's *Northern Securities* case in 1904. The conditions that allowed this were development of corporate law with limited liability and of modern capital markets. Merger was preferred to collusion because the latter was prima facie illegal after 1890. Stigler wrote: "I am inclined to place considerable weight upon one other advantage of merger: it permitted a capitalization of prospective monopoly profits and a distribution of a portion of these capitalized profits to the professional promoter. The merger enabled a Morgan or a Moore to enter a new and lucrative industry: the production of monopolies" (1950a: 30). Economists of the period failed their public according to Stigler:

> It is sobering to reflect on the attitudes of professional economists of the period toward the merger movement. Economists as wise as Taussig, as

> incisive as Fisher, as fond of competition as Clark and Fetter, insisted upon discussing the movement largely or exclusively in terms of industrial evolution and the economies of scale. They found no difficulty in treating the unregulated corporation as a natural phenomenon, nor were they bothered that the economies of scale should spring forth suddenly and simultaneously in an enormous variety of industries – and yet pass over the minor firms that characteristically persisted and indeed flourished in these industries. One must regretfully record that in this period Ida Tarbell and Henry Demarest Lloyd did more than the American Economic Association to foster the policy of competition. (1950a: 30–31).

Since 1900 a number of American industries had become less concentrated and mergers typically produced oligopoly rather than monopoly. Stigler judged that though less severe than outright monopoly, oligopoly was still a problem of which jurists were insufficiently concerned. He came to what he termed "inconclusive conclusions." These were that diseconomies of scale were largely absent as a barrier to merger for monopoly, that the Sherman Act was an effective barrier in raising the expected risk of merging for monopoly, also that improvements in capital markets had worked against competitive markets, and that new entry was the primary defense of competition.

In April 1950 Stigler testified in hearings on the steel industry by the Subcommittee on the Study of Monopoly Power of the House Committee on the Judiciary. He opened by warning against expecting to find instances of pure competition or pure monopoly. In the American steel industry there were significant elements of competition. But the question was whether this industry was sufficiently competitive to dispense with social controls. His answer to this question was that the industry was not sufficiently competitive. His evidence was:

1. Use of basing point pricing (which had been ruled collusive for the cement industry).
   Under this system, which has dominated steel pricing during the life of United States Steel, the following behavior was prescribed. One steel producer in the Chicago area would absorb large freight charges to sell near the Pittsburgh market, but this Chicago producer would

not reduce his mill net price by a penny to take Chicago customers away from another Chicago producer. No amount of terminological dexterity can disguise the essentially collusive nature of this system of pricing (1950b: 117).

2. Price rigidity, with prices being held up in depression and held down in inflation with the resulting excess supply and excess demand for individual firms. He quoted testimony of a vice president of U.S. Steel to the committee on the infrequency of price changes.
3. Uniformity of sealed bids for U.S. Government contracts, price discrimination against foreign customers, and the absence of public complaints from either rivals or customers of steel companies.

   The Supreme Court once thought this was a cause for admiring United States Steel (United States v. U.S. Steel Corporation, 251 U.S. 447–49); to me it is a sinister manifestation of pervasive strength. In any free area there is dissatisfaction; there are dissatisfied Macy customers; there are dissatisfied students of professors; there are dissatisfied constituents of Congressmen. I suggest that when the steel industry testifies, one of its minor tasks is to provide a long list of dissatisfied customers if it wishes to dispel the suspicion of widespread fear (1950b: 118).

Stigler recommended corrective policy on three fronts: (1) continued pursuit of antitrust detection and prosecution for practices such as basing point pricing, (2) supervision and regulation to prevent abuse of monopoly power, and (3) dissolving the largest firms. The third policy was the most important. He recommended breaking up U.S. Steel, and perhaps Bethlehem and Republic Steel, into firms that produced no more than ten percent of the ore, ingots, or major fabricated products. He thought the first strategy would be insufficient in an industry with dominant firms. On the other hand regulation of price was fraught with difficulties. "Experience with public utilities tells us that the already overrigid steel price structure would become still more rigid" (1950b: 119). Detailed regulation would reduce the competition that remains in the industry and retard innovation in technique and product. "I believe that, instead of adding to our regulated industries, we should make a serious effort to restore some regulated industries such as motortrucking to private, competitive enterprise, where they belong" (1950b: 119). Breaking up the large steel firms:

> seems vastly superior to both the existing organization, and to detailed regulation. The industry will not be able to agree on or enforce strongly monopolistic pricing policies. Nor will it be necessary to examine every transaction for hidden motives and effects, or to haul the industry before a congressional committee every time prices are changed, or to embark on the stagnating policy of detailed control. (1950b: 119)

Stigler continued by explaining his view that the purpose of antitrust policy was, in addition to protecting consumers from exploitation by monopolists, to protect business from detailed regulation. "If the smaller companies in industries like steel learn this lesson, we will have fewer 'united fronts' being presented against antitrust actions. Our antitrust policy is designed to harass not our private business community but the individuals who object to private enterprise and wish to be rid of it" (1950b: 120).

Stigler testified again, as the final witness before the hearing, on May 11, 1950. He opened with his surmise that the purpose of the hearings was broader than the steel industry and was concerned with deficiencies in antitrust law in light of current industrial structures. His remarks centered on antitrust law, size of firms, and the type of policy he thought appropriate in the circumstances. Antitrust law was designed for two problems, conspiracies and trusts. In part because of success with trust-busting a new problem had arisen, oligopoly, with few firms or dominant firms. He regarded policies based on the Sherman Act as inadequate to the task of dealing with tacit collusion. "A conviction often leads to a court order to the oligopolists to forget that they are oligopolists" (1950b: 992). They will neither be able to ignore what their rivals are doing or to not know that monopolization is more profitable than competition.

He explained aspects of the oligopolistic structure of the steel industry such as allocation of market share to imports (as required by the Webb-Pomerene Act) and allocation of steel ingots to subsidiaries and independent fabricators. He concluded:

> One and only one policy will deal effectively with such oligopolistic industries: the policy of increasing the number of firms and eliminating the dominance of one or a few corporations. As the number of independent companies increases, and the size of the largest companies decreases, the kind of tacit collusion I have been describing loses scope and power. …Since there are a considerable number of firms, none dominant in size, collusion cannot be tacit – based upon infrequent meetings and informal understandings – and must become overt, formal, and continuous. And this is a kind of collusion with which the Sherman Act deals effectively. (1950b: 994)

Stigler concluded his prepared remarks by placing his recommendations on antitrust policy in a political economy framework:

> My own thinking on the oligopoly problem is partly political. I state it explicitly, not because I am an expert in politics, but for the opposite reason. I am not an expert, and the members of this committee are – I say this with envy – so they may properly appraise it. The steel industry is no longer a very private industry. It must defend every price increase in Congress; it must have a Presidential board in labor disputes. … As I see it, the question is not whether something will be done with industries like steel, but what and when. The attitude of the leading firms seems to be that it doesn't matter much what is done, provided it is not done now. …
>
> This leads me to my final observation. Who are the radicals and who are the conservatives in the field of monopoly policy? Perhaps this is not a very important question, because one of the most striking of all American traits is the desire to improve on the past. I will not pass over in silence, however, the proposition so frequently heard in recent weeks that those who believe in competitive private enterprise are wild radicals and those who believe in gentlemanly cartels are admirable progressives. The traditional American policy is that of fostering competition. It is based on the sound historical judgment that our progress and our prosperity – and I think also our liberties – depend upon the free play of the ingenuity and resourcefulness of many men, not on the benevolence and wisdom of a handful either of corporate presidents or public officials. Competition cannot be painted as a radical utopian proposal of economic theorists – who, by the way, are much better at theorizing on economic problems than businessmen. Competition was the policy of England during

> its majestic nineteenth century, and it has been fundamental policy of the United States for two centuries of unmatched growth. Neither the old-fashioned monopolists nor the new-fashioned oligopolists are going to be able to make us forget this. (1950b: 998)

Stigler returned to mergers and antitrust after the 1952 amendment of Clayton Act and conclusion of the Attorney General's National Committee to Study the Antitrust Laws, of which he was a member. In "Mergers and Preventive Antitrust Policy" (1955) he made a distinction between preventive and corrective antitrust policy, noting that U.S. policy was almost all corrective, designed to eliminate existing monopoly power or curb its use. Section 7 of the Clayton Act, which concerned mergers, authorized antitrust policy intended to prevent behavior that would lead to monopoly. In Stigler's view the only justification for antitrust policy of either type was belief in the desirability and feasibility of a competitive economy. Belief in its feasibility implied belief that the underlying forces in a market economy were competitive, so that a modicum of antitrust activity was sufficient to preserve competition. This implied that preventive antitrust could be and should be targeted narrowly and with care. One category of business behavior stood out as conducive to monopoly, and that was mergers.

> By definition, they involve at least a temporary reduction in the number of independent enterprises in the industry. This is, indeed, their chief purpose; historically the other purposes of mergers (of large firms) have been incidental and unimportant. The control of mergers since 1890 would have given us an industrial structure substantially less concentrated than that we now possess. (1955: 177)

Stigler viewed cooperation between firms as an incomplete form of merger, but there were legitimate and illegitimate ways that firms might cooperate with one another. This meant that there could not be a fixed rule against cooperation. When Stigler wrote this article, Section 7 of the Clayton Act was untested, and the Attorney General's Committee had failed to lay out guidelines for policy. So Stigler laid them out. The relevant mergers, according to Stigler, were horizontal mergers. Vertical mergers posed no monopoly problem in themselves, only if there was monopoly in one of the stages of production of the merged firm. So this problem amounted to horizontal monopoly power.

Some single mergers would reduce competition below an acceptable level. Others would not, but a succession of mergers could. So the enforcement question was how to detect the mergers that should be prohibited. There were two approaches for this. The legal criterion would base the decision on a proposed merger on the past antitrust record of the firms. For instance, a merger of firms with a record of conspiracies or attempted conspiracies would not be allowed. The economic approach would consider the consequences for competition of allowing the proposed merger and others to proceed. Here there could not be any clear and certain lines. The competitive nature of the industry would depend on the power of the merged firm relative to existing firms and on potential for new entry. Stigler suggested a rough consensus of economists that if the leading firm had less than 10 percent market share the industry is competitive. If it had 40–50 percent or if two to five firms had 75 percent the industry is monopolized. In light of the uncertainties and risks on both sides of too lenient and too strict a policy, Stigler suggested the following:

1. There should be a presumption that every firm with less than five to ten per cent of an industry's output (after merger) may engage in the merger. Within this range, the percentage should be lower, the larger the industry.
2. Every merger by a firm which possess one-fifth or more of an industry's output after the merger shall be presumed to violate the statute.
3. In the situations that lie between these limits, the merger should be investigated by the enforcement agencies if the aggregate annual sales of the merging firms will exceed some absolute level—say five million dollars—after merger (1955: 182).

In closing he returned to the need for an effective merger policy, suggesting that conservatively half the major cases brought under the Sherman Act would have been obviated had there been such a policy.

In November 1955 the University of Chicago celebrated the 25th anniversary of the Social Sciences Building with a conference on the state of the social sciences. Stigler presented a paper on "Industrial Organization and Economic Progress" (1956) that focused on monopoly and competition in economic theory and their connections with economic growth. He began with Adam Smith, whose idea of monopoly was restricted to enterprises with restrictive charters. Monopoly was a privilege granted by the state. Competition was the processes that prevailed unless thwarted by government. Granted for the long-term, monopoly privileges protected companies from pressures of competition, allowing them "'to support the negligence, profusion, and malversasion' of companys' employees" (1956: 269, quoted from AS, Wealth, Modern Lib ed., 712). Competition promoted efficiency and progress.

Beginning in the 1870s neoclassical economists refined the definition of competition into what we know as "perfect competition." Refinement reached its zenith in Frank Knight's *Risk, Uncertainty, and Profit* (1921a).

> It was now essentially a concept of the stationary economy: competition could not be unique in its workings if men did not know the consequences of their actions, and they could not know these if unpredictable change – which means most kinds of change – were present. In Knight's famous list of the conditions of perfect competition, therefore, two of the conditions served to specify stationary conditions – fixed flows of resources and consumer demands and fixed methods of organization and techniques. (1956: 270)

Stigler pointed out that with competition conceived as perfect competition in a stationary economy, it was incompatible with economic change and growth. Joseph Schumpeter (*Capitalism, Socialism, and Democracy*) drew the conclusion that monopoly was the engine of economic change and growth. He acknowledged the allocative inefficiencies from monopoly but argued that they would be more than compensated for by progress that monopoly would allow by giving innovators space to recoup their uncertain investments. Capitalism's dynamism was the product of the creation and destruction of monopolies.

Stigler thought Schumpeter's conclusion came from an overly restrictive definition of competition—Knight's perfect competition. Thus Schumpeter considered every departure from perfect competition monopoly. "If we are to escape from such vacuous terminological circularity, it is necessary to contrive a definition of competition for the changing economy" (1956: 272). Stigler's definition was:

> An industry is competitive if, once it is established, it meets two conditions: (i) no individual (and independently acting) firm can appreciably influence prices in the long run; (ii) there is no contrived barrier which prevents the entrepreneur from operating in the industry and at the output he wishes.
>
> An industry becomes established once its prospective costs and demands have been ascertained with a fair degree of confidence. (1956: 272–73)

Stigler highlighted two differences between his definition and perfect competition. High elasticities of demand and supply apply only in the long run and expectations of the future are not all realized. He gave examples of new industries that fit his definition, but were not either perfectly competitive or monopolies as conventionally defined. These were frozen foods that developed from Clarence Birdseye's quick-freeze innovation in 1924 and general merchandizing begun by Aaron Montgomery Ward in the 1870s and Richard Sears in the 1880s.

With Stigler's definition, provision of information was one of the key functions of the entrepreneur. Competition rather than monopoly was the engine of economic progress. The methodological advantage of his definition was that the definition of competition did not predetermine the effects of competition. This left room for empirical evidence to influence the conclusion. Stigler's evidence of the competitiveness of industries was both general ("broad facts") and numerical, but not econometric. He did not contend that his evidence was definitive, but that it was better than nothing. "Still, in an area where prejudice and wisdom are distinguishable chiefly by the parentage of thoughts, even a meager beginning may have some interest" (1956: 276).

For example, he gave a list of industries that he said for a time had a single or two firms because of patents: aluminum, nylon, movie cameras, rayon, electrical business machines, zipper fasteners, and safety razors. Others without patents, such as automobiles, frozen foods, and petroleum refining soon had numerous firms. Stigler gave numerical evidence of technological progress—changes in labor requirements per unit of output over 38 years (1899–1937). He also gave concentration ratios for twenty-nine industries. Because the number of firms varied from the earlier years, rather than correlating the two measures he put them into three concentration classes: (1) high concentration throughout the period, (2) declining concentration through the period, and (3) low concentration throughout the period. The summarized data showed that the greatest decline in labor requirements, i.e., progress, was in the industries with declining concentration. This suggested "that it is the competition of new rivals within an industry, not the competition of new industries, that is associated with rapid technological progress, and it hints that industries with lower concentration had higher rates of technological progress" (1956: 278).

The word "hint" in this concluding statement is one of several ways that Stigler indicated his conclusions were tentative. He referred to his "slight study" and anticipated more formal and comprehensive tests of his hypothesis. But he concluded that in the meantime "there is no prima facie contradiction of the classical view of the positive relationship between competition and progress or, indeed, as much support for the contrary view as the devil usually provides for clever heresy" (1956: 278–79).[21]

Comparative Economic Systems

In February 1958 Stigler delivered the first Henry Simons Lecture at the University of Chicago Law School, on "The Goals of Economic Policy" (July 1958b). His topic was "the proper goals of economic policy," but he began with an account of the actual goals in Western European and American history. These were maximum output of goods and services, which had come to be identified with full employment and efficient production, and economic growth. To these was added in recent times equality of income. Virtually every economic policy was geared to one of these goals. Echoing his teacher Frank Knight, Stigler suggested that although these goals were currently well met in America, they would not be considered well met for long because once a standard is reasonably well met the bar is raised. This did not bother Stigler because he thought that a goal that was precisely defined and achievable was a poor goal. Goals should be beyond reach.

But Stigler did see a problem with American goals for economic policy. They were no different from the economic policy goals of the Soviet Union. Presumably American civilization and Soviet civilization were different. The goals of economic policy should reflect this difference. "One would expect two great powers to have carried into their economic goals some elements of the political philosophies that lead to their antipathy and rivalry. … [In foreign policy] we offer the same goals, and differ chiefly in promising less with respect to their fulfillment" (1958b: 172). In the late 1950s it was presumed that the Soviets were achieving economic goals as well or better than the United States. Stigler's concern, however, was not with American rivalry with the Soviets around the world. "Even if the United States were the only body of land on earth or in space, we should urgently need to give direction and emphasis to our economic policies. It is high time that we set aside the details of managing a comfortable dormitory and concern ourselves with the kind of society we wish to inhabit" (1958b: 172).

To Stigler the supreme goal of the West was development of the individual, with a maximum of personal freedom and corresponding responsibility. This was, he said, our concept of a humane society. "The 'cult of individualism' for every man, if you will – is the very foundation of democracy, of freedom of speech, of every institution that recognizes the dignity of man. I view this goal as an ultimate ethical value; others may wish to reach it through powerful utilitarian arguments" (1958b: 172). Stigler thought faith in the individual was in decline, and this from three sources. One was the relatively new doctrine of environmental determinism, an instance of which was Marxism. The second was the return, after the nineteenth-century expectation of benefits from universal education, to an old belief that most people are unable to manage their lives. This showed up in economics in the belief that advertising gave business control over consumers. The third development was the increased complexity and interdependence of urban industrial society. Stigler observed that while social and economic changes raise real questions about individuals' abilities to manage their affairs and to do so without harm to others, the same changes create new opportunities for free individuals and their communities. As freedoms and controls are revised it is important to remember that "men are not mere social animals, to be governed into prosperity or tranquilized into non-unhappiness" (1958b: 174).

Stigler regarded the first two goals of economic policy, maximum output and efficiency, as ethically neutral except for the presumption that the products should be those chosen by free men. He judged that to the extent that desire for income equality was a humanitarian concern for the welfare of the poor, this was a goal that all shared. Debate was chiefly over means. Beyond this, in light of the amount and type of income inequality in the United States, Stigler suggested that the goal of minimum inequality carried dangers for individual freedom. A decline in the value placed on freedom was also evident in policy regarding monopoly and competition. Giant enterprises were coming to be seen as the engines of progress. The federal government's record was mixed—"spiritless action against the more blatant forms of conspiracy and monopolization. While the federal government has been opening up these back lots to individual freedom, it has quietly been erecting barriers to individual action through the prairies of economic life, with its paternalistic small-business programs and the regulation of competitive industries such as agriculture, motor trucking, and housing" (1958b: 175). He suggested that programs to enhance mobility should replace programs that prop up failing industries. Economic and social policies should be revisited with the goal of individual freedom and responsibility as a guide-post.

Three Retrospectives

This review of George Stigler's work in price theory has covered the period from the beginning of his career as a graduate student in 1933 into the late 1950s. In 1958 he moved from Columbia to the University of Chicago. By then Stigler was a mature scholar with the contours of his research program well set. We will conclude by jumping ahead two decades to the early 1980s to consider three articles in which Stigler looked retrospectively at the history of economics and at his role in this history. The retrospectives provide benchmarks for our account of the development of his scholarly program. The articles are Stigler's G. Warren Nutter Memorial Lecture on "Economists and Public Policy" (1982a), his Richard T. Ely Lecture on "Economists and the Problem of Monopoly" (1982b), and his Nobel Prize Lecture, "The Process and Progress of Economics" (1983).

Stigler put himself and his economist colleagues under the microscope in the Nutter Lecture. What he saw was inconsistency borne of an urge to reform society. The inconsistency was in treating people in their activities as consumers and producers as rational maximizers, but in public affairs as irrationally ignorant. He recanted in part the thesis of his 1965 article, "The Economist and the State." The part he did not recant was that economists have "a deplorable habit of giving emphatic advice on public policy without bothering—even if they live long after—to see whether their predictions of the effects of the policy were correct" (1982a: 13). He recanted the second part of the thesis, that when economists did make a practice of testing theories for sound policy advice, the public would act on their advice. Like others, Stigler had presumed that the public did not know the effects of policies. But what Stigler actually did was not so much recant this thesis as modify it. He modified it to bring it in into conformity with the "Chicago Credo." According to Stigler the credo is that people act efficiently in their own interests—consumers, producers, union officials, voters, officials, and clients of government.

Stigler continued to believe that truth eventually wins out over error. If this is so, then the public's presumed imperviousness to sound advice from economists on policies such as minimum wage law and free trade needed to be explained. Economists' sound advice is usually grounded in elementary principles, not complex theory. It is not difficult to understand, and economists have given the same advice over and over. So is it true that the public does not understand? Perhaps, Stigler suggested, people do understand the economics of these policies. Perhaps they base their support for policies that economists abhor on sound knowledge of their effects. Perhaps interest groups even understand the effects of some policies before economists do. Perhaps they have different goals; members of the public may not share economists' concern with efficiency across the economy. Economists have been blind to the fact that interest groups effectively pursue their own interests, and do not need instruction from economists on what their interests are and how to pursue them.

Stigler acknowledged that his critique presumed that economists were irrationally ignorant, so he was open to the same Chicago Credo critique. Do economists who give public policy advice not act efficiently in their own interests? So he concluded on what he suggested was a "truly old fashioned note." Perhaps his criticism of economists was unfair, because "I must assume that these reformers are employed by sensible, informed people, and they must be providing useful services" (1982a: 17).

Stigler used his 1981 Ely Lecture to reflect on the history of economists' views on monopoly—what the views were, how they changed, why they changed, and what effect economists had on policy. He began with Adam Smith and the classical economists, who did not recognize monopoly apart from monopolies with government sanction such as the British East India Company. The implication of this view was that antimonopoly policy need to be nothing more than laissez faire. When they considered collusion, classical economists expected it to be unstable. In the final third of the nineteenth-century English language economists developed interest in economies of scale and oligopoly. Their interest arose with development of railroads and public utilities, and from new access to the work of Continental economists such as Cournot. Yet, even after the Sherman Act economists were little interested in antitrust. Their presumption was that large scale organizations were inevitable in a modern economy. They provided efficiencies of size, so breaking them up was unwise.

By the 1950s economists had become more favorable toward antitrust. [22] Stigler posed three questions: (1) why did economists' attitudes change, (2) what has been the effect of economics on antitrust, and (3) what has been the effect of antitrust policy on economics? For the first question he ruled out a change of opinion based on scientific evidence of the effectiveness of the Sherman and Clayton Acts. There was, he thought, no such body of evidence.[23] He regarded the shift in antitrust enforcement away from trust busting to preventing collusion as a reason economists came to favor antitrust. The presumed commonplace economies of scale were not a factor in collusion cases. And economists had long opposed cartels. A second reason for the more favorable view of antitrust was the opportunities it presented for regulation and control. Economists' taste for these did not abate despite failures of regulatory policies. Along the same self-interest lines, antitrust cases were a lucrative source of income for economists.

Stigler believed economists had little influence on antitrust policy. This was because they did not have a body of tested scientific knowledge of its effects. "The real reason for doubt is that no economist had any professional knowledge on which to base recommendations that should carry weight with a skeptical legislator" (1982b: 6). The "evidence" that did carry weight was from muckrakers. Stigler thought lawyers held more sway than economists in the Antitrust Division of the Department of Justice and the Federal Trade Commission.

> I freely grant that our economic analysis is better than J.B. Clark's. I hope Professor Clark agrees. If we have improved, our influence should be somewhat greater than it once was but that does not mean that it should be large. We need to be humble in a day when the greatest function of the antitrust laws appears to be to arm the defenses of the corporate officials who, when a takeover proposal is made, seek to maintain their tenure against the avarice of their stockholders. (1982b: 8)

Stigler's final question was what effects monopoly policy had on economics. On the basis of topics of journal articles, he saw less interest in monopoly and regulation than there had been in the early twentieth century. More curious was the paucity of interest in foundational questions for antitrust policy, such as the size of markets. The size of markets was a regular point of dispute in antitrust cases. Stigler suggested that the typical antitrust case was an "almost impudent exercise in economic gerrymandering. … My lament is that this battle on market definitions, which is fought thousands of times what with all the private antitrust suits, has received virtually no attention from us economists" (1982b: 8–9).

The sole bright point for Stigler was technical improvements in price theory, particularly in the theory of imperfect competition and oligopoly. Economists had better understanding and appreciation of competitive markets than they had in the past. Stigler concluded with reference to his article "The Politics of Political Economists" (1959) in which his thesis was that professional economists were more favorable than other academics toward markets. "We have trouble enough showing how economics influences our society, so it is of some consolation to assert that it influences us!" (1982b: 18).

Stigler's Nobel lecture, "The Process and Progress of Economics" (1983), continued the history and sociology of economics theme in a general way. How are new ideas introduced and accepted as part of a developing body of scientific knowledge? The science of economics required a community of economic scientists working on a common set of problems. Adam Smith was the key transitional figure, for his *Wealth of Nations* was "so broad and authoritative an account of the known economic doctrine that henceforth it was no longer permissible for any subsequent writer on economics to advance his own ideas while ignoring the state of general knowledge [as Smith had done]" (1983: 532). Soon after Smith, no economist could expect that his errors would not be exposed by others. There was a scientific community of economists.

So how did this scientific community operate? Stigler rejected the "environmental view" associated with institutionalism, that economic ideas are specific to the problems of particular places and times with no enduring doctrine. Yet economic doctrine does not develop independently of practical problems, for its central purpose is to help deal with problems of the real world. Thus, for there to be a body of scientific doctrine that grows over time and is widely applicable to practical problems there must be "a set of fundamental and durable problems" (1983: 533). These, according to Stigler, are the problems of determination of value. The theory of value is the scientific doctrine of economics. "Without the base of persistent theory, there would be no body of slowly evolving knowledge to constitute a science. Without the challenges of unsolved, important problems, the science would become sterile" (1983: 535). Yet the evolution of doctrine is a messy affair, for there are no "omniscient scholars." Stigler viewed economic scientists much like entrepreneurs. In the speculative search for new ideas "the [economic] scientist is stumbling about in a jungle of ideas or facts that seem to defy system or logic, and usually he fails to emerge with anything but scratches" (1983: 543–44).

How are new ideas accepted? On Stigler's reading of history, sometimes they are accepted readily and without struggle. This is the case when an innovator opens the door to a set of interesting problems with potential for application of the new idea. Such was the case with his article "The Economics of Information" (1961). Prior to Stigler's innovation economists had simply posited the amount and type of information held by market participants, rather than deriving it from economic principles. Like the entrepreneur whose new product or technique is "obvious" once introduced, Stigler's application of the principle of utility maximization to the acquisition of information was obvious once he presented it. It quickly became part of accepted economic doctrine, the tool kit for applied economics.

Other new ideas meet resistance, but are eventually accepted out of "necessity." Stigler's example here was public choice, which was resisted because it cut against deeply held, if unconscious, presumptions about superior knowledge and wisdom of economists and public interest motivations of government officials. The innovators, Anthony Downs, James Buchanan, and Gordon Tullock, began by questioning these presumptions. Stigler's route to acceptance of their innovations was more empirical. In his work on the history of regulation Stigler found that there was no body of evidence on the effects of regulatory policies, which led him to question the aims of regulatory policies. Thus the theory of regulation expanded the domain of the theory of value. Stigler suggested that outside of macroeconomics, few theoretical innovations were tested in the textbook fashion of empirical contests between explanations of the same phenomena. Instead, the contest was based on theories' applicability to a wide set of problems.

Conclusion

George Stigler wrote that Adam Smith's *Wealth of Nations* "is a stupendous palace erected upon the granite of self-interest." There may be a bit of autobiography in this for we have seen that Stigler's scholarly work from the beginning to the end of his career made use of mathematically (i.e., calculus) informed formation of this granite. This was that people act efficiently in pursuit of their interests. This tenet was the key to his work in industrial organization, the economics of information, and the economics of regulation (public choice). Stigler began work on neoclassical value and distribution as an M.B.A. student at Northwestern, writing lengthy term papers on "The Theory of Value from Adam Smith to Stanley Jevons" (1931–1932) and "A Theory of Distribution" (1932a). Before entering the Chicago doctoral program he came to view Adam Smith as the "foremost of all economists," whose *Wealth of Nations* "masterly superseded effectually all antecedent hypothesis." This was despite setting out to omit "criticisms … [except to] clarify and distinguish doctrines" (1931–1932). He wrote that Alfred Marshall's reconciliation of cost and utility theories of value "turned academic England from an Armageddon to the more tranquil path of refinements of the principle, which even today is the substantially accepted doctrine of value" (1931–1932: 71). He had the self-confidence as an M.B.A. student to develop his own theory of distribution of which he concluded:

> Quite apparently, this paper has been neo-classical in treatment. The writer is not completely satisfied with distribution theory as it is here presented; it seems to him, however, to be the most logical and consistent theory yet devised. It is in accord with the normal price doctrine, and in fact this same pricing process has been applied to each of the factors here presented. (1932a: 78)

Throughout his career Stigler criticized, defended, applied, and developed neoclassical value theory.

Stigler believed economics was a science and he sought to preserve what he considered the marks of science. There was an enduring but developing body of doctrine, value theory, with wide applicability across time and location. Applicability was important, for economic science was grounded in the real world. Like his friend and colleague Milton Friedman, Stigler saw the value of economic theory in helping with real-world problems. Theory needed to be tested for both its internal logic and its conformity with facts. But in a time when both economic theory and statistical testing were being made more technically precise, Stigler's tests of theory were low-brow. There was little difference for him in application of theory to a problem and testing of theory. The tests were tests of application to explain behavioral puzzles. As an economic scientist Stigler was demanding and patient. He was demanding of himself and others to be patient. Do not jump to conclusions about policy without good evidence to back up advice, and do not jump from policy to policy as an adviser or advocate without reviewing the record of the last advice you gave.

Stigler began his work in industrial organization as a reformer, analyzing and advocating or criticizing antitrust and regulatory policies. But in the 1960s, after years spent observing economists and policymakers, he became a reformed reformer. He came to doubt that the public and public officials really wanted the advice of economists. As his work took a sociology of science and public choice bent Stigler's urge to understand remained but his urge to reform receded. This turn was possibly also an effect of Stigler's commitment to democracy, his classical liberalism. He opposed the tendency of intellectuals and government officials to think that they knew better than common people what they needed.

> The proposal that certain products be subsidized for the poor consumer, because they are good for him, seems a bit out of place for an economist per se. It is just possible that on equally valid grounds some people may prefer beer and others pork chops. In a democracy, if such utopian construction may be permitted, it is fundamental that desirable changes in consumer's tastes (desirable, that is, in the light, say of medical knowledge) be effected by education, not by manipulation of relative prices. (1938: 584–85)

For a man who could appear haughty, one who did not suffer fools, George Stigler had high regard for common people's abilities.

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Notes

1. See Rutherford (2011).
2. Among the secondary literature by historians of economics are Nik-Khah (2010) and Freedman (2008, 2012, 2016).
3. Frederick Shipp Deibler, 1876–1961, was chairman of the Northwestern Department of Economics from 1916 until his retirement in 1942.
4. Here we see for the first time in print George Stigler's self-confident criticism of economists' scholarship wrapped in his ironic wit.
5. For references he pointed readers to Edward Chamberlin's *Theory of Monopolistic Competition*.
6. "The Economics of Information" (1961).
7. He does not here make a distinction between managerial and entrepreneurial functions.
8. Later in his career Stigler would argue that much of what economists perceived as irrational behavior was in fact rational.
9. See "The Extent and Bases of Monopoly" (1942a) and "The Economies of Scale" (1958a).
10. As in for example "The Theory of Economic Regulation" (1971).
11. Henry Simons, *Personal Income Taxation* (1938: 13–14).
12. He quoted Wesley C. Mitchell from *The Backward Art of Spending*: "If quantitative analysis can give us empirically valid demand curves and coefficients of elasticity for numerous commodities, shall we not have a better theory of demand than qualitative analysis can supply" (1937: 24).
13. This would be a feature of mid-century Chicago price theory. See Hammond et al. (2013).
14. This point was directed specifically at Henry Schultz.
15. Stigler and Becker (1977).
16. The assumption of quasi-perfect knowledge refers to perfect knowledge of demand and costs, but not of rivals' reactions.

17. Stigler pointed readers to J.M. Clark, "Toward a Concept of Workable Competition" (1940).
18. In the Preface to *The Theory of Competitive Price*, Stigler wrote: "In the not-too-distant future I hope to add sections on imperfect competition, multiple products, capital theory, and certain other topics which usually receive attention in advanced theory courses" (1942b: v).
19. This would soon become a matter of debate with Milton Friedman. See Hammond and Hammond (2006).
20. He referred readers to his forthcoming "Five Lectures on Economic Problems" (1949b).
21. He had referred to Schumpeter as "a distinguished iconoclast of our profession."
22. Stigler presented Henry Simons as a pioneer economist on antitrust in the classroom and in *A Positive Program for Laissez-Faire* (1934).
23. He cited his own attempt to find such an effect in "The Economic Effects of the Antitrust Laws" (1967) and other University of Chicago studies.

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George Stigler: Knowledge, Preferences, and (Self-Interested) Choices

Marina Bianchi

Introduction

> One should hardly have to tell academicians that information is a valuable resource: knowledge is power. And yet it occupies a slum dwelling in the town of economics. Mostly it is ignored: the best technology is assumed to be known; the relationship of commodities to consumer preferences is a datum. And one of the information-producing industries, advertising, is treated with a hostility that economists normally reserve for tariffs or monopolists. (Stigler 1961: 213)

Yet this neglect of ignorance in economics is avoidable, according to Stigler, since, once information is allowed to play a role, we can look with fresh eyes at many relevant, but neglected, problems of consumer behavior and of organizations. In particular, in the 1961 paper cited

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above, *The Economics of Information*, he focused on how the existence of consumers' ignorance and the costs of search allow one to explain price dispersion, a phenomenon that could be observed even in the case of homogeneous goods. By simply comparing the costs of search with the expected savings that depended both on the degree of dispersion and the amount of expenditure, the optimal amount of search needed to reduce ignorance could indeed be found. And, by allowing the problem of information a place among the arguments of the utility function, Stigler reached what he thought were the three assets of a successful theory: generality, manageability, and congruence with reality.

The search for greater generality and a greater ability to explain the many facets of economic behavior is also at the basis of the famous *De Gustibus non est Disputandum* paper Stigler wrote jointly with Gary Becker (1977). This paper indeed represents a major effort to expand the reach of the maximization of utility principle into domains that were previously precluded from economic analysis and regarded as either beyond the scope of economic theory or an instance of its failure. The formation of habits, the problem of assessing quality—a problem that was "studiously" avoided in the paper on information costs—and the existence of fashions and fads, constituted a set of phenomena that had seemed to rest on various forms of "irrationality": erratic changes of tastes, dependencies, manipulation by advertisers, and social status effects.

Not so in Stigler's and Becker's alternative approach, where knowledge, skills, and past experience are allowed to enter the consumption capital of economic agents, and through it affect the shadow prices of their objects of choice. Indeed, what previously appeared to be an inexplicable shift in preferences might well be simply a reaction to a change in the constraints set by prices and incomes, the traditional tools of the economist.

The strength of Stigler's and Stigler and Becker's new approach cannot be undervalued. By allowing information and experience to play a role in agents' choices, it enlarged the domain of economic theory into realms that previously seemed out of reach. Yet, it says more for the flexibility and potential generality of the theoretical apparatus of rational economic choice it purports to defend than for the richer understanding of human choice and action that it claims to have achieved.

For dealing with knowledge has its perils. Stigler circumvents them by representing knowledge as information and information as a commodity that can be treated like any other economic commodity in the market. Yet when knowledge is allowed to enter the process of choice and action, it does not stop at being an input for the reduction of given costs, but inevitably becomes a means for producing change and novelty. It is a process that intervenes directly into the judgment of the goals of choice, on those "tastes" that Stigler and Becker insist are stable and uniform and as such not a matter that should be discussed.

An enlarged alternative view of the role of knowledge in human choices was in fact maintained by Frank Knight, who was Stigler's teacher and under whom Stigler wrote his dissertation. For Knight the problem of knowledge was above all a problem of understanding and discovery, a process of creating new values. For him people did not rest content with given wants but kept striving for what they thought was better for them. This implied that, before efficiency could be discussed, it was necessary to have a measure of usefulness, of value, and this was particularly true when there is more than one form that the usefulness of output might take, or in the case of a costly input. Yet, by the time Stigler was thinking to "take account of the cold winds of ignorance" (Stigler 1961: 224), his view, and that of his fellow Chicago economists, had taken a direction different from Knight's.

Knowledge Is Power

John Stuart Mill, Stigler reminded (Stigler 1988: 217), had written in the 1840s that in a competitive market one can expect only one price for the same commodity. The reason is that informed buyers would always seek the lowest price and informed sellers would always seek the highest bidder, with the result that initial differences in prices will eventually disappear. But, Stigler adds, Mill's "theorem" is true only if we assume that information is perfectly free. In fact this is not the case and we generally find a variety of prices for the same commodity, a variety that depends largely on the costs of information.

It is this simple but rather powerful idea that lies at the basis of Stigler's successful study of the costs of information. "Price dispersion is a manifestation - and, indeed, it is the measure - of ignorance in the market" (Stigler 1961: 214). It is true, he added, that price dispersion is still a biased measure of ignorance, because homogeneity among goods is never absolute: some sellers might offer better services than others, a greater variety of models, or even a more pleasant selling/buying environment. Yet, these differences notwithstanding, Stigler says, it would be "metaphysical, and fruitless" to attribute price dispersion entirely to heterogeneity (Stigler 1961: 214). Because of ignorance, a buyer—or a seller—who wants to discover the most favorable price must engage in a process of search, a process that will be worthwhile only if the costs that search involves will be repaid by the gains derived by discovering better prices.

The structure of Stigler's model is simple. The expected gains to a consumer from a given search will reflect the degree of price dispersion (the greater the dispersion the greater the expected savings), the size of his expenditure and the anticipated reduction in price. Correspondingly, the cost of search will depend on the number of identified sellers and on the cost of the consumer's time. The latter will vary among consumers and reflect both differences in tastes and the fact that for people with larger incomes time is more valuable. By equating the cost of search with its expected marginal return, the optimum amount of search will be identified and, "if the conditions and participants in the market were fixed in perpetuity, prices would immediately approach uniformity" (Stigler 1961: 219).[1]

The number of sellers and buyers, however, constantly changes over time, as does the variability of supply and demand and the size of the market. These changes make the accumulated knowledge obtained through search obsolete, thus constantly imposing new costs and renewing the sources of price dispersion. The persistence of search costs throws new light on the market arrangements that might reduce them. One phenomenon that emerges is the pooling of information, as when clusters of sellers of services and goods concentrate in dedicated markets. Another arrangement, over which Stigler puzzled at length, is that provided by advertising. Thus:

> Advertising is, among other things, a method of providing potential buyers with knowledge of the identity of sellers. It is clearly an immensely powerful instrument for the elimination of ignorance - comparable in force to the use of the book instead of the oral discourse to communicate knowledge. (Stigler 1961: 220)

The greater the number and stability of the customers who can be reached by advertising, and the greater the market power of the firm, the greater will be the incentive for firms to engage in advertising. Yet the problem of knowledge, Stigler recognized, extends beyond the knowledge of prices and involves problems usually not addressed successfully by economics, among them the search for better profit opportunities, what jobs to take and their location, and, importantly, the search for knowledge about the quality of goods. a problem however that for its complexity has been, he declares, studiously avoided in this paper (Stigler 1961: 224).

Years later, Stigler, in his *Memoirs of an Unregulated Economist* (1988), going through his contributions to economic theory, judged the treatment of information as a valuable commodity that is produced and purchased in the market as probably his most important contribution (Stigler 1988: 80). He had reasons to be proud. Not only did his model of optimal search explain the apparent anomaly of persistent price differentials for the same commodity but it did so leaving intact, in fact reinforcing, the model of perfect competition and Mill's 'theorem" of one price. Information costs, conceived as an obstacle to a complete search for a better price, served also to rescue this model from its detractors, such as Chamberlin, whose alternative model of monopolistic competition Stigler had fiercely attacked years earlier (Stigler 1949).[2]

The new approach took off and expanded rapidly, opening new issues, such as, Stigler still noted in his *Memoirs*, models of asymmetric information (George Akerlof), and the informative role of advertising (Phillip Nelson). The result was that within little more than a decade the literature on the subject had become so extensive as to give rise to a specific autonomous subfield of economic research (Stigler 1988: 80–81). It came almost as a surprise to Stigler that such a study could be so rapidly accepted without controversy. Yet, as he noted in his address upon receiving the Nobel Prize in Economics for his work on market structures and regulation (1983), the reason was simple:

> … no established scientific theory was being challenged by this work: in fact, all I was challenging was the neglect of a promising subject. Moreover, the economics of information was susceptible to study by quite standard techniques of economic analysis. The theory immediately yielded results which were intuitively or observationally plausible. Here was a Chicago theory that didn't even annoy socialists! (Stigler 1983: 539)

Contrary then to approaches such as Chamberlin's, whose theory of monopolistic competition with its product differentiation and strategic interaction among firms did challenge the established scientific corpus (Chamberlin 1933), Stigler could claim to have realigned theory and reality by simply giving more analytical power to existing economic tools, and this, as he would often iterate, was one of the marks of a good theory.

The approach that Stigler found complementary to his own was, instead, Coase's theory of transaction costs. Stigler himself recounts how Coase in 1960, in a meeting become famous (Stigler 1988: 75–77), conquered a whole audience of skeptical Chicago economists with the simplicity and brilliance of his idea, namely that in a world of zero transaction costs there would be no cause for externalities because, whatever the assignment of legal rights for damages (costless) negotiations between the involved parties would occur that would internalize the externality with no effect on the equilibrium outcome. Stigler was so taken with the idea that he introduced it in the third (1966) edition of his *Theory of Price*, labeling it the "Coase theorem" and contributing largely to its success.

In Stigler's hands the Coase theorem became another instance of the possibility of extending the range of economic theory to explaining phenomena that, like imperfect knowledge or the existence of externalities, were thought to be a failure of the competitive process to assure an efficient outcome. With the Coase theorem, instead, the presence of externalities is simply the result of the costs that accompany any transaction. This, in Stigler's eyes, meant that, with the exception of externalities that involved a great number of transacting parties, a system of contracts could be legally envisaged to solve the problem, without having to recur to any form of Pigovian tax. This solution, however, went well beyond the intentions of Coase himself, who thought that the existence of costs of transactions was precisely what made it difficult and often impossible to find the optimal contractual solutions in disputes.[3]

What Are the Criteria that Contribute to the Success of a Theory?

Stigler had already addressed this question. In his *Development of Utility Theory*, published in two parts in the *Journal of Political Economy* in 1950, he traced the history of utility theory from Smith to Slutsky. His purpose was to explore whether, by identifying the major steps in the development of a branch of economic theory, the understanding of modern economics might be improved. This historical review however also opened up new questions, which Stigler addressed in the final section of his "Development" paper, where he asked: "Why do economists change their theories?" (Stigler 1950a: 307).

In a section headed "A Theory of economic theories," Stigler, drawing on the lessons of the history he had just recounted, tried to identify those characteristics which might make a theory successful or not, where success is measured in terms of acceptance by leading economists. What he found was that the bases on which economists choose between theories may be summarized under three headings: generality, manageability, and congruence with reality (Stigler 1950b: 392).

As for the criterion of generality, the more successful theory was always more general than the one it supplanted. Thus, the marginal utility theory was more general than the classical theory of value because it allowed economists to analyze the values of non-producible goods and the short-run values of producible ones (Stigler 1950b: 392–393). Again, the generalized utility function was more general than the additive utility function because it allowed the analysis of interrelationships among the marginal utilities of goods. The nonmeasurable utility function was more general than the measurable that implied ungrounded intrapersonal and interpersonal comparisons of utilities. On the other hand, the Bernoulli theory of choice over lotteries was rejected as arbitrary because it was based on an assumption on the shape of the utility function of the individual's wealth for which there was no evidence and no test (Stigler 1950b: 375).

In sum, there was no important instance of a more specific theory supplanting a more general theory (Stigler 1950b: 393). Sometimes, as in the case of the nonmeasurable utility function, greater generality simply meant the introduction of a weaker assumption that sufficed to reach the conclusions of interest. More often, however, greater generality meant the encompassing of a set of phenomena that had previously been outside the domain of the theory, and which allowed for a wider range of predictions (Stigler 1950b: 393).

The second criterion that Stigler employed for choosing between theories was manageability. Economists long declined to accept the generalized utility function and remained attached to the additive one because its mathematical analysis was more complex (Stigler 1950b: 393). The same happened with the non-integrability of indifference curves, which long remained unpopular. Manageability however should not be confused with easy manipulation. Rather than being a virtue, this aspect shows a lack of enterprise and imagination since there is always more than one route to the solution of economic problems.

While the criteria of generality and manageability are formal, the third criterion, that of congruence with reality, deals with the empirical element of a theory. For a new theory to be successful it has also to "systematize and 'explain' a portion of the empirical knowledge of the times" (Stigler 1950b: 394). In fact this third criterion, which for Stigler would remain one of the major tenets of an acceptable theory, should in his view have been adopted by economists much more willingly than it was.

What happens, in practice, is that sometimes, when the implications of a theory did not seem to pass the empirical test, there is a tendency "to reformulate the theory to make the test ineffective." Here the example he gives is exactly the puzzle he would try to solve in the "De Gustibus" paper written with Becker,

> … when it was suggested that there might be increasing marginal utility from good music, as one acquired a taste for it, this was interpreted as a

> change in the utility function. Yet if in the time periods relevant to economic analysis this phenomenon is important, it is a significant problem the defenders had no right to rush to the dinner table. (Stigler 1950b: 395)

It is not difficult to understand then why the introduction of search costs in the utility function of individuals and, in Stigler's view, also Coase's theory of transaction costs, passed the test of a successfully progressive theory on all the three counts. Stigler's new approach included a larger set of phenomena that could be addressed and explained. It was not analytically complex; and it was based on hypotheses that could be tested.

The same could not be said of theories that, as with Chamberlin's monopolistic competition, focused on variables other than price and quantity. These alternatives stressed the specificities of quality characteristics, emphasized the relevance of the particular against the generalizing approach of the perfectly competitive model. The result was a multiplicity of competitive prices due to the monopoly power of product differentiation that clearly challenged the established theory (see Ekelund and Hébert 1990; Keppler 1998, for the methodological implications of Stigler's critique). In the eyes of Stigler, then, Chamberlin's was a theory at fault on all three tests: it was particularizing, as opposed to increasing generality, it complicated what could in fact be explained with the existing tools of perfect competition and it lacked any verification in the real world (Stigler 1949).[4]

The Costs of Generality and Manageability

However, the effort to expand the general coverage of a theory has its drawbacks. Stigler himself hinted at these in his *The Development of Utility Theory* paper where he noted that generality may often be only nominal, and it "is always easy and usually sterile to introduce a new variable into a system, which then becomes more general" (1950b: 394). What then of Stigler's enterprise? The introduction of information costs (as with transaction costs) did make the theory more general.

Yet, this was true only for those cases where information concerning prices exists and the goods involved are homogeneous or almost perfect substitutes. What was left out was the problem of the heterogeneity of goods, of their quality differentiation (both horizontal and vertical) as the origin of price differentials. He was aware of the problem but dismissed it by simply asserting that heterogeneity could not entirely explain price dispersion. What was left out then at this stage was exactly what for Chamberlin justified the monopolistic power of those firms that were able to compete, differentiating their products on the basis of quality, design, and variety of characteristics.

Stigler, however, would address this problem of quality differences as markers of heterogeneity a few years later, when dealing with the choices of oligopolistic firms and in particular with their incentives to collude. In this context he showed that heterogeneity belonged both to products *and* to buyers. The extent of these two forms of heterogeneity remains difficult to measure and depends again on the costs of information. But for oligopolistic firms to disregard these differences when choosing among different price structures (as well as their collusive strategies), amounts, for Stigler, to accepting a lower rate of profit. In fact, even in the case of a homogeneous product, a firm with a monopoly power that ignores the differences among buyers acts as if it was imposing an excise tax upon them, but one that is not collected by the firm (Stigler 1964). Having stated the problem, however, Stigler, when further exploring the methods and conditions of collusive practices, does not let it play any additional role.

By Stigler's own standard of congruence with reality, then, knowledge of quality and of differences of tastes remained an unsolved problem. He knew that the problem was difficult and represented a challenge to economic theory. Dealing with quality means dealing with complexity since goods and activities have a multidimensionality that cannot be reduced to the information over single objective variables as is the case with prices. And even if it were possible, even if differences in qualities were reduced or eliminated with the diffusion of information, still differences among people's values, interests and inclinations, would reproduce these differences. Knowledge implies not just information but evaluation, comparison, and judgment. In fact, it is in the choice of quality, of the specific characteristics of the goods one might expect and enjoy—not simply in the search for the lowest price of *one* model, but the very choice of the model—that the real problems and challenges of choice reside.

Within this framework, not only the assumption that preferences are given but also the notion that utility is marked by decreasing returns becomes dubious, since complex goods can be utilized and combined in multiple ways that change through time and renew their "utility" thus defeating satiation. Moreover, called into question is also the representation of search as always costly since the exploration and discovery of yet undetected characteristics can be pleasurable, a fact that firms know very well when they challenge consumers by introducing new characteristics that increase, rather than decrease, the required amount of information a consumer must take into account.

The exclusion of the problem of quality may, then, have repercussions also for the criterion of empirical testing. Once we admit that the acquisition of knowledge is not limited at the minimization of search costs but can create product differentiation and express differences in tastes, it becomes difficult to test the predictive power of Stigler's information theory. Differences in prices even for goods that are homogeneous or slightly imperfect substitutes may well reflect different consumers' subjective evaluations, their different inclinations toward search, and different ways in which some goods complement each other in different individuals' consumption sets—all differences that do not necessarily cancel out with the reduction of information costs. Stigler nonetheless believed that the utility theory, with its corollary of rational constrained maximization, could meet these challenges and he welcomed challenges.

De gustibus non est disputandum

Stigler returned to the role that the relation between utility and choice had on the understanding of (relative) exchange values in a 1972 article, entitled "The Adoption of the Marginal Utility Theory." He began with a reflection on Bentham's felicific calculus, remarking that whatever the role of utility as a moral guide (and for him it was an "intellectual tragedy" that Bentham confined utility mainly to this role), it is undeniable that the principle of utility maximization is "an all-embracing theory of purposive conduct" (Stigler 1972: 72). Anticipating the consequences of an action always implied, for Stigler, taking into consideration the desired vs the undesired consequences, i.e. pleasure vs pain. Even if not a theory of relative prices, Bentham's analysis went a long way, for Stigler, toward explaining how the calculus of pleasure and pain extended through all of human behavior and could be an effective rule for establishing the proportion between gains and costs, or, in the case of Bentham himself, between the gains of offenses and the value of punishments, or inflicted costs (Stigler 1972: 74).[5] It was however not until the simultaneous rediscovery, after a long dormant phase, of these two governing principles of choices, by Jevons, Menger, and Walras around 1870, that utility theory finally provided, according to Stigler, a unified explanation of behavior. Everyone was a utility maximizer and all economic problems became simply a matter of tastes and obstacles to satisfying them (Stigler 1972: 78).

Yet he was aware, as we have seen, that the potential of the generality of utility theory had still to be explored and that there were gaps to be filled. Critics of the theory, coming both from inside the economics discipline and from neighboring ones, such as psychology and sociology, pointed them out. The assumption that tastes were given and stable in particular seemed to be a weak point of the economic theory of value. Not only quality differences and variety seemed to reflect and express differences in tastes, but tastes seemed to be shaped by experience and cultural specificity, to be prone to the manipulation of advertising or to depend on the influence of others, as in the search for status and with fashions. Cases such as fads showed in addition that tastes may be fickle and subject to erratic changes. Conversely, as in the case of habituation and dependencies, tastes seemed to become unresponsive to existing utility gains.

It is exactly in order to address these challenges that the paper "De Gustibus Non Est Disputandum" that Stigler wrote with Gary Becker (1977) took form. In a conversation with Craig Freedman, Gary Becker relates what the origin of their joint work was (Freedman 2016: 348–349). One day Stigler went to Becker with a quote from Alfred

Marshall that seemed to cast doubt upon the principle of decreasing marginal utility. The quote, which we have seen Stigler had already referred to in his 1950 paper, pointed out that in the case of classical music, it seemed that the more one listens to it, the more one likes it and the more one demands of it. "How do we explain that?" Stigler asked Becker.

What they were trying to do at the time, Becker pointed out, was to go behind what people want when they demand goods, to discover something more basic (Freedman 2016: 348). Becker had already tried to do this in his *Theory of Human Capital* (1993 [1964]) and in his *A Theory of the Allocation of Time* (1965). In this paper the two of them endeavored to apply that framework to new and challenging problems (see also Becker 1962, 1963). The solution they found was in a sense brilliant and original. It implied two strategies.

The first was to turn upside down the usual interpretation of the saying "de gustibus non est disputandum." This is commonly meant to indicate that where tastes are concerned there cannot be a debate since they are both as different as people are different and constantly changing. In Stigler's and Becker's new interpretation, instead, tastes cannot be discussed for the opposite reason, to wit: because they are stable over time and similar across people.

> … tastes neither change capriciously nor differ importantly between people. On this interpretation one does not argue over tastes for the same reason that one does not argue over the Rocky Mountains—both are there, will be there next year too and are the same to all men. (Stigler and Becker 1977: 76)

The difference between these two interpretations is fundamental, they stress. Under the first viewpoint, to explain an economic phenomenon as an expression of a change in tastes amounts to putting aside the economic explanation altogether and deferring to other disciplines (to: psychology? anthropology? phrenology? or sociobiology? They ask).

> On our preferred interpretation, one never reaches this impasse: the economist continues to search for differences in prices or incomes to explain any differences or changes in behavior. (Stigler and Becker 1977: 76)

Employing this strategy, categories of behavior traditionally used to show that tastes change, can in fact be explained by a Benthamite calculus of utility maximization, without invoking the qualification "tastes remaining the same." In other words, the proposition that tastes are stable and uniform allows one to shift the burden of explaining the differences among men and times from a discussion over tastes to an analysis of the effects that incomes and relative prices—the traditional tools of the economist—have on these differences. The thesis they are proposing is "a thesis that does not permit of direct proof because it is an assertion about the world, not a proposition in logic" (Stigler and Becker 1977: 76). But ultimately the choice between the two views—changing vs. stable tastes—must be made on the basis of their comparative analytical productivities. "If the latter approach yields more useful results, it is the proper choice" (Stigler and Becker 1977: 77).

The second strategy relied upon involved a reformulation of consumer theory, thanks to which the "power of stable preferences" can indeed be extended to "a wide range of behavior" (Stigler and Becker 1977: 77). Consumers—within this new approach—are no longer simply demanders of market goods but also the producers of their own desired commodities. They are transformed from passive maximizers of the utility drawn from market purchases into active maximizers who are also engaged in extensive production and investment activities. The utility function is now effective over objects of choice—here called commodities—that the same consumers produce using market goods, their own time, their accumulated knowledge, their skills, and other human capital.

Thanks to these two strategies, many of the paradoxes that were thought to undermine the traditional approach seem to dissolve. In the case of the consumption of music—Marshall's example—consumers are producing a commodity called "music appreciation." They do so by using their own time devoted to listening to music, as well as the market goods related to music. Investing in music consumption in the past has the effect—similar to what happens in the realm of production—of increasing the consumer's stock of human capital, in this case music capital. This, in its turn, increases the productivity of the time spent in listening to music. The result is that the opportunity cost—the shadow price—of producing music appreciation, relatively to alternative commodities, decreases, thus causing the demand for music to increase.

The framework is the same in the cases of habits that are negative and cause harmful effects, as with many drugs. The difference here is that the accumulated stock of human capital from past consumption depreciates over time, causing the opportunity cost of producing a commodity such as "euphoria" to increase. The reason why the demand for euphoric drugs nevertheless does not decrease is because the demand for these goods is inelastic due to both addiction and withdrawal.[6] Habits and addiction, both positive and negative, then, are easily explicable without involving any change in tastes, by simply applying the rational equilibrium rule of utility maximization.

The new approach to consumer behavior allowed Stigler to return, now with the partnership of Becker, to the problem of the costs of information. They could address the problem of quality, price dispersion and differences of tastes, a problem that, we have seen, Stigler had left in fact unsolved. They do so by considering the role that advertising has in specifying the characteristics of goods and informing consumers. The ultimate objects of choice, even in this case, continue to be the commodities produced by each household with market goods and their own time, knowledge, and other inputs. The mechanism of the model too is the same: an increase of advertising, by making consumers believe—correctly or incorrectly—that a unit of the advertised market product can deliver an increased output, has the effect of lowering the shadow price of the commodity, thus increasing its demand. What appears as a shift of the demand curve of the market good (and therefore as a change in tastes) is in fact a movement along a stable demand curve for the commodity that registers the effect of a change in shadow prices (Stigler and Becker 1977: 84).

This, Becker and Stigler insist, is not a mere rhetorical device. In the conventional approach, firms acting in a perfectly competitive market have no incentive to advertise because consumers have perfect knowledge of the quality of goods. In their alternative approach, instead, since consumers have imperfect information, firms have an incentive to differentiate their market products advertising their specific characteristics and to charge different prices. And this they do, still remaining perfectly competitive in the commodity market. In fact, the more competitive the commodity market is, the greater the incentive to advertise may be, since a higher elasticity of demand translates into a greater effectiveness of advertising on the shadow price of the consumer of this or that commodity, thus increasing the marginal revenue of advertising.

The role of information in consumer demand, thanks to the new approach, Stigler and Becker suggest, is capable of extensions in yet further directions. For example, since the stock of information and the demand for knowledge by individuals is affected by their formal education, age or period of residence in a community, the model allows for the exploration of systematic variations of demand for advertisements that take into account these additional variables (Stigler and Becker 1977: 87).

In conclusion, then, the great advantage "of relying only on changes in the arguments entering household production functions is that all changes in behavior are explained by changes in prices and incomes, precisely the variables that organize and give power to economic analysis" (Stigler and Becker 1977: 89). With the result that all the alleged suspensions of the maximization of utility that are dealt with in the paper—addiction, advertising, customs, and fashion—respond only "to the constraints imposed by the theorem on negatively inclined demand curves" (Stigler and Becker 1977: 89).

De gustibus est Disputandum

In addition, the new consumer theory developed by Stigler and Becker provided, reflected by the intentions of the authors, all those methodological advantages that might favor, in line with Stigler's 1950 paper, their new approach over those of alternative theories: i.e. generality, manageability, and congruence with reality. The new approach was constructed precisely in order to cover a set of economic phenomena previously excluded from economic theory, or, worse, considered an instance of its failure. Employing this approach, Stigler was able to close the few gaps exhibited by his information theory. Becker would further extend this approach to even more irksome problems, such as the study of crime, marriage and family planning (Becker 1976). Additionally, the new theory did not require an overwhelming dose of analytical complexity and provided results that, for the authors, could be tested empirically (see Becker et al. 1994).[7]

Yet if we look at the new approach more closely, things are not so seemingly smooth. Consider generality. As in the case of Stigler's introduction of search costs, this new extension of economic theory into the larger domain of individual choice implied also the introduction of some yet undetected costs—the subjective costs of a consumer's investment decision. This move is perfectly legitimate provided there is also a recognition that we are focusing on one aspect of a theory of choice, the technical analysis of cost reduction. Left out are those motivations, goals, and values that inevitably affect costs and that, even if not explored, were present in the "unreformed" model of choice. The logical slip in Stigler's and Becker's analysis however is to represent—implicitly and often explicitly—economic choice as if it implied only cost reduction or even, letting the word "economic" drop, as if cost reduction is all we need to know about any choice (even if with some allowable exceptions, see Becker 1976: 8–9). In fact, costs are meaningful and accessible only within a well-specified set of goals and values. This conclusion brings us to the role of testability and consequently to the "productivity" of the new approach.

In the traditional economic theory of choice the goals were subjective preferences. In the new one proposed by Stigler and Becker, the goals to be maximized are replaced by the commodities produced by the householders themselves. Yet commodities such as music appreciation, style, or recreation, are so general that they can be made compatible with almost any sort of maximizing behavior, thus making the "efficiency test" inconclusive. Therefore if a profitable opportunity exists but it has not been exploited, the new approach has only to postulate the existence of some costs that prevents us taking advantage of that opportunity.[8] Maximizing "good health," then, can explain the choice of both smokers and nonsmokers, drinkers and teetotalers, since for a smoker or a drinker, to be a nonsmoker or a nondrinker is postulated to be more costly. For the same reason maximizing a commodity such as "euphoria" can accommodate both playing video-games and shopping, gambling and taking drugs, annulling any distinction among the very different behavioral patterns involved. Maximizing social distinction can fit instead almost everything including, obviously, *not* pursuing social status. As Rosenberg (1979) observed, Stigler and Becker simply replace one unobservable, utility, and preferences, with another unobservable, the produced commodities.[9]

As the efficiency test at the level of individual household is inconclusive so is the same test at the market level. It requires a great effort of imagination to identify the boundaries of a market for a commodity such as euphoria, or music appreciation, or status, let alone to measure and test its degree of perfect competitiveness. If we lift the veil of the commodity market, we are left with nothing but the strategic competition of product differentiation that Stigler had so much criticized.

In the economic theory of choice we are presented with by Stigler and Becker, changes in tastes are replaced by changes in shadow prices, but these are not less subjective and more testable. There is not a great difference in saying that through experience one appreciates more music because subjectively it costs less or because it is liked more: their rules of change are unspecified and unverifiable in the first case as they are in the second.[10] In the new model too we continue not to know why music is more liked, or felt to be less costly with experience (see Bianchi 2002). We do not know the reasons that make "music appreciation" a potentially non-satiable good.

Economists have always been content to deal with goods and activities of proven—and stable!—hedonic values, such as basic needs. They then extend the motivations that are supposed to be behind them to all forms of human activities.[11] What is left out, however, are all those dimensions of goods and activities whose value must be discovered and created. Consumers do not rest content with awaiting for changes in constraints, they actively intervene to modify them. Consumers are producers but also entrepreneurs, they discover and innovate (Bianchi 1998).[12]

Despite the alleged greater generality, and logical compactness, of the new theory of consumption, then, we are left with an impoverished theory of choice: motivations are absent and goals are empty boxes that can always be filled in ways that rationalize the choices made, thus depriving the theory of testable predictions. The plea for greater generality calls for a different form of generality that implies also an enlargement to the contributions of other social disciplines; but this was beyond the scope of Stigler and Becker, who believed that no alternative theory offered the same advantages as economics and who used generality to reinforce the marketability of the existing theory (and its ideological offspring).[13] In this sense their efforts are valuable because they test how far the productivity of the economic theory of choice, with their established set of constraints, can go.

The Ethics of Competition

Frank Knight, in his "The Ethics of Competition" (Knight 1935a [1922]) discusses the relation between ethics and economics, an important pairing since both are recognized as dealing with the problem of value. To clarify this relation, however, rests squarely for him on clarifying the nature of human wants, and their role in human actions, which in turn calls into question their being treated as data and left in various categories of the unknown:

> … these wants which are the common starting-point of economic reasoning are, from a more critical point of view, the most obstinately unknown of all the unknowns in the whole system of variables with which economic science deals. (Knight 1935a: 20)

Yet for Knight:

> Wants.. not only *are* unstable, changeable in response to all sorts of influences, but it is their essential nature to change and grow; it is an inherent inner necessity in them. The chief thing that a common-sense individual actually wants is not satisfactions for the wants which he has, but more, and *better* wants. (Knight 1935a: 22, italics in the text)

What man strives for, Knight insists, is not actual desire but what a person thinks he/she *ought* to desire. Indeed the true achievement for

Knight is the refinement of the plane of desires, the cultivation of tastes and "all this is true *to the person acting*, not simply to the outsider, philosophizing after the event" (Knight 1935a: 23, italics in the text). This is why want-satisfaction cannot be regarded as a final criterion of value. Wants themselves are not final:

> … instead of resting in the view that there is no disputing about tastes, we dispute about them more than anything else; our most difficult problem in valuation is the evaluation of our wants themselves and our most troublesome want is the desire for wants of the "right" kind. (Knight 1935b: 42)[14]

In Knight's hands, then, the problem of knowledge was preeminently a problem of understanding and discovery, and a process of creating new values. People, in his view, did not rest content on the given but kept striving for what they thought was better for them. Life, in short, is fundamentally an exploration in the field of values and not, or not merely—a matter of producing fixed values (Knight 1935b: 43).[15] The implication of this, Knight adds, is that, before efficiency can even be discussed, it is necessary to have a measure of usefulness, of value; and this is particularly true in the case in which there is more than one form of usefulness of output, or a costly input (Knight 1935b: 43).

These preceding reflections, for Knight, are necessary if one wants to judge the role of competition as the system that allows values to be freely discussed and expressed. The scope of economic activity, besides being a means for satisficing wants, is at the same time an agency for wants and character formation, and a field where self-expression can be made manifest as part of a competitive game (Knight 1935b: 47).[16] Competition, for him, was better than any other known system for allocating resources, yet it does not imply equal access to resources, nor does it involve perfect rationality and complete knowledge (Knight 1935b: 49ff.). There is in fact a conflict in the ethics of competition between the liberty and the equality that are necessary for the competitive game to remain creative and fair, and the efficiency that is required for the satisfaction of wants. The system is indifferent and unselective as to which wants are worthy to be satisfied. Though Knight was rather skeptical about the possibility of resolving or doing without this conflict, he continued to believe in the importance of social discourse about values.

Stigler had been a student of Knight and one of the few who successfully wrote a dissertation with him. Additionally, the volume in which Knight's essays on the ethics of competition appear was edited by Stigler, Milton Friedman, Homer Jones and Allen Wallis in celebration of Knight's 49th birthday. Yet the two worlds of understanding economics—and the role played by knowledge, wants, and competition—by Stigler and his cohort, and Knight, were and remained far apart.[17]

In his "The Ethics of Competition" (1981) Stigler discussed the economist's canons of ethics and in expressing them he also took a position with respect to Knight's arguments. He recalls how, when first reading Kight's essays years earlier, he thought that they provided a definitive refutation of the "productivity ethics" according to which each productive factor under competitive equilibrium receives a compensation that equals its marginal contribution (Stigler 1981b: 19).

However, when he reread them after some decades he was shocked by the propositions they contained.[18] The main bone of contention was Knight's judgment of the workings of competition. As Stigler would repeat a few years later (1987), Knight, in his stressing of the shortcomings of competition, passed judgments that lacked any empirical basis or supporting evidence. Nor did Knight ever reveal what the ethical content of his "acceptable ethical system" might be (though it was clear, also to Stigler, that Knight's ethical system did not involve any precept but was the expression of a free social interaction). Moreover, by focusing only on polar cases, he did not recognize that only a small portion of human activities is given to the search for new wants or the exercise of curiosity while in fact the wants of most human beings are stable (Stigler 1987). Against Knight, Stigler argued instead that efficiency is the main normative prescription of the economist. He, the economist, needs no ethical system to criticize errors: "he is simply a well trained political arithmetician" "and since he is simply pointing out to the society that which it seeks, perhaps inefficiently, he need not quarrel with what it seeks" (Stigler 1981a: 9).[19]

Stigler's objections to Knight then simply iterate his position without really engaging in a discussion with Knight's line of argument. Yet they are interesting because they also reveal a second, and deeper, element of contention, involving the role of economics in relation to the other social disciplines. For Knight saw economics as only one element in a larger dialogue with other social disciplines, notably, ethics and aesthetics (see Knight 1935c). As Stigler clearly puts it, for Knight, the primary role of economic theory was "to contribute to the understanding of how by consensus based upon rational discussion we can fashion a liberal society in which individual freedom is preserved and a satisfactory economic performance achieved" (Stigler 1987: 58). Yet, accepting this larger undertaking inevitably implies that the role of the economist is vastly reduced, and the economic prescription of efficiency represents only "a pathetically small part of human activity" (Stigler 1987: 58).[20] Stigler could not accept this since his major effort, as we have seen, was always to enlarge the domain of economics, bringing back to the rationality principle and the notion of efficiency all those forms of behaviors that seemed to contradict them.

Yet, as he shows also in his comments on Knight, he reached this result by constantly downplaying, and often hiding, all the possible limitations of his alternative approach: namely, the exploratory nature of human activities and the evolving dimension of human wants where knowledge is change and mistakes and inefficiencies are not only inevitable but necessary. By so enlarging its domain Stigler, ironically, also made of economics a closed system, tightly sealed against, thus immune to, the contributions of other social disciplines. He left to those other disciplines a deeper understanding of the role of change in choices and of how knowledge and affect interact with one another.

Conclusions

Of the three methodological principles that for Stigler mark how a theory is able to succeed and win over alternatives, i.e. generality, manageability, and congruence with reality, it is generality for which Stigler strove more strongly, even at the expense of the other two. For him, as he iterates in his "Memoirs of an Unregulated Economist," an economic problem is always a problem that involves a choice of the most efficient among alternative uses of scarce resources "whether the resources are dollars, a bowl of whipped cream, available time, or even a reputation for honesty and skill" (Stigler 1984; 1988: 193). Anything worth having must be scarce and anything scarce is costly to obtain. To unearth these costs and to make economics thereby the study of all purposive human behavior Stigler devoted himself relentlessly. Yet there are costs also in Stigler's choice of such a vast application of the "no free lunch" principle.

Stigler laments in his 1961 paper that knowledge occupies only a slum dwelling in the realm of economics, but his answer to this neglect that allows information costs to enter economic decisions represents a small segment of the problem of knowledge. Still unanswered is not only how to know the best technology, the best price, or the best quality but how to bring them into existence at all; how to discover and perfect them. In this process efficiency alone is of little guidance; in fact it could be a hindrance that prevents the taking of risks and searches for initially more costly and uncertain alternatives. But taking into account these larger implications of the problem of knowledge would shake the equilibrating mechanism of the perfectly competitive model and bring to the fore the particular and the specific that, for Stigler, would lead to description and literature, not science.

Description, literature, and psychology would also take care of the motivations and preferences that inform choices. But to eliminate them, as Stigler and Becker do, also means providing a sanitized view of choice, where the most difficult and interesting task is to discover and strive for what is desired and achieved. But for Stigler these larger implications were not useful economic concepts, unless, as he says in answer to Leibenstein's concept of waste, "one is prepared to take the mighty methodological leap into the unknown that a nonmaximizing theory requires" (Stigler 1976: 216).

Notes

1. If, additionally, purchases are repetitive (and the correlation of successive prices of sellers is positive) then the expected savings will include the present value of discounted savings on future purchases (Stigler 1961: 216). Therefore, the larger the fraction of repetitive (and, as a consequence, more experienced) buyers and of repetitive sellers (with positive correlation of future prices) the larger the amount of search that will be undertaken in a market of given geographical extent.
2. As he had done for another approach that questioned the equilibrium dynamic of the market, Paul Sweezy's kinked oligopoly demand curve (Stigler 1947, 1978).
3. A thorough discussion of the relationship between Coase's view of transaction costs and Stigler's can be found in Medema (2011). Coase, however, had always appreciative words for Stigler (see Coase 1991).
4. The same charges would be raised against Leibstein's concept of X-efficiency (Stigler 1976): no empirical support and no insights that could not be embodied in the established more general economic theory. See Freedman (2002) for an analysis of Stigler's argument.
5. Yet for Bentham "pleasure" was not an indifferentiated category but varied according to intensity, duration, and the amount of time occupied that affected both memory and anticipation. See Nisticò (2017).
6. No role here for conflicts of motives or problems of self-control as in Schelling (1984).
7. For Hoover (2017), Stigler and Becker's "De Gustibus" indeed represents an attempt to reconcile a commitment to empirical content with Robbins' deductivism.
8. Becker himself admits that the new reformulation might seem tautological but, he adds, the assumption that preferences are stable provide a "foundation for predicting the resposes to various changes" (Becker 1976: 7). But this would be true only if changes in costs could be assessed objectively, i.e. independently of the commodity to be maximized, which is not the case in the new model. Also, Stigler had rejected the imputation of tautology and affirmed that economists escape this charge by simply assuming that on average people are correct in anticipating the consequences of their actions (Stigler 1984: 302).
9. Rosenberg's critique of Stigler and Becker's assumption of stable preference also contains a second argument. We could replace the proposition of stable preferences with the more acceptable and less paradoxical

proposition of a stable schedule of needs. But if we do this, then it is more difficult to maintain that any need is as urgent as any other and preserve a theory that is value-free (Rosenberg 1985, 1992).

10. Cowen (1989) asks: even if we admit that the market for good "music" has become addictive, why should the commodity produced by that market good be addictive? Caplan (2003), on the other hand, points to the contributions of recent psychological research in providing empirical evidence for preference-based explanations.
11. See on this point Bianchi (2016).
12. Already in the period in which Stigler and Becker were developing their new approach to consumer choice, experimental psychology had made notable strides towards the study of curiosity and exploratory behavior and of how pleasure responds to novelty and change (Berlyne and Masden 1973; Berlyne 1978). More recent developments in experimental aesthetics (Scherer et al. 2001; Silvia 2006) show additionally that the alternative according to which preferences are either fixed or change arbitrarily is a false alternative. Even if not predictable, the way preferences change can be understood and explained (Bianchi 2014).
13. See Craig Freedman (2005).
14. "…what we call progress has consisted largely in increasing the proportion of want-gratification of an aesthetic or spiritual as compared to that of a biologically utilitarian character, rather than in increasing the 'quantity of life'." (Knight 1935b [1923]: 41).
15. Patinkin (1973) stressed also how for Knight it was the process of learning by doing and the creation of new knowledge that defeated the "law" of decreasing returns.
16. Ross Emmett (2006) takes on the task of articulating Knight's answer to the Stigler-Becker view of stable tastes. His discussion is a valuable companion to the relative restrictedness of the Stigler/Becker position articulated above.
17. As has been remarked (Freedman 2016), Stigler may be read as having relied much more on the approach of Jacob Viner, who taught the introductory graduate course of price theory at the University of Chicago from the 1920s until 1946 and whose students included, besides Stigler: Milton Friedman, Paul Samuelson, Kenneth Boulding, Martin Bronfenbrenner, Don Patinkin among many others. Viner embraced a view of economics as syllogistic logic that was not concerned either with the origin of value, or with the validity of premises or conclusions concerning it (Viner 2013: 20).

18. Stigler however was greatly appreciative of Knight as a teacher and of his ability to stimulate students' independence of judgment and critical attitude (see Stigler 1973).
19. Man is eternally a utility-maximizer, he concludes, and what we call ethics in this approach is a set of rules that prohibit both myopic self-serving behavior and behaviors that impose large costs on others and small gains on oneself. To specify a content for what has to be maximized and avoid tautology is not a task that can be pursued. A rational person always learns from mistakes with the result that there cannot be systematic biased errors in prediction. There cannot be a theory of mistakes (Stigler 1981c: 35–36).
20. In short, Stigler says correctly, Knight saw man as "an explorer and experimenter, a seeker for unknown and perhaps unknowable truths", but he soon adds, "a creature better understood through the study of literature than by scientific method" (Stigler 1987: 59).

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Textual and Scientific Exegesis: George Stigler and Method in Economic Science

M. Ali Khan and Edward E. Schlee

> … it takes an economist to read an economist. Even the best of men is a strange mixture of insight and partial blindness. Most professors do not know how to read a scientific work well, and this skill is developed only with purposeful practice.[1] (Stigler 1969, 108–109)

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> [W]e must beware the fallacy of demanding the immediate where the mediate must suffice. Languages are powerful mediatory structures, and to act in and upon them is to act upon people immediately perhaps, but by changing their means of mediation, which is often done indirectly and takes time. (Pocock cited in Pagden 1987, 37–38)

> Only the actual participants can correctly recognize, understand, and judge the concrete situation and settle the extreme case of conflict. Each participant is in a position to judge whether the adversary intends to negate his opponent's way of life and therefore must be repulsed or fought in order to preserve one's own form of existence.[2]
> (Schmitt 1976, 27)

Prolegomenon

Stigler's (1965) distinction between *textual* and *scientific* exegesis has provoked one or both of the authors of this essay for at least the last twenty-five years. Thus Khan (1993a) connected it to a practice that is non-historical, and a style of writing in which, to use the words of Pocock, the "practitioner is not concerned with what the author of a statement made in the remote past meant by it so much as what he in his present can make it mean: what he can do with it for purposes of his own, which may or may not and therefore do not have to coincide with those of the author[3] (Pocock 1971, 7)." The distinction was seen as engendering two polar cases not unconnected from the determination of a space in which economics was to be done and written. Khan brought to bear a host of writers, including Winch, Ricoeur, and Rorty, to understand the distinction, but satisfied himself with the retreat that:

> Future work will have to determine where analysis of style falls between Stigler's polar cases of scientific and personal exegesis; meanwhile we have

re-emphasizing the relevance Carl Schmitt, to Muhammad Hussain for an afternoon instruction of MSWord, and Swayam Bagaria, Andres Carvajal, Ying Chen, Andrew Farrant, Mark Setterfield, Metin Uyanik and Xudong Zheng for their questioning and sympathetic encouragement. Khan acknowledges with immeasurable gratitude more than a decade of indispensable correspondence and conversation with Hülya Eraslan, David Levy and Eric Schliesser.

> to rest with the conclusion that the extent to which style is "grounded in the nature of economics as a discipline is something still to be established." (Khan 1993a, 57)

However, Stigler's distinction was not the primary subject of the piece, but was being referred to in passing in a review essay on a collection devoted to "economics and language."

Khan and Schlee (2016) return to Stigler's distinction was in the context of McKenzie's 1957 paper on demand theory,[4] and this time invoked less in passing. Their abstract states:

> This implicit theorizing of the process of theorizing then forces us to consider Stigler's distinction between textual and scientific exegesis and confront it to a second-order level of theorizing, and thereby bring out the continuity, possibly not quite seamless, between a theorem and its history. (Khan and Schlee 2016, 589)

The authors refer to their "engagement with Stigler's methodological imperatives," and see Stigler as a "skilled practitioner of second-order theory among distinguished (first-order) theorists." (Khan and Schlee 2016, 591, 594). In their concluding section on the framing of McKenzie's result in the context of "higher-level theorizing," they write:

> Stigler is important for this essay precisely as a counterpoint to Kermode: by fixing the contemporaneous view of science as an evaluative criterion, he fixes the context, and in this closure of higher-order thinking, valourizes the exclusions and what is taken as the tacit and the essential. Thus, the entire tenor of our argumentative thrust stands at an angle to Stigler's findings. (Khan and Schlee 2016, 625)

Continuing on, they again "single out Stigler and his methodological writings [as] rather important to [their] essay" and ask:

> How can a text be evaluated by the criteria that it itself forged? to evaluate prices by the criteria of the market-maker who makes them? (Khan and Schlee 2016, 626)

Their reader, the 1993 one as equally as the 2014 one, may be forgiven for not being able to suppress obvious questions that these oblique statements provoke, whatever their intended purpose and however unintendedly designed. What is the meaning of this distinction between scientific and textual exegesis, between retrieval and recruitment, between rational and nonrational reconstruction? And why does one need a panoply of distinguished authors, historians, philosophers, and literary critics, to understand it? What is this distinction between first- and second-order theory? How does Stigler stand at an angle to Kermode? to historians such as Pocock and Winch? to philosophers such as Cavell, Ricoeur, Lyotard, and Rorty? How is style relevant to theory and theorizing? How does a text forge its own criteria? And why are these questions relevant to an economic scientist working out, and on, his or her science in our day and age? What is the signature of his or her science, and if an upgrading or downgrading of a piece of writing is involved, a refereeing in the current professional jargon, how is it to be executed?

In fact, already in 1993, Khan found himself prompted by Shackle, by McCloskey and by the authors of the volume he was reviewing, as well as by the "facts that resources are finite and that research is supported by self-seeking institutions," and by authors in the pay of these institutions, to ask questions of his own.

> First, is the history of economic thought or more generally the thought of deceased economists a prerequisite for economic theory? Second, are methods of literary criticism a prerequisite for the doing of economics just as calculus and statistics have now become for the doing of economic theory? Third, is theorizing about the process of theorizing an essential activity within economic theory? Fourth, would anthropological and sociological narratives about the economics profession teach us any economics?
>
> (Khan 1993a, 63)

Concluding yet again with the rhetorical retreat that "it is always easy to ask questions than to answer them," Khan hoped for an absence of closure, and that the questioning would identify and invite

> more productive metaphors, more disciplined interpretations, more inviting projections, more appropriate subspaces, more adequate translations.[5]
> (Khan 1993a, 63)

This chapter then is written in answer to such questions, and addresses not only their former readers, but perhaps also younger and more contemporary ones grappling with evolving market norms as to what is and what is not economics. However, its primary motivation comes from a larger ongoing project on the mutual imbrication of the theory and its history: on how one rests on, and draws its identity from, its other; on how a history of economic thought is to be read and written; on how crucial is the distinction between the history of economic doctrine and the history of economic analysis; on where and how the historian writing the history places himself or herself. These questions again lead us to Stigler, arguably *the* doyen of the history of economics, at least for the last quarter of the twentieth century, a "historian, essayist, empiricist, wit, philosopher, and mathematician," and his methodological imperatives: (Bernstein 1976, 136–138).[6] But this chapter also has an important secondary motivation, one entirely disconnected from a potentially auto-biographical or a restitutive one, out to fill in expository lacunae in earlier work. It is motivated by what the authors see as an alarming tendency toward the writing of a fictional biography as a substitute for the writing of the history of a chosen subject: to write the history of economic thought outside the discipline of economics rather than within it, to write the history of the proof of a theorem without stating, or even possibly without a rudimentary understanding of the theorem itself. Stigler's views, however problematically conflicted the authors may find them to be, and this is to be explicated in all that follows, offer a reassuring antidote to the excesses of such a view.[7]

More sharply and concretely motivated, we attempt to understand what George Stigler understood to be economics and politics, and more generally, political economy. We believe this attempt to be not only warranted but overdue. We pay particular attention to what he thought to be the boundaries of his subject-matter, and how he thought his subject itself should be read and understood, both in the past as well as in the present. We are especially alert to the point that Stigler was an influential editor of a house journal that has maintained a sustained reputation of being one of what is now referred to as the top-five mainstream journals of the economics profession. He decided what was or was not legitimate and well-grounded investigation of the past. He chose the referees, and ruled on a text, decided on its lasting impact on what he saw to be his subject, and on how it furthered his view of the subject. He was a market-maker.[8] It is this that leads us to to think it important to know the values he brought to bear on the history of economic thought, but more generally, on what went into making the *Journal of Political Economy* what it came to be for the economics profession at large, a profession that eventually began to gauge its identity from the journal itself.

The outline of the chapter is then straightforward, and the epigraphs lay out the basic composition. In section "A Chicago Analogy and Stigler's Exegetical Distinction," we begin with Stigler's conceptual distinction and move on to his struggle with form and content, words and ideas, and with the neutrality or non-neutrality of language. In section "Professional Reception of Stigler's Categorization," we turn to the reception, largely non-reception, of the distinction both in Chicago and in the profession at large. In particular, we relate it to the largely ignored interpretive tussle between Patinkin and Friedman on what Keynes really meant, and their resort to econometrics to find out; and more generally, as a method for writing history. With this dissonance placed squarely on the table, we turn to Stigler's *professionalism* and what he saw to be his professional identity. This is a necessary subtext to the essay, as is testified to by his colleagues both inside and outside of Chicago, and indeed by some even outside his profession. After this necessary diversion, we return to a prelude to a fuller identification of the originality of Stigler's distinction. We ask what we have just said above that we ask: whether language is simply, and only, a garment for the body of ideas, or does it shape the very idea. We do this by a selective reading of John Pocock's work in section "Pocock on Concurrent Language Games." In particular, we take Pocock's reliance on Saussure's *parole-langue* distinction, based in turn on Jakobson's *synchronic-diachronic* dichotomy: his insistence on the recognition and subsequent empirical identification of the simultaneous circulation of different languages in discourse. Next, in section "Stigler's Mathematical Method and Its Psychological Pitfalls," we read Stigler (1949) on the mathematical method in economics. A careful reading of this text allows us to provisionally identify, perhaps even on an as-if working hypothesis,[9] the mathematical economist as Stigler's "other." It is the crucial text for this essay.[10] Section "Schmitt's Concept of the Political: The Enemy–Friend Dichotomy" is a necessary digression on this, a scholar's "other." It recalls for the reader Schmitt's concept of the "political," rooted as it is in the clarity of the *friend-enemy* separation, and it takes us to what is in some sense the concluding thrust of this chapter in its section "Toward an Overview: A Collection of Strands." This section attempts a summation and a collection of these various strands of the argument, both in terms of understanding Stigler, and in terms of what is the authors' current position in their program concerning the writing of the history of economic thought.

A Chicago Analogy and Stigler's Exegetical Distinction

In a piece that we see neglected[11] and deserving of a more forceful engagement with/by historians of economic thought, be they historians of doctrine or of analysis, Stigler (1965) drew an analogy between doing statistical empirics and the writing of the history of ideas.[12] He referred in particular to a "problem often met within the history of economics: which passage in a man's writings do you accept when several passages are inconsistent?" In contesting the way a particular proposition of Ricardo's was to be interpreted, he was clear that the "problem of inference which is posed by such an example is more interesting than the example itself." He put forward an equivalence result.

> If a substantive economic relationship were under discussion ..., we would never dream of establishing its validity by citing one or two or three facts (observations on pairs of years, say) – let alone by citing two "facts" against it and one "fact" for it. We would all agree that larger, more objectively chosen bodies of evidence be brought to bear upon the problem. Why should we allow the hand-picked quotation to carry an

> interpretation when we would reject the hand-picked fact as an empirical test of a hypothesis? In fact the two problems are basically the same. (Stigler 1965, 448)

This basic similarity of practice, and a hankering for "objectively chosen bodies of evidence," ran in his writings before 1965 and would surely run on later in his more mature writings.

Keeping to the matter at hand of this section, let us see how this analogy, and the resulting equivalence that it implies, is single-mindedly pursued.

> A successful hypothesis accounts for the important relationships in the appropriate data, but it need not account for random variation. Similarly, the textual interpretation must uncover the main concepts in the man's work, and the major functional relationships among them. The interpretation need not account for careless writing or unintegrated knowledge. (Stigler 1965, 448)

It is indeed a remarkable analogy: the intellectual historian as a statistician, weeding out outliers in his or her data, and giving coherence to what he or she sees as a mass of empirical detail. This detail is to be integrated and brought into service as regards the hypothesis that is presumably already formulated, and one that is to be accepted or ruled out at a level of significance that has already been assigned to the power of the statistical tests that are being brought to bear on it. Stigler continues.

> The processes of testing a quantitative hypothesis and an analytical interpretation differ in details. The basic way of increasing the confidence in the statistical test of a hypothesis is to enlarge the sample. We should not be so literal-minded as to count the passages in a book to decide an author's general position because the passages are not of equal importance. We increase our confidence in the interpretation of an author by increasing the number of his main theoretical conclusions which we can deduce from (our interpretation of) his analytical system.
> (Stigler 1965, 448)

The issue whether the "sample" is to be taken from a single book, or other books of the author, if not those of his or her contemporaries, is bypassed. It will prove to be a substantive bypassing with Stigler bypassing it not once but twice in the text that we are reading. He concludes his text with the following theme.

> Let us recognize the fact that the interpretation of a man's position - especially if the man has a complex and subtle mind - is a problem in inference, not to be solved by the choice of quotations. (Stigler 1965, 450)

Anyhow, the relentless pursuit of this analogy yields the dichotomy that this section is intended to bring into salience.

This is Stigler's categorical distinction between the "principle of scientific exegesis" and the "principle of personal exegesis." As regards the first,

> The man's central theoretical position is isolated and stated in a strong form capable of contradictions by the facts. The net scientific contribution, if any, of the man's work is thus identified, amended if necessary, and rendered capable of evaluation and possible acceptance. The test of an interpretation is its consistency with the main analytical conclusions of the system of thought under construction. If the main conclusions of a man's thought do not survive under one interpretation and do under another, the latter interpretation must be preferred. This rule of interpretation is designed to maximize the value of a theory to science.
> (Stigler 1965, 448)

This *idée fixe* of maximizing the value of a theory to science is a recurring refrain, and Stigler returns to it in a 1969 essay in his interrogation as to whether "economics has a useful past?"

> The purpose in seeking to understand the man's theoretical system is not to be generous or malicious toward him, but to maximize the probability that his work will contribute to scientific progress. Only if the analytical system is well defined and cleansed of irrelevant digression and inessential

> error may we determine whether it is a worthy addition to the corpus of science, or at least a line of investigation that ought to be explored further. (Stigler 1982a, 110)

This integration, cleansing, purification, and the weeding out of errors and confusions by a master of his subject is a constant drumbeat. Once this Whiggish idea of progress is yoked to science, it becomes a demarcation of what is useful and worthwhile, and what is not.[13]

This kind of activity is sharply distinguished from another kind, that of "personal exegesis." This is the "other" of scientific progress.

> One may seek to determine what the man really believed, although this search has no direct relevance to scientific progress. One will then invoke a different criterion to choose between conflicting passages: that interpretation which fits best the style of the man's thought becomes decisive. This may be called the principle of personal exegesis. (Stigler 1965, 448)

Personal exegesis is then seen as tangential to policy and to "scientific" pursuit. It is a stance to the subject that openly acknowledges the importance of a scholar's education and culture to be hardly relevant to his or her solving the particular puzzle that needs solving, the problem that nags and calls for the compelling explanation. Science has only one style: there is no room for any idiosyncrasy. In any case, this exegetical dichotomy leads to a stark and static methodological conception, a pursuit of an analogy that lands into a distinction between retrieval and recruitment and between appropriation and expropriation. Even though he does not cite his 1965 paper, Stigler returns to biography eleven years later in his 1976 article on the two Mills, and poses the question as to "What is biography and how may it be distinguished from scientific development;" see (Stigler 1982a, 91). He writes

> The primary task of scientific history is to become scientific: to subject hypotheses to objective tests which the hypotheses are capable of failing. I have a singularly low estimate of the scientific value of sermons on methodology [but wish] to remind all of us how easily illustration can be confused with evidence. (Stigler 1982a, 97)

Stigler's dichotomous method inveighs against weak-kneed and self-reflective doubts: the method outlaws methodology, outlaws, as indeed any enforcer of law must, a theory of theorizing.

The interesting question concerns Stigler's conception of science on which his pronouncements rest? this valorized term that provides the *leitmotif* for his distinguished *oeuvre.* Referring to writers of "high and low science [as] sentient beings," Stigler writes:

> Yet the web of mortality that ties them to their time and place is *not* science: science consists of the arguments and the evidence that lead *other* men to accept or reject scientific views. Science is a social enterprise, and those parts of a man's life which do not affect the relationships between that man and his fellow-scientists are simply extra-scientific. When we are told that we must study a man's life to understand what he really meant, we are being invited to abandon science. (Stigler 1982a, 91).

Stigler is nothing if not an aspiring scientist, and finds the un-packaging of the sociological and anthropological "we" as not really part of science. It is the scientist's "other," the non-scientists who are luring "us" into territories not "ours." Stigler invokes Stone's term *prosopography*, and reiterates in 1976 what he had already affirmed in 1965: that there is an "essential deception in handpicking congruencies between a man's life and his ideas."

> Biography is information, but it is not the kind of information, if indeed any information is of that kind, which speaks for itself. [D]etailed biographical knowledge is irrelevant to the interpretation of an individual's scientific work. I therefore firmly agree with the contrary position: to understand the *scientific* role these men played in the evolution of economic theory, that role was played with the words they wrote, not with the ideas they intended to express. (Stigler 1982a, 92)

This is a pioneering expert of the economics of information holding forth on information.[14] It is a difficult passage to interpret in its confronting of words and ideas, and presumably of form and content, though it seems to privilege form rather than content in establishing the irrelevance of form. Anyhow, the point is how to recover the gold from the gold dust, to sift the science from the non-science. Stigler illustrates by considering the scientific value of the writings of the two Mills, and in 1980, returns to Merton and the history of science proper. He ties biography to heroes and hero-worship, and perhaps also offering an antidote to Keynes' celebrated {\it Essays in Biography}

> A science is conducted by a society of scholars who jointly pursue the development of a coherent body of knowledge including a central theoretical core. Because the pursuit of science is a social enterprise, only knowledge shared by its members is scientific. As in other social enterprises – economic, military, political – it is superficial and misleading to view the progress of the society as the product of a few heroic figures. (Stigler 1982a, 98)

Never one to forego tautology and irony, Stigler turns in the article to Merton, and writes "Merton's contributions have been so fundamental as to constitute almost a self-refutation of his thesis of science as a social enterprise!" As we see, it was also Stigler's public thesis. This ambivalence as to whether there are, or are not, market-makers[15] whose presence would blemish the truth and the normativity of the market can surely be seen to persist in Stigler's *oeuvre*, but let us now turn to the reception of Stigler's important exegetical distinction itself.

Professional Reception of Stigler's Categorization

We prepare the ground for the reception of Stigler's distinction by first considering how Stigler's writings on the history of economic thought were received in Chicago itself, received by the School that he saw as his own, and by the professional colleagues he deemed professional. Of these, Aaron Director and Gary Becker were preeminent. Becker writes:

> Stigler's main scientific contributions were to the history of economic thought and to microeconomics, with a special emphasis on industrial

> organization. His interest in the history of thought was not to discover who said what first but to trace the evolution of economic ideas. … Stigler became the world's greatest expert on the history of economic doctrine. (Becker 1993, 762–763)

In this condescending encomium, with its dazzling confounding of the categories of history and science, the natural question to ask is what one means by a scientific contribution to a history, and whose, and which, history is at issue. Becker is clear that it is the history of economic thought, but when does a thought become economic, and shed its political, sociological, perhaps even anthropological, *habillements*. Even within a register deemed, and seen, as unambiguously economic, is there a jostling of ideas within it? one trajectory vying for preeminence with another? One would think these questions to be basic to any historiography. But one ought not to cause hold-ups and quibble needlessly: the point is that what we have here is an acknowledgment of Stigler as not only a theorist of the first rank, but as the "greatest expert on the history of economic doctrine."

To be sure, this issue of "who said what first" is a rather amateur, if not a primitive, view of intellectual history, a collector's view of the subject. Becker continues:

> [H]e recognized that this field attracted less and less interest from the vast majority of economists. He did not oppose[16] its abandonment[17] as a requirement for the Ph.D. degree at Chicago, a decision that mirrored what was happening in other major departments.[18] He continued to believe, however, that the great economists of the past were far more stimulating to read than all but a small fraction of contemporary writing on economics. (Becker 1993, 762–763)

One has to be careful here. It is surely not simply a matter of idle stimulation. The avoidance of biography, the finding of a thinker as interesting for his or her own sake, is precisely what Stigler's dichotomy warns the reader about: not to get diverted but to focus on the content, the scientific meaning, of what is being said. This is perhaps what is connoted by "scientific history." Instead of a question of belief and biography, Becker's reference to the abandonment of the history of economic thought appears to have been a major event for the department of economics at Chicago. Rosen also refers to the event, but in this way.

> Stigler proposed and supported the decision of the Economics Department at the University of Chicago to abandon its history of thought requirement in 1972, before many other departments did. There are good reasons to think that the disappearance of the history of economic thought from the core curriculum is a manifestation of progress in our discipline. It is also good economics. (Rosen 1993, 810–811)

Progress? Good economics? Rosen also refers to intellectual history being a stimulating read, but it is not clear that Rosen is not conflicted.

> [Stigler's] interests in and commitment to the history of thought had both personal consumption and investment aspects, which happened to create benefits for the economics community at large. Studying the development of ideas in an academic discipline produces a special kind of public good. (Rosen 1993, 810–811)

But what kind of a public good? What public purposes does it serve? Rosen sees it as a matter of score-keeping, Becker's who-said-what-first, a possible distinction between basic research and useful research that market more immediate rewards.

> The social organization of an intellectual society requires institutions for proper attribution, scorekeeping, the assignment of individual credit and property rights, and maintaining standards in order to maintain individual research incentives (Merton 1973). Basic research activities are fraught with too much common property for self-interest alone to work efficiently in creating new knowledge. Establishing priority, imperfect as it is, remains necessary to generate the peer esteem, public notice, and sometimes pecuniary wealth, in order to encourage intellectual prospectors to work hard at staking their claims. And, comparisons to the past help to set and maintain standards of accomplishment in a profession.
> (Rosen 1993, 810–811)

More than a manifesto for Chicago economics, it is a ringing endorsement of a utilitarian outlook, as read, for example, by Ferguson (2004). But let us stay on track and with Stigler.

Introducing Stigler as Adam Smith's best friend, Rosenberg also mentions the consumption-investment choice, a matter of work and leisure, indeed of love.

> Stigler's work in the history of economic thought … was not the product of a casual interest. It was, in fact, his first and lasting love. He was, I think it fair to say, he was never far away from the subject.[19]
>
> (Rosenberg 1993, 835)

This treatment of love as a commodity is, to be sure, fraught with analytical complexities—whether its production is subject to increasing returns to scale and therefore needs subsidizing, or whether it increases with its use, and therefore not fully amenable to economic discipline—but Rosenberg makes an additional distinction that can be interrogated further.[20]

> Stigler's approach to the history of economics has a consistent central feature, one that is better conveyed in fact by describing it as an interest in the emergence of economic analysis, or even economic science, rather than economic thought. (Rosenberg 1993, 835)

This preoccupation with categories continues, as it must, but it is never itself problematized. The difference between economic analysis and economic thought is surely a difference in substantive and productive consequence. Later on in the essay, after listing past thinkers Stigler wrote on, Rosenberg continues, and concludes as does Becker.

> Stigler's contributions to the history of economic thought have no doubt received far less attention than most of his other writings. Rather, I shall call attention, in an admittedly somewhat subjective way, to the high points of this large body of material and, especially, to what I regard as some very original thought that has received relatively little attention from professional economists. There is a straightforward economic

> explanation, of course. As Stigler himself once pointed out, this subject "is perhaps the last unsubsidized research area in economics."
> (Rosenberg 1993, 834–835)

But to get back to Rosen's (1993) piece, he turns to the question of retrieval, holding it to be identical to recruitment, and in underscoring his normative, as opposed to a positive, stance to the abandonment of intellectual history.

> Much writing in the history of thought is meant to clarify previous controversies in the light of current understanding. Recall, for instance, the vast number of articles written on what Marshall, John Maynard, or Milton Friedman *really* meant. Somehow the ambiguity of language and the rhetoric of selecting quotes of an earlier or current authority to support one view or another don't hold so much interest now. Stigler's (1965) sophisticated empirical approach of textual exegesis to root out ambiguity was not picked up by others, and perhaps he himself may have lost confidence in it. Perhaps, also, the profession has needed fewer authorities as the supply of economists, and of authorities for that matter, has grown.
> (Rosen 1993, 810)

Rosen, and the memorialized Stigler, never were to be bothered by any identification problems: with the question observational equivalence under which the man's thoughts survive under more than one interpretation, that the argmax correspondence is typically not a function as in elementary texts of price theory. But rather than regression analysis, Rosen delves into capital theory to clothe his ideas, and delivers a verdict by supplying the adjective "political" to self-interest.

> Yet there are good economic reasons to be skeptical that Knight's claim is valid for the history of ideas. Stigler never put much stock in the avoidance of error argument in any case. Why hold an ever-lengthening laundry list of all previous ideas in every successive generation's intellectual inventory? It is more economical for the scientific process to work sequentially. Successful thoughts are added to the social stock of intellectual capital and are maintained there, more or less, while mistakes are allowed to depreciate and vanish. Perhaps the fashion of marketing

> intellectual views to exploit political self-interest is a first cousin to the positive political economy Stigler spent much of his later career studying. (Rosen 1993, 810)

The bottom line, as Sowell (1965, 553) puts it in his recommendation of Stigler's first set of essays to scholars and sophomores, is that Stigler "treats the history of economic thought as something to *think* about, not as a field for leisurely cataloguing or a source of decorative allusion." This conflicted celebration of abandonment of a subject's roots, a finger in the eye of Schumpeter and Gerschenkron at Harvard and Heilbroner at the New School, was surely a scientific assertion about the contemporary state of affairs being some sort of a Nash equilibrium or a temporary Walrasian equilibrium: the vast majority of economists being self-seeking are simply responding to incentives and to the equilibrium context they find themselves in.[21] The interesting question relates to the mechanism designers and the market-makers, the extent to which Stigler was a price-taker or a price-setter.

Outside Chicago, the reception in Princeton and Harvard was a little more reserved. In his review of Stigler's autobiography, Baumol warns the "general reader that Stigler's writing is unrepresentative of the practitioners of the field in a number of ways."

> His enthusiasm for the accomplishments of economics is somewhat greater than the norm. His conclusions, the set of writings by others that elicit his approbation, and even his research methods are all to some degree colored by his membership in the "Chicago school." [A] characteristic of the school's research is its recurrent use of the premise that humans calculate with considerable rationality the course of behavior that best promotes their economic self-interest (broadly interpreted) and act accordingly with some consistency. (Baumol 1989, 1261)

In an earlier review, Dorfman had already coined an acronym for this premise: *SMP* standing for Stigler's major premise that man is an inveterate self-defining and self-regarding utility maximizer. *SMP* is the hammer that was taken to all problems, a hammer that defined a community and its conception of "science." Dorfman talks of the tension between the premise and Stigler's "contention that we enunciate views that appeal to congenial interest groups;" (Dorfman 1984, 106). Baumol continues to see the matter this way:

> It is perhaps ironic that this premise, which can with some justice be described as "economic determinism," is widely associated with the writings of Marx, who probably never used it, but is instead actually favoured by the group of modem economists most generally considered to constitute the center of political conservatism. (Baumol 1989, 1259)

The point, as far as the authors of this chapter are concerned, is that neither Baumol nor Dorfman focus their attention on Stigler's exegetical distinction being considered here, his dichotomous ruling that motivates and orients his interest in the emergence of economic analysis, his "thinking about" of the boundaries of economic science. They do not ask how a reading that was purposely driven, a stimulation serviced by the purpose of systematizing a scientific system, was to be executed.

The only author to have taken Stigler's piece seriously, though with the hopeful qualifier that he not be considered "unfair [in singling] out the book's shortest piece," is Samuels (1984). He offers a reductive paraphrase of Stigler's problem.

> If x is our evidence of reality (including as reality a system of thought) and if X is that reality itself, the ultimate epistemological and critical interpretive problem is whether x is representative of X. (Samuels 1984, 327)

Samuels enlists the aid of Stigler's fellow Chicagoan, Don Patinkin. He reads Patinkin in the following way:

> … if we try to interpret a man's work, and even more so the development of his work, from the viewpoint that everything that he said is necessarily consistent with everything else that he ever said, or even necessarily consistent with everything that he said at roughly the same time in other contexts – and that is the basic, if implicit, assumption (and consequently weakness) of the frequently referred to Talmudic method – then we'll

> obtain a distorted picture of how the man actually thought and worked.
> (Patinkin and Leith 1978, 123)

Samuels offers two suggestions: that we "remain content with a diversity of actual interpretations of what an author's main analytical conclusions are," and that "we ought not to expect an author to be consistent by every possible criterion (especially criteria derivative from later theoretical development and controversy)." He concludes on a tentative note that "Textual exegesis is a legitimate scientific problem, but perhaps we ought not to be overly exclusivist in the conclusions we seek or for which we settle." However, he does not really confront Stigler's dichotomy, face the fact that to understand the meaning of a single book, one needs to have recourse to many books, and that even in an auto-biography, others are involved.[22]

The important point for us here is that Patinkin also subscribes to the analogy that links the historian to the statistician.[23]

> [T]he study of the history of doctrine should be looked upon as an empirical study … I see it as an empirical science that in the broader sense that as historians of thought we are like econometricians fitting a multivariate regression equation to a man's writings: we're trying to pass a regression line through them that will best explain them.
> (Patinkin 1969, 125)

In fact he elaborates and develops the analogy going well beyond Stigler to specification bias and omitted variables.

> Now, one thing about a regression line is that there are always points off of it, and then the question is whether they are random departures from the line, or whether they reflect a systematic influence you have not taken into account of. And the same is true when you pass a regression line through a man's writings: there will generally some passages in a man's writings that you have not explained. And then you have to decide what is the true meaning of the man, what is his regression line and what is a chance phrase, a chance formulation, or perhaps even a mistake, whose departure from the regression line makes us change our view of the nature of the line. (Patinkin 1969, 125–126)

This is Chicago as scientific as Chicago can get: the true meaning of a man determined by his regression line.

And so this analogy may well be part of the oral tradition at Chicago, and it reduces to the choice of the sample. In asking "how one should approach the problem of understanding and following of the development of an idea?" Patinkin introduces his "principle of fallibility." He continues beyond what Samuels cites.

> [T]here is fallibility in the broad sense of lack of completeness: the individual does not see the full implications of his argument at a given point of time. Now, I said that 'he does not see,' and I immediately add that 'we don't see,' for we are all in the same position. (Patinkin 1969, 47)

The point is that there are subtleties and nuances involved in the invocation of the analogy. Patinkin's view, and his use of "we," carries altogether a different hue than that of Stigler's "we": the former rests on doubt and humility before the sources, the latter on over-weening confidence and aggressive *hubris* toward them.[24] Indeed, in a complaint about Friedman offering "no supporting evidence for his interpretation of the Chicago tradition," Patinkin (1969, 47) resorts to the views of a plurality.

> For questions about the history of economic doctrine are empirical questions. And the universe from which the relevant empirical evidence must be drawn is that of the writings and teachings of the economists in question. No operational meaning can be attached to the existence of a "tradition" which does not manifest itself in one or both of these ways.[25]
> Patinkin cited in Patinkin and Leith (1978, 125)

The reader will surely note how Patinkin uses the pluralized version of Stigler's singularized imperatives pertaining to the man's thought: the teachings of the *economists* in question. If one must limit oneself to the historian-statistician analogy, it all boils down to the choice of the sample, and to the very notion of what statistical inference means.

A fitting conclusion to this section is to ask how Patinkin and Stigler differ from their subscription to the Chicago oral tradition: how does

Stigler's exegetical distinction take the tradition venerated by what Dorfman calls the *SMP* and give it a sharpened fundamentalist tone? Perhaps the answer lies in the very opening sentence of Stigler's 1965 article: the implication of the acceptance of the null hypothesis rather than a failure of its non-rejection: "which passage in a man's writings do you accept when several passages are inconsistent?"[26] There is an unmistakable tilt toward scientific provisionality in Patinkin (a contemporaneous good-enough explanation until a better one arrives in the future) that one does not readily see in Stigler. Rather than using a pre-conception of science to weed out errors and outliers, Patinkin is clear on extracting the tradition from all the data that is available, on extracting a coherent view, as coherent as can be determined, of what the practice of science entailed at the time the text was being written; using the text and the textual resources to determine what is science rather than using "science" to determine the meaning of the text.

Stigler's Professionalism and Self-Image

There is by now a substantial scholarship on Stigler the scholar and the man, and even though this essay has the rather limited objective of identifying him only as a historian of ideas—the methodological preconceptions under which he worked and the style with which he wrote—we briefly digress to do what Stigler explicitly forbade the scientist-historian to do: we come awfully close to indulging in *belle-lettres* biography using Demsetz and Friedland as indispensable guides to the subtlety and complexity of the man.[27]

Demsetz (1993, 794) refers to Stigler as the "mid-century neoclassicalist" and makes the case that Stigler's "work was strongly guided by a penchant for defending and extending neoclassical price theory." Taking this as the point of departure for his reminiscence, he writes:

> He wore his passion for measurement on his sleeve, but he did not similarly call attention to the importance of neoclassical price theory to his work. Hence, this paper. (Demsetz 1993, 795)

So Stigler's neoclassicism was primarily empirical rather than theoretical.[28] In any case, Demsetz makes his understanding, and presumably also that of Stigler's, explicit. Neoclassicism rests on three postulates: (i) rational trades are rewarding trades, (ii) aggregate, rather than individual, actions are solely of consequence, and (iii) aggregate action is encapsulated in the Marshallian two-bladed scissors of demand and supply. Demsetz rejects the hypothesis of "survival of the fittest as one of the pillars of neoclassicism." Whereas this "faith was adhered to by neoclassical economists, it was a belief shared in by many who were outside the neoclassical school and even who opposed the school," see Demsetz (1993, 795). Within this backdrop, he identifies three fissures: (i) disinterest in the internal organization of the firm and indeed a lack of interest in theorizing about the firm, (ii) the devaluation of game theory and the attempts of von Neumann and Morgen- stern to view it as productive analytical engine, and finally, and (iii) what he considered "red herrings" in microeconomic theory. A quick romp through these headings is worthwhile.

The *eureka* chapter/moment in his autobiography notwithstanding, Demsetz notes that in his famous 1951 article on the "Division of labour is limited by the extent of the market," Stigler does not cite Coase's famous 1937 article on the "Nature of the firm" that deals explicitly with vertical integration. Demsetz writes:

> This omission, I believe, was due to Stigler's disinterest in the internal organization of the firm for he generally was quite careful to cite relevant work of others. Neoclassical theory assumes that persons attempt to optimize and that, subject to cost-justified errors, the market functions to ensure that the more successful optimizers survive. Market clearing works to weed out the inefficient. Neoclassical price theory, especially in its perfect competition model, leaves no room for the survival of incompetency. In equilibrium, owners of firms, as suppliers of goods, do not waste resources. (Demsetz 1993, 802–803)

In his turn to game theory, Demsetz puts Stigler's bias as refusal to abandon basic price theory.

> Supply and demand, entry and exit guided by profit considerations, marginal productivity theory, and aggregations of the behavior of large numbers of individuals played, and still play, no important role in game theory. Game theory virtually ignored the tools of traditional price theory. It also showed little concern for empirically testable deductions and for theorems of great generalizing power. On all these counts, game theory could not and did not appeal to Stigler, whose own work was guided by his desire to extend the reach of neoclassical theory. He believed that the past work of economists had created a useful set of tools by which to solve economic problems. He could not abandon these tools for an untested set that elevated negotiations between individuals to the level of a central paradigm of thought. He refused to play the game.
>
> (Demsetz 1993, 799–800)

Could it be that the reasons lie in game theory being not very loud about self-interest? About it being rooted in a game and thereby having a collective rather than an individualistic context? Its existence theorems being social existence theorems? on the impossibility of preferences being purely self-regarding, by being defined over strategy profiles, not consequences.

We leave it to the reader to pursue Stigler's pet peeves: Chamberlin's theory of monopolistic competition, Gardiner Mean's claim about the pervasiveness of administered rather than free-market prices, Liebenstein's notion of *x*-efficiency, Sweezy's kinked demand curve for oligopolistic industries.[29]

> Yet its negativism probably hindered further development of an important subject that is especially useful for problems of spatial competition. The economics of product differentiation is one class of problem that Stigler tended to overlook and finesse in his own work. It has proved important even to our current empirical understanding of search and price dispersion. And if Smith's and Stigler's insight on specialization and the process of economic growth has the validity that many of us think it does, the (internal) scale economies that cause it may be logically inconsistent with pure competition or pure monopoly. That problem cannot be studied very well by imposing theoretical limitations on forms of market organization. (Rosen 1993, 816)

But surely Demsetz' verdict stands.

> From whichever angle we view Stigler's contributions to industrial organization, they take on a decidedly neoclassical pose. This midcentury neoclassicalist influenced industrial organization considerably, but not solely through his writings. (Demsetz 1993, 806)

The question, when delineating biases and prejudices as we do here, is whether science is lapsing into biography. But let us now move to pure biography where no such ambiguities lurk. We turn to his long-time associate, Claire Friedland, references to whom are ubiquitous in Stigler's oeuvre. We can do no worse than cite the preface to his two-paragraphed autobiography. After commenting on Babbage in his final paragraph, Stigler begins his second paragraph with "I wish to thank friends who have read and commented upon most or all of this autobiography in manuscript. Foremost is Claire Friedland, who as usual did much to mend my error-prone ways."[30] In her 1993 reminiscence, Friedland is explicit about the difficulty of understanding George Stigler.

> A well-known Swedish economist's … first words to me were, "Now tell me what George Stigler is *really* like." I had worked with George for over 20 years at that time, but I had the delicious pleasure of telling him truthfully that I didn't exactly know. After 33 years of working with George, I still can honestly say I don't know the answer to that question. A student once asked me what was "beneath George Stigler's hard, sarcastic exterior." How could I resist answering "A hard, sarcastic interior"? If his humor was a cover, it was a delightful one, and I miss the wonderful fun we had together. Well, so be it. (Friedland 1993, 780, 783)

But Friedland is unduly modest—she is an important witness with important insights into an important postwar figure in economics, a leader of a school.

> He identified very much with the profession. He cared whether the profession moved ahead. He identified with the University of Chicago. He cared about it, not just about his own progress. If something was good for the Economics Department, he approved. If the Department

was in danger of losing somebody, whom he felt would be a loss, he cared and he worked on it.[31] Friedland in Freedman (1997)

Friedland stays with his professional loyalty and writes on Stigler's irrational commitment to rationality.

> Although I had often said that George was irrationally rational, in certain areas he was irrationally generous. George's sense of loyalty also bordered on the irrational. Not his loyalty to his friends, which involved an obvious return, but his loyalty to cold, impersonal institutions. the University of Chicago, the Department of Economics, or the Graduate School of Business. But how do you explain these irrational loyalties? All loyalty is irrational, except, of course, if you get to freeload. (Friedland 1993, 780)

Next, Friedland turns to the family, a concern as important as the concern with the state.

> When George was skeptical as I said about the altruism issue I said, "Look George, look how generous you are to your children. Are you doing that out of self-interested motives? Who are you kidding? You're not doing it out of that." And he looked at me and he didn't answer and he knew he wasn't doing that out of self-interested motives.
> (Friedland (2012) in Freedman (2012))

It is difficult to appreciate the deadly seriousness of these conversations—they go to the heart of the Chicago worldview, at least in this postwar period, and naggingly ask whether they constitute ideology or science. It ultimately comes down to what an "explanation" of a phenomenon, economic or otherwise, means: things have not to be opened too much. Friedland continues.

> He was actually very generous with his family, as he was with people in general. So, I think George, when pressed hard like I did there, would admit there is a motive beyond simply selfish self-interest. It could include his concern about others and generosity and so on. He didn't think we should bring that up too often, probably because he thought if we keep bringing that up, it's going to be a crutch that were going to rely

> on just to use when the going gets tough. I think that's what he would answer. Yes, it's there. Sometimes it's crucial. Maybe especially in the family, but let's not rely on that for all the explanations. A lot of them have a more narrow self-interest, selfish interpretation. And it would be too easy if we opened it up to too many motives. (Friedland (2012) in Freedman (2012))

Claire Friedland is nothing if not herself a loyal adherent to the Chicago school[32] she herself cuts her world through the knife of rationality and of self-interest.

> He really had this funny identification, you see. What do I care if the Department moves ahead? I only care if Claire Friedland moves ahead. But George really cared. And he cared about the profession in the same way. So much of his work dealt with issues concerning the profession. If you look at my catalogue of his papers, you'll see what I mean. There are a lot of categories under the heading 'Professionalism'. In fact, his loyalty even extended to abstractions: the Chicago School or neoclassical economics. Much of his work centered around saving the damsel in distress, neoclassicism, from her attackers: hence his work on the economics of information and his enthusiasm for the Coase theorem.[33]
>
> (Friedland 1993, 780)

It is here that Friedland connects to our invocations of the Demsetz and Rosen testimonials on what Stigler considered to be "red herrings" in microeconomic theory, on what is purely theoretical and consequently of little practical role. The adjective "purely" has no signal role, if it is not to be read as signaling this.

> What he thought about monopolist competition, aside from its being inconsistent with neo-classical economics, was that it wasn't productive. He felt very much the same way about game theory. He didn't question it as a theory. How can you question it? There is nothing to question. Strategies are all very fascinating. If they do this, I'll do that, and if they do that, I'll do this. How can you argue with it? It is purely theoretical. But George didn't see it as moving the profession along. It didn't seem to have any practical applicability. There wasn't much possible in the way of testing the theory. Maybe some people did feel that they were testing it. But there was nothing that he was impressed with. That's how he felt

> about extremely mathematical stuff too. But now I've gotten off the topic and I've gotten off of George. Friedland in Freedman (1997)

Stigler's exchanges with Samuelson on welfare economics, and the way that he initially adopted and defended his teacher's views on ethics in economics only to discard them in his Tanner lectures later, is a fascinating *volte face* that deserves an extended treatment.[34] Here we can only give Friedland's testimony some concluding play.

> He described externalities as that for which there are no transactions *at the present time*. On the working of the market, he was moved to say that "the degree of monopoly is in competitive equilibrium" and "competition results in offering fraud at a cheaper price." On the free-rider problem, he said, "If lunches aren't free, then rides aren't free; what we really have is the cheap- rider problem." In a debate with a former chairman of the Securities and Exchange Commission, he made his famous remark, "It is of regulation that the consumer must beware." He introduced his Ely lecture to the American Economic Association on "Economists and the Problem of Monopoly" by saying that if he were speaking to the American Monopolists' Association an equally appropriate title would be "Monopoly and the Problem of Economists."
>
> (Friedland 1993, 780–783)

And so where did Stigler stand on the two fundamental theorems of welfare economics? Did they constitute an apologia for a free-market, or were they impossibility theorems that brought in time and uncertainty, and thereby testified to the impossibility of such an apology. But are theorems, interpreted literally as theorems, which is to say interpreted tautologically as if-then statements, have anything to do with ideology or with science? We again fall back on the difficulty of distinguishing the science from the *belle lettres* biography.

Pocock on Concurrent Language Games

In the 1989 preface to his essays on "politics, language and time," John Pocock writes of the need for historians to "understand that the history of discourse is not a simple linear sequence in which new patterns overcome and replace the old, but a complex dialogue in which these patterns persist in transforming one another." Referring to neoliberal efforts to enlist the state toward the destruction of a mixed economy and the enforcement of universal privatization, he talks of the "Ogre now wearing a different face," and through the imposition of values of cost-effectiveness and profit-making speculation, openly showing its intention "to demolish universities and replace them with some species of tertiary education."

> Liberal learning, attacked twenty years ago because it did not lend itself to instant politicization of the culture, is now under attack because its products cannot quickly enough be repackaged and sold at a profit. [But] if speech is in any degree autonomous, it may fight the Ogre to win leisure for thought; language may reconquer time for politics.
>
> (Pocock 1989, xii)

In an earlier 1980 paper, representing a conversation between *ego* and *alter* in order to formalize the idea of speech as power play, he had already stated with a Wittgensteinian flair:

> Ego behaved as he did because he sought the power and the freedom to determine what he was going to talk about. He had reached a decision as to what it was that he wanted to talk about, and he declared that decision; whereof he could or would not speak, thereof was he silent, and he compelled Alter to be silent also. The affirmation, "If so and so were said, then the reply would be such and such," carried with it the plain implication, "And I do not want to discuss anything else." The erection of a set of conditions – intellectually scrupulous though it may have been – was a power play, a bid to set the rules of the game to be played.[35]
>
> (Pocock 1980, 146)

Pocock connects Ego's move to a "view of language in its Hobbesian state of nature, … a means of escaping from the ordinariness of ordinary language, even if this is only escape into second-order talk about ordinary language." As such, it may very well be the case that all interaction ceases, and Alter submits to Ego's power, but if "[o]ur universe is not to be transformed into a series of laboratory caves for Ego to reign over in the role of the mad scientist," the conversation must continue.

> If Ego and Alter succeed in carrying on conversation, each will be engaged in discovering what the other says and means to say, and – as an inescapable corollary – what he says and means to say himself.[36]
>
> (Pocock 1980, 147)

Ego is finding himself not by solipsistically introspecting about himself but by conversing with his "other." Pocock invokes Oakeshott's distinction between the "self-made and self-worshipping rationalist and the inhabitant of a continuum of behavior." He elucidates what he means when he refers to a continuum of behavior, and his elucidation merits extensive quotation.

> It is the presence of what we mean by ordinary language that gives political philosophy its character as history. Strictly speaking, the language in which it is conducted is academic and extraordinary, and it might be better to call it a tradition of discourse. But it shares with ordinary language and other kinds of tradition the characteristic that it is a continuum of behavior, compounded of material from many sources and available to many users in such a way that no one of them has unlimited power over it.
>
> (Pocock 1980, 147)

With this insight at hand, Pocock turns to a radiographic metaphor to explore what "tradition" means.

> Perhaps the key characteristic of tradition is that no single transmitter has complete knowledge or complete control of the messages he either receives or transmits; there is always the element of the implicit and perhaps the contradictory, which must escape his attention at any one single moment of transmission. The chain of transmitters must formally be thought of as open- ended and immemorial. It is the use of language – of language not perfectly controlled by its users – that constitutes the historicity of political philosophy as of many other activity.
>
> (Pocock 1980, 147–148)

And now we now have it: "always the element of the implicit and the contradictory." It is not clear whether one is controlling language or being controlled by language; whether the particular register the ego so willingly stepped in will not allow him to look beyond it. And to be sure, there is a communal consideration.

> The situation I have been describing is one in which the philosopher learns that he and his method share the world with other beings and other methods, some of which may be induced to counterpoint, enrich and limit the operations he practices. In a world thus described there is a plurality of powers and there are relations between these powers, and the political biases of my description may be said to be liberal.
> (Pocock 1980, 156)

Pocock not only wants to check ego, but also grant him freedom and his liberty.[37]

> Ego, then, has an unlimited right, but not an unlimited power, to set himself free from his interlocutor in a conversation or a tradition, or – what amounts to the same thing – from the uncontrolled historicity of his language. (Pocock 1980, 149)

The point, of course, valid as much for the narrow purposes of this essay, as it is for the writing of a history of ideas, is that intellectual history does not possess a unified and narratable trajectory. Pocock has recourse to the metaphor of *hypogriffs* at their simplest, models; and at their richest, myths. As we have seen, he counters the historical by the experimental.

> Speech-acts and utterances in political philosophy, when viewed as historical phenomena, must be considered as multi-dimensional. There must be a diversity of contexts in which, a diversity of levels on which, they may be and have been interpreted; and the content of any given message can be exclusively specified only under conditions which are experimental rather than historical. (Pocock 1980, 150–151)

It is experimental in that "reality" is being boxed in and curtailed by a hypothetical laboratory-like situation.

> It is the endeavour of Ego in the monodrama to substitute experiment for history, … and [while] at the end of the transmission, he is at liberty to reshape the message being received in the form that he desires to receive and respond to it, there may be going on elsewhere in the continuum … the effort to reconstitute the message at which it may have been at the time of emission, or at any intervening moment of transmission.
>
> (Pocock 1980, 150–151)

Pocock sees two advantages in the training of a philosopher, theorist if one prefers, to see utterances in their historicity.

> First, [the] capacity to receive and process information of different kinds will be vastly enlarged, while nothing [would] diminish his capacity to screen out information which he desires to omit from consideration. [Second,] this training will enhance his capacity to view himself as a historical being and the acts of thought and speech he performs as historical acts: to recognize that he is making choices and transmitting messages which are conditioned but which modify the structures that condition them, and that the historical world he inhabits is shared with others and governed by the law of heterogeneity of ends, so that not all the consequences of his acts will be those he can foresee, intend or desire.
>
> (Pocock 1980, 154–155)

Pocock begins his magisterial 1987 paper with the observation that "one cannot verbalize a practice without offering a theory" but that he does not want to find himself "affirming and defending a general theory of language and how it operates in politics or in history, still less to offer an account of [his] kind of historian as himself, a historical actor or agent." As Khan (1993a) already pointed out, Pocock (1987, 21) sets out general methodological guidelines, and provides his own criteria: the currency test, the Monsieur Jourdain test, the experimental or refutability test, the serendipity test, the anachronism test.

> We have to get from the merely possible to the theoretically testable, to frame hypotheses about a realm of meaning which may have been present and operative, such that they will enable us to say whether, as a matter of historical fact, it was or not. The latter part of this operation – the verification – must obviously be conducted by methods which are rigorously historical; the former may be, but need not. The point of immediate importance … is that a heuristic construction does not become a historical hypothesis until it is reworded in such a form that it can be tested by the rules of historical evidence.[38] (Pocock 1985, 30–31)

This insistence of the inquiry being "rigorously historical" is precisely an insistence that it not be experimental. Ironically, Pocock's prose is reminiscent of Friedman's (1953) prescriptions on the methodology of positive economics. Friedman rules on a model by confronting its implications with the data; Pocock (1975, 390) rules on a hypothesis by confronting it with the "language patterns available in the author's time," a recognition that the historian is also a historical actor and agent, a player in his or her own right, and one needs constant assurance that the language he or she identifies is not her own fabrication. Instead of rewording, Friedman insists on redefinition so that the variables are "observable and measurable." However, this distinction between "figures of speech" and "figures of arithmetic" is not an unimportant one,[39] especially when the distinction between the histories of economic doctrine and of economic analysis is being drawn. This is a reworking of the Patinkin-Stigler contestation but in a register that is not statistical.

It is an amusement perhaps afforded only to those with an interest in intellectual history to see how the word "classical" keeps floating from one body of work to another. It is thus fitting to ask with Condren. Whether free-ranging answers involve exploitation and use, emblem and authority, inherent and purposive ambiguity,[40] all having to do with eristic debate and disputation, and pointing out the illegitimacy, if not the mischief, of our substitution of the word "economic" for "political" in his text.[41] Indeed, it is this substitution, lack of complementarity if one prefers, that leads us directly to the difference between the writing of the history of economic thought, a story of doctrine and disputes, and the writing of the history of economic analysis, a story of theories and theorems.

Does an evaluation, a placement, necessarily involve grappling with categorial distinctions, say of Pocock and Winch, that revolve around expropriation versus appropriation, on recovery rather than recruitment?[42] In the writing of the history of economic theory, is there a permissiveness that sanctions books from Plato to Nato, Aristotle to Arrow,[43] and sees absolutely no danger of the seductions of doxography, to the writing of books which

> … start from Thales or Descartes, and wind up with some figure roughly contemporary with the author, ticking off what various figures traditionally called 'economists' had to say about problems traditionally called 'economic,'… knowing in advance what most of their chapter headings are going to be.[44] (Rorty 1984, 62)

In some sense, the criterion for one type of history has been stood on its head for another type of history. At best, Stigler's personal exegesis, perhaps not-altogether scientific identification of the rubbish, flotsam and jetsam, of economic science, comes closest to *geistesgeschichte*, Rorty's identification of a subgenre within "rational reconstruction."[45] This involves an attempt to give "plausibility to a certain image of philosophy, rather than to give plausibility to a particular solution of a given philosophical problem – to describe the philosopher in terms of his entire work rather than in terms of his most celebrated arguments … a self-justification in the same way as rational reconstruction, but on a different scale (pp. 56-57)."

We can then ask whether there is a need to distinguish a history of economic thought, of doctrine and ideas, from the history of economic analysis and of economic theory? and if so, how is one to do it? When we are writing the history of economic doctrine, of eighteenth-century arguments for free trade, or of the seventeenth-century views of the poor as a productive resource,[46] it is the self-interest and the self-deception of the writer that must be somehow policed. It is the asking for policies that protect not only the historian from himself, but also his constituents: in his overweening ambition to obtain a dominating position for himself, a singularly commanding view, and a consequent surrender of his discipline to science, relevance, reality or whatever coin currently in service, he may be landed out of his profession with neither a view nor a position, a success totally ruinous, socially and privately, because the guardian's intellectual standards have become so rigid and constricting that they cannot but be seen as anti-intellectual. Skinner (1969) focusses on the identification of intention as a possible antidote.

> No agent can eventually be said to have meant or done something which he could never be brought to accept as a correct description of what he had meant or done. [This excludes] the possibility that an agent's account of an agent's behavior could ever survive the demonstration that it was itself de- pendent on the use of criteria and description and classification not available to the agent himself. (Skinner 1969, 28)

For a historian of economic analysis, perhaps one can draw a parallel to the activities of the historian of art.[47] One painting by itself is not sufficient—the entire *oeuvre* must be considered, and perhaps this consideration must be supplemented by a fuller consideration of the painter himself. Rather than simply stop at the scientific, one would like to have a better understanding of how science is being conceived, to get to know an author better, to see how he or she translates problems of economic life, present or the past, into constructions of mathematical science, a translation of the good and the beautiful into the optimal and the efficient. And rather than the final product, the dynamics of the struggle toward the finishing rather than the rationally finished product, the stutterings and stammerings of the striving toward the rational are to be noted. As such, the division between theory and history, even if one limits oneself to technical economic analysis, is elusive and ephemeral indeed.

Stigler's Mathematical Method and Its Psychological Pitfalls

Stigler's fourth LSE lecture turns to the mathematical method in economics, and is worth a careful reading not only for the substance of his claims but the rhetorical structure that he gives to those claims.

He begins the lecture as an outsider to mathematical economics, leave alone to mathematics, and considers four claims for the subject to be included as an integral part of mainstream economics. Next, by necessity, he expands on what he considers to be the aspirations of scientific economics, and then concludes with suggestions on the avoidance of the “psychological pitfalls of the mathematical method” in economics. It is an unguarded, off-the-cuff *tour de force*.

But to begin at the beginning.

> Mathematical economics is riding high. It is the non-mathematical economist who is becoming apologetic. Indeed he is becoming, in some circles at least, a sort of eavesdropper on serious economics, gleaning what instruction he may from the words between the formulas. As one such eavesdropper, I imprudently propose to offer an estimate of the proper role of the mathematical method in economics. (Stigler 1949, 37)

It is clear that Stigler here is altogether too modest in referring to himself as a non-mathematical economist, and is rhetorically setting the stage for what is to be his seminal contribution to the economics of nutrition, pioneering what were to later become the state-of-the-art results in linear programming.

Indeed, it is rather extraordinary that delivering his lecture a good two years after the *Foundations of Economic Analysis*, and a decade after *Value and Capital,* had been published, he felt the need to praise mathematics. He set the stage

> Let us set the stage by admiring mathematics. Mathematics is a powerful and a beautiful method of reasoning – it is the poetry of logic. Its flexibility and versatility are extraordinary. Mathematics is indeed the queen of the sciences. These praises should be set forth emphatically. It is not a tautology that the proper use of mathematics never hurts an economic analysis and often improves it. [It] is the premier language of logic.
> (Stigler 1949, 37)

Stigler repeats his need to “offer an estimate of the proper role of the mathematical method in economics”: this need was a derived need.

> Having lavished great, but only proper, praise on mathematics, I wish to examine its proper role in economics. I wish to explore special claims for the mathematical method, claims that transcend its admitted power and usefulness. (Stigler 1949, 38)

To be sure, Stigler is scrupulous about propriety, and the word "proper" is important to him. His self-image of an eavesdropper and a layman leads him to characterize his suggestions as "imprudent" [and] "necessarily based upon authoritative hearsay."[48]

> Because mathematics is the premier language of logic, it is a method: a method of drawing exact deductions from given premises, and of verifying the logical consistency and adequacy of the premises. It follows that mathematical economics is a thing without content. (Stigler 1949, 7)

This is a point of view of some importance. Stigler elaborates his skepticism even about the use of the appellation "mathematical economics," arguing that one "revolts" against "$\chi 2$ economics or historical economics."

> Economics may be divided into various more special disciplines, but surely only the substantive bases for classification have any usefulness. The theory provides another tool of research, but we should all revolt at the thought of $\chi 2$ economics. History is another eminent resource of the economist, but we have all revolted against historical economics. A classification of branches of economics by method is not even clear-cut: our most mathematical economists make frequent use of words and occasional use of empirical material. (Stigler 1949, 37–38)

What Stigler seems to be doing here is to distinguish between the technical and substantive registers. But this is a contextual matter. Every register has its own sub-registers. Probability has its substantive and intuitive mainstream—coins, roulette wheels, dice, a pack of cards—as it does its mathematical underpinnings in measure theory. Statistics has its methods and its theory. Leaving such disaggregations aside, let us proceed with the rhetorical composition of the lecture. Stigler contests four claims for the mathematical method.[49]

Claim 1: *Mathematical method necessarily leads to good economic theory.*
Claim 2: *Mathematical exposition has an inherent clarity.*
Claim 3: *Mathematical method is indispensable in certain types of analysis.*
Claim 4: *[T]opics involving complicated formal reasoning are so numerous and important and pervasive in economics that mathematics is virtually indispensable and should therefore be a part of every economist's equipment.*
Corollary to Claim 4: *Mathematics is a suitable language for communication.*

Stigler restates the second claim "more conservatively."

Claim 2′: *The mathematically trained economist states his concepts more clearly, on average, than the unmathematical economist.*

With regard to his first claim, Stigler dips into Straussian waters: the esoteric versus the exoteric, the explicit versus the implicit.[50] He writes

> Rarely, if ever, is so bold a claim made explicitly, but there are implicit positions that come close to it. G. F. Shove, in his excellent centenary essay on Marshall, seems to have taken this position when he explained the theoretical skeleton of the *Principles* as the product of Ricardo's theories and Marshall's differential calculus. We know that this is not true: the *Principles* is the product of Marshall's magnificent brain, not of his calculus. Mathematical training will not turn a mediocre economist into a great economist – a statement most mathematical economists will immodestly accept. (Stigler 1949, 38–39)

Indeed—who said that it would?

In the context of the second claim, Stigler cites Samuelson, and locates it in Poisson.

> Even when the mathematical economics is wrong (and it is not an indictment of a method that it can be misused), it is clearly wrong and eagerly invites correction and improvement. [M]athematics has no symbols for confused ideas. (Stigler 1949, 39)

Through a contentious digression into the work of one N. F. Canard,[51] Stigler writes

> The belief, however, has neither historical nor logical foundation. The Poisson view is objectionable not merely because it is untrue, but because it is almost the opposite of the truth. It is an insulting restriction on the usefulness of mathematics to credit it with the ability to deal only with clear concepts. The history of science gives us good reason to believe that every concept of modern science will be found to be ambiguous at some future time. (Stigler 1949, 40)

At this point, the authors, if not the modern reader, is at sea with what Stigler is trying to say. In any case, he delivers the punchline.

> Therefore a snobbish mathematics would be unusable at present. It is as if one were to assert that language is only for the expression of pure thoughts: we have also mathematical pornography. (Stigler 1949, 40)

Stigler turns to his amended claim, finds it ambiguous and relapses into second-order thinking on the "clarification of clarity." He writes

> I suspect that in this more plausible version the claim is ambiguous: clarity may be confused with familiarity or susceptibility to logical manipulation. But even if the concept of clarity can be clarified, it is difficult to conceive of a method of testing the claim; it seems necessary for each person to accept or reject it on faith. (Stigler 1949, 40)

Stigler concedes the third claim but is conflicted regarding it: his concession is based on the criteria of speed, certainty, and completeness.

> It is not a sweeping claim: it does not say that complicated theorems cannot be invented without the mathematical method, nor does it say that complicated theorems cannot be understood without mathematical knowledge. But it is a substantial claim: there are types of economic analysis, some of which even the non-mathematical economists have deemed important, that can be executed more swiftly, more surely, and more completely with the mathematical method. (Stigler 1949, 40)

Finally, we get to the fourth claim, one that Stigler deems to be the most important for the mathematical method in economics. The text elaborates that from which the claim draws its legitimacy.

> [T]he topics involving complicated formal reasoning are so numerous and important and pervasive in economics that mathematics is virtually indispensable and should therefore be a part of every competent economist's equipment. Even if it is possible, it is certainly foolish to be a non-mathematical physicist, and the same thing is true of economics. (Stigler 1949, 40)

However, when Stigler takes the claim and the corollary together, he concludes that "At present [it] this cannot be judged conclusively: one's decision must turn on his view of the proper nature of economic research in our times. I personally reject the claim, the analogy to physics, and the view that mathematics become a mode of communication among economists."[52]

What is most interesting is the devaluation of mathematical methods, and of economic science more generally.

> The economist as political adviser may feel the need for acting on a larger and livelier stage, but on this stage his effectiveness will depend chiefly upon his unsystematized knowledge and intuition – his judgement, if you will – and very little upon his scientific knowledge. In this early stage of scientific economics, the mathematical method is not very important: there are not enough established economic uniformities to permit of useful generalization on the grand scale. (Stigler 1949, 41–42)

Along with a devaluation of mathematics, there is a valorization of what is seen as its "other."

> Verbal methods of reasoning can still solve most problems efficiently and economically: surely one need not argue the proposition that verbal reasoning can also be extremely subtle and complicated and rigorous – and even beautiful. It is for this reason that we are able to find among the leading economists of our generation a large number of wholly

> non-mathematical economists. Hence I conclude that mathematics is not indispensable. (Stigler 1949, 41–42)

All that Stigler is willing to grant to mathematics is that it is useful, but he gives with one hand what he takes from the other.

> It is useful, and so is formal statistics, and so is knowledge of foreign languages, and so is knowledge of other disciplines. The study of mathematics is a most desirable part of one's education, and so are the other disciplines; none can yet make a claim to special recognition in economics. Which is most useful still depends upon the man and his problems. (Stigler 1949, 41–42)

Earlier, Stigler had conceded to the liberal impulse to leave the choice of the problem to the chooser, but when Cournot makes his choice explicit, and more importantly, makes the criteria of his choice explicit, Stigler retreats and reacts.

> Cournot, one of the very greatest of the mathematical economists, begins, "I have put aside questions to which mathematical analysis cannot be applied…" This method of choosing problems is doubly and pathetically wrong. (Stigler 1949, 43)

What is of interest is that it is wrong on two counts that are in some sense orthogonal to each other. It is wrong because of two limitations, a limitation of the subject and the limitations of one who is studying the subject.

> It is wrong, first, because almost always the economist has a very limited command of mathematics and must therefore limit severely or contort the problems he examines. Perhaps he must avoid the study of stocks of goods – he does not know the theory of integral equations; perhaps he must assume that time processes move smoothly – he had only the beginning course in differential equations. It is wrong, second, because mathematics, for all its vast power, is dreadfully weak when alone. It cannot answer so simple a question as whether a locomotive has a higher cost

> of production than a toothbrush, or whether monopolists usually seek to minimize profits. (Stigler 1949, 43–44)

At this point in the composition of his lecture, which is to the point after he had addressed his four claims, Stigler turns to the "psychological pitfalls of the mathematical method." He lists three options: *abandonment, immersion* and *translation*. We take each in turn.

His dismissal of the *abandonment* of mathematics is of epistemological consequence.

> [Abandonment] cannot appeal to a real scholar. We should have to abandon every weapon of research – and with them the search of truth – if we retained only those which cannot be abused. (Stigler 1949, 44)

It is of consequence, because like most of Stigler's claims, his assertions prove fecund and fruitful for the moving of thought forward. And so one can usefully interrogate what appear to be the most straightforward of his *dicta*. What makes a scholar real? What is real scholarship? Is it useful scholarship, and if so, to whom? And to him, her or it, what is the use and purpose that it serves? Or is it scholarship for itself? a fulfilment of the ends that it, itself, has set for itself? Curiosity toward some end or idle curiosity which may eventually engender its own ends? And the distinction between use and abuse of the weapons of research? And research itself? what kind of search is that?

What we refer to as *immersion* here is put forward simply as neutral prescription and possibility that "economists can study more mathematics."

> This choice has merit: by increasing our mastery over mathematics, we can decrease its mastery over us. The increase of mastery over mathematics, however, is not free. The budget equation of the mathematical economist applies also to himself: he purchases mathematical literacy with economic illiteracy. An economist, after all, is not an unemployed mathematician. (Stigler 1949, 44)

But why cannot both kinds of literacy increase simultaneously?[53] What kind of partition of knowledge is this that necessarily involves a zero-sum aspect to it? Does mastery of one necessarily exclude mastery of the other. Why such a static conception? Stigler persists and gives into a liberal impulse.

> Each person must decide his preparation for himself, in light of his aptitudes and interests. But he should make this choice with open eyes, and he should not assume that gaps in his non-mathematical training are necessarily easier to fill than those in his mathematical training.
> (Stigler 1949, 44)

And to be sure, the friend–enemy dichotomy is implicit: it is "them" versus "us." But more importantly, there is a closure which is the cause of the problem. It is mastery itself rather than an open-ended striving for mastery that is at issue.

And Stigler, nothing if not an influential text-book writer,[54] does not ignore teaching and the curriculum.[55] There is talk of "negative costs and replacements."

> Although the study of mathematics has a substantial alternative cost for the graduate student, the cost would be small or negative at an earlier stage. Differential calculus would be an admirable replacement of high-school economics and civics. (Stigler 1949, 44)

By the time that Stigler gets to the question of *translation*, the psychological pitfall slides into an imperative. It is the profession, and the efficient allocation of the resources of the profession that is at issue. There is a clear distinction between the members of the profession and their "others."

> It is undeniable that the profession contains many very able economists whose mathematical attainments are meagre or less. If the mathematical economist's results are suggestive or useful, these people have a right to know them. If the results are tentative and conjectural, these people have a right to test them. (Stigler 1949, 45)

Use has again entered the picture but without a specification of whose use is at issue, and when does an instrument, a weapon of research, become useful from being merely basic to its identity. And who are "these people" whose mathematical attainments are counter-balanced by their non-mathematical attainments (economic attainments if one prefers) to such an extent that their position within the profession is unquestioned and secure. And again, one is naturally led to inquire into the writer's stance regarding these people. Is he one of them? Or only one with an attunement and understanding with them? Are they his people? The text is back to the business of *real* scholarship, and it is here the identity of the people at issue is unmasked and the question of *submission* made explicit.

> [I]n his publications the mathematical economist can provide along with his equations a translation of his results into words. This solution, I submit, is the correct one. (Stigler 1949, 45)

And once the solution is found, it must be implemented.

> The mathematical economist can, if he wills, always meet this obligation. Even when the details of the proofs must be shrouded in a fog impenetrable to the non-mathematical economist, the assumptions and the conclusions can always be stated clearly in the language of words, and heuristic derivation of the conclusions is probably always possible. It is the fundamental obligation of the scholar to submit his results and methods to the critical scrutiny of his competent colleagues in a comprehensible fashion. The failure to provide these translations is a renunciation of the canons of scholarship. (Stigler 1949, 44–45)

Desirability is confronted by necessity: translation is good for the translator but "not merely desirable but absolutely necessary for the profession" whose doors the mathematical economist is knocking at. But he first considers "desirability from the viewpoint of the mathematical economist himself"—the secondary hazards that are involved.

> The fundamental point is that from the viewpoint of the profession, the translation is absolutely necessary, not merely desirable. It is the fundamental obligation of the scholar to submit his results and methods to the critical scrutiny of his competent colleagues in a comprehensible fashion. (Stigler 1949, 45)

Stigler does not ask any questions as regards the payment to the translator, the incentives that lead the translator to became a translator—the translator's budget constraint, and the price and the professional valorization that undergirds the activities and choice of the action of the translator.[56] Never one to leave an action un-interrogated, the author lists three causes for this failure: laziness, snobbishness and a sense of shame, excuses for the mathematical economist not providing the translations that are necessary.

> The failure may be justly, if harshly, attributed to one of three causes. Laziness. Or snobbishness. Or a sense of shame at the abstractness of the analysis – accompanied by an illegitimate desire to talk of the real world before one has taken into account its central features. These are not good excuses, but I can find no other. (Stigler 1949, 45)

And finally a concluding lament.

> The queen of the sciences should not be made a puppet of a scientific oligarchy. (Stigler 1949, 45)

Lest one think that this reading is highlighting a young, perhaps even immature, George Stigler, it is important to realize that his views remained fairly constant over the years. Thus, in his review of Hicks' "Revision of demand theory," Stigler continued with the technical-substantive distinction, and devalued the technical.

> [T]he volume elicits strong admiration, for it is a superb pedagogical performance: lucid, orderly, and ingenious in the exposition of modern utility theory. On the other hand, the volume is of little substantive interest: its chief purpose is to reach well- known theorems by the use of elementary tools. One is compelled to question the significance of the task while admiring the performance. (Stigler 1957, 169)

Stigler remains fixated on the reader's budget constraint.

> The derivation of the theorems on consumer behavior previously presented as the mathematical appendix to Part I of Value and Capital is

> presented here in the language of preference theory. Hicks persuasively argues for the greater economic meaningfulness of an elementary derivation of these results (pp. 18-19), but it is an open question whether the cost in terms of concentrated attention leaves much reader's surplus. (Stigler 1957, 169)

But it is really the 1988 review of *The New Palgrave* that is most revealing. The editor of the *Journal of Political Economy* evaluates the editors of the dictionary, and finds their performance wanting: his criticisms are listed under the headings of "minor" and "major" aspects, with the latter classified under four further categories as follows: (i) exclusion of empirical material, (ii) severely technical articles, (iii) essay on living economists and finally, on the (iv) Marxian perspective. The first two are the most important for this essay. With regard to the first, Stigler gives two reasons.

> The first is that many empirical findings of economics have histories that are at least as interesting and important as most theoretical developments.[57] The second reason is the falsity of the implied obverse,[58] that theoretical discussions are of permanent interest. The profession has its fads and misdirections, and our past is strewn with them.[59] It is not necessary for my purposes to predict which of these essays are terminally ill, because I am arguing only for a selective inclusion of empirical articles. (Stigler 1988c, 1701)

In 1988 he continues to refer to mathematics as weapons of war in their use in *Palgrave* articles, and remains the defender of the "tolerably competent economist." Stigler feels the need to give arguments against having such articles.

> The first is that they are never completely necessary: It is always possible to give heuristic or intuitive explanations of the problem, its method of solution, and its solution.[60] The second reason is that the technical expositions are often extremely compact, so they are really accessible only to fellow experts (who scarcely need them); learners will be driven to more spacious presentations. The final reason is that these "state-of-the-art" essays will rapidly go out of date: A dictionary is no rival to a journal. (Stigler 1988c, 1731–1732)

The expert on perfect "competition, historically contemplated" has no compunction in ignoring entries on *perfect competition*; the pioneer of the economics of the information does not use the entries on *incomplete markets* and on *hidden actions* as an opportunity to reflect on the fact that the more incomplete markets are, the poorer a description price theory becomes of economics. This simply refers to the tension in Stigler in defending price theory and doing work in information economics and industrial economics that undermines the centrality of prices as "aggregators of information and incentive-providers to act on that information," in the language of his colleague, Hayek.

We conclude our reading of Stigler (1949) by seeing the extent to which his views regarding the threat of mathematical economics were still operative as late as 1973. Leijonhufvud was to say in his ethnographic study of the economics profession:

> It has become increasingly clear that the Econ associate certain, to them significant, beliefs with every modl, whether or not they also claim that modl to be a "useful tool." That taking "usefulness" as the point of departure in seeking to understand the totemic culture of this people leads us into a blind alley is particularly clear when we consider the Math-Econ caste. The Math-Econ are in many ways the most fascinating, and certainly the most colorful, of Econ castes. There is today considerable uncertainty whether the "priest" label is really appropriate for this caste, but it is at least easy to understand why the early travellers came to regard them in this way. (Leijonhufvud 1973, 333)

Schmitt's Concept of the Political: The Enemy–Friend Dichotomy

What has Carl Schmitt's "concept of the political" to do with George Stigler's views on the use of mathematics in economics, or how the economics of the past masters is to be read, interpreted and understood, by and in the present? Why Schmitt?[61] In answer, we can do no worse than begin with an extended citation from his influential classic.

> The friend and enemy concepts are to be understood in their concrete and existential sense, not as metaphors and symbols, not mixed and weakened by economic, moral, and other conceptions, least of all in a private individualistic sense as a psychological expression of private emotions and tendencies. They are neither normative nor pure spiritual antitheses. (Schmitt 1976, 27–28)

Mouffe (1993, 1999) builds her political theory on "pluralistic agonism" and on the "return of the political."[62] In the delineation of the "we," and of the "us versus them," Schmitt's sentences that we use as the third epigraph to this chapter, are crucial. The sentiment they articulate is violent, and it has rather untoward and disagreeable consequences. In particular, it connects to Stigler's sentences used as the first epigraph of this chapter. They assert that only an economist can understand an economist, only a woman can understand a woman, only a member of the LGBTQ community can understand what are the fears of an individual who belongs to that community, only a Pakistani-Scot can understand a Scottish-Pakistani. More sharply, and bringing the point closer to home, only the non-mathematical, mainstream economist can understand and check the anxieties that are generated by his or her mathematical "other" wanting to be seen as part of his or her community. Schmitt continues on the autonomy of the distinction.

> Emotionally the enemy is easily treated as being evil and ugly, because every distinction, most of all the political, as the strongest and most intense of the distinctions and categorizations, draws upon other distinctions for support. This does not alter the autonomy of such distinctions. (Schmitt 1976, 27)

And as every practicing mathematician knows, a proposition typically has its converse.

> Consequently, the reverse is also true: the morally evil, aesthetically ugly or economically damaging need not necessarily be the enemy; the morally good, aesthetically beautiful, and economically profitable need not necessarily become the friend in the specifically political sense of the word. Thereby the inherently objective nature and autonomy of the political

> becomes evident by virtue of its being able to treat, distinguish, and comprehend the friend-enemy antithesis independently of other antitheses. (Schmitt 1976, 27)

To emphasize, the friend–enemy category is indigenous and autonomous, and does not derive its identity from other categories. It is in this connection that Schmitt roots the friend–enemy antithesis in the public–private antithesis. In an important footnote, he connects his antithesis to Plato's distinction between the "public enemy and the private one." He approvingly cites the eighteenth-century philologist Forcellini in an important footnote.

> A public enemy *hostis* is one with whom we are at war publicly … in this respect he differs from a private enemy. He is a person with whom we have private quarrels. They may also be distinguished as follows: a private enemy is a person who hates us, whereas a public enemy is a person who fights against us. (Schmitt 1976, 29)

Schmitt elaborates the registers involved. The enemy in the "political sense need not be hated personally, and in the private sphere only does it make sense to love one's enemy, i.e., one's adversary."[63] Furthermore, the idea of a collectivity is essential to his notion of an enemy: how "an enemy exists when, at least potentially, one fighting a collectivity of people confronts a similar collectivity." It is an existential struggle, with war and annihilation of the "other" as the only action available. Are you now, or have been a member of this or that party fighting against us? The choice of an enemy trumps that of the friend.

> Liberalism in one of its typical dilemmas of intellect and economics has attempted to transform the enemy from the viewpoint of economics into a competitor and from the intellectual point into a debating adversary. In the domain of economics there are no enemies, only competitors, and in a thoroughly moral and ethical world, there are only debating adversaries. The concern here is neither with abstractions nor with normative ideals, but with inherent reality and the real possibility of such a distinction. The enemy is not merely any competitor or just any partner of a conflict in general. (Schmitt 1976, 28)

In a brilliant reading that connects Schmitt to Levinas via Hobbes, Botwinick (2016) explores the objectivity of Schmitt's distinction as grounded in Hobbes' *nominalism* and *conventionalism*. He writes

> The thesis that our knowledge claims can never be fully grounded, Schmitt takes over from Hobbes the nominalism (the theory that only individuals and no abstract entities, such as essences, classes, or propositions exist) and the conventionalism (the notion that some of the most enduring institutions of social life such as language and government are formed by agreement or compact) that, in their skeptical import, converge in highlighting how theory is underdetermined by fact and words are underdetermined by things. (Botwinick 2016, 350)

The point is a subtle one. When there are no criteria, and basic terms remain allusive and undefined, the lack of criteria becomes the criterion. When there is only subjectivity, that becomes the objectivity. Botwinick puts it all this way.

> In a strange and paradoxical way, it is rather the absence of such criteria that renders the objectivity of the political possible. From Schmitt's perspective, the political in his sense as the embodiment of the friend/enemy distinction is triumphantly objective because it excludes reference to all other criteria (especially moral, aesthetic, and economic) in its delineation and differentiation of itself. One might almost say that for Schmitt it is the absence of objective criteria in the conventional sense – those stemming from adjacent disciplines and sectors of experience such as morality, aesthetics, and economic – that makes the political objective in his sense. Schmitt has to first subvert objectivity before he can claim it for his conception of the political. The missing intervening premises between that subversion and his revised conception of the political as institutionalizing the friend/ enemy distinction appear to be the Hobbesian postulates concerning skepticism. (Botwinick 2016, 351–352)

So who is George Stigler's friend and who is his enemy? What is his self-image? and his image of his chosen profession? and how does it feed into giving productivity to Schmitt's dichotomy the bearing that this dichotomy on the development and teaching of economic theory.

And how can a mathematical economist be the enemy of a mainstream economist? What existential issues are at stake? What absurdity is this? Perhaps here, the following thought also helps?

> In classical liberal theorizing, the notion of a self (as well as the allied notions of friend and enemy) like the kindred container notions of *thing, substance,* and *physical object* are all highly questionable.[64] In this stream of theorizing, there is always presumed to be an unwarranted leap between experiencing both an individual sense-datum and a phenomenologically grasped putative physical object or person, and naming. One can never be sure that the name exhausts or even covers the experience and one even has to question ones questioning of the adequacy of the name. (Botwinick 2016, 350)

It is all a question of naming and necessity: science, economic science, political science, biographical science, social science. But the point goes to the basic issue whether you form judgments about reality through the lens of one's self or the other way round: how one conceives who one is. Let us give Botwinick the concluding words for this section.

> The notion of a self can be floated but only at the cost of recognizing that the movement from individual impressions of engagement in thought and action to the extrapolation of a self involves a leap whose distance can never be rationally neutralized. The notion of a self might be a total fiction, or we might be constituted by multiple and successive selves. The other in the guise of friend or enemy involves comparable leaps and elisions. (Botwinick 2016, 355)

But it is at this point that the chapter doubles back and connects to Claire Friedland and Stigler as they present themselves and each other in section "Professional Reception of Stigler's Categorization" above.

Toward an Overview: A Collection of Strands

This has been a repetitive and meandering essay, and in this concluding section, there is a need to bring its meanderings and repetitions to some sort of closure.[65]

The word *scientific* is an important technical term for George Stigler.[66] The point of this essay has been based on it: based on the consideration of Stigler's dichotomy between textual and *scientific* exegesis, and how it undergirds the analogy—false, productive or otherwise, but rampant in the Chicago of the time—between a statistician and a historian. However, as we have seen in section "Professional Reception of Stigler's Categorization," even within Chicago there were tensions and dissonances in the application and execution of this analogy to what was being read at the time. But leaving these aside, the point is that the analogy draws its power by giving, and assuming, a pre-eminent position to a contemporaneously monolithic view of what *science* is, and therefore what economic *science* is. In the preface to his celebrated anthology on the *Chicago Studies in Political Economy*, Stigler talks of economics as an "optimistic *science*," one that involves "frontal attacks" versus "higher levels of comprehensiveness and precision."

> Our adherence to free trade despite the perpetual state of protectionism entitles us to be called practitioners of the optimistic science. [I]t is for society at large to make what use it wishes of this new and better knowledge. (Stigler 1988b, xvii)

It is a cumulatively progressive view of economics based on a cumulative progressive view of science, perhaps that of an optimistic mid-century social science positivist rather than an altogether more recent conception of science as a social enterprise. The historian of economic thought is to read the history of his subject with such a view at hand. The view has, by necessity, to be unyielding if it is to yield all that it is expected to yield: a professional history of the subject as conceived by the profession, and the officers of the profession who have made the profession their own. He has to read and write an official history. Whatever the profession thinks to be *scientific* is to be extracted, homogenized and brought into alignment, and the rest is to be discarded. It is an important counter to Oakeshott's 1933 offhand, but seminal, remark that "thinking is not a professional matter."

This view of writing the history of economic thought is clearly ill-at-ease with the distinction between the history of economic doctrine and the history of economic analysis. The history of doctrine is not easily extracted through the disciplinary pincers of a particular subject in its contemporaneous identity, be it of economics or of politics, or indeed, of sociology, anthropology, or behavioral psychology. It must be seen as without arbitrary exclusions. Stigler himself had asked a basic question regarding boundaries within the field of political economy.[67]

> Once we raise the question of the nature and functions of the political institutions within which our economy operates, where do we begin and where do we stop? (Stigler 1988b, ix)

He had proposed yet another analogy, but only to reject it.

> An ideology … is a commanding set of beliefs, beliefs that are probably not grounded upon self-interest or are related to the interests of the holders in so subtle and obscure a manner as to make it more useful to treat beliefs as data. Ideology therefore plays the role in analyzing political phenomena that tastes play in analyzing ordinary economic behavior of individuals. (Stigler 1988b, xii)

Stigler's rejection was based on his expertise in market, or industrial, organization.

> The analogy is not satisfying, however, because political activity is conducted by organized coalitions, and a closer economic analogy is to the theory of the firm, and the association of firms (cartels) – where tastes play a most modest role overshadowed by the pursuit of profits.
> (Stigler 1988b, xii)

But leaving these two analogies of statistics/history and tastes/beliefs aside, and going back to the *pastiche* that constitutes this essay, several other questions come to mind. What is the distance between the ego-alter dichotomy that we see Pocock delineate in section "Pocock on Concurrent Language Games," and the enemy–friend categorization that we see Schmitt articulate in section "Schmitt's Concept of the Political: The Enemy–Friend Dichotomy?" How does the unscrambling of transmitters, a conversation and friendly rivalry, a spirit of authorial competition, become an existential life-and-death struggle? If the connections that are made in this essay are worth making, one can then ask, if only as an as-if[68] working hypothesis regarding their fecundity, who is George Stigler's enemy and who is his friend? And a glimpse of an early answer, at least as a first iteration, can be had in the chapter on the "economists' traditional theory of the economic functions of the state" in Stigler (1975).[69] There he describes two views: a "reading of historical reality" versus one "dictated by the necessities of optimal economic organization." He mentions Arrow/Samuelson as holding to the first view and Pigou/Lange as subscribing to the second. And with these strawmen in place, concludes:

> Neither the cynicism of the first view of the state nor the unreasoning optimism of the second view provides a basis on which the economist can make responsible policy recommendations. (Stigler 1975, 113)

But to stay with the question, are these his enemies or simply professional debating partners. And how about Gerard Debreu and Milton Friedman respectively.[70] And given the *hegira* of the Cowles Commission from Chicago to New Haven, is there some transitivity in these relationships: Tjalling Koopmans is also an enemy by virtue of being the enemy of a friend.[71] But why is a mathematical economist Stigler's "other"? Is it simply ideology? Does the abandonment of the history of thought have anything to do with it? Why did Stigler and Friedman try to keep articles on mathematical economics out of the *Journal of Political Economy* and give the Chicago organ the tilt that they did? To be sure, a great scholar is always best understood by the trappings and pronouncements that he or she uses to understand and trap others, and the answers may well lie more in Stigler's worldview than in his expertise of industrial organization.

They lie also perhaps in the commodification of economic science to which he was totally committed. A commodity that has to be sold to those who demand it, the state and the commercial corporations that are part and parcel of it.[72] And surely for, and in, this commodification, there ought to be no ambiguity, and consequent plurality, of the nature of the commodity that is being sold. If the nature of the commodity is not clear, the equilibrium price can hardly be the right price: we are in the world of Akerlof, Spence, Stiglitz where market failure is constantly courted, and more often than not, not overcome. Thus it all comes down to the matter of packaging and branding, and through this, in the identification of who the sellers are.[73] Other criteria and categorizations needlessly interfere in this. Despite Stigler's protestations concerning truth and science—sometimes, he surely protested too much—it is not really a matter of truth but of use, not really a matter of science but of commerce. Surely, market structures are determinative in how a commodity is to be defined and identified, or to rely on the more modern Dixit-Stiglitz vernacular of Sen's original rendering, it is all a matter of the "choice of technique" and the optimality of the equilibrium commodity spectrum: the point at which the wine is to be drunk and the tree to be chopped down. And presumably, that is how the subject shades also into curriculum design—into the abandonment of economic thought and the necessity of keeping the technicalities of mathematics at bay, a measured distance to both.

But this is only an exploratory essay—these second-order theoretical speculations must be subjected to rigorous empirical tests. This is the minimum that the authors owe to a great twentieth-century economist.

Notes

1. These fragments are from a section titled *How to Read a Book* in a 1969 article titled "Does Economics have a useful past?" Stigler continues, "Let me say at once that a large fraction of the historians of economics meet this test very imperfectly." It is of some tangential interest that Paul de Man (1971) titled his volume, "Blindness and Insight."
2. Since translation is a consideration of consequence for this essay, we remind the reader that these are translated words.

3. This is also referred to as "Whiggish history"; see Mayr (1990) and his references for one (biological) perspective on the phrase; also the authoritative rendering of Pocock (1985). We return to the subject in the concluding lines of section "Pocock on Concurrent Language Games."
4. To be sure, Stigler is a recognized authority on twentieth century demand theory; in addition to his celebrated surveys on utility theory written in 1950, see his review of John Hicks' classic work in Stigler (1957).
5. The same sentiment appears a quarter of a century later as follows: "Keynes …was always searching for sharper articulations, more lucid metaphors, and compelling examples, not only for his various interlocutors, and audiences, but for also for himself." (Seybold 2019, 278). Also see the essay on Dickens in Husvedt (2011).
6. Demsetz (1993, 794) puts it this way, "Intelligence, insight, wit and style were evident in his writings. His articles and essays could not be ignored. They provoke readers to think and often to follow his lead. For some readers, they simply provoked." Stigler (1988c, 213) himself puts it this way. "Even to be demolished is better for one's self-esteem and reputation than to be ignored: It requires some ability to excite and especially to outrage one's professionals." Also see Baumol (1989) and Dorfman (1984) invoked below, and with Leijonhufvud's (1973) ethnographic study at hand.
7. See Khan (2019b) for seeing some recent work as working within this genre of fictional biography.
8. We warn the reader that in financial economics, for example, a "market maker" is someone who passively adjusts a price in response to demand; see, for example, Glosten and Milgrom (1985), O'Hara and Oldfeld (1986), Plott and Sunder (1988), Gode and Sunder (1993) and their references. Our use of the term relates more to the idea of a "gate-keeper," but this term does not seem quite apt in light of the fact that the "gate-keeper" is also deciding on the criteria that regulate entry to all that he is gate-keeping.
9. We turn to this hypothesis below; see footnote 50 and the text it footnotes.
10. Indeed, this essay can also be seen as reading this text in the company of Pocock and Schmitt.
11. One exception is Warren Samuels' (1984) review of Stigler (1982); we come back to it in section "Professional Reception of Stigler's Categorization." In a passing mention, Rosen (1993, 810) writes,

"Stigler's (1965) sophisticated empirical approach of textual exegesis to root out ambiguity was not picked up by others, and perhaps he himself may have lost confidence in it."

12. The fact that it is an analogy deserves emphasis. Stigler is not arguing for, or indeed against, cliometrics. In the conclusion to Stigler (1992, 466), he was to write, "No discipline welcomes a broad-scale invasion by an alien and complex body of doctrine and method. Most economists stubbornly ignored mathematics from the 1890s until almost World War II. The invasion of history by cliometrics appears to the outsider to have had no warmer a reception."
13. We refer the reader to footnote 5 and the references it lists. The reader can perhaps now appreciate why Whig history is a keyword for this essay.
14. See the pioneering paper, Stigler (1961); also Stiglitz (1975) and Arrow (2001) in ignoring it, and Stiglitz (2000) in confronting it. Mirowski and Nik-Khah (2017) attempt at a history of the subject is a useful one; see Khan (2019a).
15. We refer the reader to footnote 9 to remind him or her the spin that we are giving to the term in this essay. It is a spin of substantive consequence.
16. As we shall see below, Sherwin Rosen refers to this very event by using "proposed" instead of the word "opposed."
17. This "abandonment" will become a drumbeat of this essay. It will slip from the "abandonment" of the history of economic thought to the "abandonment" of mathematics; see section "Stigler's Mathematical Method and Its Psychological Pitfalls."
18. The issue is of course whether Chicago was a leader or a follower in this abandonment. Current confusions notwithstanding, there is a difference between economic history and the history of economic thought. We leave it to future work the explorations of the abandonment of the requirement of economic history in the core graduate curriculum.
19. Rosenberg notes that "Stigler's master's thesis at Northwestern, and his Chicago Ph.D. dissertation written under Frank Knight both dealt with issues in the history of economic thought." As is well-understood by the Stigler *cognoscenti*, out of the two distinguished Knight students, it was Buchanan who remained loyal to Knight's commitment to ethics and economics. As Stigler said in his Tanner lecture, "Dear Professor Knight, please forgive your renegade student, but I do so contend, if it was a splendid year of claret." We refer the reader to Stigler (1982a, 19)

to see the context. The uneasy relation between Stigler and Knight and that between Buchanan and Knight is well-worth exploring.

20. But see Khan (1993b) on seeing the "commodification of love" as an issue that divided Boulding from Samuelson. Also see footnote 66 below and the text it footnotes.
21. In his autobiography, Stigler (1988a, 215) puts it this way, "I cannot be confident that it would be profitable for a young scholar to study the history of his subject. If a young economist does immerse himself in the history of economics, he will learn that every proposed innovation is first produced in a highly imperfect form, and only gradually will the larger imperfections will be removed. He will also learn that the sponsors of the new theory and program exaggerate the deficiencies in the previous knowledge they are seeking to displace. I know of no important exceptions to this pattern of aggressive academic salesmanship." But see Friedman (1992) for a dissonant view.
22. We return to this point below; see footnote 49 below and the text it footnotes.
23. We are reading Stigler and Patinkin here as statisticians; we leave it for a future sociological/anthropological investigation whether they were econometricians, and thereby the identification of one community vis-à-vis another.
24. But as we shall see below in a reading from his autobiography, Stigler (1988a) was conflicted on this issue.
25. Patinkin in Patinkin and Leith (1978, 125) repeats this passage, and claims a sort of priority for it.
26. On the fact that a "statistically non-significant result does not 'prove' the null hypothesis" or that "statistically significant results 'prove' some other hypothesis," see Amrhein et al. (2019).
27. In his autobiography, Stigler (1988c, 207) puts the matter this way, "One can find more about contemporaries than is easily learned about the dead: Are they stingy or generous, do they love their spouses, are their children monsters or gems, do they know a lot about subjects they do not write? One can of course dispute the usefulness of such knowledge, which seems more helpful in judging a scholar's character than in in understanding his ideas." As the reader will see, we call on Claire Friedland below to get some faint glimpse of Stigler's character.
28. As is by now well-known, the fiasco of the first Chicago offer was based on the President of the University of Chicago finding Stigler as one with concerns that are too empirical and not theoretical enough.

As Stigler (1985, 40) writes, "I went to Chicago, met with the President, Ernest Colwell, because Chancellor Robert Hutchins was ill that day, and I was vetoed! I was too empirical, Colwell said, and no doubt on that day I was. So the Professorship was offered to Milton Friedman, and President Colwell and I had launched the new Chicago School." George Stigler was then to make this apparently perceived failing as his signatorial success. The title of Demsetz paper is "George J. Stigler: Midcentury Neoclassicalist with a Passion to Quantify."

29. Craig Freedman has scrupulously and authoritatively documented these exchanges: on Chamberlin, see Freedman (2016); on Sweezy, see Freedman (1995); on Liebenstein, see Freedman (2002); on Means, Freedman (1998a).
30. He then lists the others: Rose and Milton Friedman, Charlene and Richard Posner, John Hause, and Virginia and Stephen Stigler. It is interesting that he does not mention Director or Becker. In the concluding paragraph of his preface to his anthology on *Chicago Studies in Political Economy*, he uses the same valorizing adjectives for her that are typically used by his colleagues for himself: "attractive combination of intelligence, care and wit." However, the authors could only locate one joint publication, Stigler and Friedland (1989). On Director, see Van Horn (2010).
31. Demsetz (1993, 794) puts it this way, "It was coupled with a joy in verification and with a strong work ethic and sense of duty to his profession."
32. It is perhaps important to emphasize that there are at least three time periods in which the trajectory of the Chicago School can be bifurcated: one associated with the names of Knight, Viner and Buchanan; the second with Becker, Friedman and Stigler; and the third with Lucas. For the Buchanan's Chicago School, one can do worse than see Buchanan (1992).
33. Friedland is referring here to his seminal 1961 paper on the economics of information. It may be worth pointing out here that Stigler's search theory severs the link between prices and marginal rates of substitution in consumption and production. However it is not clear how much of an interest in welfare economics Stigler retained after his 1943 brush-up with Samuelson.
34. Stigler (1943) is crucial as the origin of the exchange to which we can hardly do justice here. The reader can follow the trail through Levy (2016), Schliesser (2016) and their references. Also the articles of Boulding, Friedman and Samuelson in Hook (1967). On the

commonality of social values which goes to the heart of this exchange, Stigler (1975a, 168) writes, "Everyone wants a good deal of purity … erudition and or at least incomprehensibility in professors, and sunshine on weekends. Everyone wants poverty eliminated, families united – no matter what the cost in psychiatric care – and crime abated, or at least confined to certain localities." Also see footnote 69 below and the text it footnotes.

35. It is not known to the authors whether Stigler read Pocock, but the consonance of understandings is remarkable. After identifying himself as "undoubtedly conservative, and only hopefully fair-minded," Stigler (1975a, 3) writes, "The purpose in labelling an individual person an extremist is to put his views outside the range of discussion – they are simply too absurd to merit the attention of normal people. An extremist is an intellectual lunatic – allowed loose if he does not communicate violence, but without an admission ticket to ordinary discourse." Stigler admits that the "lone dissenter with the absurd view will prove to be right, but if we gave each lunatic a full meticulous hearing, we should be wasting vast time and effort." Presumably, Stigler desk-rejected.
36. Stigler (1975a, 3–5) writes, "The issue of extremism is, so to speak, the extreme form of the problem between the liberals and the conservatives of America. Neither side seems to understand the other's position. It is as if there were a dialogue between two men, each of whom spoke the same words, but with a different meaning. Let me try my hand as an interpreter."
37. It is amusing to read Pocock's passages through the lens of the Galbraith-Stigler exchanges read in Freedman (1998b).
38. This important passage brings out Pocock's dependence and use of the vernacular of information theory; also see footnote 17 for a reference to Stigler's pioneering 1961 paper, and the text it footnotes.
39. It is that of Poovey (1993).
40. Stigler (1988a, 219) was surely aware of "purposive ambiguity": in his autobiography, he writes, "We all wear glasses that carry a date in time and the name of some geographic area, and even with the keenest of vision these glasses allow us to see only limited distances and partial motion of the world." It is thus a little reckless to think that context and purpose do not actively figure in his perspective and in the formalization of that perspective.

41. The first question concludes the first paragraph of Condren's concluding chapter titled *Toward an Explanation of Classic Status*, but with "a political" instead of the "an economic" in our reproduction; for this and the other two questions, see Condren (1985, 253–254).
42. See Pocock (1971) and Winch (1971). Winch introduces his book on Adam Smith by distinguishing his own enterprise from a "Whiggish" history of economic thought. He writes, "This essay can be seen as a contribution to a more general argument in favour of a historical interpretation of Smith's intentions and achievements (p. 1). I regard this as an essay in recovery rather than recruitment (p. 5)."
43. Decorum forbids us to give examples of such "histories of economic thought"—we are sure the reader will have no difficulty in identifying her own favorite examples.
44. Again, we substitute "economists" for "philosophers" and "economic" for "philosophic."
45. See Rorty (1984, 55) for "rational reconstruction", an activity in which "we are free to indulge in as much as we like as long as we realize what it is that we are doing."
46. See, for example, Chapter 6 in Appleby (1978) titled "The Poor as a Productive Resource."
47. In addition to Khan and Schlee (2016), see Khan (2004, 2014) for elaborations of this point of view.
48. Footnote 1 refers to the Courant-Robbins book *What is Mathematics?* and Stigler writes that the "layman's appreciation must necessarily be based upon authoritative hearsay."
49. Since the words are Stigler's, we given precise citations. These are respectively: the second full paragraph on p. 38, the first full paragraph on p. 39, the second and third full paragraphs on p. 40.
50. See, for example Drury (1985) and his references. The classics are of course Strauss (1952, 1959).
51. It is the authors' conjecture that the choice of Canard may have more to do with his name than with his place in the history of mathematical economics.
52. However, in his autobiography, Stigler (1988a, 36–37), he puts the matter of communication this way. "Family members use words that have special meaning for them. A reference to 'Z' brings to mind a tedious bore or a remarkable procrastinator; in our family, to 'Lizzie Bean' was to lead out immediately all one's aces in bridge. So it is with every person, and that is why intimate association makes communication

between people efficient and accurate. If I had known David Ricardo, I would be better able to understand his written words. That would be a help, because to this day the meanings of his theories are much debated." What is fascinating is that he does not draw the parallel to discursive communities, and how they may have their own vernaculars and their own language games with their specialized vocabularies.

53. Stigler, and presumably also Becker, would point out the opportunity costs of time. But it is precisely this kind of zero-sum thinking that is at issue. Time surely expands with enthusiasm and shrinks without it; how can one read a proof without reading drama?
54. It is an interesting exercise to read Stigler (1942) and its 1952 revision in the light of modern textbooks to gauge the progress of economic science.
55. The reader can do worse than look at Chapter 12 titled "A Sketch of the History of Truth in Teaching," in Stigler (1975a). Elsewhere (p. 178) in the volume, he writes, "The consumer—and the investor and the laborer—have always been subjected to vicissitudes arising out of chance, ignorance, neglect, and fraud. Some are essentially inescapable: no sovereign has discovered a way to insure that everything taught in school is correct."
56. Stigler (1975a, 171) would then put it this way. "A rational man must be guided by the incentive system within which he operates. The carrot and stick guide scientists and politicians as well as donkeys."
57. Never one to forego evidence, he gives three examples: the consumption function, the Cobb-Douglas production function, and the effect on money supply on price levels.
58. But see Karni and Shapiro (1980), a wonderful satire on empirical work that Stigler accepted in the JPE. As his professional colleagues continually observe, his was a "subtle and complicated mind," presumably not averse to saying yes and no at the same time.
59. Stigler continues "Workable competition is obsolete, although it was in high fashion for several decades, and it is omitted from Palgrave II. Correspondingly, a fair number of the essays in Palgrave II have short expected lives."
60. Stigler invokes Arrow, "This is done, and done well, in numerous essays, for example by Kenneth Arrow on Arrow's theorem."
61. Mouffe (1999, 1) begins her introduction by asking "Why should we read Carl Schmitt today? Does his friend-enemy conception of politics retain some pertinence in our 'post-political' age?" Our question, in her

vernacular, is whether his conception give us any insight into George Stigler, and through this, into the economics profession.

62. She writes, "In the domain of collective identifications, where what is in creation is the creation of the political 'we' by a delimitation of a 'them', the possibility always exists that this we/them relation will turn into a relation of the friend/enemy type; in other words, it can always become political in Schmitt's understanding of the term" (Mouffe 1993, 2–3).
63. On the non-transitivity of the friend or enemy, see Solow responding to Freedman (1997). "And George and I just fell for each other. And we just enjoyed each other's company, more than I can tell you and we spent a lot of time together. And we just, we just hit it off. We were good friends." And after referring to a scene at Stigler's birthday celebrations, he moves to Friedman and says, "I don't think there was ever the kind of depth of personal friendship that I had with George." He also mentions Samuelson, "I had been here [at MIT] for seven or eight years and I knew I loved it. I have had, since the year 1949 or '50, the next office to Paul Samuelson who is the closest friend I have, and I couldn't imagine", ellipses the interviewee's. Also see Leijonhufvud (1973), footnote 23 above and footnote 74 below.
64. This leads directly to Mouffe's project. As she writes, "What is at stake in answering Schmitt's challenge, therefore, is devising ways in which *antagonism* can be transformed into *agonism*. How can one envisage a democratic form of commonality which makes room for conflictual pluralism" (Mouffe 1999, 5).
65. To the extent that this essay has a point of view, and is an essay in persuasion of that point of view, we draw some assurance from Stigler's (1998a, 211) words, "A scholar who cannot convince himself that what he proposes is certainly true and possibly important is asking a good deal of others to generate enthusiasm for the idea. [An] aspect of this salesmanship is the heavy use of repetition, perhaps the most powerful of arguments." On repetition, see Husvedt (2011, 51). I thank Eric Schliesser for drawing my attention to Husvedt's work.
66. One way to see this is through the eyes of Stigler's Knightian sibling, James Buchanan. Buchanan (1992, 152) writes, "There are at least three distinguishable sources of my criticism of orthodox political economy. First, I have been influenced by Frank Knight and by F. A. Hayek in their insistence that the problem of social order is not scientific in the standard sense." This goes to the heart of the differing identity of the Virginia School; also see footnote 30 above.

67. Stigler (1992, 467) writes in the context of the study of legal institutions and doctrines, "The answers to these questions are not exclusively legal and economic—indeed, they obviously involve the workings of the political system. Understanding the source, structure, and evolution of a legal system is the kind of project that requires skills that are possessed but not monopolized by economists, for it is in good part an empirical project addressed to rational social policy."
68. A celebrated procedure associated with Friedman (1953), and before him by Vaihinger (1925), a connection that surely merits exploration. Also see Hammond and Hammond (2006) and Stigler (1988a) for Stigler's intimate involvement in the discovery and formulation of this procedure.
69. This chapter is useful for comparing how Stigler's view regarding welfare economics evolved since 1943; also see footnote 36 above and the text it footnotes.
70. See the correspondence between Friedman and Stigler relating to the editorial policies for the JPE in Boumans (2016).
71. On all this Hammond and Hammond (2006) and Mitch (2016) are priceless sources of information. Also see Freedman (2008, Chapter 5) for bringing in Galbraith and seeing him as Stgler's alter-ego. On intransitivities, also see footnote 66 above. One of the authors may also be forgiven for recommending a possible look to/at Khan (2019b).
72. To be sure, it can also be sold to sister disciplines. Stigler (1992, 466–467) writes, "No discipline welcomes a broad-scale invasion by an alien and complex body of doctrine and method. Once this natural reaction is taken into account, the width of the foothold that economics has obtained in law schools is impressive. Most major law schools have one tenured economist, and some have two." In this selling of economics to the law profession, he saw the commodity to have two uses: "to provide its expertise on points requested by the lawyers, [and] in the study of legal institutions and doctrine." Always wary of ambiguity and disruption, he cautioned "Such studies are not necessary and are possibly even disruptive in a discipline whose fundamental task is to train practitioners." Leijonhufvud' (1973) ethnographic study is useful in this regard.
73. See the brilliant reading of Bentham and utilitarianism by Ferguson (2004); also Husvedt on Dickens in Husvedt (2006, 166–167). A close reading of Stigler as a utilitarian is overdue, one in which one can see Bentham's *panopticon* as a price system regulating and monitoring the actions and behavior of all who, willingly or unwillingly, have submitted themselves and their actions to it. Also see Stiglitz (1975, 1985, 2000).

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Stigler on the Science of Economics: A Tale of Two Knights

Peter J. Boettke and Rosolino A. Candela

Introduction

"A science," according to George Stigler, "is an integrated body of knowledge, and it is pursued and developed by a group of interacting practitioners called scientists," the extension and validation of which is the intellectual goal of such scientists (1983: 530). Economics "is a substantive science dealing with economic phenomena" (Stigler quoted in Kitch 1983: 172), but it also "requires both the persistent and almost timeless theories that naturally ignore the changing conditions of their society" upon which to understand economic phenomena (Stigler 1983: 534–535). The persistent and timeless basis of economic theory, and

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the basis that grounds the economist's assessment of economic policy, is the assumption that man is "a reasonably efficient utility maximizer," an assumption which Stigler argues is "singularly ill-suited to assuming that the political activity of men bears little relationship to their desires" (Stigler 1982: 9). Therefore, the economist qua scientist has no public policy relevance other than to provide general understanding of the cause and effect of public policy, not to advocate for public policy.

Stigler's extension of the body of economic science included not only the history of economic thought, but also the development of new subfields, including the economics of information, economic regulation, and industrial organization. For these seminal contributions, the Royal Swedish Academy of Sciences awarded Stigler the Alfred Nobel Memorial Prize in Economic Science in 1982, particularly for "research on market processes and the causes and effects of public regulation."[1] As Harold Demsetz remarks in a remembrance of Stigler, the development of these subfields, not to mention his understanding of market processes, are no accident, but "strongly guided by a penchant for defending and extending neoclassical price theory" (1993: 794).

Our focus in this chapter will be on the methodological role that Stigler played in validating what he regarded as the science of economics that he had inherited from his own teacher, Frank Knight, and how this affected his understanding not only of economic theory but also public policy. Our point here is not to downplay Stigler's stature within the Chicago School by interpreting him through a Knightian lens. Given the complexity and subtlety with which Knight treats economic science, this cannot be the case (see Emmett 2009). Rather, given that Stigler was such a mythic figure in the transition from the Old Chicago School, prior to WWII, to the New Chicago School post WWII, his understanding of economic science, viewed from a Knightian perspective, will help historians of economic thought to answer the following questions regarding twentieth-century neoclassical economics: first, what was lost and what was carried forward from Knight by the generation of Chicago economists following WWII? Secondly, how did Stigler's understanding of Knight contribute or detract from the mainline of economic science and its relevance to public policy? Our contribution is an attempt to answer these questions.

Ludwig von Mises has characterized the evolution of economic science as a "march" from a "science of wealth" to "a science of human action" (von Mises 1960 [2009]: xv). Historically, twentieth-century neoclassical economists have understood economics as a science of human action from two overlapping, yet distinct, approaches: as a *price-theoretic* approach and as a *choice-theoretic* approach. As a price-theoretic science, economics illustrates an *indirect* link between rationality and equilibrium via comparative institutional analysis. According to this approach, individuals in their assessment of the *expected* costs and benefits of their decisions (i.e. rational behavior), and through their interaction with other individuals, *unintendedly* generate institutions, including money prices, which in turn guide them in their future assessment of expected costs and benefits toward a coordination of their independent plans (i.e. equilibrium).

As a choice-theoretic science, there is a *direct* link between the rational agent and the efficiency of market equilibrium. This means that equilibrium is already assumed to exist *ex-ante*, and therefore equilibrium outcomes are directly reducible to and constructed from an aggregation of individual choices. The public policy implication of this choice theoretic approach is that institutions and public policy are not unintended outcomes, but are a deliberate and efficient result of rational actors optimizing their goals.[2] The distinction between the two is subtle, yet related, but the degree to which the two have been conflated has both waxed and waned among economists throughout the history of economic thought in the twentieth century (Boettke and Candela 2017: 732, fn. 11). For Stigler, the distinction between the two was somewhat conflated, given that they also reflect different points of emphasis that he inherited from Knight himself. The point here is that one's understanding of economic science, including Stigler's, is never irrelevant to the economist's role in public policy. Economic science has public policy implications.

Stigler was, predominantly, a choice-theoretic economist, in which partial equilibrium is the anchor that foregrounds economic analysis, and that Stigler's public policy conclusions were a by-product of this understanding of economic science. Like Knight, however, this did not imply he always neglected to tell a background story of dynamic adjustment of relative prices to changing circumstances, in which error-prone actors are being guided toward equilibrium. Moreover, it does not imply that Stigler neglected comparative institutional analysis. As he himself stated in his presidential address to the American Economic Association, the competence of the economist as a scientist "consists in understanding how an economic system works under alternative institutional frameworks" (1965: 2). However, Stigler goes on to state that "[i]f they have anything of their own to contribute to the popular discussion of economic policy, it is some special understanding of the relationship between policies and results of policies" (Stigler 1965: 2). Therefore, for the economist in the role of the scientist, the assumption that a given set of institutions and policies are inefficient, or to say that a set of policy results are ineffective in fulfilling a set of policy intentions, would imply that individuals are "irrational" or "wrong" in their choice of policies, a conclusion that Stigler would regard as contradictory to the economist as a value-free scientist, in which science is defined almost entirely by the logic of choice.

For the economist to remain a value-free scientist, and not become a "preacher,"[3] they are restricted to taking the policy choice as given, and assess its costs and benefits *as perceived by those individuals who have chosen that policy*. The public policy implication is that the economist as a preacher may recommend a set of policies that increase economic wealth, such as free trade. However, once he or she has taken into account the costs of implementing such a policy, such as lost rents to special interest groups benefiting from such a policy, the economist qua scientist is methodologically straitjacketed from offering such a policy recommendation. This public policy conclusion, however, presents a fundamental dilemma for the advocate of the market (of which Stigler was a fervent proponent): is there a non-normative way for the economist to remain in their role as a scientist while fulfilling their role as an economic reformer? From a purely Stiglerian perspective, the answer would be no.[4] But if we regard Stigler's methodology as a subset of Knight's broader understanding of economic science, then there is another Knightian answer to this dilemma, which answers this question in the affirmative, the solution to this dilemma being provided by another of Knight's Nobel Prize winning students, James Buchanan.

George Stigler and the Chicago School

In a recollection of George Stigler as a political economist, Warren Samuels described Stigler's understanding of economic science in the following way:

> George was a neoclassicist; a certain type of neoclassicist; if he was more than a neoclassicist he was also an Austrian economist[5]; his version of neoclassicism was an interesting blend of Marshallian and Austrian economics. I am more eclectic than George, and certainly an institutionalist of the old school. In any event, I think he appreciated not only that I took the Chicago School seriously but that I lauded the old, Frank Knight-Henry Simons version of the School relative to his version. (Friedland et al. 2002: 642)

George Stigler's intellectual biography offers an important lens into the evolution not only of the Chicago School in the twentieth century, but also how he contributed to the evolution of neoclassical economics in general. In the post-WWII period, George Stigler, along with Milton Friedman, were instrumental in reconstructing what would later become known as the Chicago School of economics. Only in retrospect have economists made a distinction between the "Old Chicago School" of Frank Knight, Henry Simons, and Jacob Viner, and the "New Chicago School" of Milton Friedman, George Stigler,[6] and later Gary Becker (see Buchanan 2010; Hammond et al. 2013; Mitch 2016; Reder 1982). Prior to the end of World War II, there was no identifiable Chicago School[7] (Stigler 1988: 148). A full exposition of why this was the case is beyond the scope of this chapter, but we can outline both a *methodological* and an *analytical* reason, which are both illustrated by Stigler's role in the Chicago School.

The first reason is with regard to the relationship between economic science and public policy. Economic science is not a public policy conclusion, but a set of methodological and analytical propositions that yields public policy conclusions as a by-product of scientific analysis. It would not be unfair to associate the study of free markets with the Chicago School, including the Old Chicago School. However, it would be misleading to then conclude that Chicago economists *advocate* public policies consistent with economic freedom. This is a commonly oversimplified characterization by proponents and advocates of the Chicago School, leading to the claim that "Chicago economics" is inconsistent with public policy goals that are commonly associated with government intervention, such as the reduction of income inequality. However, in Stigler's publication of *Roofs or Ceilings?* (1946), co-authored with Milton Friedman (and the only piece they would ever co-author), he and Friedman had argued that free pricing was the most effective means to reducing income inequality, not as a matter of public policy advocacy, but as a matter of science in terms of studying the results of a public policy in terms of its desired intentions. *Roofs or Ceilings?*, published by the Foundation for Economic Education (FEE), presents a study of the housing market in San Francisco in 1906, when it experienced a destructive earthquake, in comparison to San Francisco's housing market in 1946 with extensive rent controls. Although the city had lost more than half of its housing in three days, during the crisis, "*there is not a single mention of a housing shortage!*" (Emphasis original, Friedman and Stigler 1946: 7). In 1946, however, there were extensive reports of housing shortages due to rent controls, even though there had been no physical destruction of housing by earthquakes. In an exchange of correspondence, reprinted in *Making Chicago Price Theory* (2006), a dispute between FEE, Stigler, and Friedman emerged not over the findings of the study, but over what FEE regarded as what might be interpreted as an advocacy of collectivism (Watts 1946 [2006]). Friedman and Stigler's contentious passage went as follows:

> For those, like us, who like even more equality than there is at present, not alone for housing, but for all products, it is surely better to attack directly existing inequalities in income and wealth at their source than to ration each of the hundreds of commodities and services that compose our standard of living. (1946: 10)

The irony of this story is that George Stigler and Milton Friedman, two founding members of the Mont Pelerin Society, were accused of advocating collectivist goals! This is a common misunderstanding among free-market proponents regarding the relationship between economic science and policy, particularly in the Chicago School, namely because, as Stigler has written elsewhere, economists since Adam Smith "have always been opposed to inequality of income" (1949: 1) as a policy objective. Put differently, the notion that there was an identifiable pre-WWII Chicago school, substantially differentiated from other neoclassicists, which *advocated* public policies consistent with laissez-faire would be a misunderstanding. Acting as scientists, Stigler and Friedman were employing the doctrine of *wertfreiheit*, or value-freedom. In order for economic analysis to remain scientific and value-free, it must remain neutral with regard to ends. However, it was not the empirical testability of propositions that kept science value-free, as science came to be understood in the New Chicago School.[8] Rather, value-freedom in Stigler and Friedman's economic analysis was preserved by using price theory to *illustrate* the indirect and unintended effects of rent controls in a way that contradicted the goals of those who desired rent controls in the first place. In other words, it was an analysis of the effectiveness of a set of means for a given set of ends.

In a letter to Leonard Read, President of FEE, Friedman makes this point: "I believe it is essential to make it clear wherein we are criticizing means and wherein ends. Failure of liberals to emphasize their objectives seems to me one of reasons they are so often labeled reactionaries" (Friedman 1946 [2006]: 21). Where economists have differed is over the most efficient set of means to reduce income inequality, not over the objective of reducing income inequality itself. But as a science, the economic theorist introduces objectivity into the analysis by taking the stated objective of policy as given, in this case the reduction of income inequality, and assesses whether or not the chosen policy, in this case the existence of rent controls, is logically congruent to its stated objective.

Stigler and Friedman were "seeking to convince the open-minded, not those who already favor our position" (Stigler 1946 [2006]: 20), that if one wished to decrease income inequality, then using rent controls as a means to make housing cheaper for lower-income families would have the opposite effect, thereby illustrating in a logical manner that the policy of rent controls was self-defeating to those who desired it in the first place. With rent controls, the unintended consequence of a price ceiling is to create shortages, which in turn will redistribute income from the poor to the wealthy, since the distribution of housing will go disproportionately to those who can pay the higher real price of housing. The unintended consequence would be to increase the inequality of income. To reiterate, economic science is not a set of public policy conclusions, but a methodology that yields public policy conclusions. This methodology is a way of thinking that takes stated ends as given, and then assesses the effectiveness of alternative means to achieve those ends—a scientific practice that had been shared by economic scientists including Knight, Mises, Robbins, and Weber.

This brings us to the second reason one could not point to a distinctive "Chicago School" prior to World War II, which was the fact that, among neoclassical economists, which included Austrians, Marshallians, and Walrasians, there existed a shared analytical understanding of markets that overshadowed any substantive differences between the different schools of thought. As Stigler puts it, "1930s economics appeared to be little different at the University of Chicago than elsewhere" (1988: 148). For example, in Jacob Viner's lecture notes to Economics 301, the graduate course on price theory, dated June 17, 1930, Viner remarks the following:

> Neoclassical economics is a sympathetic evolution of the English Classical School. Included under neoclassical economics is the English-American version in Taussig and Marshall and also the Austrian school, whose differences are not as important as the resemblances to the Anglo-American type. Included also is the Continental Equilibrium School or the Mathematical School, such as Walras, Pareto, and their followers. They have much more in common with the neoclassicists than in dispute. (Viner 2013: 19)

This shared and implied understanding included a study of markets in terms of dynamic processes of adjustment, not in terms of equilibrium. In short, economics was understood to be a price-theoretic science. This does not imply that equilibrium analysis was not unimportant. Rather, equilibrium analysis was utilized as a method of contrast, or a theoretical tool by which to understand the institutional conditions necessary to generate a *tendency towards equilibrium*. These conditions include not only free pricing, but also private property and freedom of contract under the rule of law. In this shared understanding, there coexisted not only an appreciation for competition as a rivalrous process, but also an appreciation for the degree to which markets tend to approximate the conditions of perfect competition. Throughout Stigler's work, there are elements of both understandings of the market, but the dominant strand is an emphasis on *choice theory*, rather than price theory as it was traditionally understood prior to the mid-twentieth century. The unifying theme that connected these two generations of Chicago economists, and that later differentiated it from other schools of thought in the post-WWII era, was the consistent and persistent application of price theory, particularly that of Alfred Marshall's *Principles of Economics* (1920 [2013]), to understanding economic phenomena.

Stigler, however, "believed not only in neoclassicism but in the Chicago version of neoclassical economics" (Friedland et al. 2002: 642), which "proceeded from the assumption that modern price theory is a powerful weapon in the understanding of economic behavior, not simply a set of elegant theoretical exercises suitable for instructions and the demonstration of one's mental agility" (Stigler 1988: 162). This "Chicago version" of price theory that was consistently and persistently applied by Stigler and that later came to define the New Chicago School, most consistently practiced by Gary Becker,[9] included three distinguishing characteristics.

The first characteristic is the pervasiveness of efficiency. "Indeed, every society that is purposive," Stigler states, "seeks to do efficiently whatever it seeks to do" (1975b: 286). The presence of efficiency is based on a universal fact that, when faced by a set of alternative opportunities constrained by scarcity, individuals will choose that alternative that gives them more rather than less satisfaction. In other words, individuals are rational and *strive* to maximize utility. Based on the postulate of utility maximization, individuals will not only succeed in maximizing all the gains from trade and innovation, but also succeed in minimizing waste and error. This implies that goods and services will be produced and sold at the lowest possible cost to consumers, and therefore generate the highest possible profit to producers. The presence of inefficiency in production, however, would imply the existence and pursuit of other more desired outputs, in which case (1) inputs are more highly valued in alternative lines of production and (2) producers have not chosen the lowest cost method of production. This can only be the case, according to Stigler, if (1) the economist imposes value judgments upon consumers that they never accepted (1976: 214) or if (2) the economist has not accounted for the cost of gathering information; if information is costly to gather, then adopting another production method may be inconsistent with utility maximization (Stigler 1961). For example, advertising is an efficient response to positive information costs on the part of producers and consumers. In a world of perfect information, consumers, and producers would converge instantaneously. The presumption that producers are able to manipulate consumers and exercise monopoly power through advertising implies an instance of market failure. However, this conclusion only follows if (1) consumers are fully aware of other competitors and (2) other competitors are selling at a lower cost. Once the cost of producing information is accounted into the production function, then advertising proves to be the most efficient way to coordinate buyers and sellers at the lowest cost, both monetary and non-monetary, consistent not only with utility maximization, but also perfect competition.

The second characteristic is the ubiquity of competition in society. Competition, Stigler writes, refers to "the independent rivalry of two or more persons" (1957: 1) and "a process of responding to a new force and a method of reaching a new equilibrium" (1957: 2). Given that scarcity cannot be eliminated, neither can competition be eliminated as well. Given the postulate of utility maximization, regardless of the market structure, competitive pressures will generate an outcome that approximates that of perfect competition. On the concept of competition, Stigler remarked that "it was unfortunate that perfect market was made a subsidiary characteristic of competition, for a perfect market may also exist under monopoly. Indeed, in realistic cases a perfect market may be more likely to exist under monopoly, since complete knowledge is easier to achieve under monopoly"[10] (1957: 14–15). What this implies is that "the processes of obtaining, defending, sharing, and eliminating monopoly positions are far more important and interesting than the exercise of monopoly power" (Stigler 1988: 164). Both rent-seeking and profit-seeking will exhaust the gains from trade and innovation, approximating a situation in which all profit opportunities have been discovered and rivalry no longer exists (i.e. perfect competition).

The ubiquity of efficiency combined with the omnipresence of competition also yields public policy conclusions, in that it undercuts, for example, the rationale for anti-trust policy. It is not simply the case that the effects of monopoly power are negligible in the long-run due to the threat of entry and exit by other competitors. Rather, according to Stigler, monopoly power is often an instance in which policymakers have defined the relevant market so narrowly so as to merit government intervention to preserve or generate what already approximates a perfectly competitive market in the first place (see Stigler and Sherwin 1985). For example, in "The Extent and Bases of Monopoly" (1942), in sectors of the economy, which have allegedly become monopolistic through market competition, such as the domestic automobile industry, a "reasonable approximation is all that is needed" (1942: 3). Stigler identifies two "grave defects" with the use of statistics measuring concentration ratios for particular industries. In the case of automobiles, the first is the omission of imports that compete with domestic car manufacturers. By including imports, the extent of monopoly in the domestic automobile industry would be greatly minimized, making "it reasonably certain that monopolistic powers are in general small" (Stigler 1942: 8). Moreover, the inclusion of secondary markets, such as a used-car market, into statistical analyses of market concentration would further minimize the extent of monopoly power. Taking into account the constant erosion of monopoly profits by competitive pressures, it is no wonder that "persistent monopoly problems" most often "are created by governmental regulations" (Stigler 1988: 165).

This last point leads us to a third characteristic, not only in the extension of Chicago price theory, but also in the evolution of Stigler's economic thought from the Old Chicago School, namely the application of equilibrium analysis to explain the existence and persistence of government regulation. Like his own predecessors of the Old Chicago School, Stigler, by his own admission, regarded the existence of monopoly power as a persistent problem in the economy,[11] and its persistence justified the use of anti-trust policy not only to break up concentrated industries but also punish collusive behavior in order to approximate the conditions of perfect competition (1988: 97–99). It was after his return to Chicago from Columbia in 1958 that he was increasingly persuaded by his colleagues, most notably Aaron Director, Ronald Coase, and Harold Demsetz,[12] but also Sam Peltzman and Lester Telser, about the insignificance of monopoly in an unhampered market, even one whose structure did not conform to the conditions of perfect competition. Moreover, his own work on public regulation, particularly in industrial organization (see Stigler and Friedland 1962; Stigler 1966), led him to conclude that utility regulation and anti-trust laws have a negligible intended effect of curtailing monopoly power. "The declining importance of monopoly as a problem in public policy or as a hypothesis to explain business behavior," Stigler writes, "is an important reason why my own research interests shifted increasingly to the government regulation of economic life – and the reciprocal regulation of government by economic groups" (1988: 109).

If public policy has, at best, negligible intended effects, or more likely, unintended consequences, as in the case of rent controls described above, is it reasonable to conclude, based on the assumption of rationality and the ubiquity of competition, that policy makers are persistently mistaken? The presence of a mistake implies waste or error, and therefore unexploited gains from trade, which implies individuals are not striving to maximize their utility. "To say that policies are mistaken is to say that no can explain them" (Stigler 1975a: x). If the economist is to remain positive and explain why such policies persist, then we must "assume that political systems are rationally devised and rationally employed, which is to say that they are appropriate instruments for the fulfillment of desires of members of the society" (Stigler 1971: 4). This implies that regulations persist because industries have expended time and resources to "capture" the discretion of a regulatory body as a means to effectively pursue their own private goals. For Stigler, political parties are analogous to firms in markets (Stigler 1972), and just like in any other commodity, there exists a market for regulation governed by supply and demand conditions. As Stigler states, the "representative and his party are rewarded for their discovery and fulfillment of the political desires of their constituency by success in election and the perquisites of office," but this cannot be achieved without the support of special interest groups demanding a variety of regulations for their own private gain. The refusal to "sell" regulation and abolish subsidies, price controls, tariffs and the like would promote the general welfare, but this benefit would be dispersed among the masses of rationally ignorant voters. In turn, such refusal will be a concentrated cost to the politician, in terms of votes and campaign contributions forsaken to competing political parties. As such, regulation will be "bought" by special interest groups and "sold" by political parties, resulting in an "efficient" political outcome. Stated differently, to argue that such policies are "inefficient" implies that the costs of removing protective regulations, in terms of "buying out" the present discount value of rents accrued to protected industries, are less than the gains in consumer surplus that would be created by a fall in price due to entry of new firms into the industry. If this is not the case, however, the existence of regulation implies that consumers and voters "desire" such policies because the alternative policy—economic freedom—is more "expensive" under the given circumstances. This positive conclusion, as we shall see in section "Economic Science and Public Policy: A Knightian Solution to Stigler's Dilemma via Buchanan," led Stigler to conclude that neoclassical economists are extremely limited in their ability to engage in value-free welfare economics. This public policy conclusion, as we discuss in the next section, is based on Stigler's definition of economic theory as rational choice itself.

A Tale of Two Knights: Statics and Dynamics

Elsewhere in an extended, earlier version of our paper,[13] later published as "Price Theory as Prophylactic against Popular Fallacies" (2017), we had suggested that in the history of the Chicago School of Economics, there is "a tale of two Knights" that could be told, a Knight that leads logically not only to Stigler's choice-theoretic emphasis in economics, but also a Knight that leads to the Buchanan's price-theoretic emphasis in economics. Given the focus of this chapter, however, we will concentrate primarily on Stigler, but also discuss Buchanan in more detail in section "Economic Science and Public Policy: A Knightian Solution to Stigler's Dilemma via Buchanan." Stigler's rendition of Chicago price theory had come to emphasize a more choice theoretic approach compared to the price-theoretic emphasis displayed by the Knight-Viner-Simons generation of Chicago economists.[14] The method by which a choice-theoretic approach to economics explains economic phenomena is by fitting aggregations of rational behavior directly into an equilibrium construct, in which *equilibrium prices* are rendered sufficient for market-clearing outcomes. However, this approach trivializes, as Henry Simons put it, the "central conception of price theory," which "is that of an equilibrium *adjustment* with respect to relative prices and relative production" (Simons 1983: 6, emphasis added). Individual plans not only are adjusted by relative prices, but relative prices themselves in turn are adjusted by the mutual adjustment of individual goals. In such a world of *disequilibrium prices*, individual ends cannot be simply reduced to an equilibrium construct, because the knowledge necessary to define equilibrium—perfect foresight of individual ends—does not exist *ex ante*. Rather, the knowledge necessary to define the conditions of equilibrium are defined through a process of interaction, where private property, *disequilibrium prices*, and hence the emergence of profit and loss, are generated by such interaction to set forth equilibrating *tendencies*. Individual ends, though taken as given for scientific analysis, only *become known* through the process of equilibration. For example, the economic scientist can deductively conclude, as a given, that individuals strive to maximize their utility, but what entails utility maximization for the individual requires subjective knowledge about the content of one's utility function, which can only be translated into publicly available information through the price system, and such knowledge can only be communicated through a process of monetary exchange. Without such knowledge, the choice-theoretic economist can only construct an equilibrium by inferring intentions from outcomes.

However, Stigler's choice theoretic emphasis should be not mistaken for ignorance or neglect, given his training by Knight and his appreciation for the Old Chicago School. As Stigler himself wrote, the "merging of the concepts of [perfect] competition and the market [process] was unfortunate, for each deserved a full and separate treatment" (1957: 6). Before we elaborate on those aspects in which Stigler diverges from the Old Chicago School, and arguably Knight himself, let us outline the basis upon which economic science was understood when Stigler received his training at the University of Chicago. The idea that both classical and early neoclassical economists outlined a *shared understanding* of the methodology of economic analysis, including Frank Knight, is often misunderstood by modern economists. For both economists of the Old Chicago School as well as the Austrian School, economics is understood as a social science that could derive laws "as universal as those of mathematics and mechanics," yet account for the complexity of the human experience (Knight 1935 [1997]: 127). As Knight states, economics is composed of three, interconnecting "methods of treatment which must logically be sharply differentiated," the first two parts of which constitute the theoretical core of price theory.[15] The first part refers to pure theory, or in today's terminology the logic of rational choice, which is "largely deductive in character, of the more general aspects of economic cause and effect," and forms the basis for "those tendencies of a price system which are independent of the specific wants, technology, and resources" (1935 [1997]: 135). The tendencies of the price system are not based on any laws regarding the content of rational choice, but only on laws regarding the *form* of rational choice, which states that individuals prefer more of a good rather than less. The second part refers to applied theory,[16] which combines rational choice with subsidiary empirical conditions of time and place, such as institutions, in order to understand the concrete manifestation of rationality itself. Applied theory is the realm of spontaneous order analysis, from which the unintended emergence of money prices and institutions are traced back to rational decision-making. The third division of economics is history, which includes the realm of statistical analysis. The economic scientist takes the arguments one constructs in pure and applied theory, and then develops a framework of analysis that aids the empirical interpretation of events and provides an economic assessment of those events.

Within this methodological framework, equilibrium analysis is not absent from economic science. Rather, it serves as a necessary backdrop, or a method of contrast, to understanding how the application of theoretical abstractions, such as rational choice theory, combined with institutional analysis generates concrete equilibrating tendencies that manifest themselves in a particular time and place. This methodological understanding of economic science, as Knight understood, was not new, but how all economists since Smith understood it: "Whether or not the use of the method of exact science is as necessary in the field of social phenomena as the present writer believes, it will doubtless be conceded, even by opponents of this view, that it *has been* employed in the great mass of literature since the modern science of economics was founded" (emphasis original, 1921 [2006]: 13). Given that Knight considered himself to be articulating what was considered the mainline of economic thinking, the primary theme in Knight's understanding of economic science, we argue, is one in terms of dynamic processes of change. For example, consider how Knight explains the role of the price system in *The Economic Organization*:

> The system of social organization does more than reduce individual values to a common denominator or scale of equivalence. In large part the individual wants themselves are *created* by social intercourse, and their character is also largely dependent upon the form of organization of the economic system upon which they are dependent for their gratification… the science of economics is largely taken up with description and analysis of the process by which this common denominator of things consumed and produced by the economic system is arrived at, that is, with the *problem of measuring values*. (emphasis original, 1933 [1967]: 9–10)

Knight's emphasis here is on the *process* by which money prices emerge out of human action, though not of human design—individual values are neither directly embodied in the price mechanism as mere aggregations of individual demonstrated preferences, nor are they directly reducible to individual ends. Moreover, information regarding individual wants are neither fixed nor given. Rather, knowledge is an ever changing and multifaceted flow of new ends that are created and discovered only through the context of market exchange (see Boettke 2002). Economic knowledge embodied in the price system is an emergent whole that is not directly reducible to the sum of all individual valuations; it only emerges within the context of private property and monetary exchange. From a choice-theoretic perspective, information is an aggregation of individual search according to marginal benefits and marginal costs; equilibrium is constructed as if it were directly reducible to pure theory, or rational choice. With regard to whether or not comparative statics or dynamic processes are the focus of economic theory, Knight also states the following:

> The problem of conditions of equilibrium among given forces – "statics" in the proper sense – is often important in economics, *but is after all subsidiary*, as indeed it is in physical mechanics. *The larger question is that of whether the forces acting under given conditions tend to produce an equilibrium*, and if so how, and if not what is their tendency; that is, it is a problem in dynamics. This type of problem has been too largely passed over hitherto, leaving a fatal gap in the science. The crying need for economic theory to-day is for a study of the "laws of motion," the *kinetics* of economic changes…*The centering of economic theory about the possibility and condition of equilibrium has caused the study of the laws governing economic changes in time to be neglected*. (emphasis added, Knight 1935 [1997]: 133–134)

This shared understanding regarding economic science between the Austrians and the Old Chicago School, as outlined by Knight, was based not on a particular *method* of analysis per se, which was the case for Stigler (and more explicitly for Gary Becker), but on *economic activity* with a particular analytical understanding of the coordinative properties of private property, freedom of contract, and money prices. This analytical understanding of the market as an open-ended discovery processes, as expressed by Knight, is most prevalent in Buchanan's "What Should Economists Do?" (1964), where he states that

> A market is not competitive by assumption or by construction. A market *becomes* competitive, and competitive rules *come to be* established as institutions emerge to place limits on individual behavior patterns. It is this *becoming* process, brought about by the continuous pressure of human behavior in exchange, that is the central part of our discipline, if we

> have one, not the dry-rot of postulated perfection. A solution to a general-equilibrium set of equations is not pre-determined by exogenously-determined rules. A general solution, if there is one, emerges as a result of a whole network of evolving exchanges, bargains, trades, side payments, agreements, contracts which, finally at some point, ceases to renew itself. At each stage in this evolution towards solution, there are *gains* to be made, there are exchanges possible, and this being true, the direction of movement is modified. (emphasis original, Buchanan 1964: 218)

However, another plausible interpretation of Knight is that he understood economics in terms of stationary equilibrium,[17] the latter of which Stigler pushed to its logical conclusion in terms of positive analysis and public policy. Though indeed Stigler regarded economics as a substantive science dealing with economic phenomena, the analytical focus of explaining economic phenomena came to be dominated by pure choice theory, not price theory per se. It is plausible to trace Stigler directly back to Knight, since it is unclear in Knight to what degree the realm of price theory can be reduced to the pure logic of choice. Knight commonly equated economic theory with the pure logic of choice itself: "A large part of the extant body of economic theory would be as valid in a socialistic society as it is in one organized through exchange between individuals" (Knight 1935 [1997]: 131). This strand of Knight that views the market in terms of stationary equilibrium leads him to conclude the following:

> [T]he problems of collectivism are not problems of economic theory, but political problems, and that the economic theorist, as such, *has little or nothing to say about them*. This holds true whether we consider the problem to be the scientific one of predicting what the collectivist economy would be like in structure and activities, or whether we look at it *practically from the standpoint of the right objectives to be pursued and the right principles to be followed in realizing them*. (emphasis added, Knight 1936: 255)

Here we see the purest form of Stigler in Knight, from which we see Stigler's positive assessment of economic science and his normative assessment of public policy, the latter of which we discuss in section

"Economic Science and Public Policy: A Knightian Solution to Stigler's Dilemma via Buchanan." If economic science is taken literally as the maximization of given means and given ends, calculating the relative values of scarce capital resources is not an economic problem that is *contextually dependent* on private property and monetary exchange. Rather, it is a political problem of incentivizing bureaucratic agents, who are imperfectly informed, to gather information that is already given, so as to approximate the *equilibrium prices* necessary to the clear the market of any surpluses and shortages.[18]

What this reveals is that not only that Stigler's economics of information, but also his economics of regulation and public policy conclusions, follow from a conflation of an important distinction between *information* and *knowledge*. Information for Stigler is treated as a commodity acquired through *deliberate search*, which implies an individual who searches for such information already knows of its existence, but invests in acquiring it according to the marginal benefits and costs of search. From this perspective, the informational content embodied in prices are the equilibrium outcome of nothing more than the aggregation of individual search activity. This is different from knowledge, which is spontaneously and unintendedly generated through interaction. For the choice-theoretic economist, this introduces an inherent paradox: *deliberate search requires that we know beforehand what we don't yet know*. Stigler, by his own admission, is aware of the limits of this approach, since the paradox that arises from such conflation renders economic theory unable to "explain how the comparative values of different goods and services are established" (Stigler 1983: 533). As Stigler writes:

> The near-universal tradition in modern economic theory is to postulate a maximum possible output from given quantities of productive inputs – this is *the* production function – and to assert that each firm operates on this production frontier as a simple corollary of profit or utility maximization. The merit of this conventional tradition is also its demerit: *it eliminates the problem of the choice of technology*…What one may lament, however, is the failure…to recognize the problem of determining which technologies will be used by each firm (and, for that matter, each person).

> *The choice is fundamentally a matter of investment in knowledge: the costs and returns of acquiring various kinds and amounts of technological information* vary systematically with various characteristics of a firm. (emphasis added, Stigler 1976: 214–215)

Returning to one of the questions outlined in the introduction for a moment, this passage exposes in several ways what was lost and what was carried forward from Knight by the generation of Chicago economists following WWII. If, as Stigler argues, that "a science requires for its very existence a set of fundamental and durable problems," the most fundamental of which is "the theory of value," then indeed the "gap" in economic science to which Knight referred follows from pursuing a static view of markets in terms of choice of given means and given ends. Therefore, the consequence of pursuing economics exclusively as a choice-theoretic science is to turn the subject-matter of economic science exclusively into a *technological* problem of gathering information regarding given resources and given goals, not an *economic* problem of discovering the means to satisfy competing ends that are open-ended and defined by the process of competition itself. Stigler's focus on stationary equilibrium conditions leads him to conclude that markets are always and everywhere efficient with respect to given means and chosen ends by individuals. To say otherwise implies that the economist has (1) misunderstood the given constraints or (2) imposed a value judgment on the chosen ends of utility maximizing agents.

Economic Science and Public Policy: A Knightian Solution to Stigler's Dilemma via Buchanan

Is there a non-normative basis for the economist to remain scientific without economic science losing its public policy relevance? This depends upon, as we suggested above, whether the economist treats science as price theory or simply the logic of rational choice. "Social policies and institutions, not individual behavior," Stigler states, "are the proper object of the economist-preacher's solicitude. *This orientation is demanded by the very logic of economic theory: we deal with people who* *maximize their utility*, and it would be both inconsistent and idle for us to urge people not to do so" (emphasis added, Stigler 1982: 6). Based on his view of economic science as the logic of choice, Stigler intends this statement to be an indictment that limits the economist from offering policy recommendations or "preaching." However, economic science from a price-theoretic approach suggests that this very statement also offers a non-normative basis for the economist to offer policy advice that is consistent with economic science, namely by suggesting institutional changes, as Buchanan would argue. "If the utility function of the choosing agent is fully defined in advance, choice becomes purely mechanical" (Buchanan 1964: 217), and therefore the economist scientist is extremely limited in their capacity to suggest Pareto improvements that are not normative. "On the other hand, if the utility function is not wholly defined, choice becomes real, and decisions become unpredictable mental events," from which the economist can suggest policy measures that expand the scope for mutually beneficial exchange (Buchanan 1964: 217).

Another way to understand the public policy relevance that Buchanan and Stigler attributed to economic science is in terms of a quote each has credited to Frank Knight. Stigler has quoted Knight as saying that "anything which is inevitable is ideal!" (Stigler 1982: 6), or Buchanan has put it, "to call a situation hopeless is equivalent to calling it ideal" (Buchanan and Tullock 1962: 204). For Buchanan, economic theory is defined in terms of the "non-ideal" disequilibrium of the market process, which is dynamically adjusting to changing conditions. For Buchanan, the fact that we live in an imperfect and open-ended world of discovery implies not only a scope for change, but also hope for a better world. Given that economic science, for Stigler, is defined in terms of the "ideal" of competitive equilibrium, the economic scientist is restricted from "preaching," since he or she has no hope of changing what is inevitably an efficient outcome.

Stigler argues that at "the level of economic policy, then, it is totally misleading to talk of ends as individual and random; they are fundamentally collective and organized. If this conclusion be accepted, and accept it we must, the economist may properly exceed the narrow confines of economic analysis. He may cultivate a second discipline, the determination of the ends of his society particularly relevant to economic policy" (1943: 358). By inferring the intentions of voters from the outcomes of public policy, Stigler concludes that the role of the economist is to infer that voters have chosen such a policy because they wanted it, and to say otherwise would mean the economists are substituting his or her value judgments for those of voters, abandoning their role as a scientist for that of a reformer. Moreover, the presumption that there are benefits to the overall economy to a change in public policy, such as the abolition of tariffs against imports, according to Stigler, would imply that the economist has not properly identified all of the costs that would entail changing such a policy. If it would have been less costly for policymakers to compensate interest groups the capitalized value of the rents they derive from an existing policy, then it would have been efficient for policymakers to have done so already. Moreover, the idea that free trade would be a more beneficial policy to society neglects the fact that those individuals who benefit from restrictions on trade at any moment have supported laws which are designed to prevent trade. Stigler takes the prevailing social consensus as the efficient equilibrium from which to assign a rational choice explanation as to why societies choose particular policies. The welfare implications of Stigler's theoretical paradigm economics for public policy is best stated by Stigler in his "Law or Economics?":

> [E]very durable social institution or practice is efficient, or it would not persist over time. New and experimental institutions or practices will rise to challenge the existing systems. Often the new challenges will prove to be inefficient or even counterproductive, but occasionally they will succeed in replacing the older system. Tested institutions and practices found wanting will not survive in a world of rational people. To believe the opposite is to assume that the goals are not desirable. (1992: 459)

Unlike Stigler, Buchanan combines the reformist zeal of the economist in a manner not inconsistent with his role of a scientist. Rather than approaching political economy from an approach that takes tastes and preferences as stable and given (see Stigler and Becker 1977), Buchanan argues that there is a non-normative basis for economic reform, which "may be summed up in the familiar statement: *There exist mutual gains from trade*" (italics original, Buchanan 1959: 137) from which individuals can benefit, and that such gains from trade can be realized by suggesting adjustments in the rules of the game. For Buchanan, the task of economists is broader than the study of the efficiency propositions of the market; it also includes "the study of all such cooperative trading arrangements which become merely extensions of markets as more restrictively defined" (1964: 220).

The political economist contributes to science and reform by analyzing alternative institutional arrangements, and offering changes in the rules of the game as hypotheses to be tested in the arena of collective action. In devising such changes in the rules of the game Buchanan stresses two critical building blocks. The first building block concerns the position of the status quo. The positive political economy of reform must begin with the "here and now," and never some imaginary start state where opposition to change is non-existent. In doing this, Buchanan is not attributing any normative weight to the status quo. All he is doing is insisting that "it is what it is" and that must be the starting point of any assessment of relevant alternatives. The second building block follows from the recognition that we begin with the "here and now," and that is the compensation principle. Any shift in the rules of the game will change the nature of the payoffs in the game. Those who currently gain from the status quo will lose, while others currently not in a position of privilege with respect to existing institutions will gain from the change. The winners must compensate the losers in the proposed change, not because the losers have any normative claim to their existing benefits but because unless compensated the beneficiaries of the status quo will fight to defeat any proposed changes in the structure of rules. For example, compensation can come directly from "buying out" the present discounted value of rents derived from changes in policy, such as monopoly profits lost by protected industries from the abolition of a tariff. From a more dynamic standpoint, however, compensation is paid not from redistributing a fixed pie of economic wealth, but from an economic pie that grows due to realizing the increasing returns to the gains from trade and innovation that are unleashed from an expanding the scope of the market. However, this dynamic conception of efficiency in terms of *discovering* ever greater gains from trade via institutional changes, as suggested by Buchanan, is incommensurable with a conception of static efficiency emphasized by Stigler, in which all of the gains from trade are already known and fully exhausted.

Conclusion

Stigler was a dominant figure in the evolution of the Chicago School. Given that he was a crucial transitional figure in the Chicago School, first as a student of Frank Knight, and later as a faculty member of the University of Chicago, analyzing his contributions to economic science and public policy helps us to understand what aspects of Knight's conception of economic and public policy were filtered through to the generation of Chicago economists of the post-WWII era, most notably Gary Becker, and those aspects of Knight's work that became deemphasized. We are not suggesting here that Stigler was merely a Knightian vessel, with no original insights of his own. Rather, his own original contributions to the development of economic science reveal that there is "a tale of two Knights" in the Chicago School, and that Stigler's particular understanding of Knight illustrates that what were *shared* methodological and analytical propositions held by early neoclassical economists later became different substantive points of economic analysis, ultimately generating different conclusions with regard to the public policy relevance of economic science.

Notes

1. https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/press.html.
2. The choice-theoretic approach is also outlined by Demsetz, in what he describes as "neoclassical price theory," in the following terms: "First, the theory exudes confidence that rational behavior succeeds in realizing mutually beneficial exchange opportunities. Second, it counts the individual—whether consumer, laborer, or business owner—as

unimportant, despite its reliance on self-interested individual behavior; it uses aggregations of the behavior of individuals to construct its equilibria, and in doing so it deprives the individual of any force in the economic system. Third, it relies on Marshall's two-bladed scissors, supply and demand, to construct these aggregations of the behavior of individuals" (Demsetz 1993: 795). The choice theoretic approach provides an atomistic view of individuals, in which individuals yield all economic power to the market and are in turn free to respond to the resulting market prices. Individuals thus are passively responding to exogenous changes in prices, rather than initiating price changes. They are free to choose, yet given their preferences, choice is essentially pre-determined.

3. Stigler's defines preaching in the following way: "I suppose that it is essential to state what I mean by preaching. I mean simply a clear and reasoned recommendation (or more often, denunciation) of a policy or form of behavior by men or societies of men" (1982: 3).
4. As Stigler states in a letter to Milton Friedman, "if a pure scientist—one believing only demonstrated things—is asked his opinion on policy, he must decline to answer—and listen to his intellectual inferiors give advice on policy" (see Stigler 1948 [2006]: 96).
5. Samuels' association of Stigler with the Austrian school is based not upon similar public policy conclusions, but based upon shared analytical propositions regarding economic theory. In his review of Stigler's *Memoirs of an Unregulated Economist*, Samuels writes the following: "We also find a Chicago School emphasis on the economy as *process*: on the implications of scarcity, on people responding 'rationally' to changes in the incentive structures around them, on the flexibility and adaptability of markets, and on competition in nonstructural, behavioral terms. For all the relevance of tight prior equilibrium, Chicago is not neo-Walrasian; it much more resembles neo-Austrian economics" (emphasis original, Samuels 1990: 409).
6. Though beyond the scope of this chapter, much has been written regarding the circumstances of this transition. Knight would continue to serve as on the economics faculty until his official retirement in 1951 (Emmett 2009: 84). In 1947, Aaron Director, brother-in-law of Milton Friedman, joined the Law School (Stigler 1988: 158), succeeding Henry Simons; the causes of Simons' death in 1946 are still being disputed (see Van Horn 2014). Jacob Viner moved from Chicago to Princeton in March 1946, which prompted a hiring process, in which Milton Friedman emerged as the compromise candidate between the

Knightian faction of the department, who had wanted to hire George Stigler, and the Cowles Commission (now the Cowles Foundation at Yale University), who had wanted to hire Paul Samuelson. As Mitch states, "It was arguably this mix of technical skills and serious policy interests that made Friedman viable as a compromise candidate in February 1946 garnering support from Marschak and Koopmans of the Cowles Commission as well as Frank Knight and his protégés" (Mitch 2016: 1727). Stigler would eventually move from Columbia University to receive a joint appointment in the School of Business and the Department of Economics at Chicago in 1958, when he was recruited by Allen Wallis, the then Dean of the Booth School of Business (and a former classmate of Stigler's at Chicago), as the Charles R. Walgreen Professor of American Institutions (Stigler 1988: 157).

7. As Stigler mentions on this point, though Knight and Viner were identified with classical liberalism and *laissez-faire* economic policy, the "rest of the faculty were highly varied in their policy preferences: Paul Douglas favored a large economic role for the state; Simeon Leland was a traditionalist in taxation; Harry Millis was an old-fashioned labor economist; Lloyd Mints wrote only on central bank policy; Henry Schultz stuck to his mathematical and statistical knitting; and Oskar Lange was a socialist" (Stigler 1988: 149).
8. The emphasis on quantification and empirical testing of theoretical propositions as a matter of public policy relevance is made quite explicit later by Stigler in his 1964 Presidential Address at the American Economic Association, where he stated the following: "The age of quantification is now full upon us. We are now armed with a bulging arsenal of techniques of quantitative analysis, and of a power—as compared to untrained common sense—comparable to the displacement of archers by cannon… It is becoming the basic article of work as well as of faith of the modern economist that at a minimum one must establish orders of magnitude, and preferably one should ascertain the actual shapes of economic functions with tolerable accuracy… It is a scientific revolution of the very first magnitude—indeed I consider the so-called theoretical revolutions of a Ricardo, a Jevons, or a Keynes to have been minor revisions compared to the vast implications of the growing insistence upon quantification. I am convinced that economics is finally at the threshold of its golden age-nay, we already have one foot through the door. The revolution in our thinking has begun to

reach public policy, and soon it will make irresistible demands upon us" (1965: 16–17).

9. As Becker writes, the "combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it" (1976: 5). But even Becker, who stresses the equilibrium conditions of a market, must also provide a background discussion about the process of equilibration for markets in disequilibrium. As he writes in his textbook, *Economic Theory* (1971: 92), the "stabilizing force is the negative slope of the demand curve and the positive slope of the supply curve because they imply that demand exceeds supply below the equilibrium price, and supply exceeds demand above it. A market can overcome this force and become unstable only if lags are introduced that require demanders or suppliers continually to make erroneous decisions."
10. With regards to monopoly and anti-trust regulation, this was not always the view of Stigler or of the Chicago School.
11. This is further evidence that a sweeping generalization of the Chicago School as always having propagated and idealized the efficiency of the market is a retroactive misrepresentation. For example, in "The Ethics of Competition" (1935 [1997]: 44), Frank Knight argued that "the workings of competition educate men progressively for monopoly, which is being achieved not merely by the 'capitalist' producers of more and more commodities, but by labor in many fields, and in many branches of agriculture, while the producers of even the fundamental crops are already aspiring to the goal." Moreover, Henry Simons, in his pamphlet, *A Positive Program for Laissez Faire: Some Proposals for a Liberal Economic Policy* (1934), went so far as to propose that poor regulation of alleged natural monopolies in utilities required direct nationalization of industries, including telephones and railroads.
12. Among the most important and critical works challenging the necessity of regulating natural monopolies as well as anti-trust legislation against market concentration, see for example Demsetz's "Why Regulate Utilities" (1968) and "Industry Structure, Market Rivalry, and Public Policy" (1973), the latter of which had sparked the Airle House Conference, which was held between March 1 and 2, 1974 in Virginia, debating the merits of anti-trust policy. The papers from this conference were later gathered in an edited volume entitled *Industrial Concentration: The New Learning* (1974).

13. This paper can be viewed and downloaded at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2710201.
14. Being in the mainline of economics (see Boettke 2012), this price-theoretic emphasis in the Old Chicago School can trace itself back to Adam Smith, but its more recent and direct origins can be found in Marshall. As Stigler states, Marshall's "treatment of competition was much closer to Adam Smith's than to that of his contemporaries" (1957: 9). As Marshall himself wrote, "Economic laws are statements with regard to the *tendencies* of man's action under certain conditions" (emphasis added, 1920 [2013]: 32).
15. This shared methodological understanding regarding price theory can be found both in Marshall and Böhm-Bawerk as well. As Marshall writes, "Some parts of economics are relative abstract or *pure*, because they are concerned mainly with broad general propositions: for, in order that a proposition may be of broad application it must necessarily contain a few details: it cannot adapt itself to particular cases; and if it points to any prediction, that must be governed by a strong conditioning clause in which a very large meaning is given to the phrase 'other things being equal.' Other parts are relatively *applied*, because they deal with narrower questions more in detail; they take more account of local and temporary elements; and they consider economic conditions in fuller and closer relation to other conditions of life" (emphasis original, 1920 [2013]: 31, fn. 1).
    Böhm-Bawerk restates this in similar terms: "Accordingly, it seems to me expedient to divide the problem of the theory of price into two parts. The first part concerns the necessity for developing *the law of the basic phenomenon in its purest form*…under the supposition that all persons participating in an exchange are actuated by the one single motive of the quest for the attainment of an immediate benefit through exchange. The second part of the problem consists in incorporating into the basic law the modifications which result from the contributory activity of other motives and factual circumstances…this second part is also the proper situs for revelations concerning the function performed by certain highly concrete institutions" (emphasis original, 1888 [1959]: 212).
16. Ludwig von Mises, whose economic methodology is often misrepresented as purely deductive, or aprioristic (see Boettke and Leeson 2006) makes reference to Frank Knight, with whom he shares a similar methodological perspective on economic theory. As Mises states: "Economics does not follow the procedure of logic and mathematics.

It does not present an integrated system of pure aprioristic ratiocination severed from any reference to reality. In introducing assumptions into its reasoning, it satisfies itself that the treatment of the assumptions concerned can render useful services for the comprehension of reality. It does not strictly separate in its treatises and monographs pure science from the application of its theorems to the solution of concrete historical and political problems. It adopts for the organized presentation of its results a form in which aprioristic theory and the interpretation of historical phenomena are intertwined" (von Mises 1949 [1966]: 66).

17. Following Knight, what we mean by "stationary" is "the use of given resources in accord with a given system of technology to realize given ends" (1936: 259).
18. Following the Socialist Calculation Debate between Austrian Economists, such as Ludwig von Mises and F.A. Hayek, and market socialists, such as Oskar Lange and Abba Lerner, this distinction was made more explicit and developed by the Austrians, particularly Israel Kirzner (1973, 1979), but is still conflated by many economists today. For example, the "tale of two Knights" theme that we have discussed is also embodied in Harold Demsetz, an important economist who straddles between the Old and New Chicago School and whose scholarship embodies elements of appreciation for dynamic market rivalry (see Demsetz 1968, 1973) and elements of appreciation for markets understood in terms of perfect competition (see Demsetz 1993). Following Knight, however, he regards the problem of economic calculation as one primarily about incentives, not about knowledge. As he notes: "Mises and Hayek note that the price system is much better at using knowledge and improving calculation than is central planning, but they emphasize the price system too much. It is not the price system per se that improves knowledge utilization. If it were, a socialist society could implement a price system, and some economists have urged just that…A socialist price system would yield prices that differ from those that would arise if ownership were private. *The problem is not knowledge acquisition* per se *but motives to marshal and use particular kinds of information*" (emphasis added, Demsetz 2002: S664, fn. 18). Though Demsetz is one of the leading scholars in the economics of property rights, in this quote, he ironically misses the point that Mises and Hayek were making an institutional argument regarding private property being a fundamental prerequisite for economic calculation.

As another student of Knight, G. Warren Nutter, states this point, market prices without property is a grand illusion (see Nutter 1968).

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James Buchanan and George Stigler: Divergent Legacies from Frank Knight

Richard E. Wagner

James Buchanan and George Stigler: Divergent Legacies from Frank Knight

George Stigler and James Buchanan were both students of Frank Knight, and each held Knight in high regard as evidenced by their respective scientific research programs being to a significant degree resident within Frank Knight's *oeuvre*. With their common source in Knight, it would be reasonable to expect significant points of similarity between Stigler's and Buchanan's research programs. All the same, surely more notable were the differences in the legacies Stigler and Buchanan took from Knight. To some degree, that difference in direction was a matter of Stigler and Buchanan choosing to aim their research programs at different substantive topics, as reflected in Stigler's initial concern with the theory of perfect competition and Buchanan's initial concern

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with the theory of public finance. Beyond different substantive topics, Stigler and Buchanan erected their research programs on different analytical hard cores, to recur to the methodology of scientific research programs. There is both complementarity and antagonism between their research programs.

The methodology of scientific research programs (Lakatos 1970, 1978; Latsis 1976; Weintraub 1993) construes scientific research programs as constituted through a hard core of propositions that a theorist accepts as data to provide a platform for the erection of theories. From that platform, a variety of propositions are developed to illustrate the scientific value of that core, and to provide protection against challenge from other research programs. The sociologist Randall Collins (1998) further explains that scientific inquiry is a social activity that generates competing schools of thought. Scholarship entails competition among scholars for limited attention space, due to limits on the abilities of scholars to read and digest other scientific formulations. Hence, a scientific map would resemble a variety of solar systems of variable circumferences. Most thinkers' bodies of work can be described abstractly as circles around their hard core, and with thinkers differing in the circumferences needed to contain their work. This characteristic fits Stigler and Buchanan. It does not, however, fit Knight. In terms of abstract geometry, Knight would surely have to be represented as resembling a Koch snowflake, with its indefinitely long circumference.[1] This feature of Knight's work enables scholars to construct different analytical cores while using Knight as a point of departure, as Stigler and Buchanan did.

Frank Knight was a common presence in the work of both Stigler and Buchanan, and yet our theorists of interest theorized in different analytical directions, reflecting the different hard cores on which they based their work. Knight stands clearly visible in the scholarly work of both Stigler and Buchanan, only Stigler and Buchanan appropriated different legacies from Knight. Before exploring that different appropriation, it will be helpful first to say a little about Knight's *oeuvre*, especially as it pertains to Stigler and Buchanan, and with Ross Emmett (2009) providing a masterful treatment of Frank Knight.

The Point of Origin: Frank Knight's *Risk, Uncertainty, and Profit*

Risk, Uncertainty, and Profit (1921) is a revision of Knight's doctoral dissertation from 1916 which he wrote at Cornell University, and which he titled "A Theory of Business Profit." After two preliminary chapters on methodological issues and theoretical forerunners, the book is divided into two parts. The first of these parts is titled "Perfect Competition." Over the preceding half-century, economists had been wrestling with articulating a useful and meaningful concept of a condition of perfect competition. Such a condition would mean that a situation would have been reached where it would be impossible to imagine any alternative allocation of resources within a society that could increase the well-being of one person without simultaneously rendering at least one other person worse off. Within this condition of perfect competition, a market economy will have enabled everyone to exploit fully the potential gains from social interaction, given their various initial starting points. These four chapters provided the capstone for a half-century of inquiry into the notion of a perfectly competitive system of economic organization.

The next section of *Risk, Uncertainty, and Profit* was titled "Imperfect Competition through Risk and Uncertainty." These chapters were antipodal to the chapters on perfect competition, in that they recognized that perfect competition was an ideal-type of construction that could not be a faithful representation of reality. Knight's treatment of imperfect competition explored the limitations of perfect competition to explain the operation of an enterprise-based economy. These chapters presented Knight's distinction between risk, for which insurance can be purchased, and uncertainty, for which insurance is impossible. This distinction has created consternation among economists that has persisted to this day, perhaps because many economists want to believe in the reality of a system of perfect competition. Risk is congruent with that belief, uncertainty is not.

The central feature of a system of perfect competition is a pattern of prices of outputs and inputs where the pattern of production reflects relative consumer desires and the inputs employed in production are paid according to the marginal value they add to production. When everyone receives what they produce on the relevant margins of action, moreover, the total output is exactly exhausted when it is distributed among the suppliers of inputs within a system of perfect competition. There is no surplus value within such a system, nor is there any deficiency that could be described as exploitation. For instance, if one occupation is more dangerous than another, leading to a higher rate of injuries among practitioners, wages will have to rise to attract workers, thereby compensating workers for their expected losses. This compensation can happen under two alternative institutional arrangements, one *ex ante* in character and the other *ex post*. Under the *ex ante* arrangement, individual workers will be responsible for their personal losses, with each worker thus self-insuring by receiving sufficiently higher compensation to attract the number of workers employers desire to hire. Under the *ex post* arrangement, those workers who have accidents will be compensated for their losses. Under this *ex post* arrangement, the risk of injury will have been shifted onto workers as a class, where under the *ex ante* arrangement the risk will reside with those workers who are injured. Either outcome is consistent with the theory of perfect competition. In either case, wages will be higher and output lower than would have been the case had there been no occupational hazards, illustrating the ability of a perfectly competitive system of economic organization to achieve full exploitation of gains from trade among market participants.

Perfection vanishes in the presence of uncertainty, according to Knight's conceptual framework. Furthermore, uncertainty is inherent in life as an unavoidable feature of innovation and change. The linchpin of Knight's argument was the gap between risk and uncertainty, and with that gap being an inherent feature of human action. The typical economic action entails making a commitment of resources today, knowing that the result of that commitment won't be known until some future moment. A decision to produce a product must be made prior to the sale of that product. The marginal productivity theory of distribution envisions factors of production being paid the values of their marginal products within a competitively organized economy. There is, however, a logical difficulty in this common formulation, and with that difficulty pointing to the gap between perfect and imperfect competition.

Creating a plan to produce a product requires knowledge of consumer demands for that product as well as of the prices of the inputs necessary to produce that product. Within the theory of competitive equilibrium, those inputs are paid the values of their marginal products under free competition. To be sure, there is no direct calculation of such marginal products. Rather, that outcome is a feature of the logic of a system of perfect competition. If hiring an input at its current market price will add more to an enterprise's net worth than it costs the enterprise to hire it, the enterprise will hire it. And the enterprise will continue to hire such inputs until the enterprise no longer will be able to increase its net worth by hiring further inputs.

This logic of competitive equilibrium might make sense within what could be denoted as a stationary or evenly rotating economy where economic activity is a monotonous parade of repetition. As demanders of products, consumers will always buy the same things, never venturing to try something new—for doing this would change the demands for different products. As suppliers of productive inputs, people will never tire of their jobs and thus will never choose to change jobs. And as employers of productive inputs, firms will never create new products, but rather will continue indefinitely to repeat what they have been doing as far back in time as what one can recall. With this scheme of thought, the model of competitive equilibrium is taken to be a snapshot of a situation that has carried forward from the past and will persist into the future—unless there is some exogenous disturbance that elicits some type of reaction from within the economic system. Otherwise, the system of competitive equilibrium is quiescent.

Knight recognized that actual economies are far from quiescent, which means in turn that the theory of perfect competition does not provide an adequate framework for understanding an enterprise-based economic system. What was lacking from the theory of perfect competition was any internally-generated source of change. To incorporate internal change into an economic system, however, violates the framework of perfect competition, creating a system that Knight described as imperfect competition where entrepreneurial action gives creative direction to patterns of resource utilization within an enterprise-based economic system.

Suppose the gap between production and consumption is one month. There are two ways in which production can be organized, one collectivistic and the other individualistic. Within the collectivistic mode, workers will not be paid until the product they produce is sold, so they will have to finance their consumption during the month through their capital. This arrangement recognizes that the value of the workers' efforts during the month won't be known until the next month when the product is sold. Workers are effectively investors in the enterprise, as illustrated by a worker-managed firm. Within the individualistic mode, an entrepreneur will guarantee a wage to workers while production is occurring. In doing this, the entrepreneur is projecting a sales price for the product, with that projection guiding the demand for labor during the month. Any gap between that projection and the resultant experience when the product is sold redounds to the gain or the loss of the entrepreneur. This gain or loss is unknowable and so uncertain, in contrast to risk which is subject to exact calculation. For Knight, imperfect competition did not mean market failure of some type that could in principle be corrected through collective action. To the contrary, it was an inherent feature of the forward-looking character of an enterprise-based system where people accept contractual terms prior to the sale of the products that are produced when governed under those contractual arrangements.

Aside from his *magnum opus*, *Risk, Uncertainty, and Profit*, Frank Knight was mostly an author of essays, many of them book reviews. Those essays show Knight to be fascinated with the many paradoxes that reside in the penumbra between the ideal of perfect competition and the reality of imperfect competition. That fascination extends, moreover, to the place of politics within the economic process, and with much of his thinking summarized in the set of lectures published *as Intelligence and Democratic Action* (Knight 1960). While Knight entered scholarly life as an economic theorist, much of his subsequent writing would be better described as social philosophy, with James Buchanan's writings also containing a good deal of social philosophy. Knight's social philosophy centered on the value of liberty, and the problems of maintaining a system of liberal political economy due to cacophonous currents resident within human nature. In many writings, Knight explored incongruences between the liberal vision of a free economy and the political economy of liberal democracy.

In a highly perceptive essay, Angus Burgin (2009) explored the numerous paradoxes that infused Knight's thinking. While Knight valued individual liberty probably as highly as anyone, he thought that people were generally incapable of exercising the discipline required for liberty to flourish. For instance, Knight thought that a freely competitive economy and a democratic form of government could be highly complementary, but only so long as political processes operated though discussion and the formation of genuine consensus. Human nature, however, tended to convert discussion into persuasion, thereby replacing genuine discourse with eristic discourse. Politics becomes the activity of professionals who are particularly adept at manipulating symbols and channeling public discussion in directions they prefer. Furthermore, people who are highly successful in market activity will often congratulate themselves for their success and look down on people who haven't been so successful. Knight thought a market economy would generate a degree of inequality that could not be reasonably justified, and which would foment opposition to a social order based on freedom of enterprise. At the same time, Knight doubted the ability of political action to maintain the tensions between opposing forces and principles that a system of liberal political economy required, as the essays collected in Knight (1947) illustrate. This paradoxical side of Knight, Stigler avoided while Buchanan wrestled with it.

Imperfect Competition and the Years of High Theory

George Shackle (1967) describes the period 1926–1939 as the *Years of High Theory* in economics. Starting in the 1870s, economists had come increasingly to refine what they meant by a liberal system of economic activity organized under conditions of free competition within an institutional framework of private property and freedom of contract. The last (8th) edition of Alfred Marshall's *Principles of Economics* had been published in 1920 and Knight's *Risk, Uncertainty, and Profit* was published the next year. What Shackle described as the *Years of High Theory* represented a shift in theoretical direction away from further elaboration of ideas about how a system of free enterprise enables societies to flourish, shifting the analytical focus toward ways in which a system of free enterprise will fail to promote flourishing, encapsulated in Pigou (1920). Where imperfect competition for Knight (1921) referred to entrepreneurial sources of progress, the newer concepts of imperfect competition described impediments to flourishing, and placed those impediments into two categories. One category pertained to ways in which an enterprise system might benefit some people at the expense of others. The other category pertained to the possibility that an enterprise system might experience systemic collapse.

In both cases, economic theory underwent transformation from a largely scientific orientation concerned with explaining how economic activity reflected generally coordinated patterns of action toward a focus on how political action might be employed to overcome the market failures associated with the non-Knightian notion of imperfect competition. During the years of high theory, the attention of economists shifted for the most part onto sources of market imperfection or failure, thereby transforming most economists' vision of their discipline from science to engineering. Economics now became an instrument of statecraft where economics creates knowledge of how to keep the economic engine working well, in contrast to the earlier focus on explaining how an economic system generates generally orderly patterns of activity without there being engineers to manage the system—as distinct from there being numerous engineers who work to manage individual enterprises within the system.

Knight's scheme of imperfect competition was nothing like the theories of imperfect competition and market failure that came into economics in the years of high theory. For Knight, though not for Stigler, imperfect competition pertained to reality and not some ideal-typical notion of perfect competition. Imperfect competition could not be transformed into perfect competition, for that transformation could occur only if people stopped the activities associated with learning, imagining, and experimenting. Within a theory of perfect competition, it would be reasonable to speak of all inputs as receiving the values of their marginal products, for this was a necessary condition for systemic equilibrium. Not, however, for a theory of imperfect competition. In this case, entrepreneurs are receiving payments, possibly negative, for bearing the uncertainty that someone must bear if other members of society are to receive fixed contractual payments for their work when the value of their work won't be determined until some later date when the products they helped to produce are sold.

There are two paths along which a theory of imperfect competition can be pursued. One was Knight's path; the other was the path that characterized the years of high theory. For the branch of theory that took shape during the years of high theory, imperfect competition meant that an enterprise-based economy failed to achieve some full exploitation of the potential gains from trade within a society. In the presence of market failures, a large literature on market failure and welfare economics created a large and growing encyclopedia of cases of market failures along with calls for political actions to overcome those failures. To be sure, not all instances of alleged market failure were thought to be suitable objects for perfection through political intervention, for there may be some tasks that lie beyond state competence. Still, the theory of imperfect competition was thought, within this analytical branch of the theory, to obviate the default presumption in favor of the beneficent qualities of an economic system of free competition.

Knight's analysis, however, took the other path. Progress within a social economy arose through entrepreneurs disturbing existing configurations of enterprises by creating new products, establishing new businesses, and rearranging methods of doing business. Entrepreneurial action starts with the belief that some new product or organizational change will increase an enterprise's net worth. The elemental logic of production and sale requires an entrepreneur to decide to produce a product prior to consumers offering their judgments on that product. Values of the marginal products of productive inputs can't be established until consumers have responded to entrepreneurial offerings. This raises the question of what kind of commercial organization will organize production if inputs can't receive payment until after products have been sold. Production could be organized as producers' cooperatives, where workers manage firms and provide the capital. Rather than receiving wages, they would receive dividends.

Stigler and Buchanan in Relation to Knight

Frank Knight supervised George Stigler's dissertation which Stigler finished in 1938 and which he published in 1941, after revision, as *Production and Distribution Theories, The Formative Period*. Stigler's book treated the development of the neoclassical theory of production and distribution during the formative quarter-century starting with William Stanley Jevons and ending with John Bates Clark, and with the other theorists examined being Philip Wicksteed, Alfred Marshall, Francis Edgeworth, Carl Menger, Friedrich Wieser, Eugon Böhm-Bawerk, Léon Walras, and Knut Wicksell. These ten theorists were in the forefront of developing a coherent theory of the structure of production and the distribution of output within an enterprise economy which largely characterized the western world at the time. Those theoretical efforts culminated in the marginal productivity theory of production and distribution. Within this theory, inputs were allocated among activities to achieve equality of marginal products among uses. Open competition, moreover, priced inputs so that each input received the value of its marginal product. Significant effort among these theorists was devoted to what was called the adding-up problem: if each input received the value of its marginal product, would such a system of competitive pricing exactly exhaust the total product?

For Knight, perfect competition was an ideal-type of economic model. As an ideal-type, it could be useful in organizing thought, but the construction of an ideal-type should not be confounded with the actual system of economic organization in place today or at any other historical moment. The model of perfect competition could provide a useful window for looking onto economic and social reality, but it was not a flawless representation of that reality. The final six chapters of

Risk, Uncertainty, and Profit were devoted to explaining many sources of deviation between the ideal of perfect competition and the reality of imperfect competition. In contrast to Knight, Stigler took on the task from early in his academic life, as illustrated by the set of lectures collected in Stigler (1949), to explain that perfect competition provided the best available analytical window we had for observing economic reality. Stigler diverged from Knight in confronting claims that perfect competition did not fit reality well.

Stigler denied that apparent divergences were significant. There could be divergences at the level of description, but those divergences may have little or even no impact upon production and distribution within a competitively organized economy. There were two avenues along which Stigler pursued his denial of the significance of deviations from perfect competition. One was through an imaginative use of theory. The other was through a creative effort to bring statistics to bear on theoretical categories, often with those two avenues coming together in the same paper.

Frank Knight did not supervise James Buchanan's dissertation, Roy Blough did that, with Knight serving as a secondary reader. In Buchanan's mind, however, Knight was the effective supervisor of his dissertation. It was Knight with whom Buchanan consulted, and whose opinions Buchanan treasured, about economics as well as about life in general. Buchanan wanted to write a dissertation on public finance, with Blough being the person in public finance in Chicago at that time. Buchanan's dissertation work culminated in papers in the *Journal of Political Economy* (Buchanan 1949) and *American Economic Review* (Buchanan 1950). Buchanan's reverence for Knight is revealed both in Buchanan's own testimony in many places as well as by recognition that portraits of Knight and Knut Wicksell were the only portraits he had in his office. So, Buchanan and Stigler can both be reasonably regarded as immediate students of Knight, even though Knight supervised Stigler's but not Buchanan's dissertation.

Stigler and Buchanan differed in the use to which they put Knight's vision of perfect competition. Stigler mostly ignored Knight's account of entrepreneurship as something that could not be theorized about systematically, any more than mistakes provided useful material for economic analysis. Stigler also brought politics into the penumbra of the theory of perfect competition. In contrast, Buchanan sought to bridge the gap between Knight's formulations of perfect and imperfect competition, while also emphasizing differences between markets and politics. Buchanan also tried, modestly, to incorporate some of Knight's repeated tergiversations about economic theory in relation to reality into his work, in contrast to Stigler's comparative contentment with the state of economic theory.

In contrast to Stigler, Buchanan embraced Knight's recognition that the theory of perfect competition was an ideal-type construction that was not an accurate description of reality. Buchanan also embraced to a fair degree Knight's sense of the paradoxical qualities that reality presents for thought. In contrast to Stigler, Buchanan distinguished between the form a theory takes and its substantive content. As a formal matter, economic theory pertained to some universal qualities of human action and interaction which were not subject to empirical testing. How particular theories might be brought to bear on various substantive situations were suitable topics for empirical examination, but the underlying conceptual ideas were to be judged logically and not empirically. Buchanan tried to bridge the gap between Knight's formulations of perfect and imperfect competition, while also emphasizing differences between markets and politics.

For both Stigler and Buchanan, Frank Knight's presence is apparent in their bodies of work. Both received scholarly legacies from Knight, but the content of those legacies differed. For Stigler, Knight's formulation of perfect competition provided a navigational beacon for Stigler's construction of his scholarly *oeuvre*. In making his construction, Stigler deviated from Knight in three significant ways. In contrast to Knight, Stigler put no credence in theories of imperfect competition. Also in contrast to Knight, Stigler thought that the claims of economic theory should be tested empirically, and with Stigler's subsequent empirical work illustrating in many ways the significant explanatory power of a properly formulated model of perfect competition. Furthermore, Stigler brought political action and phenomena within the rubric of the theory of perfect competition.

Stigler as Economic Theorist

Stigler's economic theory was a refinement of Knight's theory of perfect competition, with Knight's recognition of the unrealistic character of the model removed. Stigler accomplished this removal by embracing a version of logical positivism, which was in robust play at the time and which was anathema to Knight all the same. Within this positivist orientation, which Milton Friedman (1953: 3–43) later elevated to canonical status, a model was not to be rejected because some of it its assumptions did not seem to accord with reality. Once you recognize that all models are reductions from a reality that is necessarily more complex than the reality, and that all economists work with models as Mary Morgan (2012) explains, a failure of some aspect of a model to conform to some element of reality is no cause for rejecting the model. Models rise or fall according to their ability to offer predictions that can be either confirmed or disconfirmed, when compared with the data of reality, within Stigler's methodological framework.

There can be many reasons why a model of perfect competition does not seem to fit with reality and yet fits well with the data of experience. Likewise, there can be many models of imperfect competition whose assumptions seem to fit more sensibly with our sense of reality, and yet which offer us no insight into the world we experience. Sense perceptions can lead us to think we see things that aren't there, and it requires empirical work to determine whether this is the case. Stigler started his work on economic theory with the presumption that the theory of perfect competition generated superior insight into the economic organization of enterprise-based systems, and then sought to explain how the world of experience could be rendered intelligible through the theory of perfect competition, despite the superficial appearance that perfect competition described a situation that was not found in reality.

Stigler (1949) offers two essays that are pertinent to this point. The first (1949: 12–24) explains that the theories of monopolistic competition that had been advanced during the years of high theory offered no ability to illuminate economic situations or processes. Those theories offered greater descriptive detail, but all this did was add clutter without adding offsetting illumination. As compared with a theory of perfect competition where firms face infinitely elastic demands, leading in turn to straightforward implications that firms produce at minimum average cost, what is gained by working with an elasticity of–100, (and some small gap between price and marginal cost) and a gap that varies among firms because each firm is now heterogeneous? Stigler's answer was that nothing is gained. The second essay (1949: 46–62) brings data to bear on two common claims about the imperfectly-competitive quality of an enterprise system of economic organization. Against the claim that we live amidst a gang of monopolists, Stigler presented evidence that competitively organized industries were more than twice as significant as monopolies. With respect to trends in the proportion of competition and monopoly since 1870, Stigler found a modest increase in monopoly, and with the bulk of that increase being a product of the strong decline in the importance of agricultural employment. Stigler's overall reading of the evidence was that competition had been increasing modestly up to the time of his examination, with Warren Nutter (1951) offering a book-length treatment that supports Stigler's general theme.

A literal reading of Knight's theory of perfect competition required that everyone effectively be omniscient. Yet omniscience is in no one's grasp. But how close to omniscience might someone try to get? Stigler (1961) recognized that this is a matter of the cost and gain of acquiring knowledge. Armed with this simple insight, Stigler asked whether actual market processes did an effective job of generating useful information for people to use in making transactions. Suppose the only source of information people could acquire required them to visit vendors. In this setting, would people be so ignorant that they could be fleeced repeatedly by price-gouging vendors? A fool and his money may very well be soon parted, but Stigler supplied reasonable grounds for thinking that people pursue useful knowledge in a cost-effective manner. Searching for prices entailed the expense of visiting vendors in Stigler's model. The gain from searching manifested in finding a lower price than had previously discovered. People who acted prudently to guard against getting fleeced by paying exorbitant prices would make more effort to search for prices when the expected gain from the effort was higher. Finding a lower price for an item will generally offer a higher gain for a high-price item than a low-price item. The empirical record Stigler examined showed less price variation for high-value durable goods than for low-value goods. Consumers are not omniscient, no one is, but they seem to be prudent in making purchases without acquiring suitable information.

To be sure, searching for prices by visiting vendors is not the only source people have for obtaining information. It might not even be the best source. For one thing, vendors advertise their offerings and prices. If a vendor can supply information more cheaply than consumers can solicit it, elementary economic principle tells us that the competitive efforts of vendors to attract customers will economize on the search for information by consumers as a by-product of the search for profit. In this and in numerous other instances throughout his career, Stigler was able to put the theory of perfect competition to good use, despite what might seem to be descriptive shortcomings. For Stigler, however, those shortcomings were not a counsel to replace perfect with imperfect competition. To the contrary, they were a counsel to dig more deeply into the material to find ways in which market process have been fashioned so as invariably to capture gains from trade whenever someone sees an opportunity to do so.

George Stigler and Gary Becker (1977) is a piece of methodological writing that further illustrates the imaginative as against the literal use of the theory of perfect competition. Stigler and Becker were reacting against the proclivity of many economists to attribute change through time or variation in actions among groups of people to variations in preference. In contrast, Stigler and Becker recognized that using changes in preferences as an explanation for some phenomenon could be an easy refuge for a scoundrel to seek, and so advanced the working hypothesis that preferences were invariant across time and place. This injunction did not represent some essentialist fact of life in an effort to reduce economics to ethology, but represented a methodological counsel to pursue as far as possible the use of variations in prices and incomes in explaining differences in actions across time and place. It's not that utility functions are truly invariant across time and place, for no one has seen a utility function anyway. The point is rather that taking recourse to changing preferences can easily serve as a way of avoiding scientific analysis rather than engaging in it. Throughout his body of work, Stigler showed how the theory of perfect competition could explain many economic phenomena that seemed on superficial examination to violate the assumptions that commonly accompanied the theory of perfect competition. Stigler showed that the model of perfect competition can offer a fertile framework for developing subtle explanations regarding the world we experience.[2]

Buchanan as Economic Theorist

Buchanan's economic theory likewise descends from Knight, only from a different branch of Knight's multi-faceted scheme of thought. Like Stigler, Buchanan thought the theory of perfect competition could be put to good use, only not universally so. Wagner (2017: 19–23) describes Buchanan's economic theory as residing between stasis and chaos. Knight's vision of perfect competition was a vision of stasis, and Buchanan recognized that useful analytical purposes could be served in working with this model on some occasions, though not on all occasions. The model of perfect competition has no room for internally-generated change. The only source of change in that model must come from outside the model. Knight recognized this feature of perfect competition, and set forth imperfect competition as his means of recognizing that the internal generation of change is a persistent feature of modern economic life. Buchanan likewise recognized this situation and the theoretical problems and challenges it presents, which led Buchanan to center economic theory around exchange and the institutional arrangements that both facilitated exchange and emerged through exchange, in contrast to the common focus on resource allocation (Buchanan 1964). Buchanan's analytical framework led to his regarding the material of economic theory as residing somewhere between stasis and chaos.

The challenge for Buchanan's approach toward economic theory is how to inject internal change into the economic process. The Stigler and Becker (1977) framework does not allow internal change, for there is no way people can change their actions without prior changes in prices or incomes. One might pose some sort of exogenous shock, as in a crop failure or destructive storm; otherwise, life will proceed repetitively *ad nausea*. Buchanan (1969) recognizes the non-separable quality of cost and choice. Any choice must be among at least two options, with the cost of the choice being the value of highest valued option that is not chosen. We may bring in imagination and experimentation, which Buchanan brought along from his association with Frank Knight. Here, the question becomes whether people are modeled as having completely ordered utility functions or only partially ordered functions. Stigler worked with complete orderings. Buchanan opted for partial orderings, because doing this offered a path into the treatment of enterprise-based economies as being sources of invention and innovation.

In a highly imaginative essay, Ross Emmett (2006) asked what Frank Knight would have thought about Stigler and Becker (1977). The challenge Emmett faced in writing this paper is that Frank Knight died in 1972. Nonetheless, Knight had amassed numerous essays where he spoke of individual liberty not in terms of maximizing utility but in terms of people being free to make of their lives what they choose, one fact of which was self-improvement. Related to this concern is whether what economists denote as preferences are genetically determined or whether they are to some extent environmentally influenced. If genetically determined, economics is a branch of ethology. If environmental influences are relevant, economics acquires elements of a philosophical and moral science. Thirty of Buchanan's essays collected in volume 17 of his *Collected Works* are titled "Moral Science and Moral Order."

Buchanan did not object to the model of perfect competition. He objected to assuming a state of perfect competition, as against explaining how markets might become competitive through the continual entrepreneurial search for profit. Any model of imperfect competition will contain unexploited gains from trade among market participants, and those unexploited gains are simultaneously profit opportunities for entrepreneurs to capture once they figure out how to do so. In seeking to place the analytical focus on how markets become competitive through entrepreneurial action, Buchanan was seeking to bridge the gap between Knight's formulations of perfect and imperfect competition, in contrast to Stigler who tried to show that that gap was an illusion that could be exposed through the right pile of statistics.

Stigler's Theory of Political Economy

Stigler's belief in the explanatory power of the theory of perfect competition likely generated an analytical path to his subsequent work on regulation and political economy. Theories of monopoly, and its imperfect competition cousin, generally claimed that political control could transform monopoly gains into consumer gains by inducing monopolies to act as if they were competitive industries. By forcing a monopoly to set price equal to marginal cost, a political entity can force monopolist to act as if it were a competitive industry. What would have been profits for a monopolist are thus transformed into a higher output and a lower price. To be sure, the literature on regulation contained numerous caveats when Stigler started writing about regulation. A rule of marginal cost pricing would work on the whiteboard for industries where production was subject to constant returns. But if there was a significant range of increasing returns, setting price equal to marginal cost would force firms to produce at a loss, which was incompatible with keeping producers in business. That loss could be covered by public subsidy, but such subsidies would impose losses elsewhere in the economy. At this point, the theory of regulation enters the perilous world of second best theorizing, which can be summarized by the aphorism that nearly anything is possible.[3]

This was the conventional theory of the political regulation of monopoly until Stigler and Friedland (1962) asked just what it is that regulators can regulate, and did so within the context of electricity. Stigler and Friedland approached this question empirically, and found that the presence of political regulation had little detectable impact on the prices of electricity or on the rates of return producers received. Stigler followed his study of electricity regulation with other studies, several of which are presented in Stigler (1975). While Stigler's claims about the accomplishments of regulation have been challenged, the facile presentations that were once common about the need for regulation to curb enterprise monopoly have subsided to a great degree. As Stigler (1975: 24) declares: "We can get on a bus labeled Economic Reform, but we don't know where it will take us."

But if regulators can't transform monopoly into competition, it is reasonable to wonder just what regulators are doing and accomplishing. Regulators incur cost, for the resources used by regulators could have been used elsewhere. If regulators can't truly regulate, one can't help but wonder why regulation is still prevalent. This question is addressed in Stigler (1971), where he advances the notion that regulation is pursued to promote the interests of the firms being regulated. Stigler's insight into this question inspired a prolix variety of studies of how regulation might benefit at least a portion of the industry supposedly being regulated. As a particularly dramatic instance, the Civil Aeronautics never allowed a single air carrier to engage in interstate transportation from the time it was created in 1938 to the time it was abolished in 1978. Yet this period saw an immense growth in air travel, which would have entailed the entry of numerous competitors under open competition. Indeed, the dramatic growth in air carriers after abolition of the CAB, as well as the disappearance of several carriers that were well established during the CAB's reign, are hard to reconcile with claims that regulators protect consumers, but fit easily with claims that regulators protect competitors, or at least some of them.

From this explanatory theory of regulation came a more general examination of the economics of politics, to which Stigler (1988) applied the rubric Chicago Political Economy. The chief feature of Chicago political economy is the sweeping of political economy into the framework of perfect competition, with a difference that takes into account some differences between market arrangements and political arrangements. This sweeping of politics into economics was countenanced by Frank Knight (1947: 242–243): "The political and economic structures of society are so closely related that they are ultimately little more than aspects of the same organization. … This means that either politics or economics can be regarded as a sub-division of the other." By incorporating politics into the rubric of perfect competition, Stigler did not claim that political outcomes were identical with market outcomes.

Political outcomes entailed redistributions of wealth that were not present in market outcomes where all transactions were agreeable to all participants.

Political actions entailed winning coalitions imposing costs on losers, as a more general feature of the recognition that regulation benefits some firms in an industry at the expense of other firms. Stigler's claim here, however, and which is amplified in Becker (1983), is that winning coalitions will extract their gains from losers in an efficient manner. Politics will redistribute wealth, but will do so in an economically efficient manner, meaning that any observed volume of redistribution is accomplished in a manner that minimizes the excess burden associated with that redistribution. This Stiglerian line of analysis leads readily to the incorporation of a democratic polity into the marginal productivity formulation of perfect competition, as Donald Wittman (1995) exemplifies.

Buchanan's Theory of Political Economy

Buchanan's political economy also has features that can be traced to Knight, but Buchanan hit upon different features of Knight than did Stigler, just as was true for their economic theories. Stigler entered political economy indirectly through his desire to explain the superiority of the theory of perfect competition over that of the notion of imperfect competition that arose in the 1930s, and this desire led him to the study of regulation and political economy. Buchanan entered the study of political economy directly from his dissertation research through his effort to put the theory of public finance on an explanatory footing, and with Marianne Johnson (2014) examining the relevant archival record regarding Buchanan's desire to construct a different approach to public finance from what he encountered at Chicago. The public finance that Buchanan encountered during his student days was almost exclusively normative. Public finance was typically thought to be a branch of social engineering or applied statecraft. Public finance was concerned with such matters as setting forth the features of what would comprise a good or a just system of taxation. Differences among theorists would reflect their presuppositions about goodness or justice.

In contrast, Buchanan wanted to place public finance on more of an explanatory setting where patterns of taxation, among other fiscal phenomena, reflected interaction within some set of political and fiscal institutions where people were trying to pursue their various objectives. An explanatory theory of public finance sought to explain why tax rates are what they are rather than some different rate, why some forms of income are excluded from the tax base, or why one base is selected among several possible bases. In all such cases, the explanation would play out in terms of individuals pursuing their interests in a world where people differ in what they desire, and with differences in such desires intermediated through some set of political institutions. Buchanan's theoretical interests were merged with his normative individualism, leading to a scheme of thought that took seriously and not just formally the common declaration that democracy was a system wherein people governed themselves. Yet masses can't govern themselves directly, recognition of which led Buchanan to explore how different constitutional frameworks might promote or impede genuine self-governance (Buchanan and Tullock 1962; Buchanan 1975; Brennan and Buchanan 1985).

In taking this approach, Buchanan, like Stigler, was reflecting Knight's recognition that there were no essential differences between economics and politics. In this respect, Harold Lasswell (1958) described politics as the study of who gets what, when, and how. Lasswell's characterization of the domain of politics is about the same as Knight's (1951) presentation of any economic system as necessarily addressing questions of what is produced, how it is produced, and for whom it is produced. Buchanan, like Knight, had democratic values, in that both believed in governance through consensus. But how to describe an institutional framework that would promote consensual governance, in contrast to promoting governance by dominant groups over others, posed a severe analytical challenge that animated the bulk of Buchanan's scholarly work, as Wagner (2017) explains. In pursuing this analytical agenda, Buchanan was instrumental in creating the approach to political economy now known as Public Choice, as well as in spearheading the approach to political economy that has been known from the early 1960s as Virginia political economy (Buchanan 2006;

Boettke and Marciano 2015; Wagner 2015b; Wagner 2004), which has both similarities and differences with Chicago political economy. Most significantly, Buchanan's analytical framework held the future open within limits, in contrast to Stigler's closed framework where there was no escape from the data of systemic equilibrium. To some extent the future world we experience is an emergent quality of imaginative and creative actions and interactions, even though the emergence of the future out of the present is still subject ex post to apprehension through the logic of economizing action suitably articulated.

Stigler and Buchanan: Their Varied Legacies from Frank Knight

As legatees of Frank Knight, George Stigler and James Buchanan surely put their inheritances to good use. Stigler received the Nobel Prize in economics in 1982, and was cited for his work on industrial organization, information and market processes, and public regulation. Buchanan received the Nobel Prize in 1986, being cited for his work on public choice, political decision-making, and the constitutional structure of political entities. Both, moreover, are associated with contemporary schools of thought. The Chicago school of economics and political economy is associated with the universality of the theory of perfect competition, contrary to Knight, while also extending perfect competition to politics. The Virginia school of political economy is associated with recognition of significant divergences between market interaction and political interaction, recognizing in either case that people invariably seek to make the most of their situations as they understand themselves and their situations. Stigler and Buchanan have both passed along impressive bodies of work and models of scholarly conduct for those who choose to follow in their wakes. In this, they reflect well upon Frank Knight as well as on themselves, while providing inspiration for those who come after them.

Notes

1. A Koch snowflake is generated by starting with an equilateral triangle, removing the middle of each line segment, and constructing another equilateral triangle on that gap. That procedure can be repeated indefinitely, creating a highly jagged circumference of indefinite length.
2. Hammond and Hammond (2006) collects the correspondence between George Stigler and Milton Friedman until they were together again at the University of Chicago in 1958.
3. The seminal formulation of second-best theory is Lipsey and Lancaster (1956). A highly skeptical examination of this theory from the perspective that any such instance of market failure implies simultaneously the presence of profit opportunities through institutional or organizational arrangement is Wagner (2015a).

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