

PALGRAVE STUDIES IN ECONOMIC HISTORY



AN ECONOMIST'S GUIDE
TO ECONOMIC HISTORY

*Edited by
Matthias Blum and Christopher L. Colvin*



Palgrave Studies in Economic History

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Editors

An Economist's Guide to Economic History

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*To Valeria and Elisabeth,
and to Sue and Glenn,
for supporting us, always.*

INT. EDINBURGH - BOW BAR - EVENING

EOIN McLAUGHLIN, CHRIS COLVIN and MATTHIAS BLUM are in a deep discussion about their very important economic history research.

EOIN

What's your favourite movie related to economic history?

CHRIS

'Mary Poppins'.

EOIN

Why?

CHRIS

Because of the bank run.

EOIN

And yours, Matthias?

MATTHIAS

'There Will Be Blood'. Because of all the resource depletion and unbridled capitalism.

EOIN

Both quite different.

CHRIS

So, what's yours then, Eoin?

EOIN

'The Terminator'. Overinvestment in ICT.

FADE OUT

An Economist's Carol

Volume 1

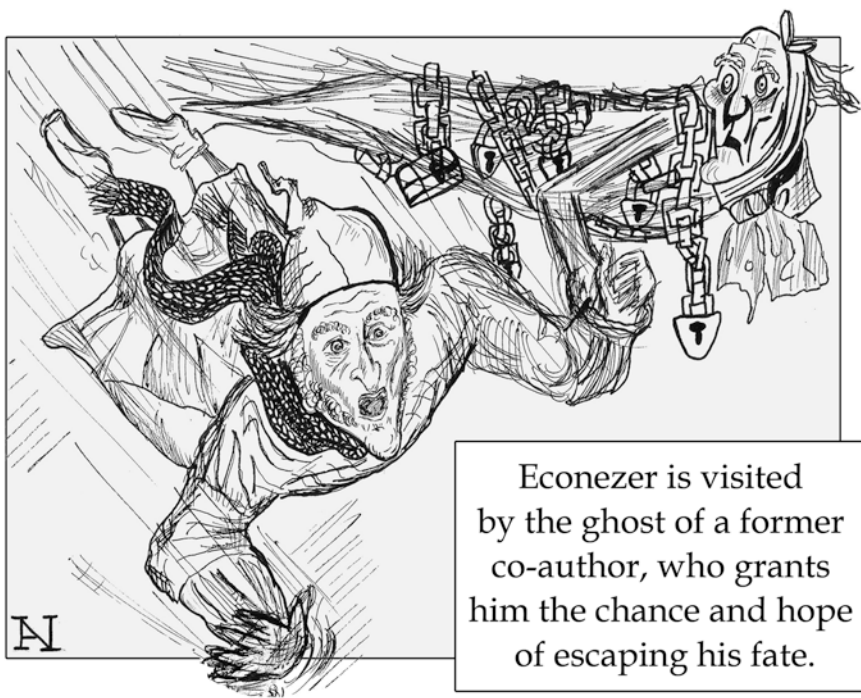


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Meet Econezer Scrooge PhD,
an old econo-miser.

An Economist's Carol

Volume 2



An Economist's Carol

Volume 3



the Economic Past,

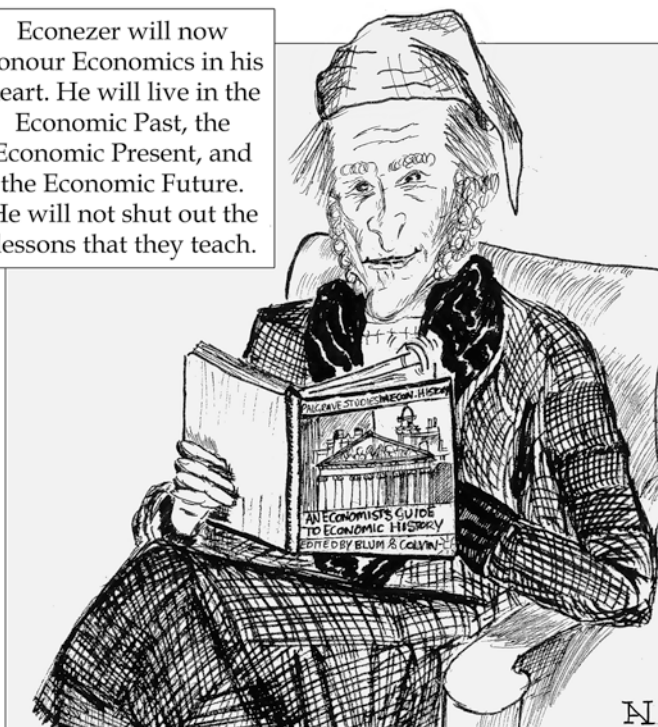
the Economic Present,

and the Economic Future.

An Economist's Carol

Volume 4

Econezzer will now
honour Economics in his
heart. He will live in the
Economic Past, the
Economic Present, and
the Economic Future.
He will not shut out the
lessons that they teach.



Praise for *An Economist's Guide to Economic History*

“Every economist who appreciates the importance of economic history should read this book as they will learn so much from the wealth of the material it covers. More importantly, all the other economists, those who do not yet know why economic history matters to them, should read it too. Whatever the subject—financial crises, innovation, trade wars, competition policy, and many others—understanding the historical perspective is essential to sound economic analysis and must be part of what students of economics are taught.”

—Diane Coyle, *Bennett Professor of Public Policy, University of Cambridge, UK*

“Economists have much to gain from studying economic history seriously. This excellent volume explains why, elaborates what this entails, and demonstrates the potential for synergies between economics and economic history. The result is a compelling manifesto.”

—Nicholas Crafts, *Professor of Economic History, University of Warwick, UK*

“The list of contributors to this project is truly impressive, as is the breadth of the topics covered. The result is a terrific teaching resource that will give students a good sense of the many ways in which economic history can help economics come alive.”

—Kevin H. O'Rourke, *Chichele Professor of Economic History, University of Oxford, UK*

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1

Introduction, or Why We Started This Project

Matthias Blum and Christopher L. Colvin

When the two editors of this volume were being trained to become economic historians not so long ago, any economists we met—with the best of intentions—would warn us our chosen field had passed its sell-by date. We were told economic history as a discipline had become parochial, that there would be no jobs for us, that we should be prepared to retrain and do something else with our lives. In some respects, the concerns of our peers and educators were justified. After all, one of us attended a university which employed a single solitary professor of economic history. And the other was based at the last remaining dedicated department of economic history in the English-speaking world.

The view economic history is under threat and, if economic historians remain complacent, is destined for extinction is not so different from that of the women and men who first established our field in the early twentieth century. Justifying the existence of economic history as a distinct field of study proves to be a perennial concern among its practitioners. In the first ever edition of *The Economic History Review*, one of our field's key journals, William J. Ashley, widely acknowledged to be the first professor of economic history at an English-speaking university, warned the creation of a separate journal could 'risk of a drifting-apart' between economic history and economics

We thank Michael Aldous, Graham Brownlow and John Turner for comments on this chapter in particular, and for supporting us more generally throughout this project. Without their input very early on, we would not have been able to get this book off the ground. We thank our publishers, Clara Heathcock and Laura Pacey, for giving us the freedom to make something a little different. Finally, we thank Adi McCrea for the data visualisations throughout the book, and Vinodh Kumar and his team for copyediting and typesetting the text.

(Ashley 1927: 4). He was concerned the new journal could ultimately lead to the irrelevance of economic history in the eyes of economists. He went on to argue we, as economic historians, should always remain vigilant and react against this risk wherever we can; ‘that each should know something of the crops and methods of cultivation in the neighbouring field would, I am sure, be good for both companies of tillers of the soil’ (ibid.).

A supplement to the first edition of *The Journal of Economic History* (published in 1941), our field’s other key journal, makes for very similar reading. Edwin F. Gay, who was appointed to Harvard University in 1902 to replace Ashley—who had by this point departed to become first dean of the new business school at the University of Birmingham—noted economic historians must play a difficult balancing act between the various disciplines that together constitute the social sciences. Gay—who himself helped establish Harvard Business School and served as its first dean—made a plea for more and deeper interdisciplinary cooperation in order to maintain the field’s relevance: ‘one of the first tasks of the economic historian today is to open the way to a more complete connection of the two disciplines’ of history and economics (Gay 1941: 14). He continued by acknowledging while an ‘adequate equipment with two skills, that of the historian and the economist, is not easily acquired, [...] experience shows that it is both necessary and possible’ (ibid.: 15).

So economic historians have always felt under threat. Or, at least, they have always felt they needed to publicly justify their existence. This vigilance is probably not even unique to economic history; other interdisciplinary fields also have to remind others of their relevance. But as editors of this volume, we think it is now high time we put an end to all this self-doubt. Because just a short decade after we have completed our own academic training, things are beginning to look very different for economic history. We contend the financial crisis which started—in the UK at least—with the Northern Rock bank run on Friday 14 September 2007 has changed everything. This time is different (apologies to Reinhart and Rogoff 2009). Among the interested general public, it appeared economists had not only failed to predict this crisis, but also failed to understand and explain the ensuing Great Recession exactly when it was going on all around them. The first of these criticisms highlights a fundamental misunderstanding among the public of what economics as a discipline actually aims to do (see, e.g. Giles 2018).¹ But the public’s second criticism is

¹ Economic forecasting is practised by a small minority of economists concentrated in just a few fields, such as financial econometrics and macroeconomics. Economics in general is a backward-looking social science in that it aims to explain the past, albeit usually the very recent past. While the prediction criti-

something economists should really take to heart. Because a little knowledge of economic history could have really helped them out.

We contend one solution to this second criticism lies within the very pages of this present volume, which we have helpfully entitled *An Economist's Guide to Economic History*. Because economists must now learn the crops and methods of cultivation of economic history, just as we economic historians have already been learning those of economics. This book helps them to do just that. We think economics and business graduates will be better trained in the art and science of economic enquiry as a direct consequence of this new economic history learning. Exposure to economic history will help them to gain valuable real-world experience by actually applying economic theories and methods to something concrete. Economic history will empower them to reflect on current affairs and everyday events and ultimately help them to form their own informed opinions.

Why is economic history the solution? Well, the recent crisis of confidence in the economics profession has resulted in serious reputational damage which economic history can help to fix. Starting with the Post-Crash Economics Society in Manchester, student groups have been set up across the world advocating for so-called “pluralism” in economics teaching (Earle et al. 2016). Feeding on this student unrest, various media outlets, especially in the UK, have complained the university economics curriculum has become too narrow, too theoretical, “other worldly” rather than real worldly (e.g. Elliott 2017).² In our view, a rapprochement between economics and economic history will repair this damage, because learning some economic history makes economists into better economists. We are not the first to make such a call (see, especially, O'Rourke 2013). Indeed, key economics employers, including the Bank of England, have vocally demanded economics students should graduate with a greater understanding of our economic past (see various contributions to Coyle 2012). And others in the sector have already started to learn this lesson; new introductory economics textbook projects have sprung up which attempt to teach about historical economic phenomena, including the causes of the Great Depression (see, especially, The CORE Team 2017).

There is clearly now new demand for our field. But is there a sufficient supply of trained academics to meet this demand? On the face of it, there is.

cism is therefore flawed, perhaps it does reveal a failure to communicate the discipline's main aim to those outside of academia.

² Although many of these journalistic criticisms are unfair in that they present a very outdated view of what constitutes modern economics. See Coyle (2018) for an impassioned discussion of this point.

Despite being apparently under threat for over a century, contributors to our field easily manage to fill many hundreds of pages of dedicated academic journal space every year. However, we argue the contributors to these journals have not done a very good job at communicating the questions that occupy economic historians to our colleagues in allied fields. In particular, we have failed to speak to economists to convince them there is a great deal we have to offer them, in terms of both research and teaching. Moreover, because economic history has been eliminated from most graduate programmes in economics and “relegated” to history departments, the majority of “common-or-garden” economists have no training in economic history whatsoever (see also Diebolt and Hauptert 2019, who make a similar point). And in the UK, the problem has been compounded by the elimination of economic history from history degrees; economics departments can no longer rely on their history colleagues to service-teach an economic history module for them because all the economic historians there have been replaced by social and cultural historians.³ Consequently, economics lecturers are now being told to teach economic history, but they have no means by which to do so other than to follow one of the few textbooks aimed at economics students which have recently emerged to fill this void (e.g. Persson and Sharp 2015; or Baten 2016). And while the arrival of these new textbooks is very welcome, they are all meant as introductory texts; aside from some recent handbook-style volumes (e.g. Neal and Williamson 2014; Parker and Whaples 2013; Whaples and Parker 2013), there is very little in the bookshop for intermediate or advanced-level economics students (history students, of course, have the great Hudson and Ishizu 2016). And there is nothing at all aimed at economic history pedagogy.

We, as editors of *An Economist’s Guide to Economic History*, advocate an alternative solution. Teaching from a textbook can be boring for both students and their teachers. It might suit a course for first-year undergraduates, but it is not a great solution for more advanced students. It does not play well to the individual strengths of the educator, nor to the interests of their students. Instead, we have commissioned and compiled here a series of short chapters, all written by experts, which can be used more flexibly in any university-level economics pedagogy. This edited volume can be employed either to design bespoke research-led economic history courses, or to insert economic history thinking into other economics courses (such as growth, development, managerial or financial economics, to name just a few). We

³ The field of economic history has become mostly unrecognisable to historians since the mainstreaming of quantification and counterfactuals (the so-called Cliometric Revolution). This is even the case among those historians who have avoided the excesses of postmodernism (see, e.g. Evans 2016).

hope a consequence of introducing students to economic history in this new way will be that they discover the various connections between our field and “economics proper” for themselves. We hope this book will provide them with the means through which they can become better economists.

Aim of the Book

This book is an introduction to economic history for readers who have thus far had little or no exposure to the field. Typical readers will already have completed introductory and intermediate-level courses in microeconomics, macroeconomics and statistical methods. They may have also completed more advanced courses in econometrics, growth or development economics or other applied fields in economics, finance and business management. They will now be eager, and crucially also able, to apply their economics knowledge in new and interesting ways. They may also be seeking a research question for their first extended piece of research work, such as an undergraduate dissertation, master’s thesis or doctoral field paper.

Economic history is simultaneously a distinct and separate discipline and a subfield of both economics and history. The aim of this book is to show how economists can access this interdisciplinary field for the first time. We hope that it may even kick-start the “conversion” of some readers to being fully fledged economic historians. Economic history is a field which addresses some of the most profound questions in the social sciences and should therefore prove an attractive proposition for new economists. Many of us attempt to research issues which have the potential to overturn the way we understand the origins and development of the modern world. Even where our questions are small, their answers still have the scope to revolutionise our interpretation of the drivers of human interaction. Instead of being theoretically or methodologically narrow in our approach, we tend to be rather agnostic (or secular) with respect to theory and methods, choosing (and adapting) the most relevant framework to the question, and context, under study. We are not microeconomists, macroeconomists or econometricians; we are not orthodox or heterodox—we are empirical social scientists intent on understanding our past; we are economic historians!

What is this book? In short, it is an introduction to economic history unlike any other, in that it is aimed specifically at economists. All chapters have been crafted with this audience in mind. The book is intended to be read non-linearly; we are keen for educators and their students to discover for themselves the sequence in which they wish to use the material in these pages. Each chapter includes a short reading list which readers can then use to further

explore the particular theme, context or method being introduced. The works in these lists are always discussed in the chapters themselves. They include classics in the field, but also recent works which push the reader towards the field's frontiers.

What is this book not? Well, it is, of course, not the final word on the field of economic history. And it is definitively not comprehensive. While we are proud our book includes a diverse set of aspects of our discipline, we are aware that it has gaps. We hope to address these gaps by providing additional content on the website which accompanies this book (<http://www.blumandcolvin.org/>) and, eventually, in a second edition. Quite clearly, this book is not a traditional textbook either; readers are not expected to read it from cover to cover but rather to “dip in” selectively and make the connections between chapters for themselves. We encouraged our contributors to keep reading lists short; the absence of extended citations is entirely intentional in order to aid new economic historians to start engaging with our field.

Structure of the Book

Each contributor to this book was given the brief of writing an introduction to a particular theme, context or method from across the field of economic history. We introduce each of these pillars separately below. At the start of this book, in Part I (*Purpose, Philosophy and Pedagogy of Economic History*), we also include some more introductory material which we hope will help readers to ascertain the best way in which they can use this book in their own teaching and learning.

Part II (*Questions and Themes in Economic History*) constitutes the core part of this book. It introduces readers to some of the most important research topics and puzzles in our field. These include long-standing questions which remain unresolved, in addition to new topics which have emerged in the last decade thanks to innovative interdisciplinary research by economists and historians. These themes are not usually bound by a time and place in history but rather appear in different ways across history. Each of the chapters provides an introduction, discusses the theme's scope and relevance, sketches potential contributions and provides a reading list for further study. Borders between some of these topics are fluid and the list of themes addressed by contributors is not exhaustive. We have decided to order the chapters in this pillar roughly by their principal *Journal of Economic Literature* code to make them more easily navigable for economists.

Part III (*Eras, Regions and Contexts in Economic History*) addresses the fact our primary targeted audience—students and teachers of economics and

business studies—have limited or even no “exposure” to history in their professional environment. Therefore, a series of chapters links together the aforementioned contents and discusses them in light of long-run developments of world regions and historical eras. They provide both an overview of historical backdrops to economic enquiry and introduce the burning economic questions which those geographies pose, in addition to some interesting time periods which are overlooked by economists. The attention paid to context in scientific enquiry is something that makes economic history distinct and different to other fields of economics, and so this pillar is particularly important for those readers who have never before encountered our field.

Finally, Part IV (*Methods and Techniques in Economic History*) provides an overview of various methodologies and techniques used in economic history research today. Contents of this part of the book build on the standard syllabi of economics departments and business schools; a major task of the authors of these chapters has been to bridge the gap between methods and their use in economic history. This pillar also introduces methods which may be rather more alien to readers but from which, in our view, economists may stand to gain useful new scientific insights.

The intention of the two editors of this volume is these pillars can be navigated in any which way the student (or their instructor) wishes. Depending on the goals of the reader, themes, contexts and methods can be connected in various alternative ways. Our colleague, Matthias Flückiger, has helpfully provided a set of examples of this non-linear approach in the introduction pillar to this book (see Chap. 7). Further examples are available on our website (<http://www.blumandcolvin.org/>).

Birth, Death, Resurrection, Ascension

This book has been made possible thanks to the excellent work of our contributors. Each chapter has been carefully crafted by these experts, who have followed our challenging brief to write short, accessible introductory texts. They have all had to make tough editorial choices, presenting only the “tip of the iceberg” on their assigned subject. Most contributors work in economics departments and business schools and have thus already faced the unique challenges of teaching economic history to economics and business students. Some are based in history departments but have frequently faced the task of communicating the usefulness of their historical insights to economists. We boast a mixture of both junior scholars who are just starting out in their careers as economic historians, and senior scholars who have had a lifetime of research

and teaching experience. All of them are experts in their field and highly committed to teaching. Together, they demonstrate economic historians are more than just economists ‘with a high tolerance for dust’ (Solow 1985: 331). We thank them for their endeavours, and we dedicate this book to them.

Similarly, this book would not be possible without the support provided to us by our own institution, Queen’s University Belfast. Over the past decade, Queen’s has become one of academia’s key players in the field of economic history. This is quite an achievement for an institution which counts itself among the most geographically isolated and peripheral universities in Europe! Unlike “the other places” traditionally more famous for economic history, we are based within a business school and teach students registered on economics, business and finance programmes. Uniquely, our numbers now put us in the majority of research-active scholars in the economics group within our school. This scale has allowed us not only to teach economic history as a distinct field of study, but has also enabled us to insert economic history thinking into the syllabi of other field courses. So, we teach econometrics using cliometrics. We teach business economics using business history. And we take a long-run perspective on fields from development economics to industrial organisation. Students tend to like this approach as it gives them concrete applications of economic theory. It enables them also to better understand the particular context in which these theories first emerged. Even our colleagues in the economic theory group now use economic history in their teaching! We thank them all for providing us with this unique setting.

Of course, Queen’s has a long history in economic history—we are merely dwarfs standing on the shoulders of giants. Thomas E. Cliff Leslie, professor of jurisprudence and political economy at Queen’s from 1853, is regarded as one of the founders of the English historical school of political economy—an approach which opposed theoretical economics and advocated empirical and historical analysis of the economy.⁴ Then, the first bona fide chair in economics at Queen’s was held by an economic historian, Hugh Owen Meredith—who, incidentally, was replaced at his previous affiliation by John Maynard Keynes. Meredith published what is perhaps the first English-language economic history textbook (Meredith 1908). Fast-forward 50 years, and Queen’s economics lecturer R.D.C. Black is researching his seminal work on the relationship between economic theory and economic policy in the context of nineteenth-century Ireland. Black (1960) mixed together two fields that are unfortunately rarely combined today: economic history and the history of economic thought.

⁴ See Backhouse (2011) for a discussion of Cliffe Leslie and the English Historical School.

In many ways, our institution's subsequent history of economic history mirrors the history of the field elsewhere in (UK) academia. Economic history at Queen's separated from economics and history in the mid-twentieth century to become its own distinct discipline; first established by Ken H. Connell in the early 1960s, the university maintained a separate economic history department all through Northern Ireland's Troubles.⁵ Members of this department later included Bruce Campbell and Liam Kennedy, both now emeritus professors at Queen's. But as elsewhere, this separate department was then collapsed into a combined history school in the late 1990s. And as economic historians retired there, they were replaced by social and cultural historians. The discipline looked to become extinct in this part of the Emerald Isle.

The final stage in our evolution, the re-emergence and renaissance of economic history through the colonisation of the university's business school—a process that was led by our colleague and mentor John D. Turner—is also not entirely unique to Belfast; there are signs similar things are occurring elsewhere, at least in the UK. We might be more successful than other universities in terms of scale, but there is nothing stopping other places from following suit. We hope our colleagues at these other places can usefully use *An Economist's Guide to Economic History* to further proselytise the virtues of our field and help convert new generations of economists to our cause. Because to do better economics, we share the views of our discipline's founding fathers, Ashley and Gay: economists must cooperate with us and learn about our crops and methods of cultivation, just as we have been cooperating with and learning from them.

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Part I

Purpose, Philosophy and Pedagogy of Economic History

Haiku by Stephen T. Ziliak

Why wait to value
economic history
until markets crash?

Counterfactual:
Even game theory arose
from shadow prices



2

Economics Versus History

Christopher L. Colvin and Homer Wagenaar

Economic history is an interdisciplinary field which fuses economics with history, two disciplines that often misunderstand one another. The early work of Nicholas Crafts, now one of the giants of this field, exemplifies how difficult this fusion can be. In 1977, Crafts argued histories of the Industrial Revolution all inherently suffered from a logical fallacy: affirming the consequent (since event X followed event Y, event Y must have been the cause of event X). He noted this fallacy is often present in historical comparisons of England and France, which all read something like this: ‘since the Industrial Revolution occurred in England before France, then there must be something that was different and unique about England that made this possible’.

Rather than relying on what he saw as the flawed approach of the comparative historian, Crafts argued instead that we should always interpret history with the logic of economics. What does this mean? Well, essentially, the sample size here is two: England and France. And there is just one non-replicable event: the British Industrial Revolution. There are many differences between England and France. How can we ever isolate the crucial causal factor? Instead, according to Crafts, we must analyse history in probabilistic terms. Yes, the

We thank graduate students at Queen’s University Belfast for the lively seminar discussion that underpins this chapter, and Graham Brownlow, Rob Gilles and Tom Hulme for comments on an earlier draft.

Industrial Revolution occurred first in England—but it *could* have occurred first in France as on most fronts England and France were identical.¹

While the historical statistics have improved somewhat since Crafts's polemical early work—much of which is down to research conducted by Crafts himself—his point still stands. We now know that England *was* ahead of France in certain key respects on the eve of industrialisation. But a “cliometrician”—a fancy term coined in the 1960s to describe an economist doing historical research—would still analyse the location and timing of the Industrial Revolution in probabilistic terms. Most cliometricians would now agree the probability distribution of the Industrial Revolution occurring in the two countries have different shapes—perhaps with the English distribution shifted to the right across the whole range of possible outcomes (a higher mean) or perhaps with fatter tails (the same mean but with a larger variance). Either way, the point remains that it still *could* have occurred first in France.

In contrast, archetypal historians, like the late David Landes, who wrote a direct response to Crafts's probabilistic analysis of history in 1994, view the Industrial Revolution as a series of small consecutive events, most of which occurred first in Britain rather than France. For Landes and his colleagues, there are not really two cases at all but rather a panel of two countries and several years. Attempting to understand Britain's surprising success in the late eighteenth century, Landes suggested these various small events, when put together, explain Britain's ascendancy, which he characterised as ‘the greater unity and efficiency of the British market, to the higher degree of entrepreneurial freedom, to the precocity of regional specialization’ (p. 641). He argued there is an abundance of circumstantial evidence for these events, especially in the form of contemporary witness accounts. How exactly these events combined to result in industrialisation is for historians of the economy like Landes impossible to discern, and anyway perhaps not that interesting.

The contrast between Crafts and Landes points towards a stereotypical difference in outlook between the economist and the historian. Economists borrow concepts from statistics to build models which explain trends, while historians use archival sources to tell stories about events. Economists look for approaches which can cleanly disentangle cause from effect, while historians focus on evidence which enables them to build nuanced narratives full of contingency. Economists tend to dismiss questions which cannot be answered with any degree of certainty, while historians are usually happier to tackle

¹ Anyway, it *did* occur in France, and just a few short decades later than England.

issues for which the answer can be very messy. We explore these differences in the remainder of this chapter. And while these differences are important to understand, we conclude economists and historians have much more in common than they have in contrast.

Deduction, Induction, Abduction

History is a science in the sense it is conducted using research methods and epistemologies (the “nature of knowledge”), and it adheres to the same scientific principles of validity, reliability and controllability. The knowledge it creates should be *falsifiable*, in the sense that the statements of the past are “risky” and may be refuted with further evidence (Thornton 2017). However, many of the mutual misunderstandings between the economist and the historian emerge from the fact history is also part of the *humanities*, sharing a pantheon with art, literature, theology and law. They differ in that a (social) scientist is generally interested in finding general laws and patterns (“how do markets work?”), while a historian is interested in the particular (“how did the cheese market in seventeenth-century Gouda work?”). From this distinction in outlooks, each lays a different focus on their theory and methodology.

The difference between the two disciplines exposes three different ways of doing (social) science: “deduction”, “induction” and “abduction”. Deduction is the process of reasoning from one or more premises to reach a logically certain conclusion. This conclusion is *certain*. If all premises are true and the terms are clear, then the conclusion reached is *necessarily true* (Doven 2017). In contrast, induction is the process of reasoning in which observations are viewed as supplying more or less strong evidence for the truth of the conclusion. The conclusion of an inductive argument is *probable* based upon the evidence given (Hawthorne 2018).²

Finally, abduction refers to the moment when a researcher encounters something surprising and creates a new explanation which might better account for the odd event (Doven 2017; Mullins 2002). Both “surprising” and “might” are fundamental here. Firstly, surprise, after all, refers to the previous knowledge of the observer: somehow, this observed empirical reality does not seem to fit her expectations. The surprising is therefore an invitation to rethink theory. Secondly, “might” refers to the fact the new initial explanation has the status of a hypothesis. It is an informed guess. Therefore, this new hypoth-

²What we have in mind here is a Humean/Popperian rather than a classical understanding of induction—one in which falsification plays a central role. See Thornton (2017).

esis should be rigorously tested by holding it to account, to both the necessary consequences of this hypothesis (deduction: if the hypothesis is true, then other related aspects should also be true) and the evidence (induction: is it possible to falsify this hypothesis?).

Indeed, induction and abduction are closely related as the scholar, searching for an explanation, might alternate quickly between the creation of new possible explanations and their subsequent refutation on the basis of the evidence. Nevertheless, economists tend to be viewed as relying on inductive reasoning, the testing of hypotheses derived from a body of theory, while historians tend to be viewed as relying on abductive reasoning, the creation of new hypotheses to understand the evidence they are dealt with. From this difference in focus, the economist tends to be interested in the mean and standard deviation of her tests, while the historian focuses on trying to explain the outliers.

Bad Economics and Bad History

As new scholars starting out in the field of economic history, it is important to be able to identify and then emulate examples set by good economic historians, such as the work of the various contributors to this book. More importantly still, new scholars must also learn how to spot bad practices which should be avoided. The distinction we have made between deduction, induction and abduction allows us to paint a picture of what in our view constitutes “bad economics” and “bad history”.³ We argue they are both surprisingly similar.⁴

Bad economics is deductive rather than inductive. Practitioners of bad economics may posit a new mathematised theory which links a series of events together in a logical system, largely through introspection. They may rely on some “stylised facts” to motivate their model. Rarely, however, are these truly facts.⁵ They are quite happy to “assume”. Their “economics” becomes a highly self-referential system that has little explanatory power because it is not related to an actual case, present day or historical. Bad economics does not have to be

³Our characterisations of bad economics and bad history are perhaps a little unfair on practitioners of these disciplines. But we think there is merit in attacking straw men because they can provide clean and clear models of what to avoid. For a famous example of this rhetorical approach, see the way in which John Maynard Keynes uses “Professor Pigou” (Arthur Cecil Pigou) as a straw man “orthodox economist” throughout his *General Theory* (1936).

⁴Deirdre McCloskey (2018) makes a somewhat similar argument.

⁵Graham Brownlow, our colleague, argues that a better term for stylised facts is “stylised fictions”.

theoretical; empirical economics can suffer similar problems. Simply because an “exogenous” factor works as an instrumental variable and results in an F-statistic exceeding a certain threshold does not necessarily mean an econometric analysis contributes to our understanding of the economy. Both of these archetypical economists are essentially just applied mathematicians, having fun with equations.

Bad historians *also* do deduction, although implicitly. They go to the archives, supposedly with a blank slate, open to anything. They hang out there for months on end, taking copious notes, with a pencil.⁶ However, because they dangerously believe themselves to be “atheoretical”, by not reflecting on their implicit theory and their assumptions, they are unaware of the explanation they already have in mind when they go to the archives. By merely confirming what they already thought beforehand, they are essentially carrying out a crude form of verificationism. Rather than being an empty vessel open to new knowledge, they are instead a vassal of some long-debunked theorist.

A historian is *never* truly atheoretical. To be able to observe and understand, the historian needs to rely on the body of knowledge she already knows (Gadamer 2004). If historians are unaware of this, they run a danger of “shoe-horning” the evidence into their (implicit) theory. If so, they are no different to the bad economist! For both the bad economist and the bad historian are doing unfalsifiable theory. They are both selecting the “facts” based on their theory. Both are only interested in the evidence which verifies their theory, if, indeed, they are interested in evidence at all.

Nick Crafts, in the discussion at the start of this chapter, was principally doing induction. Good economists do induction. Good economists, like good historians, are conscious of the *purpose* and, crucially, the *limitations* of theory. They take the inductive process seriously. Yes, by all means hold a set of prior beliefs, based on observation or even pure introspection. Yes, do formalise these into some sort of schema. Yes, specify a set of testable hypotheses. Yes, use these to develop an appropriate empirical framework through which these hypotheses can be tested.

But also: reject the hypotheses if the evidence does not fit. Go back to the drawing board, start again, open the (text)books, and develop a new idea to test. Or: start an abductive research process, go about explaining what you actually *did* find. If you found the opposite relationship to that which you

⁶Our advice to students visiting archives is to bring a good, simple, pencil. It has to look like a pencil (please, no mechanical pencils!). It cannot have an integrated eraser. Failure to follow this advice will usually result in an archivist confiscating your writing materials.

expected, then be brave and think of a new theory that fits. If you found no relationship, then think of a theory that fits. Move backwards and forwards, between theory and evidence. And if your evidence *does* fit, still be prepared to reject your theory. Be aware you are dealing with probabilities. Be aware of “Black Swans”. There is a chance your idea explains the phenomenon. There is a chance it does not.

If you are attempting to do good history, you too must take all three scientific processes seriously. Yes, by all means interpret the connection between the various archival materials. Yes, do approach your sources chronologically. Yes, tell a convincing narrative which fits within the wider historiography. But also: be aware you have a theory of history when you approach your sources—a paradigm. Be explicit about your theory. Be prepared to use different “theoretical lenses”. Be prepared to be surprised by the evidence and to reject the explanations you initially came up with.

Good Economics Takes Context Seriously

And so economic historians have the unenviable task of navigating between two fields which have different value systems. Economics prides itself of being context-free. It is all about “universal laws” which work irrespective of the institutional setup. Meanwhile, history is all about specificity, about circumstances, about context.⁷ An idea will only work in a particular time, in a particular place, for a particular set of economic actors; ‘the past is a foreign country; they do things differently there’.⁸

The economic historian has to find a compromise between these two extremes. We use economic theory and apply it to a particular historical setting—we have to “contextualise our economic theory”. What is context? Well, it is the *circumstances* which form the setting for an event, statement or idea. It is the terms under which that event/statement/idea can be fully understood. This contextualisation process means we have to be far more flexible with theory. We have to be able to take theories and adapt them. We cannot, therefore, always be as formal with our use of theory as economists sometimes wish us to be. However, this is our strength, not our weakness.⁹

⁷ Perhaps even too much context!

⁸ This famous line comes from LP. Hartley’s 1953 novel, *The Go-Between*.

⁹ Do not let economists tell you otherwise because *they* are the ones who are probably being delusional about theory, not you!

Indeed, the historian is suspicious of the claims of the economist that her theory is universal. Theory is created in a specific time and a specific geographical location on the basis of an abductive process based on a specific (usually historical) case study. Of course, inductive and deductive processes may have bolstered the theory by testing it on other cases as well. But there might still be *implicit* assumptions and value systems involved that do not always reveal themselves—especially if the researcher shares these assumptions with the subject under study, and even with the audience reading the study (because they are, essentially, cultural). These assumptions can only be exposed by “maintaining an open mind” and “conducting reciprocal comparison”. We explain both these ideas in what remains of this chapter.

In order to be surprised, the economic historian should “maintain an open mind”. Often, this requires an attention to detail. Much like Arthur Conan Doyle’s Sherlock Holmes might start his thinking on the basis of something as utterly everyday as the question “why didn’t the dog bark in the night?”, or like how the nineteenth-century art critic Giovanni Morelli was able to distinguish frauds on the basis of how an artist painted fingernails, the economic historian might sometimes need to look at seemingly unimportant details to develop new economic theory (Ginzburg 1979). For example, coinage is relatively common throughout most of history, with silver and gold coins circulating in Europe since ancient times. However, the relative frequency of less prestigious coins, such as copper and bronze, differs over time. Economic historians have connected this rather surprising archaeological finding with the extent to which impersonal markets for wage labour were developed: only if labour is actually paid for in coins do you need to be able to change silver coins for lower denominations. A feudal economy relying on indentured labour does not require as much copper coinage (Lucassen 2007).

The second hallmark is a principle which Gareth Austin (2017) calls a “reciprocal comparison”. When economic historians study a non-Western or historical economy, they tend to take their own current, Western, situation as the norm. As a result, the other country or time is backwards, “an earlier stage of development”. The “normal” trend is to become just like Western Europe and North America.¹⁰ Rather, the economic historian should take the *other* place and time as an opportunity to reveal things about themselves, to take this other situation as the norm and attempt to see what in their perspective would be strange about the Western economy in the twenty-first century. Only then is there hope to contextualise our economic theory, and to learn.¹¹

¹⁰Walt Rostow (1960) is perhaps one of the most well-known exponents of this logic.

¹¹Reciprocal comparison encapsulates pretty well the important lessons of “hermeneutics”, the philosophy of understanding.

In short, economic historians have to make a *judgement* about the generalisability of an idea. We need just the right amount of context-specificity. Just the right amount of context-freeness. A combination of induction and abduction. This is difficult. Economic history is difficult. But it is very rewarding.

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3

Economics, Economic History and Historical Data

Vincent J. Geloso

Every few years, a prominent economist will proclaim the rebirth of economic history or point to its upcoming resurgence. Such utterances were heard when Douglass North and Robert Fogel won the Nobel Prize in economics in 1993, when Daron Acemoglu, Simon Johnson and James Robinson published their article on the colonial origins of divergence in the *American Economic Review*, and when Thomas Piketty published his *Capital in the Twenty-First Century*. Yet, of the 1,509 academic job openings listed on the American Economic Association's website for the 2017–2018 job market, just 26 positions advertised using the *Journal of Economic Literature* code related to economic history (which is N). While many departments were looking for “all fields”, which entails that this number is an underestimation of potential hires with specialties in economic history, these numbers, nevertheless, suggest a lack of popularity for the field. The articles published in top journals confirm this tendency: Fig. 3.1 shows the percentage of articles published in the “top five” general interest journals in economics that used the economic history classification code.¹ As the figure makes clear, there is no progress in the modest interest in economic history. Alongside international economics, macroeconomics and econometrics, economic history has not seen its relative importance grow since 1970 (and the exceptions are between three and six times more popular than economic history).

¹ A related exercise carried out by Ran Abramitzky (2015) for the “top three” economics journals finds an *increase* in economic history representation over time. Abramitzky's result appear to be driven by contributions to just one journal: *The Quarterly Journal of Economics*. Taken together with my own results, this suggests that while the total quantity of economic history appearing in top general interest journals has remained constant over time, there has been a shift in interest between the editors of these top journals.

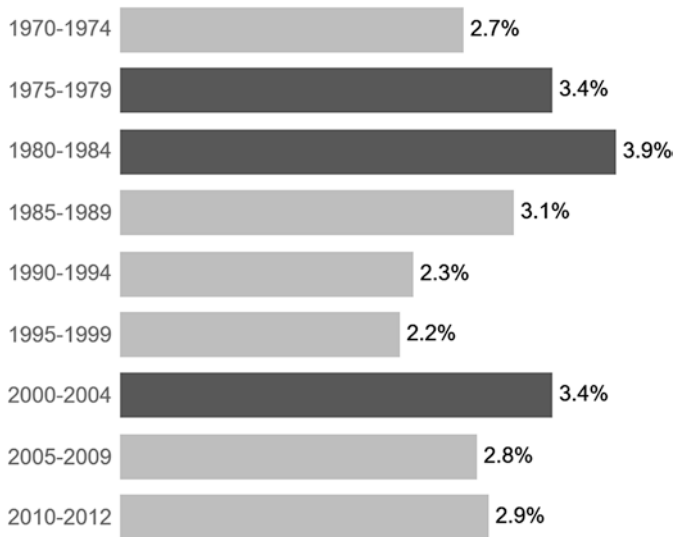


Fig. 3.1 Share of articles using the economic history JEL code in top five journals. Source: Card and Della Vigna (2013). Note: The top journals here constitute the *American Economic Review*, *Review of Economic Studies*, *Econometrica*, *Quarterly Journal of Economics*, and *Journal of Political Economy*

Why the dissonance then? To properly answer the question, it is necessary to understand that economists are still stuck with some “big questions” regarding the “big issues” for which the use of historical data has become in vogue (e.g., development, the role of the state, the role of inequality, etc.). History has become a tool with which to resolve ongoing debates between economists as it allows them to extend their data in to the past. The idea is that great datasets going back decades or centuries may unlock the big questions.² However, historical data are not the same thing as economic history. Economic history is not the same as economics that uses history. Economists will frequently use data without considering the underlying construction (see, notably, Jerven 2013, for a discussion of this point in the case of Africa), the context in which it was created (i.e., what biases might be introduced into the data), and the optimal level of aggregation (i.e., statistical aggregates may often conceal more than they reveal). This will include historical data which may be used carelessly to arrive at improper and incorrect causal statements that may lead other researchers (and policymakers) astray.

²These examples would include the World Top Income Database, the Global Price and Income History Group, the Maddison Project, the Jordà-Schularick-Taylor Macrohistory Database and the Measuring Worth Project.

The economic historian is a different beast altogether. Their task is a burdensome one as they must simultaneously explain history with economic theory and bring theory to life through history. In achieving the task, they must comb through large quantities of detail that may be of relevance to their results. In dealing with this, they must themselves avoid the production of excessive details—perhaps a desired feature of historical studies conducted in the humanities, but a distraction to good economics (on this, see, notably, de Vries 2017). Economic historians must navigate the uncertainty of the past using the “northern star” of economic theory which allows them to comb through the details. The mastery of this navigation depends on a rich understanding of theory, which takes time to mature. Finally, to arrive at convincing conclusions, they must be able to use methods that will convince readers about the *relevance* of their points (either through analytical narratives and/or econometric methods).

These are the crucial elements which must be discussed step by step as in the case of an assembly manual: the understanding of wide arrays of economic theory; the ability to comb through details to get at what matters; and asserting relevance. Each of these elements must be discussed individually.

The Understanding of Economic Theory

Economics is a deductive science through which axiomatic statements about human behaviour are derived (von Mises 1957). For example, a supplier will *never* (all else being equal) increase the quantity of the goods it produces if the price falls. Alternatively, demanders will *never* increase demand for a good if the price increases (all else being equal). These are axiomatic statements; they are not up for discussion. However, imagine a government imposes a tax on a particular good, and the tax incites a popular rebellion in the population. How can we use the axiomatic statements (the laws of supply and demand) to explain the rebellion? This requires an analysis of the distribution of the cost imposed by the tax, which entails a need to collect data, measure elasticity and arrive at an idea of the counterfactual (i.e., the no-tax) equilibrium. As it is evidently true (i.e., an axiom) that the quantity consumed will fall as a result of the tax, the estimation of elasticity is the assessment of the importance of this evidence to the question studied. And there is economic history properly done. Once the question is asked, the economic historian tries to answer which theory is relevant to the question asked; essentially, the economic historian is secular with respect to theory. The purpose of economic history is thus to find which theories matter the most to a question.

However, economists who do not define themselves as economic historians would also recognise their own craft in that description. The problem is that the economic historian, when attempting to answer historical questions, deals with a much more daunting task. Take the tax example mentioned earlier. Anyone familiar with American history could relate the example to the American Revolution, as it is often conceptualised in popular imagination as a tax rebellion. Yet, it is now well known that American colonists had a lower tax burden than their British compatriots in monetary terms and an even lighter one when we account for the greater income that the American enjoyed (Lindert and Williamson 2016; Rabushka 2010). Why would a rich lightly taxed society revolt? To answer the question, economic historians have attempted to import insights from experimental economics (de Figueiredo et al. 2006; Rakove et al. 2000), from rent-seeking theory (Geloso 2018) or the wider political economy of the British colonial empire (Greene 2000). Albeit only an example, one should notice the wide array of theory used to disentangle the elements necessary to arrive at a valuable explanation.

Further complicating this work is the fact that two statements can be true individually but conflicting with one another. Take the case of the often-cited market for used cars, where information asymmetries will push out the sellers of high-quality goods out of the market and leave consumers with only the “lemons” (Akerlof 1970). That case, while theoretically valid, conflicts with the theory of signalling, where individuals may invest in sending signals to ensure consumers of quality (think about used cars websites with consumer feedback). The signalling theory appears to provide a more relevant prediction for the used car markets as many research articles have shown that the key prediction of the market for lemons fails to materialise (Bond 1982).

The economic historian must be able to master a great array of economic theory in order to arrive at convincing explanations of key historical events. They must work in all subfields at least at an intermediate graduate level and must be an expert in the main fields they work in. In that regard, they are able to arrive at a rich overview of the situation. This will allow, with the complementary use of advanced empirical tools, to derive causal statements about key moments in economic history.

Cutting Through the Mountains of Detail

Economic history is not merely the use of historical data. One must carefully consider details about the quality of the evidence collected. This can range from more “macro” topics such as carefully estimating gross domestic prod-

uct (GDP; e.g., Bolt et al. 2018; Broadberry et al. 2015) to more “micro” details.

Consider the example of agricultural productivity in nineteenth-century Canada, where a sizeable French-speaking minority existed. That minority has been described by contemporary visitors and many modern historians as “poor farmers” clinging to outdated farming methods, which were unproductive and inefficient. It has also historically been poorer than the rest of Canada (which is itself poorer than the United States) (Altman 2003). Primary sources like the censuses appear to support this contention as outputs per unit of land were lower for French than English farms (Ouellet 1980). However, it turns out that the French farmers were reporting outputs in *minots* and land in *arpents*, while the census-takers reported figures in bushels and acres. As a *minot* was greater than a bushel and an *arpent* was smaller than an acre, the French output per land figure was 32 per cent more than the English output per land. Once economic historians like Marvin McNinn (1981) revisited the issue and corrected the figures, it was realised that the French-Canadian farmers were equally as productive as the English farmers (Geloso et al. 2017).

These findings, related merely to improving the quality of measurement, overturned a wide body of literature. In fact, my more recent set of corrections, which adjusts directly on the basis of ethnic composition rather than on whole sub-districts, suggest slightly higher output figures still (Geloso [forthcoming](#)). These corrections also show that previous corrections were “measuring away” the heterogeneity of productivity levels across the colony in a way that was biased against testing a number of key hypotheses regarding tenure institutions. This issue of cultural differences in what measurements are reported may, at first sight, appear irrelevant. Yet, as shown earlier, it matters crucially in assessing why the French-Canadians were poorer than other North Americans.

This is a potent example of the craft of the economic historian. They must discover small details about seemingly complex issues as weights and measures in the distant past. Knowing the details yielded dramatic improvements in our understanding of (non-existing) productivity differences across ethnic groups. It also shows that the economic historian does not take the data “at face value”. The data must be surveyed to see whether they are suited to the question asked. This requires us to delve into “how” the data were assembled, by “whom” they were collected and, crucially, for “what” purposes. The data cannot simply be entered into statistical software in order to mindlessly run a regression.

Another even more potent example is that of Acemoglu et al. (2001), who studied the role of settler mortality in the early days of colonisation and the

institutional quality of those colonised polities later in time. The key conclusion of that paper was that it was more likely that the colonising power would implant “extractive institutions” in locations where settler mortality was higher. And conversely, the colonising power was more likely to invest in institutions such as well-enshrined property rights in locations with low settler mortality. That theory was logically consistent, thus responding to what was mentioned in the previous section, but it was empirically flawed. David Albouy (2012) showed that Acemoglu et al. (2001) misinterpreted the data they were using by mixing incompatible types of mortality rates. Once some basic adjustments were made, the results that Acemoglu et al. pointed to were partially reversed. This shows the crucial importance of caring about the “story” behind data.

Economic Relevance

The economic historian must also show the relevance of the answer they provide. If vast sways of data are collected to show an undisputed point of economic theory—even if done to the highest level of empirical credibility—the contribution still needs to prove economic relevance. A great data design must serve a “big question” in a way that marginally brings us closer to a convincing answer.

Consider another example related to Canada. The French minority of Canada has long been considered exceptionally short in stature—something that has been labelled as a “striking exception” within Canada (Cranfield and Inwood 2007). As stature is a good proxy for living standards, this is akin to saying that the French-Canadian was poorer than other Canadians. That gap existed as far back as the late eighteenth century (Arsenault Morin et al. 2017) and still exists today. The issue of relative poverty is a relevant one, most would argue. It may even be considered a “big” question.

But the answer that is economically relevant is not necessarily found in the “big” things. In the case of the heights of French-Canadian in the distant past, this answer lies in the way by which millers were incentivised to transform wheat into flour. The French-Canadians were known for eating low-quality bread laced with dirt as the wheat that was transformed was not cleaned prior to grinding (Geloso and Lacombe 2016). The flour was thus deleterious to health. Why would this be the case? The answer, provided by Geloso and Lacombe (2016), was that landlords who received estates from the crown had monopoly rights over milling but had the obligation to provide those mills and provide the milling at a fixed rate but only on the domestic market. In

order to circumvent the price controls, low-quality flour was produced for the domestic market and high-quality flour was exported. Moreover, the landlords could make a hefty profit from the residues of milling. When wheat is processed into fine flour (for higher quality baked goods), one must sift the wheat residues (brans and middlings) from the final product. As they were the sole producers of these residues, the landlord kept increasing prices faster than the overall price level for grains. This pushed up the price of raising animals and discouraged pastoral production which could have produced calories and protein cheaply.

The example above is one where an apparently minor issue—flour quality—may have *economic relevance*. The incentives generated by the institutions surrounding the production of flour directly and indirectly worsened nutrition in ways that may have contributed to the relatively short stature of French-Canadians. The example also encapsulates my other points made earlier. To showcase relevance, the economic concepts used encompassed elements of the literature on regulatory economics, health economics and industrial organisation. It also required an understanding of the details behind the production of flour, which are then explained through the vehicle of well-mastered theory.

Advice for New Scholars

For those who consider venturing down the path of becoming an economic historian, my suggestion is to read voraciously in both economic theory and historical topics. Do the two simultaneously. The economic theory will allow you to see economic puzzles in historical events. Eventually, you will find a topic that fascinates you. Within that topic, pick a question. It would be best if you start with a modest one in order to use it as a training tool. The steps described in this chapter are those that you will develop in this process and they will turn you into—at the very least—a competent economic historian. This will require patience. However, as you will gradually master these skills, they will become marginally easier to use and will permit you to make significant contributions.

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4

Economic Theory and Economic History

Robert P. Gilles

Economics has a very difficult methodology, complicating the relationship between economic theorising and empirics. Below, I set out my view of why economics is in this dismal state. Subsequently, I discuss, as part of a possible remedy for this problem, how economic theorising could relate more productively to (economic) history. On the one hand, analytic narratives can be used to explain the particular economic aspects and recorded behaviours of a historical episode. Conversely, history can provide insights into human economic endeavours to help us formulate better general theories of economic phenomena and to advance political economy in general. Particularly, I focus my discussion on historical entrepreneurial activities and how these could contribute to formulating economic theories of entrepreneurship. This illustrates the relationship between economic history and economic theorising.

Understanding the Current State of Economics

Economics studies the human condition, which makes this pursuit dangerously fraught with significant problems. The cause for this is the very nature of the human condition. But from this, we can also understand how to alleviate these significant problems and to achieve a more functional and productive study of economic phenomena.

I thank Matthias Blum, Chris Colvin and Owen Sims for fruitful and elaborate discussions on the ideas presented in this chapter.

At the foundation of the problems in economics is the fact that humans operate in a dual reality (Harari 2014): On the one hand, human actions concern the objective reality made up of the physical substance of the human environment; on the other hand, humans collaborate and create economic wealth through fictional narratives that engender a fictional reality. Our human ability to collaborate is characterised by our ability to believe these fictional narratives blindly—as if they were of the same substance as the objective, physical environment in which we exist. The latter is the main subject of study in the social sciences—including economics—and the humanities—including history.

The physical and natural sciences—from biology to physics, chemistry and the medical sciences—concern themselves primarily with the study of the objective reality. They are endowed with relatively strong methodologies of how to conduct scientific investigations and analysis founded on a strict relationship between the empirical measurement of the natural environment and the formulation of theories that explain the observed phenomena. This methodology was set out most profoundly by Popper (1968), although his precursor Kuhn (1962) and his critic Lakatos (1978) addressed methodological issues that put doubt on the validity of the Popperian methodological perspective.¹

As the social sciences and humanities concern themselves with the human condition itself, their perspective requires the incorporation of both sides of the dual human reality: The physical objective reality as well as the human fictional reality of socio-economic institutions and politics. This makes pursuits in economics and history fraught with inconsistencies and serious problems. This is most profound in economics, since it explicitly pursues understanding how human involvement converts objective physical substance into human “use value”. In particular, in contemporary economics, this has resulted in the strict dichotomy of theoretical economics and empirical economics.

The Dichotomy of Economic Theory and Empirics

The main objective of economics is to investigate how human collaborative effort converts natural resources as well as human labour and ingenuity into economic wealth or “value”. Theorising about an ever-evolving dynamic pro-

¹ Even the natural sciences can get carried away by their adherence to the fictional narratives that form the foundation of their theories (Smolin 2006).

cess of human collaboration is complicated due to the feedback from this theorising on these processes; economists' subject matter is not objective but part of the human fictional reality itself. Therefore, economic theorising has become burdened by the economists' embeddedness in their own theories and narrative perspectives. Consequently, the development of economic theory has become propelled solely by the theory itself, without a proper empirical component.

On the other hand, empirical economics cannot be strictly Popperian since it is impossible to appropriately falsify economic theories. As a consequence, empirical economics has become more and more data-driven, without a proper theoretical component. This is further complicated by the perception that empirical economists seem insufficiently aware that empirical observation of human economic activities is actually theoretical in nature. Indeed, the measurement of seemingly objective economic phenomena is actually founded on theoretical constructs such as the demand and supply of commodities, income levels and unemployment. Thus, empirical economic measurement is informed by a certain (political) perspective of the economy.² There is no truly *objective* measurement of these economic phenomena possible.

Another consequence of the indicated problems with economics is that there evolved a long history of self-reflection and -doubt in economics centred on the difficulty of its subject matter—the “economy”—and its own necessarily ideological nature (Backhouse 2010; Foley 2006; Keen 2011; Sutton 2000). After the Great Financial Panic of 2008 and the following “Great Recession”, there emerged a large, popular as well as academic literature on the state of economics and its inability to properly explain the state of the contemporary twenty-first-century global economy (Hodgson 2008; Kirman 2010a, b; Mirowski 2010; Schlefer 2012).

Some economists have thrown up their hands and argued that economics is nothing more than telling intelligent stories, either as cleverly constructed narratives or as mathematical theories (McCloskey 1983; Rubinstein 2006, 2012). Rubinstein even seems to argue that economics is just not of much interest at all. I think that this perspective is too negative. Only very few contributions such as Backhouse (2010) really enlightened the relationship between economic theorising and empirical observations.

²An example is that of the unemployment rate. Throughout the past century, this figure has been (re)constructed from government data of unemployment benefits and related registrations. In the past decades, the unemployment rate has been redefined habitually by government agencies for the political benefit of political parties that are in government.

Empiricism, Analytic Narratives and Economics

Understanding the human condition is important and should be pursued vigorously. This can be accomplished by making its methodology subordinate to this goal. Theoretical economics ought not be concerned with proliferation of mathematical theory for its own sake, and empirical economics ought not to be solely data driven. We ought to strive for the unification of theoretical and empirical economics and unburden economic reasoning from its heavy methodologies.

I believe that historical events, cases and phenomena give us an empirical as well as an analytical test bed for economic theorising. Indeed, economic history is mainly concerned with past economic activities, which can be measured empirically as well as investigated through analytic narratives. Both perspectives of the past provide economists with evidence to construct better theories to understand the creation and allocation of economic wealth. Below, I focus on entrepreneurial activities and economic theories of entrepreneurship to illustrate the relationship between history and economic theorising.

Beyond Analytic Narratives

The contributions in the volume edited by Bates et al. (1998) introduce analytic narratives as theories that explain historical economic phenomena, using economic decision and game theory. This methodological conception explicitly focuses on using economic theories to explain historical processes. These analytic narratives “rationalise” past behaviour by constructing applied theories—such as the models developed and used in Greif (1993, 2006) to explain the historically observed contracting practices of tribal traders.

The above refers to the use of general economic theories—such as game theory—to investigate and explain historical economic phenomena. Thus, these general economic theories are specified and calibrated for application to the historical phenomenon in question and serve only to explain the observed economic aspects.

The second use of historical economic phenomena is to inform and support the formulation and design of general economic theories in political economy and economics. Hence, a generally applicable theory can be designed based on historically observed behaviour, events and processes. The theorising process is, therefore, reversed: Instead of explaining an explicit historical episode with the application of a general economic theory, multiple historical episodes are used to design a general economic theory that can explain these

historical episodes through appropriately formulated analytic narratives based on that particular economic theory.

It should be noted that there is a long tradition in economics to formulate general economic theories based on such observations of historical phenomena. In particular, I refer to the general use of casual observations of historical events and processes by economists throughout the past two centuries.³ I illustrate this with a more elaborate discussion of economic theories of entrepreneurship, some of the historical cases that inspired and framed these theories, as well as some historical cases that can be understood through application of these general theories in explanatory analytic narratives.

Case: Economic Perspectives on Entrepreneurship

Entrepreneurship plays a critical role in the development of the capitalist economy. The explanation of entrepreneurship and the related economic development has traditionally been based on the study of historical and contemporary entrepreneurial activities. This has resulted in the rise of general economic theories of entrepreneurship. I discuss the relevant economic literature in Gilles (2018, chap. 5), which I summarise here.

Schumpeter (1934, 1942) developed a very comprehensive, qualitative perspective on economic entrepreneurship in the context of his general perspective on the economy. He proposes that entrepreneurs disrupt the economy through the creation of innovative production technologies and new economic goods. These entrepreneurial actions result in the destruction of existing economic processes through the obsolescence of production technologies and of existing commodity markets. One refers to this as the Schumpeterian theory of *creative destructionism*.⁴

Burt (1992, 2004, 2005) developed an alternative perspective founded on a sociological, non-market view of the economy. He argues that entrepreneurs are exceptional networkers, who build new connections—or “bridges”—between disparate parts of the existing (trade) networks. These bridge builders

³An example of this is the formulation of the theory of money and debt founded on the historical intervention of nation states from the nineteenth century through the monopolisation of monetary instruments, the regulation of banking and the establishment of national central banks to regulate the monetary and financial system. This has resulted in the economic theory of *fiat money* and its derivatives (Menger 1892; Mitchell 1944; Sargent and Velde 2002).

⁴Historical examples for this theoretical perspective are, for example, the case of Henry Ford’s introduction of the semi-automated production of his T-model car and the case of the introduction of the compact disk to replace vinyl music recordings.

bring together disparate ideas to innovate the economy and to create new economic wealth.⁵

Baumol (1990, 2010) introduces an institutional perspective on entrepreneurship and contests Schumpeter's perspective on the entrepreneur as a driver of economic development through generating waves of creative destruction. Baumol instead observes that the distinct form of entrepreneurship within a society is determined by the institutional structures of that society and, thus, integrates institutional structures into the analysis of entrepreneurial action. Baumol suggests that some institutional environments and arrangements have historically been more compatible with productivity increasing technological innovations than others. He concludes that institutional arrangements allow a Schumpeterian entrepreneur to be more or less successful. However, Baumol also notes that entrepreneurship has historically not always been of the Schumpeterian variety. Hence, institutions tend to determine both the level and type of entrepreneurship.

In Sims (2017) and Gilles (2018, chap. 5), this Baumolian line of reasoning is extended to its logical conclusion and fully integrated with the Schumpeterian and Burtian perspectives. Based on several historical cases, we conclude that entrepreneurial activity is only relevant as far as it affects the institutional matrix that guides economic behaviour. Innovation of production technology and the introduction of new commodities only have economic impact if it affects the institutional structure of the system of commodity markets; bridge building is similarly effective only if it creates innovative network architectures; and activities that modify the institutional matrix of the economy are obviously entrepreneurial as well.⁶ All these phenomena refer to institutional features of the economy.

The Entrepreneurship of the House of Medici

One historical episodic era stands out as a unique case that combines all of these three categories of entrepreneurial activity and supports the theorising of entrepreneurial activity from all of these perspectives. This concerns the rise of the Medici bank and the establishment of the Medici family as the ruling house in the plutocracy of Renaissance Italy.⁷

⁵ A prime example of such entrepreneurship is the case of Microsoft under the leadership of Bill Gates, using acquired software to provide IBM with an operating system for its *Personal Computer* in 1980.

⁶ This general perspective on institutional entrepreneurship allows one to consider political agents as economic entrepreneurs if their actions indeed affect the institutional foundations or matrix of the economy—such as is the case for Gaius Octavianus Augustus, Napoleon Bonaparte and Margaret Thatcher.

⁷ I refer to Sims (2017) and Gilles (2018, chap. 5) for details of the following conclusions and insights.

First, the historical entrepreneurial activities of the Medici family in Florence support the Schumpeterian perspective for the innovative financial products and bookkeeping practices introduced by Giovanni di' Bicci de' Medici in his international banking network in the early fifteenth century.

Second, Giovanni's son and heir, Cosimo di' Giovanni de' Medici, placed himself and his family at the centre of the Florentine political power structure by building an elaborate marriage network with other ruling families in early fifteenth century. Cosimo's network has been formalised by Padgett and Ansell (1993) and Jackson (2008) to test several measurement tools from social network analysis to determine positional power.

Third, Giovanni was able to build his European-wide banking network based on innovative management of international branches, referring to innovation of the institutional behavioural rules that governed the international banking networks at that time. This contributed to a prolonged period of significant economic success for the Medici bank that lasted for a century, only coming to an end under the weak leadership of Piero "the Gouty" de' Medici. It also affected banking practice in Europe, in general, that paved the way for the rise of capitalism in the eighteenth century.

The case of the Medici shows that historical episodes and cases can contribute significantly in the development of qualitative, general economic theories. This goes well beyond the standard perspective on economic history founded on analytic narratives and cliometrics.

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5

Economic History and the Policymaker

Tim Leunig

Politicians, and the officials who serve them, live in the present. That is their job. But making good policy requires an understanding of the past. This is true in all fields of policy. Those who wish to win a war must study those who have won and lost in the past. Likewise, those who wish to prevent war must study those who have done so, and those who have failed to do so. History is even more important when making economic policy, because economics, unlike international relations, has many regularities that can be understood, and which yield useful predictions. Economics is a *social* science, but it is *science* nonetheless.

Small but Common Issues Versus Large but Uncommon Ones

Take the basic laws of supply and demand. Economists know that if supply falls, prices rise. We know this because it has been true throughout the ages. Wages rose rapidly after the Black Death because the supply of labour fell as people died, the price of food rises in famine situations, and so on. All economists know a little economic history, even if they do not realise it. When problems are small, and frequent, economic history is of little benefit. Everyday experience teaches us that price rises when supply falls, however interesting the Black Death may be to economic historians.

In contrast, when problems are large, and rare, a knowledge of economic history can be critical. Consider the Great Recession: the biggest challenge economists have faced for many years. On most measures, the world has not had a recession of this magnitude since the Great Depression of the 1930s. No one in any position of authority or influence in the Great Recession remembers the Great Depression first hand. It is history.

And history teaches us some pretty clear lessons. We know, for example, that when recessions hit, tariffs are bad news. The Smoot-Hawley tariff did not create the Great Depression, but it made it worse (Irwin 1998; The Economist 2008). Contemporary economists knew that tariffs were a bad idea—more than 1,000 petitioned Hoover not to sign the bill. But it had wide support, and Hoover signed Congress's wish into law. This time has been different: only one politician of any note, President Donald Trump, supports protectionism. A knowledge of economic history has at the very least delayed politicians from making a bad situation worse, even if, at the time of writing, it is unclear whether it will prevent the return of protectionism.¹

Economic historians have also helped policymakers understand which policies are more likely to work in these unusual economic circumstances. Economic historian Professor Nicholas Crafts, for example, has written extensively—including for policy audiences (Crafts 2011)—about the right policies when interest rates are at their “zero lower bound”. He was awarded a Commander of the Most Excellent Order of the British Empire (CBE) in 2014—an honour just one rung below a knighthood—explicitly for his “services to economic policy”. With Peter Fearon, Crafts edited what is the definitive book on the 1930s for current policymakers: *The Great Depression of the 1930s: Lessons for Today* (Crafts and Fearon 2013). That book deserves to be—even more—widely read.

Is This Time Different?

The second classic opportunity for economic historians is in ascertaining whether something that appears new and interesting is indeed new, interesting, and likely to be sustained. A good example is female labour market participation, and the gender wage gap. Female labour is as old as time itself, as is the gender wage gap: the book of Leviticus records that female slaves were worth three-fifths that of male slaves (Leviticus 27, 3–4). Harvard Professor Claudia Goldin, a leading economic historian working with her co-author,

¹ Academics being academics, some question whether protectionism is so bad (see Siles-Brügge 2014; note, however, that the author is a political scientist, not an economist).

fellow Harvard Professor Larry Katz, has produced some of the most groundbreaking work in this area. Their new book, *Women Working Longer*, addresses some of these issues (Goldin and Katz 2018). Women, or at least those in full time work, with a decent education, are working for more years than ever before. But that qualification, “those in full time work, with a decent education”, matters. It allows us to make a reasonable estimation of future trends. Similarly, their recent work on how pharmacies are the most family-friendly profession currently in existence is based on understanding 50 years of technological and social development (Goldin and Katz 2016). The world is path dependent, and the only way to understand path dependency is to understand history (David 1985).

Nowhere is this more true than in developmental economics. For sure, China has grown faster than any previously developing nation (Maddison 2001). But its path to development is very similar to—say—Japan or Korea before it. They in turn drew on the experience of Continental European nations, who caught up with Britain and America earlier in the twentieth century (Gerschenkron 1962). In each case, the story is the same. A nation, directed by its government, imports technology that can be operated reasonably well by relatively inexperienced workers. Dramatically lower wages in the newly industrialising country outweigh productivity shortfalls, ensuring that the nation is competitive in international markets (Clark 1987). This tells us that the best industries on which to develop are those in which consumers are price sensitive, with technology that can be learnt quickly, and with a sizeable labour input. In short, garment manufacturing. Nimble fingers and a sewing machine are all that is needed. Hence, economic historian Professor Eric Hobsbawm’s remark that ‘whoever says Industrial Revolution says cotton’ (Hobsbawm 1999: 34). As China move up the value chain, and Chinese wages rise, the next nation can learn from history too (Huang 2013). Get the basics right: the rule of law, the ability to export, reliable power, and the historically well-proven route to development opens up. People will spontaneously leave low productivity agriculture to work in factories in these circumstances, as they have done in every nation so far. History tells us so, economic historians understand why, and policymakers would do well to listen to both.

Some Practical Suggestions

If policymakers are to listen to economic historians, they need to know who precisely to listen to. There are outstanding professors in economic history in some of the world’s finest universities. It does not take long to find them on

the web, and finance ministers should—and sometimes do—pick up the phone and ask them for their advice. My experience is that no academic, in any discipline, declines to give time to their own, and to other, governments.

But what governments really need are people within government—officials, who know some economic history. That means hiring people with this knowledge and skill set. While it is possible to hire people with a degree explicitly in economic history, it is generally better to hire an economist who has studied economic history as part of their degree. A module or two is sufficient to make any decent economics student realise that the world is not like a textbook. The ideal two-module course would have one on the development of the global economy and one on the development of the relevant nation. Any economics department can offer such a course—using a syllabus found elsewhere as necessary—and such courses should be as automatic as the current trinity of micro, macro, and 'metrics. This will also have the advantage of teaching students to write, an essential skill in so many walks of life, and one not taught well in increasingly maths-based economics degrees. That is certainly the case for economists in government, where almost all decisions are taken based on arguments expressed in writing.

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6

Economic History, the History of Economic Thought and Economic Policy

Graham Brownlow

There is a problem within economic pedagogy that the economic past is patronised by academics unable to recognise its usefulness. Students suffer a reduction in their economic literacy as a result of a curriculum that marginalises the past (Kates 2013; Earle et al. 2017). In addition, there is a wider social cost that ignorance of past events and ideas gives rise to inferior policymaking. Perhaps underlying this ignorance is the comforting (but erroneous) Whiggish assumption that the current “state of the art” research techniques applied to contemporary problems offer the only relevant policy-making insights. It is noteworthy that recently both policymakers and academics have been highly critical of such a Whiggish assumption (Morck and Yeung 2011; Crafts 2012; Haldane 2016). As a consequence, it has been suggested that economic history should shift more towards policy-menu-driven questions precisely because Whiggishness does not hold (Rodrik 2003; Crafts 2012).

The discussion in this chapter revolves around two important papers by Cairncross and Eichengreen, as well as the more recent book by Offer and Söderberg. All three of these publications discuss the relationships between contemporary economic policy, the history of economic thought (HET) and economic history. This chapter agrees with the more pessimistic assessment of Offer and Söderberg concerning the optimality in the “market” for economic ideas, and it is also supportive of shifting economics in a more policy-driven direction.

Cairncross on Economics and Economic History

The thoughts of Sir Alec Cairncross on these kinds of issues, provided in the Economic History Society's 1988 Tawney Memorial Lecture, have particular resonance (Cairncross 1989). Cairncross was incredibly wide ranging. He was a policymaker par excellence, a distinguished academic and author of an influential textbook as well as work in economic history. Cairncross observed that essentially there were two kinds of economic history. A first kind was based on assembling facts and creating a coherent historical account. Such an interpretation was not based on placing itself within a more general framework. In contrast, the second kind attempted to explain historical events as essentially an illustration of more general theoretical propositions. Understandably, some have wondered if one of the main costs of "the Cliometric Revolution", whatever its undoubted benefits, is that it played a significant role in shifting economic history towards a merely an illustrative role (Solow 1985; Kindleberger 1989; Cesarano 2006). Paradoxically, an illustrative approach is argued to have accentuated the separation between economics and economic history (Cesarano 2006). Such a separation tended to marginalise the potential policy contribution of economic history because it had little that is unique to offer to the economics profession (Solow 1985).

It was, however, Cairncross's contention that the interests of the economist (be they policymaker or academic researcher) were "fundamentally the same". While it was the job of the economist to explain how a contemporary economy works, the job of the economic historian was to explain how it worked in the past. Cairncross did not end the argument there, however. He observed that, firstly, in order to understand how a contemporary economy works, one needed to rely on historical data. And secondly, for an economist in a policy-making role, he noted that in order to take an accurate view of what does and does not matter in a contemporary economy, or parts of it, you needed a historical perspective and that in order to think how an economy might work in the future, it might be necessary to look quite far back into the past. Furthermore, there was a pedagogical implication to Cairncross's lecture. He argued that the best training for interpreting current economic trends was 'a prolonged struggle with the imperfect and incomplete statistics' of the past (Cairncross 1989: 178).

Cairncross was concerned that any neglect of economic history would lead to gaps appearing within economic theory, and that such gaps could have adverse implications for policymaking as well as theoretical research: 'the value of any advance in [economic] theory is measured by the usefulness of

the questions it moves us to ask, the addition to the kit of tools with which it supplies us; and it is the same kit of tools that economic historians will find useful for their purposes' (Cairncross 1989: 175). He continues by arguing that economic history cannot neglect advances in theory, 'but if theory is not doing its job properly it can make this clear and contribute to theory by showing the importance of questions that find no place in current theoretical literature' (ibid.).

Cairncross's observation regarding the role of economic history in offering a "critical friendship" or "myth-busting" role is one shared with other economic historians (Morck and Yeung 2011; Crafts 2012). The earlier generation of scholars, mentioned by Cairncross, tended to reject the idea that theories regarding economic policy had to hold for all time. Economic historians played a crucial role in cultivating the idea that context was vital and that sometimes contemporary theory did not do its job properly (O'Brien 1942–1943).

Such scepticism towards the possibility of explaining all economic phenomena with a single general theoretical framework, and the corresponding emphasis on the importance of historical context, was not restricted to older, less technically adept economic historians. For example, Oskar Morgenstern devoted parts of his *Limits of Economics* to a discussion of generalisability and historical context (Morgenstern 1937). Morgenstern was particularly interested in discussing the role of vested interests within economic history. He observed that vested interests will hinder economic performance by slowing down technical progress. This is a rather prescient insight, which anticipates the findings within much later research. Likewise, a number of later economists and economic historians have shared in this mistrust of generalisability (Solow 1985; Kindleberger 1989; Mokyr 2005).

Eichengreen on Economic History and Economic Policy

Eichengreen's 2011 Presidential Address at the Economic History Association provides a good way to frame the more recent thinking and the relationship between HET, economic history and economic policy (Eichengreen 2012). Eichengreen observed that the Great Recession or Global Financial Crisis (GFC) of 2007–2009 was "a good crisis" for economic historians. He suggested that the GFC reminded macroeconomists in the USA of parallels with the Great Depression, in turn, such a recognition legitimised those wanting to

not repeat the perceived policy errors of the 1930s. Likewise, among economists, the analogy with the Great Depression delegitimised protectionist impulses.

Eichengreen argued that the use of such analogies is common and that choosing to draw an analogy between the Depression and GFC provided a “powerful intellectual shortcut” that provided a clear diagnosis and prescription for policymakers (Eichengreen 2012: 293). Eichengreen also discussed the possibility that decision-makers could make public reference to historical analogies in order to provide cover for decisions taken on other grounds. Eichengreen conceded that this more cynical interpretation ignored the observation that several of the key policymakers (e.g. Ben Bernanke and Christina Romer) were ‘serious students of business cycles and financial crises’ (Eichengreen 2012: 298).

Eichengreen concluded his valuable discussion by speculating that future historians of the GFC would be less sympathetic to the Whig history interpretation of the Great Depression in which the crisis created policy and institutional innovations that made it less likely that such a situation would re-emerge. Likewise, the role of ideology rather than detailed macroeconomic thought in explaining policymakers’ resort to austerity as a policy response to the GFC suggested to Eichengreen that future historians of the Great Depression will focus less on bad economics and more on political-institutional considerations in explaining the failure to effectively tackle the crisis. More generally, he concluded that the widespread use of the 1930s analogy in response to the GFC reminded economic historians that narratives are contested. This led Eichengreen to suggest that more explicit attention will be paid in future to the question of how such influential narratives are formed.

In terms of responding to Eichengreen’s arguments, three points can be made. Firstly, Eichengreen’s observations regarding the limitations of Whig history is well placed, but it was a pity that he did not discuss Cairncross’s idea of economic history providing a critical friendship role. The problematic role of economic thinking in the emergence of the GFC fitted Cairncross’s line of argument. Eminent macroeconomists had during the 2000s boasted that the problem of boom and bust had been solved (Lucas 2003; Earle et al. 2017). Cliometricians as a group conspicuously did not deflate such exaggerated claims. So, while Eichengreen’s claim that the crisis had been a good one for economic historians is accurate at one level, he did not address the failure of economic historians to provide the vital critical friend role in the run up to the GFC.

Secondly, since the GFC, the history of economics has started to repeat itself in ways that Eichengreen’s analysis also downplayed. For example,

Haldane's recent discussion of the policy lessons from the GFC calls for a more eclectic modelling approach, including a retreat from the idea of generalisability (Haldane 2016). Haldane's analysis provides additional support for economic history operating in the critical friend role and for focusing economic history more on policy-menu-based topics.

Thirdly, Eichengreen's insight regarding narrative has been developed by Shiller (2017). Shiller, using a number of examples drawn from economic history, including the Great Depression and GFC, argues that economic narratives, by which means stories that people tell each other about the economy, are important in explaining the determinants of macroeconomic activity. He concludes that economic policy needs to take narratives into account.

Offer and Söderberg on Economic Ideas, Economic Policy and Economic History

Cairncross observed that revolution in statistics encouraged the development of cliometrics, but he noted that it was curious that the historical development of statistics within economics had not been given enough attention by economic historians (Cairncross 1989: 179). Offer and Söderberg in *The Nobel Factor* provide clues as to how economic historians can contribute to an analysis of the relationship between economic history, HET and economic policy (Offer and Söderberg 2016). In their account, Offer and Söderberg note that the creation of the Nobel Prize itself stemmed from the distributional conflict that had been formed by Sweden's economic history. The Swedish social democratic government was much more egalitarian than the central bank—Sveriges Riksbank—and as a form of compensation for restricting the policy autonomy of the bank, the government allowed the central bank to keep funds. In 1968, this funding was used to endow the prize to mark the bank's tercentenary. The prize cemented a relationship between changes in economic ideas and policy outcomes, as Offer and Söderberg contend that the prize legitimised a "market turn" in economic policy, which had many consequences, including increased inequality and the shifts in finance and macroeconomics that culminated in the GFC.

Offer and Söderberg provide much food for thought for scholars of economic history, contemporary economics and HET. For example, economic historians' role within the market turn, a point Offer and Söderberg did not develop, is worth further research. Two pioneering cliometricians were awarded prizes in 1993 (North and Fogel) and a number of other prize

winners have also written about topics within economic history (e.g. Coase, Friedman, Hicks, Lewis, and Kuznets). Yet, while some of these winners (most notably Coase and Friedman) can undoubtedly be classified as promoting free market ideas, others (Hicks, Fogel, Lewis and Kuznets) cannot be so easily categorised. North's ideas changed so much over time that it is hard to pigeon-hole his policy views (Brownlow 2010).

Offer and Söderberg place the blame for growth in inequality at the door of economic liberalism; they argue that one of market liberalism's objectives was to 'take the question of inequality out of economics' (Offer and Söderberg 2016: 261). There are big claims in *The Nobel Factor* concerning issues of observed economic performance, equity and efficiency and the evolution of economic ideas. Responding to such claims will contribute to the research programme centred on the historical relationship between economic ideas, policy and outcomes.

Concluding the argument by returning to the insights of Cairncross, there is much to commend the argument that economic history should develop in a more policy-driven direction (Crafts 2012). Scholars of both economic history and HET in the future will, for example, have much to offer in trying to think about the role of free market ideas in explaining the growth in inequality. Whatever direction this future research takes, however, it can be safely wagered that the ideas of "defunct economists", to borrow Keynes's phrase, will continue to influence economic policy.

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7

Teaching Economics with Economic History

Matthias Flückiger

There are many parallels between economic history and other subfields of economics. Economic historians speak the same language as other economists; they share a common disciplinary core. They employ similar research approaches to address related questions. In fact, as this book illustrates perfectly, the overlap is sometimes so strong that the distinction between economic history and other areas becomes fuzzy at best. As Christina Romer (1994) highlights in an article aimed at university-level economics teachers, economic history has become an integral part of economics, without which we would not understand the economics of topics ranging from depressions, labour markets, growth, financial institutions or business cycles. And there is ample evidence of further convergence between economic history and other fields since Romer's contribution to pedagogy in economics. Indeed, Peter Temin (2016) goes one step further to argue that economic history and development economics have such a degree of crossover that they are essentially one and the same.

However, many non-economic historians still see it as a marginal subfield. Some even seem to be misled by the term "history" and assume economic history must be part of the humanities. This has led to (relatively) little awareness of the field and its results among (most) economists. Insights from economic history, therefore, do not feature in many economics syllabi. This omission is to the detriment of economics students and junior researchers as understanding the past is often the key to making sense of the present. Furthermore, historical examples are often better suited to illustrate an economic theory to

students than current situations. The benefit of hindsight, a great advantage of economic history, often allows for a much clearer identification of cause and effect, and consequently, the deduction of insights and policy implications. Making economic history an integral part of economics lectures can improve understanding and learning outcomes.

Given the convergence between economic history and other fields, inserting insights from economic history into the economics syllabus should not prove an impossible task, even for those educators never before exposed to the field. Despite the small extra effort this may require, this should be desirable because, as Eugene White (1996) argues so forcefully in his essay on the relationship between economic history and economics, ‘good economic history is essential to good economic research’. Together, the contents of this present volume are a great starting point. They provide a resource for both lecturers and students. Incorporating its content into your economics pedagogy will help bring economic concepts to life, spark the interest of students and, ultimately, improve teaching quality and success. Below, I outline several ways in which the book could be used to enrich and complement the syllabus.¹ I do so from the particular vantage point of someone who has recently been teaching growth, development and macroeconomics to undergraduate students.

The Book for Lecturers

The book can form the basis for whole lectures. All chapters are sorted in the book according to their *Journal of Economic Literature* code, which makes finding relevant material very easy. For example, the Chaps. 14 (Vellore Arthi) and 15 (Sascha O. Becker) can be used to develop a lecture on the relationship between human capital and growth. The most important aspects, both theoretical and empirical, are succinctly discussed and many illustrative examples, along with the relevant references, are provided. Similarly, Chaps. 31 (Matthias Blum) to 38 (Leonardo Weller) give a good overview of the importance of “deep fundamentals”, such as geography and institutions, in explaining economic disparities today. What makes the chapters of this book particularly compelling is that they illustrate the changing influence of one and the same factor over time and space. These heterogeneous, context-specific effects are

¹ Other places to look for inspiration on teaching economic history are the collection of economic history syllabi collected on the Economic History Association’s *EH.net* (<http://eh.net/course-syllabi/>) and a similar resource provided by the Business History Conference (<https://www.thebhc.org/syllabi>). For ideas on teaching economics more generally, and syllabi from other fields of economics, see The Economic Network’s learning and teaching pages (<https://www.economicnetwork.ac.uk/resources>).

often neglected when looking solely at the modern era but are of critical importance when deducing policy implications. They should therefore be highlighted to students.

The stand-alone nature of the chapters also makes it easy to integrate specific (new) topics into already existing lectures. For example, incorporating a discussion of the measurement of innovation in Chap. 47 (Gerben Bakker) and frontier analysis in Chap. 48 (Pieter Woltjers) would perfectly complement my own lecture on total factor productivity (TFP) growth. These chapters will give me a good sense of what a particular measurement methodology is, what the associated problems are, and—most importantly in the context of teaching—provide me with illustrative examples. And all without having to turn to an econometrics textbook for help.

More generally, the book can be used to enrich lectures. I always try to contrast economic theories and concepts with real-world data. However, finding compelling examples can be a time-consuming task, particularly for teaching material that lies outside one's own direct area of expertise. This search is greatly facilitated by the book. It includes, and makes reference to, numerous examples that illustrate the effects of policy changes in Chap. 9 (Alan de Bromhead), natural disasters in Chap. 28 (Eoin McLaughlin) or technological change in Chap. 25 (Gerben Bakker) on economic growth. Leafing through the book may also help identify gaps in one's module outline. For me, this gap was female agency, the focus of Chap. 17 (Humphries).

The book can be used in various types of student assessments. For essays or presentations, students can be given selected chapters, which are to be used as starting point for further analysis of a topic. For instance, mapping Chaps. 18 (Richard H. Steckel) and 26 (Christopher L. Colvin) onto 37 (Price V. Fishback) could be used as the basis for an essay (or presentation) in which students analyse the relationship between culture, institutions and economic growth in a particular geographic setting of North America. Or mapping Chap. 19 (Rowena Gray) onto Chap. 49 (Noel D. Johnson) can compel students to propose how they could use geospatial research methods to gain new insights into the questions that occupy economists studying crime.

Similarly, students could be asked to critically assess the content of one specific chapter and outline the possibilities and limitations of using historical evidence to inform policies today. For example, students could be asked to identify the main factors driving migration from Chap. 10 (Sebastian T. Braun), analyse to what extent these factors are still relevant and deduce policy implications. Such a task could feature in a short-answer question in an end-of-term exam. But it could also form the basis for setting a take-home "research exam", where students are given a limited number of days to write up a fully referenced essay.

The Book for Students

Many economics modules will touch on aspects covered in the book without discussing them in any depth. When students are interested in learning more about these topics, they can easily be pointed towards the relevant chapter in this book. During my lectures, for example, I discuss the importance of the Industrial Revolution in changing the trajectory of economic growth. A rigorous analysis of factors explaining the timing and location of the Industrial Revolution lies outside the scope of my module. However, some students may be interested in exactly this question. Reading Chaps. 23 (Jared Rubin) together with 24 (Christopher L. Colvin and Alexandra M. de Pleijt) will give them an up-to-date overview of the literature and provide them with references for further reading.

Students themselves may also be encouraged to navigate the book independently. When I was an undergraduate student, I was frustrated that my degree required me to follow a set of business management modules that I felt were either not very rigorous or not well integrated into my economics degree. Recommending this book to students who are struggling with similar issues may help them to find the connections between management and economics. For instance, they might discover the connections between the material in their business strategy module and Chap. 20 (Michael Aldous), which discusses the historical origins of the corporation and shows that this legal form is not always the most appropriate means of organising the firm. Or they might find the connections between their human resource management module and Chap. 22 (Andrew Seltzer), which shows how this field is essentially just an application of information economics.

When students decide to write a thesis, undergraduate or postgraduate, they often struggle to find a research question. Browsing through this book can help them to find one. Students get an overview of what some of the “big questions” are in economics and economic history. This may be enough to spark their imagination. The fact that the chapters succinctly summarise the current state of the literature further helps identifying gaps worth investigating. Most of the chapters explicitly discuss open questions and avenues for future research. What may particularly appeal to students is that each chapter includes a multitude of references that they can use to start their literature review.

The book further highlights the importance of acquiring a technical toolkit in order to rigorously address and understand economic issues. A great example of this is Chap. 45 (Matthias Blum and Arcangelo Dimico), which

discusses how methods that have become popular in applied econometrics can be used to solve problems in economic history research. It also illustrates that historical data often lend themselves to the application of state-of-the-art techniques and thereby contribute to the understanding of current issues, such as in Chap. 49 (Gabriel Geisler Mesevage) for the case of network analysis. In short, this book may inspire and motivate students in their own research.

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Part II

Questions and Themes in Economic History

Haiku by Stephen T. Ziliak

Neoclassical
economic modeller
afraid of the past?

To hungry scholars:
The proof's not in the pudding
but in the archives



8

Money and Central Banking

John D. Turner

The study of money and banking, and the prominence of historical enquiry within this study, goes through cycles. The undergraduate and graduate economics programmes in elite universities at the beginning of the twentieth century had courses in both money and banking and on the history of money and banking. However, the growth of general equilibrium theory, and the move away from an institutional approach to economics, precipitated the demise of such courses. This demise was accelerated by the Great Moderation, because money and banking became a technical issue that “we had brought under our control”. Studying it was boring and studying its history was perceived as antediluvian at best.

The 2008 global financial crisis, however, has changed this attitude. Students of economics today need to understand the history and evolution of money and banking so that we do not repeat the mistakes of the past and do understand why our modern monetary systems have evolved in the way they have. Furthermore, the study of the history of money and central banking helps illustrate key concepts in monetary economics. Most important of all, monetary and central banking history confronts students with an existential question—why do money and central banks exist? Unless students can address this question, it is difficult for them to grasp the purpose of monetary policy and central banking in the present and into the future.

The Origin of the Specie

A large part of the discipline of economics is the study of markets. To function, markets need a medium of exchange, that is, money. Economics is also interested in measuring wealth, debts and value. Money helps us do this. In addition, economics has an intertemporal dimension—how can we store wealth from this period and take it into the next period? Once again, money is an asset which easily helps us do this. The history of money is the study of how a particular asset came to possess these three functions. It is important for students to understand this story because it illuminates the purpose and function of money in modern societies. In an era of cryptocurrencies and existential threats to cash, the history of money provides insights not readily available elsewhere.

The best-known description of the evolution of money is that of Menger (1892). The Mengerian theory starts in a barter economy with its double coincidence of wants, which greatly encumbers trade and the division of labour. This situation resulted in individuals, led by their own self-interest, without any agreement or legislative compulsion, to exchange their goods for other more saleable commodities, even if they do not intend to consume those commodities. Over time, some commodity spontaneously emerges as the generally accepted medium of exchange. This commodity then becomes the unit of account and a store of value.

The implications of Menger's theory of the origin of money are at least threefold. First, money was not invented by someone or created by the government—it evolved spontaneously from humans pursuing their own economic self-interest. Second, the commodity that becomes the medium of exchange may not be consumed by many of the individuals who use it for exchange. An interesting natural experiment which illustrates this point, as well as the role of shocks to the money supply, is Radford's (1945) description of a prisoner-of-war camp during World War II, where cigarettes supplied by the Red Cross spontaneously emerged as the medium of exchange among smokers and non-smokers alike.

Third, because money evolved spontaneously, its forms have varied across time and space. For example, in primitive agricultural societies, sheep, cattle, grain, slaves, fur and animal skins, tobacco and rice have been all used at various times (Einzig 1966; Davies 2016). Indeed, the etymology of money illustrates this point—the “buck”, a colloquial word for the US dollar, comes from the fact that buck skins operated as a medium of exchange in early colonial times.

Urbanisation and the growth of international trade resulted in the spontaneous evolution of gold, silver and copper as mediums of exchange. Their uniform quality, durability, portability, divisibility and fusibility explain why they emerged as money. In addition, unlike agricultural monies, their supply was fixed, resulting in a relatively stable purchasing power over time. Mints emerged to coin these precious metals and create what was known as specie. Gold and silver dominated monetary systems up until the late nineteenth century, when the gold standard emerged as the dominant monetary regime.

White (1999) provides a helpful extension to the Mengerian theory of the origin of money to explain how banks and paper money evolved. Merchants and individuals who had lots of silver and gold coins deposited them with goldsmith bankers for safekeeping. Over time, these deposits were used as a medium of exchange without leaving the vaults of the goldsmith, or simply being transferred between the vaults of different goldsmiths. Goldsmiths issued certificates for coin deposits and these were used as a medium of exchange. Alternatively, individuals could write cheques on their specie deposits which permits the holder to have the specified amount of specie transferred to their account.

There are, however, difficulties with the Mengerian explanation of the evolution of money. First, it does not always align with the historical evidence produced by numismatists and anthropologists (Goodhart 1998; Graeber 2011). Second, and more fundamentally, it does not explain why governments have played an important role in money across time and space. In particular, it does not explain why fiat money (i.e., inconvertible paper money which has been made legal tender by government decree) emerged. To overcome this deficiency, the Cartalists, or state theory of money school, propose an alternative story for the origin of money (Knapp 1924; Goodhart 1998).

Throughout time and across space, governments have established and operated mints and stamped the sovereign's face on their specie. According to the Cartalists, governments controlled minting because they had a monopoly of violence to protect inventories of precious metal and they could better protect the quality of the coinage from debasement, i.e., diluting the precious metal in a coin with base metal. This control of the coinage by the sovereign helped the development of the fiscal state—taxes were levied in the government-minted coins, and the coinage itself proved a source of emergency revenue (seignorage) during times of war. Henry VIII's infamous debasement of the Tudor coinage occurred because of his need for funds to fight the French. Cartalists argue that coinage and the monetary economy would have not taken off without this government intervention and that the value of money

comes from the sovereign not its intrinsic value. Mengerians, on the other hand, argue that the only reason for the state control of money was to raise revenue (Selgin and White 1999). While this power can be used to finance state survival during a war, it can also be abused by sovereigns. Possibly the principal issue in monetary economics today, and in the past, has been how to prevent the sovereign debauching the currency.

The Evolution of Central Banks

So how did we move from specie-based monetary system to today's fiat money issued by central banks? How did central banks assume such importance in modern economies? What can history tell us about the role of central banks? To answer these questions, economists can do no better than understand the evolution of the prototype central bank—the Bank of England, which was founded in 1694.¹ For the interested reader, a brief history of the leading central banks can be found in Capie et al. (1994) and Goodhart (1988).

The founders of the Bank of England solved a critical issue that was being faced by England's new democratic regime, ushered in by the Glorious Revolution of 1688. Tax raising powers in the new democracy were in the hands of parliament, who were reluctant to supply the finance necessary to the king to fight wars. The Bank of England was established by King William and his financiers with a paper currency redeemable for gold, that is, a gold standard. During military emergencies, the note issue was expanded and convertibility into gold was suspended. This unique feature allowed the currency to expand quickly and flexibly to finance defence emergencies because there was a credible commitment that after the military emergency, notes would be convertible into gold at the original conversion rate. The first formal suspension of gold payments occurred in 1695, and the suspension lasted the two years King William required to defeat Louis XIV of France. One hundred years later, in 1797, the Bank suspended convertibility into gold for over two decades to enable Britain to fight the Napoleonic Wars. This suspension resulted in a cartoonist drawing a cartoon entitled 'Political Ravishment, or the Old Lady of Threadneedle-Street in Danger'. The nickname stuck. According to Thompson and Hickson (2001), the Bank of England was an institution which was vital to the survival of the first national democracy and was therefore emulated by other nations when they became democracies.

¹ Although the Sveriges Riksbank can trace its origins back to 1668, it was not permitted to issue notes until the eighteenth century.

Convertibility was suspended once again on the outbreak of World War I and restored at the pre-war parity in 1925 by Winston Churchill, against the advice of John Maynard Keynes. However, as with previous resumptions of gold, this triggered a harsh recession. This was one of the main costs of the gold standard. In addition, the gold standard generated its own business cycle, with recessions of greater duration and amplitude than under a fiat money regime. By the mid-1930s, the gold standard was abandoned by all but the US. After World War II, the Bretton Woods system re-established a *de facto* gold standard, when Western democracies agreed that their paper currencies should be convertible into US dollars as long as the dollar maintained a fixed conversion rate into gold. This Bretton Woods system was phased out in the late 1960s and came to an official end in 1971 when the US broke the link between the dollar and gold.² As a result, central banks issued inconvertible fiat money, which had value only because governments required future taxes to be paid with it.

The dominant view among economic historians concurs with that of Keynes, who described the gold standard as a “barbarous relic”. However, Thompson (2013) raises the question as to whether the cost of abandoning the gold standard has been worth it in that countries no longer have access to emergency finance, and countries with weak institutions and many external threats typically end up creating hyperinflation. As a means to tame inflation and remove the influence of government on monetary policy, central banks were made independent, pegged their currency to a major reserve currency or, in the case of the Eurozone, they handed control of their money supply to unaccountable technocrats schooled in the monetary discipline of the Bundesbank.

Because central banks, such as the Bank of England, evolved to have monopoly or near-monopoly of note issuance, they came to play important roles in banking systems, particularly during credit expansions and subsequent crises. Economic history provides us with multiple case studies of such episodes and the role of the central bank in booms and financial crashes (Bordo et al. 2016; Friedman and Schwartz 1963; Grossman 2010; Reinhart and Rogoff 2009; Turner 2014).

Should central banks prick or lean against booms and bubbles or simply clean up afterwards? The historical examples of pricking suggest that such a policy has many dangers. For example, the Federal Reserve is alleged to have deliberately pricked the bubble after the death of Benjamin Strong in the spring of 1929. The deliberate pricking of the stock market boom in 1927 by the Reichsbank was another policy intervention that did not work

²To understand the evolution of the international monetary system, economists should consult Eichengreen (2008).

out well because the intervention hit Germany at a key point in its post-hyperinflation recovery, affected investment and tipped the economy into a severe depression.

Economic history tends to reveal that central bankers prefer to clean up *after* a financial bust than prick a financial boom. The debate about the proper role of central banks during financial crises goes back as far as Thornton (1802) and Bagehot (1873). Should the central bank act as a lender of last resort and, if so, under what conditions should loans be advanced? Should the central bank go further and rescue insolvent banks? What is the moral hazard associated with the lender of last resort and bailouts?

Although the historical evidence largely supports the notion that central banks should act as a lender of last resort, there is debate as to how assistance should be provided. An excellent summary of the lender of last resort from an historical perspective can be found in Bordo (1990). There is also debate about efficacy of historical bank bailouts, with repeated bailouts resulting in a build-up of moral hazard, resulting in an even larger bailout of the system (Turner 2014). This moral hazard explains why central banks began to regulate banks. Recent historical scholarship has highlighted the relative stability of regulatory systems where bankers and bank owners had “skin in the game” in the form of double and unlimited liability (Grossman 2010; Turner 2014).

It’s the Politics, Stupid

The most important lesson of the history of money and central banking for economists is that monetary policy and central banking is not a technocratic issue. From the very creation of money and central banks, sovereigns have been involved. There is both a cost and benefit to this interference. On the one hand, government control of money and central banks means that military and other emergencies can be met, ensuring the survival of democratic states. On the other hand, the symbiotic relationship between central bankers and governments has resulted in the fragility of financial systems past and present (Calomiris and Haber 2014; Turner 2014).

Understanding the evolution of this relationship and its manifestation in historical booms and busts will prove fruitful territory for future economists and economic historians. In order to help students understand this relationship, we can have them write research papers or dissertations or blogs, which focus on a particular historical banking crisis, bubble or credit boom.³ This focus on past monetary disasters is of paramount importance to economics.

³ Reinhart and Rogoff (2009) provide a helpful list of such crises as a useful starting point.

The discipline was wrong-footed on the 2008 crisis. We therefore have a responsibility to ensure that the next generation of economists has a “lest we forget” mentality towards the carnage that can be afflicted upon an economy as a result of monetary disorder.

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9

Globalisation and Trade

Alan de Bromhead

Globalisation, defined as the increasing integration of markets for goods, capital and labour, is a process that has sometimes been portrayed as an irresistible and unidirectional force in our modern world. International markets now operate within an institutional environment of relatively low tariffs on goods, few restrictions on capital, low transport costs and almost instantaneous communication. Policies surrounding globalisation, such as those relating to trade, migration and the movement of capital across borders, have, in recent decades, been largely removed from the domestic political arena. They have been transferred instead to international and largely technocratic bodies such as the World Trade Organisation and the institutions of the European Union (EU).

Since the financial crisis of 2008–2009, however, the political economy of trade, and globalisation more generally, has returned as an important issue in domestic politics. Migration has been recognised as the single biggest issue in the referendum on the UK's membership of the EU in 2016, while the successful campaign of President Trump featured a strong anti-migrant and anti-free trade rhetoric. With Britain seeking to redefine its trade relationship with the rest of the world after Brexit, and an escalating trade war between the USA and its biggest trading partners, interest has been renewed in the costs and benefits of protectionist policies. In short, the domestic political economy of globalisation is again a prominent issue.

While this recent political backlash against globalisation may have come as a surprise to some, it would not have been a surprise to those with a knowledge of the history of globalisation. Indeed, that globalisation can in fact go

into reverse is clear from the historical record. It is a striking observation that the world of 1914 was in many ways just as globalised as the world of the late twentieth century in terms of trade and capital movements (O'Rourke and Williamson 2001). When migration is considered, the world was surely more globalised in the past and a return to the mass migration of the pre-World War I era seems an unlikely prospect. The interwar years, however, were characterised by a dramatic breakdown in the international system, with new or increased restrictions on the movement of goods, capital and labour across borders. Understanding how globalisation went into reverse in the past, and how pressures to protect the domestic economy developed, is an important reason why anyone interested in the global economy should be interested in its history. As such, this chapter focuses on how history can help economists address a number of important questions in international trade.

The Political Economy of Trade

As most economists appreciate, despite the theoretical gains from trade, any change in trade policy has distributional consequences—it creates “winners” and “losers”. Theoretically, the losers from changes to trade policy can be compensated by the winners. In practice, however, the compensation offered to those who lose out from foreign competition are rarely adequate (Rodrik 2011). What this creates is a political group which, quite rationally, sees globalisation as a potential threat to their standard of living, a constituency that can be mobilised politically. As the political economy of globalisation becomes more apparent today, history presents an opportunity to assess the link between the politics and the economics of trade.

Most studies of the historical political economy of trade utilise some variant of Heckscher-Ohlin theory to analyse the relationships between politics and trade policy. Using such a framework, interests based on factor endowments or broad sectoral interests are defined and voting preferences regarding trade policy are predicted and tested. Examples relating to historical trade policy decisions include Sibylle Lehmann's (2010) study of Germany's switch to protectionism in the 1870s. She analyses the elections of 1877 and 1878 and finds that import-competing sectors such as agriculture favoured protection but that voting preferences within agriculture depended on farm size. Likewise, Doug Irwin's studies (1994, 1995) of British elections in 1906 and 1923 also find evidence in support of the specific factor model. However, it is clear that the way in which political cleavages are defined, as well as the existence of institutional factors, have important implications for the analysis of

trade policy formation. For example, the political system can influence the interests and preferences of the median voter, and by implication, trade policy. O'Rourke and Taylor (2007) explore the relationship between voting rights and factor endowments and find that the impact of increases in voting rights depended on relative factor endowments, as Heckscher-Ohlin predicts.

The advantage of using historical elections, such as those from Germany in the late nineteenth century and Britain in the early twentieth century, is that these contests presented a relatively clear choice to voters between free trade and protectionism, while also being elections in which the issue of trade policy featured prominently. They can therefore be used to test in which ways interests and voting preferences over trade were aligned more directly, something that is difficult to disentangle with modern data as few post-World War II elections in major industrial economies have hinged on the issue of trade. History therefore offers a useful laboratory for testing both trade theory and the formation of trade policy more specifically.

Is Trade a Driver of Economic Growth?

Another important issue which history can help illuminate is how trade relates to economic growth. The World Trade Organisation argues that the economic case for an open trading system 'is simple enough and rests largely on commercial common sense' (WTO 2018). They add: 'it is also supported by evidence: the experience of world trade and economic growth since the Second World War'. Has this always been so? Is the link between trade and growth an absolute truth or one that is conditional on the economic and political environment? Again, history can help us address these fundamentally empirical questions.

Identifying a causal link between trade and growth is of course challenging; the relationship is one which is highly likely to be endogenous. In a recent paper in the *American Economic Review*, Pascali (2017) employs the historical record to address this identification problem and explores the relationship between trade and growth in the first era of globalisation in the late nineteenth century. Causality is isolated using the introduction of the steamship which produced an asymmetric change in trade distances between countries. Under sail power, trade distances depended on wind patterns. Under steam power, passage can be more direct as wind patterns no longer dictate routes. Therefore, the introduction of steam power reduced shipping times between some countries more than others.

Among a number of import results, the relationship between trade and economic development in the late nineteenth century is found to be a complex one, with less developed countries experiencing highly detrimental effects, while a limited impact was identified for developed countries. Moreover, the relationship between trade and growth depends on the institutional context, with trade having a positive impact on growth in countries with strong constraints on executive power. The results are particularly interesting when compared to papers such as Feyrer (2009a, b), which use similar asymmetric shocks to trade distances—the closure of the Suez Canal and aircraft technology—to identify a positive relationship between trade and growth in the late twentieth century. The comparison of the results of these studies suggests there may be a context-specific relationship between trade and growth that is relevant for policymakers to consider.

What Is the Impact of Trade Policy?

Likewise, the question of the importance of trade policy—such as the impact of tariffs and protectionism—can be explored using the historical record. It is arguably harder to find opportunities to test the results of these policies in the current low-tariff and relatively open international trading environment. In the nineteenth century, many countries maintained high protectionist tariffs based on infant-industry type arguments (with the US being the often-cited example). Did these policies aid or hinder economic development? The early literature uncovered a “tariff-growth paradox” in which tariffs were found to be positively related to growth in the nineteenth century but negatively related to growth in the twentieth century. Was this paradox a statistical illusion or is the impact of tariffs on growth context-specific? Empirical work by O’Rourke (2000) and Lehmann and O’Rourke (2011) explored the relationship between tariffs and growth in the nineteenth century and found that tariffs were positively related to growth. However, the structure of protectionism mattered; only tariffs on industrial goods were associated with growth.

Additionally, Clemens and Williamson (2004) argue that the changing relationship between tariffs and growth between the pre-1914 period and the post-1950s can be best understood by appreciating the prevailing trading environment. Tariffs may have helped growth in a world where average tariffs of trading partners were higher. Meanwhile, Schularick and Solomou (2011) argue that the positive tariff coefficient uncovered in analyses of the nineteenth century is a statistical illusion which does not survive the inclusion of time-fixed effects and other controls. The debate surrounding the relationship

between tariffs and economic performance is therefore far from concluded. Understanding whether, or under what conditions, protectionism can support economic development is an important question with obvious relevance for trade policy formation, particularly for developing countries. It is also one where, again, history can provide a useful laboratory.

Conclusion

The advantages of a long-run view of the evolution of trade go well beyond those discussed in this chapter. The list of questions relating to the international economy that can be addressed by studying history is extensive. It includes questions relating to how trade shapes, and is in turn shaped by, institutions such as democratic structures and the welfare state, how trade relates to development, industrialisation and deindustrialisation, as well as the consequences of economic integration and disintegration for international relations and the probability of interstate conflict. Indeed, understanding the differences between globalisation of the past and more recent globalisation is also vitally important from a policy perspective. In many ways, the rhetorical attacks on current globalisation targeting individual countries reflect an outdated view of how international trade is now conducted. Recent work by Baldwin (2016) highlights how twenty-first-century globalisation based on information technology is different to that of the trade based on the international movement of goods in the nineteenth, or even late twentieth centuries. As a result, twentieth-century protectionist measures, such as tariffs, are likely to have unintended and unanticipated consequences. Without a knowledge of the history of globalisation, or indeed a proper understanding of how this relates to the present, policy mistakes can be repeated.

So how might students of international trade benefit from a knowledge of history? It is important for economics students to understand the history of trade for the same reasons it is important for policymakers: history gives important context. For example, an introductory course that presents the theory of comparative advantage and the mutual gains from trade might complement this with an analysis of the historical record on the distributions of the gains from trade, which are determined by the terms of trade. How were the gains of trade distributed in the late nineteenth century? Can this help us to understand the Great Divergence? Likewise, more advanced classes looking at Heckscher-Ohlin theory can use nineteenth-century trade as an illustrative example—as O'Rourke and Williamson (1994) point out, the commodity price convergence that Eli Heckscher and Bertil Ohlin observed for the late

nineteenth century was precisely what motivated their theoretical framework.

For research students, the opportunities for making a contribution to our understanding of historical trade are many and are often only limited by the ability to assemble and digitise existing data—values and quantities of trade have been one of the best recorded aspects of human interaction across millennia. In fact, the workhorse model of international trade—the gravity model—has even recently been used to locate the lost cities of Bronze Age Assyria (Barjamovic et al. 2017). This highlights, in a novel and interesting way, the potential for important complementarities between economics and history.

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10

Immigration and Labour Markets

Sebastian T. Braun

The number of migrants worldwide has more than tripled since 1975, reaching 258 million in 2016 (United Nations 2017; Zlotnick 1998). Today, about 3.5 per cent of the world's population are migrants. The migrant population has grown especially rapidly in Europe, North America and Australasia. In Europe, the population share of migrants increased from 2.7 per cent in 1975 to 10.5 per cent in 2017—a nearly fourfold increase over little more than 30 years. Over the same period, the share of migrants in the USA more than doubled to 15.3 per cent, reaching levels last seen at the beginning of the twentieth century. Other regions, such as Latin America, experienced much lower increases in migrant numbers or even a decline in the migrant population share (see Ferrie and Hatton 2015, for an overview of trends and developments in international migration in the last two centuries).

The increased importance of immigration in many developed countries has sparked a heated policy debate on the economic effects of immigration in immigrant-receiving countries.¹ These policy debates, along with the increased demographic importance of migrants, have also fuelled academic interest in the economic determinants and consequences of immigration. This chapter first summarises the core topics and debates in immigration economics, and then discusses how economic history can inform these debates.

¹ The economic costs and benefits for sending countries and migrants themselves have received less attention (Clemens 2011) but are not less important. For a synthesis of important lessons from the economics of *emigration*, see contributions to the *Journal of Economic Perspectives*, Vol. 25, No. 3 (Summer 2011).

Core Topics in the Economics of Immigration

Issues in the economics of immigration that have received particular attention include the decision to migrate; the integration of migrants into the economy of host countries; and the effect of immigration on native workers' economic outcomes. Much of the economics literature on immigration—and the policy issues they revolve around—focus on the labour market, which is also the focus of this chapter.

The *decision to migrate* is a natural starting point for studying the economics of immigration. Economists and other social scientists have long highlighted that international migration is a selective process. Some residents choose to leave their country of birth, whereas others choose to stay. One key question then is who moves from origin to destination—and how the selection process depends on the characteristics of origin and destination countries. The key insight of the literature's workhorse model, the Roy model, is that the selection of immigrants depends on the relative return to labour market skills in origin and destination countries (see Bansak et al. 2015; Borjas 2014, for textbook treatments; and Borjas 1987, 1991, for seminal contributions).

In particular, the Roy model predicts that migrants will have higher skills than residents who stay behind—and are thus positively selected in terms of their skills—if the destination country exhibits higher relative returns to skills and thus greater levels of income inequality than the origin country. In contrast, immigrants will be negatively selected if the destination country offers lower relative returns to skills and is therefore more equal than the origin. Numerous studies have tested the predictions of the Roy model, generally with mixed success (see, for instance, Belot and Hatton 2012; Brücker and Defoort 2009; Grogger and Hanson 2011). In fact, many studies find contemporary migration flows to be positively selected, regardless of the relative returns to skills in origin and destination.

The selection of immigrants has direct implications for their *economic integration in host economies*, which in turn is central for the economic costs and benefits of immigration for receiving countries. Starting with the seminal contribution of Chiswick (1978), a large literature in economics studies whether newly arrived immigrants fare worse than natives do—and, if so, whether they catch up over time (see, for instance, Card 2005; Lubotsky 2007; Borjas 2015 for recent contributions).² The answers to these questions depend on immigrants' pre-existing skills but also on their investment into

² A key challenge in identifying the rate of convergence between immigrants and natives is to separate convergence from cohort effects (Borjas 1985), that is, from changes in the average skill level between

new skills after arrival in the host country. The skills of immigrants not only affect their own labour market success in host countries, but also that of their children (Dustmann and Glitz 2011). Since a significant share of migrant children come from a more disadvantaged family background than their native peers, the persistence of disadvantages across generations is one of the central impediments to the successful integration of second generation migrants (see Black and Devereux 2011, for a survey of the growing literature on the persistence of economic status across generations).

The skill level of immigrants relative to that of natives also determines the effect of immigration on *wage and employment outcomes of native workers*—at least in theory. In a simple textbook model of a competitive labour market, immigration will shift out the supply curve if immigrants and natives have the same skills and are perfect substitutes. Consequently, immigration decreases wages of competing native workers. In contrast, immigration will increase native wages if immigrants and natives have different set of skills and are complements in production. Consequently, immigration increases wages of complementary native workers.

The most common empirical approach to test these predictions is to correlate native wages and immigration across regions of a country (see Dustmann et al. 2016, for a discussion of alternative empirical approaches and their interpretation). Studies using this so-called spatial correlation approach compare native earnings in areas with a high immigrant share to native earnings in areas with a low share—and often find little or no evidence for an adverse wage effect of immigration (see Kerr and Kerr 2011, for a survey). However, immigrants might self-select into high-wage areas, severely complicating the identification of the true effect of immigration on wages in spatial correlation studies (see, for instance, Dustmann et al. 2008, for a discussion). A positive correlation between immigration and wages, for instance, might be evidence of a positive wage effect of immigration—but it might also indicate that migrants move into high-wage regions.

The Role of History for Current Debates in Immigration Economics

Migration is all but a new phenomenon. For thousands of years people have left their region of origin to improve their standards of living or to seek shelter from war and conflict. Can the analysis of historical migration episodes

successive immigrant cohorts. This requires the availability of longitudinal data that track individuals over time, or at least the availability of repeated cross-sectional data.

inform current debates in immigration economics—and, if so, how? At least three aspects come to mind: Economic history can highlight the importance of context for the applicability of economic models of immigration, provide a long-term perspective on immigration and serve as a laboratory for empirical studies in migration research.

First, historical episodes can provide the context for testing economic models of immigration. Abramitzky et al. (2012), for instance, study the selection of immigrants from Norway to the USA during the “Age of Mass Migration” (1850–1913). This is a particularly interesting episode to study migrant selection because the USA maintained a nearly open border for immigrants at the time. Migration decisions were thus unaffected by legal barriers, which govern much of contemporary migration flows. In line with the assumptions of a simple Roy model, the Age of Mass Migration thus allows researchers to test the selection of immigrants in the absence of selective migration policies. Abramitzky et al. (2012) show that Norwegian migrants to the USA were negatively selected from the sending population. This finding is consistent with the predictions of the Roy model, since income inequality was higher in Norway than in the USA during the period. Stolz and Baten (2012) come to similar conclusions. Using a comprehensive panel of 52 source countries and 5 destination countries, they show that the selectivity of migrants in the mid and late nineteenth century is in line with the predictions of the Roy model.³ Today’s often-restrictive immigration policies towards the low skilled could thus explain why the Roy model is less successful in predicting the selection of contemporary immigrant flows.

Second, a historical perspective allows us to study the long-term effects of immigration, whereas much of the existing literature on immigration has focused on the short-term effects of immigration. Clearly, short- and long-term effects could differ significantly in both magnitude and sign. Recent studies that offer such a long-term perspective include Hornung (2014) and Sequeira et al. (2017). Both studies document significant positive effects of immigration on host countries in the long run. Hornung (2014) analyses the long-term effects of Huguenot refugees to Prussia on the productivity of textile manufactories. He shows that Prussian towns with higher refugee shares

³ Measuring skills and inequality are key challenges for studying migrant selectivity in economic history. Stolz and Baten (2012), for instance, use the share of individuals who are able to report their exact age as a measure of skills and variation in human stature as a measure of inequality. More recently, Blum and Rei (2018) use height as a proxy for health and human capital when studying the selectivity of Jewish refugees from Europe to the USA between 1940 and 1942.

in 1700 experienced higher output and productivity levels in 1802. Sequeira et al. (2017) study the effect of immigration from Europe to the USA during the Age of Mass Migration on a wide range of contemporary economic outcomes. They find that counties with higher historical immigration are generally more prosperous today.

Bauer et al. (2013) take a long-term perspective on the economic integration of forced migrants, studying the economic integration of the eight million forced migrants from Central and East Europe who arrived in West Germany after World War II. They show that even a quarter of a century after displacement, first generation migrants performed significantly worse than native West Germans with similar socio-demographic characteristics. Differences in economic performance shrink, but are still visible, in the second generation.

Third, historical events can sometimes serve as a laboratory to study causal effects of immigration that are otherwise difficult to quantify. The literature on the labour market effects of immigration is a case in point. As discussed, immigrants typically self-select into booming labour markets, making it difficult to separate correlation from causation. Unique historical events can create regional variation in immigrant inflows that is independent of local economic conditions. Such “natural experiments of history” might clarify the direction of causality between immigrant inflows and native labour market outcomes (see Boustan et al. 2010; Braun and Mahmoud 2014, for recent examples).

The so-called Mariel Boatlift is one of the most famous examples of a natural experiment in immigration economics. On 20 April 1980, Fidel Castro announced that Cubans wanting to leave to the USA could do so from the port of Mariel. By the end of October 1980, 125,000 Cubans had reached Florida. The majority settled in Miami where the labour force increased—almost overnight—by seven per cent. Importantly, the inflow of Cubans to Miami was not driven by expected labour market opportunities, but by geographic proximity to the origin and the settlement of earlier Cuban migrants in Miami. Card (1990) uses the Mariel Boatlift as a natural experiment to estimate the effect of immigration on native wages and unemployment. He compares labour market conditions in Miami—relative to those of four similar cities that were unaffected by the inflow—before and after the Mariel Boatlift. He does not find any evidence for adverse labour market effects of immigration although his results are controversially discussed until today (see Borjas 2017; Peri and Yasenov 2015, for recent contributions).

Economics, History and Policy

Immigration economics addresses many of the questions that we encounter in today's heated policy debates: Do immigrants displace natives in the labour market—or do they improve labour market outcomes for native workers? How quickly do immigrants integrate into the labour market—and which policies can foster the integration process? Who chooses to migrate—and how do labour market conditions in source and origin countries affect the selection process? A key learning outcome from introducing economic history into a course in immigration economics is that none of these questions are new, and all were already relevant in the past. Comparing research findings from different migration episodes can teach students the importance of context for the applicability of economic models and, more generally, the relevance (and irrelevance) of economic history for today's immigration debates. Abramitzky and Boustan (2017) is an excellent starting point in this regard; they review side-by-side the historical and contemporary evidence on immigration into the USA and discuss how the direct comparison of different migration episodes in US history can shed light on current debates.

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11

Financial Institutions and Markets

Meeghan Rogers

A historical analysis of both financial institutions and markets can provide useful insights into how they function, and what can go wrong. For example, two recent winners of the Nobel Prize in Economics, Eugene Fama and Robert Shiller, both needed to use long-run data to derive their analysis of asset prices. And Ben Bernanke, the former Chairman of the Federal Reserve, examined the role of finance in recessions by exploring a particular historical setting, the Great Depression. By examining the characteristics and behaviours of institutions and markets historically we can more clearly understand market changes, design appropriate corporate policies and evaluate the efficacy of modern financial regulations. A knowledge of financial events in the past may be a necessary prerequisite to understanding current affairs. They may even help in preventing future financial crises.

The financial services sector plays a major role in the economy by making it possible for companies, households and governments to access capital. Corporations need to raise funds to develop new products, individuals need mortgages or credit to make large purchases and countries borrow rather than raise taxes immediately. The providers of capital, investors, can earn a return on their investments and savings, which can be used to provide a source of income or provide for retirement. Financial markets allow buyers and sellers of securities to trade with each other directly. Financial institutions act as intermediaries by channelling funds from the suppliers of funds to the users of these funds.

Financial history can be insightful for several reasons. It allows the testing of theories over time, to ensure that they are robust across different periods. This is a particularly important check for research into financial markets where “data snooping” can seem to suggest techniques to earn abnormal returns, but which are often only temporary. Examining particular periods in history can also be useful if they provide a natural experiment. The scope provided by historical data allows researchers to choose time periods that have the necessary conditions for their research. Financial history also provides evidence of the persistence of institutions. An institution exists in the set of (unwritten) rules governing human behaviour within a particular context. Financial institutions have existed for centuries. When analysing the importance of financial history, it is helpful to look at the major theories of financial economics individually and the initial research into each.

Asset Pricing

Asset pricing is a major area of finance, which looks at how much return investors can make from securities. Long-run historical analysis into the pricing of securities and other assets can indicate whether there are certain anomalies investors can make use of to earn a superior return. Three stock market factors—the excess market return, firm size and market-to-book ratio—have been found to have a substantial impact on the returns of common stocks (Fama and French 1993). Financial historians have been able to examine whether these anomalies existed in a number of early capital markets. When looking at the UK from 1870 to 1913, there exists little evidence to support the size anomaly, but there is weak evidence for the value anomaly (Grossman and Shore 2006). In pre-World War I Berlin, firm size and the earnings-to-price ratio are positively, albeit weakly, related to returns (Fohlin and Reinhold 2010).

Other long-run analyses can aid in understanding certain patterns that could be indicative of financial market crises and crashes. The Cyclically Adjusted Price-Earnings (CAPE) ratio for companies in the USA, dating back to 1871, was used to warn that investors were suffering from “irrational exuberance” before the dot-com crash (Shiller 2005). Other long-run time series of US home prices, dating back to 1890, were able to anticipate that property prices were too high before the Credit Crunch.

Financial history also provides a unique opportunities to test theories on asset price reversals, mainly bubbles, as these are somewhat rare events

which occur at different points in time. A bubble is where the price of an asset far exceeds its intrinsic value. Due to the nature of calculating intrinsic values, bubbles are usually identified retrospectively. Notable bubbles include the South Sea Bubble in 1720 (Dale et al. 2005), the Mississippi Bubble in 1720 (Garber 2001), the stock price bubble of 1927–1929 (Voth 2003), the Japanese real estate bubble in the 1980s (Barsky 2011) and the Nasdaq bull market bubble in the late 1990s (Pástor and Veronesi 2006).

The benefit of financial history is the time periods it provides to examine under what conditions certain stock market anomalies exist, if at all, and the behaviour of market participants prior to crises. Could stock market crashes be prevented or lessened by understanding the factors that influenced previous crashes? Long-run event studies could also be used to find how size and market-to-book ratio factors in returns are driven by the behaviour of earnings.

Corporate Finance

Certain aspects of corporate finance have garnered attention from researchers due to the puzzling questions that still exist regarding their policies. Should firms have a target dividend pay-out or an optimal capital structure? Are the current corporate governance mechanisms helping or hurting shareholder wealth? These questions are part of ongoing debates in corporate finance research.

Dividends are cash payments made by companies to their shareholders. In the 1940s and 1950s, almost 90 per cent of US stock market firms paid dividends (Fama and French 2001). By 1999, this number dropped to only 20 per cent. This change has largely been driven by the changing characteristics of firms that are listing on the stock market. There are now more small, low profitability, growth firms which are almost always more likely to pay little or no dividends. The disappearance of dividend-paying firms could be explained by an increase in firms without these specific characteristics. Dividends can also be used as a signal to investors (Baskin and Miranti 1997).

Agency theory suggests dividends are a means to control managerial behaviour. But in historical environments of low taxation and an absence of institutional constraints, there is little support for the agency theory of dividends. For UK firms between 1895 and 1905, there is little evidence to support the hypothesis that dividends signalled quality; dividend-paying firms did not have fewer information asymmetries between insiders and shareholders (Braggion and Moore 2011).

The ratio of the amount of debt to equity that a firm uses is referred to as its capital structure and is another major area of corporate finance research in which financial historians have the potential to make an impact. Two theories of capital structure are often pitted against one another. The first, static trade-off theory argues that there is an optimal capital structure that firms should aim to maintain, and avoid deviating from. In contrast, the second, pecking order theory, argues that firms will prefer internal financing, and if external financing is required firms should issue debt first, with equity being issued only as a last resort (Myers and Nicholas 1984). There has been a wide body of research evaluating these theories, but there is still no definitive conclusion as to how companies use debt and at what times.

Over the past 100 years, the use of corporate debt has changed greatly. In 1946, the median unregulated firm had no debt, but by 1970 had a leverage ratio of 31 per cent (Graham et al. 2015). Changes in government borrowing, economic uncertainty and financial sector development were important factors in this change. More recently, the capital structure of European firms has varied substantially, contrary to the predictions of the static trade-off theory (Campbell and Rogers 2018). Companies that have a significant amount of debt volatility tend to be smaller and less profitable.

In the majority of large corporations, the suppliers of finance—bondholders and shareholders—do not manage the firm. This can create an agency problem in which the agent (manager) does not act in the best interest of the principal (owner). A major part of corporate governance research examines ways to limit corporations' exposure to agency problems. Historical research into corporate governance mechanisms, such as economic and legal instructions, can help to examine what past reforms proved useful, or harmful, to the shareholders of a corporation. Examining remuneration of CEOs and other executives of corporations is a perennial topic of financial research interest. History provides researchers with many interesting case studies of corporate governance successes (e.g. Chandler 1992), and especially also failures (e.g. Colvin 2014). Although much attention has pointed to top executives earning exorbitant salaries and bonuses, prior to the 1990s, this was not the case. From 1930s to the late 1980s, CEOs' salaries did not rise in real terms (Jensen and Murphy 1990). And since the 1990s, the story has changed and there has been much criticism of executive pay and its association with accounting scandals and financial crises.

Shareholders may be able to ensure managers act in their best interests by using legal rights. Ultimately, the most important legal right is the right to vote. Shareholders can also choose the directors they want in annual elections

(Shleifer and Vishny 1997). Executive pay is a crucial aspect in limiting agency problem. A well-designed remuneration package may attract and retain the right executives while also motivating executives to take actions that increase shareholder value. Rather than having inside members of the board set executive pay, outside directors could carry out this task (Fama and Jensen 1983).

Within corporate finance, history is needed to understand the changes in policies by finance managers. Are there particular time periods and conditions when firms should pay out dividends? Capital structure is still a puzzle and there are opposing arguments if there should be a stable debt level. Are there certain conditions that caused changes in capital structure that can be examined in the past and applied to today? Understanding how corporate governance policies can be both a solution and source of agency problems could help to ensure managers are acting in the best interest of shareholders. The use of “natural” historical experiments will help to answer these questions.

Banking and Growth

Banks have acted as both engines and destabilisers of growth across history. The banking system is of critical importance to technological innovation and economic growth. Credit from banks is the cornerstone to innovation and new enterprises (Schumpeter 2012). Entrepreneurs need money for their investments and banks provide this in the form of credit. Corporations borrow money from banks, for example, to further invest in research and development and technological innovation. Banks also exist because they are pools of liquidity. They provide savers, who experience shocks, with funds to ease their consumption needs. Banks are able to reduce the cost of collecting and processing information about investments, which helps with duplication and freeriding (Diamond 1984). Banks can also lower liquidity risk (Diamond and Dybvig 1983) and diversify systematic risks (Allen and Gale 2000).

Historically, industrialising countries were more likely to be interested in large-scale industrialisation with already existing technologies. Gerschenkron (1962) famously posited that this type of industrialisation would be heavily dependent on banks. Thus, those countries that were first to industrialise would instead rely on the stock exchanges for capital. With the benefit of long-run historical analysis, Gerschenkron compared Britain’s industrialisation with that of France, Germany and Russia and identified certain attributes that were present in financial systems that missed the Industrial Revolution, versus those that were industrial leaders.

Both a well-functioning financial market and banking system are beneficial to economic growth. Stock market liquidity and the level of banking development are positively and significantly correlated with economic growth (Levine and Zervos 1998). However, banks provide services that stock markets cannot. Whether a financial system is market or bank based cannot explain economic development. However, the development of financial institutions and the stock market can explain changes in economic development (Beck 2003).

While they provide a necessary function in the economy, the failure of banks can cause catastrophic events. The cause of many crises is due to the pro-cyclical changes in the supply of credit. The credit supply will increase dramatically in positive periods, and will decline drastically in economic downturns (Kindleberger 2005).

Monetary and banking history are interrelated fields. The analysis of Friedman and Schwartz (1963), which takes in nearly 100 years of monetary history, highlights periods where the money supply did not move in line with macroeconomic conditions. Monetary errors by the Federal Reserve precipitated the Great Depression (Friedman and Schwartz 1963). There is a relationship between bank failures and the costs of credit intermediation after the financial crisis of the 1930s, which can explain the atypical length and scope of the Great Depression (Bernanke 1983).

Financial history is extremely useful to the understanding of banking crises. The historical details of each are crucial to understanding why these crises occurred and when they could happen again. With the large quantity of research into the Great Depression, Ben Bernanke and others were able to use history to help resolve the 2007 crisis; the US government's intervention of the near-failure of Bear Sterns and AIG came from learning historical lessons. However, it is also the responsibility of economic historians not to abuse the use of history in debates today and oversimplify the comparison between crises such as that of the Great Depression and the 2007–2008 recession.

Conclusion

Long-run and historical analysis can provide insights into a wide range of financial puzzles. Financial institutions and markets have been in existence, in some form, for several centuries. They have undergone radical changes and withstood severe crises. From these experiences, there are important lessons for the present and future. The role of the financial sector in the overall

economy and the ever-growing globalisation of financial markets throughout the world are motivation for continued research into this field.

Financial history should be a part of both research and the curriculum of classes to provide future students with the understanding of what has happened in the past and its relation to today. Teaching business and economics students about past crises, for instance, will produce better employees in the financial world, who are aware of events that have a tremendous relevance to current affairs. Including corporate governance topics in a business ethics course, bringing historical asset pricing analysis into an investments course and discussing the history of financial market crises and bank failures in a financial institutions and markets class are necessary for students to understand the historical context in which financial decisions are made today.

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12

Financial Crises and Bubbles

William Quinn

For many economic subjects, history is primarily used to provide new perspectives, out-of-sample data and natural experiments on existing theory. However, since financial crises and bubbles are rare events, modern empirical evidence on the subject is usually very limited. Economic history has therefore been fundamental to the development of the field, with much of our understanding based on insights from historical episodes. The recent revival of economic history owes much to the 2007/2008 crisis, which demonstrated the continued relevance of this area of research.

A complication of this field is that neither “financial crisis” nor “bubble” has universally agreed-upon definitions. Financial crises are typically associated with bank failures, reduced value of financial assets and a sudden fall in access to credit that leads to a recession. But whether a particular episode is significant enough to constitute a crisis is often subjective. The word bubble usually refers to a significant rise and fall in asset prices with no obvious fundamentals-based explanation. Despite the label being common among the general public, some economists consider it too vague for scholarly research (O’Hara 2008). But its use in existing historical literature renders this impractical for the field of economic history, and economic historians sceptical towards the existence of bubbles as a recurring phenomenon still find it necessary to engage with the term (Garber 1990).

Crises in the Mirror of the Great Depression

Histories of financial crises traditionally address questions about individual incidents. The intention is not necessarily to provide general economic insights; the focus is rather on the historical value of documenting events with major social, economic and political consequences. As in other areas of economic history, early research tended to take the form of narrative accounts, with data used to provide supporting stylised facts (Galbraith 1955). Later research introduced cliometric methods, but the questions being addressed were still usually specific and historical: “what caused X?” rather than “what causes X?”

The most widely researched such case study has been the Great Depression, especially in the US, where the failure of the banking sector was most profound. Few Western financial crises have resulted in comparable economic damage: between 1929 and 1932, US GDP fell by 16.7 per cent, and the unemployment rate rose to 31.4 per cent, to say nothing of its political repercussions (Crafts and Fearon 2010). In comparison, the 2007/2008 crash was associated with “only” a 3.2 per cent fall in GDP and peak unemployment of 10 per cent (IMF).

Research into the Great Depression has largely focused on two questions. Firstly, what was its cause? An early hypothesis was put forward by Friedman and Schwartz (1963), who attributed it to a sudden reduction in the money supply. This was exacerbated by Federal Reserve policy, which at that time tended to be pro-cyclical. The money supply would have been affected directly by the decision to raise the discount rate in October 1931, but the more significant factor may have been allowing so many banks to fail, which would have reduced the money multiplier.

A problem with this hypothesis was highlighted by Hamilton (1987), who noted that a monetary contraction is inconsistent with the rapidly falling short-term risk-free interest rates after 1929. Temin (1976) argued that a more plausible alternative was a drop in demand, which Friedman and Schwartz implicitly assumed had remained constant. This would be consistent with Temin’s (1975) data on industrial production and income, but posed the question of what might have caused a sudden drop in demand. The obvious candidate is the wealth effect of stock market losses, but Temin finds these to have been relatively small. According to Romer (1990), the more significant effect of the Wall Street Crash was the increased uncertainty that it signalled, which led many industries to cut production.

More recently, scholars have argued that the monetary explanation of the crisis could still hold if investors had anticipated deflation, in which case real interest rates could rise while nominal rates fall (Cecchetti 1992). However, this anticipated deflation would also need to have been caused by Federal Reserve policy for the monetary hypothesis to hold. While Romer and Romer (2013) find some evidence for this in the financial press, it is unclear how representative such views were. There is thus no established consensus on whether the Depression was triggered by the Federal Reserve, the Wall Street Crash or something else entirely.

The second research question concerns the remarkable severity of the crisis and its international propagation, which was much too significant to be explained by its immediate cause. Bernanke (1983) argues that the severity stemmed from deflationary pressures causing a cascade of banking failures, which left many businesses unable to obtain essential credit. Since several European countries, most notably Germany, depended on American credit, this also caused the crisis to spread across Europe, particularly after Austria's largest bank collapsed in 1931 (Temin 2008). The political response was hamstrung by adherence to the gold standard, which often left governments trying to resolve balance of payments deficits by decreasing wages (Eichengreen and Temin 2000). Whether the Federal Reserve's actions sparked the crisis or not, the political and monetary responses were clearly inadequate. Bernanke's response to the 2007/2008 crisis, which focused on sustaining credit, was based largely on the conclusions of this literature.

By focusing excessively on the Great Depression, however, one risks missing valuable insights from elsewhere, and less well-known financial crises have been used to provide counter-examples that pose challenges to existing theories. For example, proponents of "Free Banking" often assert that a *laissez-faire* approach to banking would prevent crises, with the instability of financial systems attributed to central banking, deposit insurance and the expectation of government bailouts (Dowd 1996). But Hickson and Turner (2002) find that this view is contradicted by the experience of Australia in the nineteenth century, when a free banking system experienced a severe financial crisis. Absent regulation, Australian banks converted to limited liability institutions to limit their downside risk. They then invested heavily in speculative assets, driving a boom in the market for land. When the land market collapsed, so did many banks, tipping the economy into a severe depression. This foreshadowed, with a remarkable degree of precision, the development of US banks after the deregulation of the 1980s and 1990s.

Financial Panics and Speculative Bubbles

The study of financial bubbles has, until recently, been dominated by economic historians. The formative modern text was Charles Kindleberger's (1978) *Manias, Panics and Crashes*, which provided both a rough framework for cyclical bubbles and a series of historical examples. Since this work largely relied on qualitative sources, an alternative literature emerged that used historical data to explore the causes of these bubbles. This work tended to play down the "irrational" or "mania" element of these events, citing instead the role of informed speculation or "bubble riding", political interference or ex-ante uncertainty (Garber 1990; Neal 1990; Campbell 2012). Others, most notably Robert Shiller, have continued to stress the role of irrationality, arguing that the presence of financial bubbles represents a critique of the efficient markets hypothesis. But the conflict between these two strands of literature is easily overstated: the majority of the "precipitating factors" of bubbles noted by Shiller (2015) are consistent with most of the rational-bubbles literature.

A classic case study in the field of financial bubbles is the Dutch "Tulipmania" of the 1630s, which now provides a boilerplate introduction for any journalist suggesting that the price of an asset might be too high. Charles Mackay's (1852) polemic on *Extraordinary Popular Delusions* is often cited, the supposed lesson being the mundane observation that "markets can sometimes go mad". But most recent studies of the Tulipmania suggest that prices were driven primarily by the Dutch political economy; the price of tulip futures rose when it appeared likely that a new law would allow them to be converted into options, which would allow the buyer to default for a relatively small fee (Thompson 2007).

In a sense, however, literature on the Tulipmania exposes a key limitation of the case study approach: it can be easy to overextend an argument based on one data point. Garber (1990) concluded his history of the Tulipmania with a brief application of the rational-pricing framework to the Mississippi and South Sea Bubbles, implying that the facts of the three episodes supported the Fama (1990) view that markets are broadly efficient and speculative bubbles may not even exist. But more detailed quantitative histories of the Mississippi and South Sea Bubbles have concluded that the assets involved were far from accurately priced (Neal 1990; Velde 2009). This does not imply that the root cause was a spontaneous "madness": the Mississippi Bubble can largely be attributed to the skilful market manipulation of John Law, whereas the South Sea scheme was deliberately engineered by the British government to change the terms of its most unaffordable debt (Velde 2009; Dickson 1967). But nor

does this research support Garber's implication that the bubbles were essentially mythical.

Other recent research has documented suspected historical financial bubbles that had attracted little previous work in order to expand the available evidence on the subject. Campbell (2012, 2013) and Campbell and Turner (2012) use new data to examine the British Railway Mania of 1845, finding that financial leverage played a significant role in driving stock prices and that investors who invested in railway shares were surprisingly experienced. My own research looks at the Bicycle Mania of 1896–1898 (Quinn 2018) and finds that technology-driven speculation is a more likely explanation for stock price movements than the efficient markets perspective of Pástor and Veronesi (2009). This area has a lot of future research potential; there has been, for example, remarkably little research on the Poseidon Bubble of 1969–1970.

The Data-Driven Approach

A notable recent trend in the history of both financial crises and bubbles has been to use large datasets to identify patterns across space and time. The most popular such study was Reinhart and Rogoff's (2009) *This Time is Different*, which contributed a global dataset of financial crises and sovereign defaults extending back to 1350. The central theme of this study was not new—the tendency of the public to repeatedly forget about the possibility of a financial crisis was commented upon by investor journals in the nineteenth century (*Money: A Journal of Business and Finance* 1896: 759). But the study demonstrated the remarkable persistence of certain features of sovereign defaults, such as the tendency for one default to be followed by several more. This type of insight could not be generated using a case study analysis, nor could it be generated from a financial dataset consisting solely of modern data.

Several studies have used similarly long-term datasets to identify the causes of financial crises and bubbles and investigate what determines the severity of their economic consequences. Schularick and Taylor (2012) have studied financial crises since 1870 across 14 countries, finding that the likelihood of a crash is greatly increased by loose credit. Jordá et al. (2013) note that recessions following credit expansions, as well as being more frequent, are much deeper. This is consistent with the findings of Eichengreen and Mitchener (2003) regarding the Great Depression, showing how the use of large datasets can demonstrate the general applicability of hypotheses generated from studies into specific incidents.

Goetzmann (2015) has undertaken a similar task for the study of stock market bubbles, using a dataset of 21 countries between 1900 and 2014. The main insight generated is that the likelihood of a crash is increased only slightly by a preceding boom, indicating that regulators should be wary of intervening when they feel that the stock market is overvalued. Again, this is consistent with previous case studies: Voth (2003) argues that the German central bank's pricking of a non-existent bubble in the 1920s precipitated a recession.

Other recent work falls somewhere between the two approaches, synthesising research on various case studies across long periods of time to reach more general conclusions. Turner (2014) uses this approach to examine British banking crises since 1800, building the argument that the limited liability structure of current banks is a root cause of excessive risk-taking. Perez (2002) takes a similar approach to the history of technologically driven financial bubbles, developing a model in which technological booms are often followed by more damaging credit booms. Such a methodology is necessarily partly descriptive and may be unconvincing to those who prefer hypotheses to be robustly tested against quantitative data. But dealing with rare events means dealing with relatively small datasets, and the gains from understanding the historical context of each event will often outweigh the loss of statistical power.

Conclusion

The traditional approach to studying historical financial crises and bubbles has recently been complemented by studies applying econometric techniques to long-term data on the subject. While such studies are very valuable, care must be taken to ensure that important historical context is not disregarded by the reduction of each individual episode to a data point. An interesting avenue for future research might also be to use the data-based approach to motivate deeper analysis of particular cases. Large datasets are excellent for producing general insights that can be broadly applied, but it is often the outliers that can teach us the most about appropriate policy responses. Why were some credit booms not followed by a financial crash? How come a small number of economies have managed to recover relatively well from a wave of banking failures? By answering these and other such questions, the history of economic and financial crises can continue to produce research with substantial contemporary relevance.

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13

Sovereign Debt and State Financing

Larry D. Neal

A perennial topic for discussion in economics classes is why the management of the national debt of one's country does not correspond to the rules for management of the personal debt of individuals, households, or business enterprises. The topic is perennial due to the constant use of the debt analogy by populist politicians railing against any government programme, other than war, that is financed by new issues of debt. The usual explanation in economics textbooks is that only the central government in today's world has a monopoly over the national money supply, as well as power to collect taxes. Modern monetary and fiscal arrangements mean that a national government can always pay the interest promised to creditors on its sovereign debt, either by raising the necessary taxes or printing the necessary money, options not available to ordinary citizens and businesses or even to local governments.

Of course, if politicians find it congenial to pledge “no new taxes” or “Whip Inflation Now”, these economic solutions may run afoul of political pressures. Moreover, economists, trained to look at the opportunity costs of any policy, note that increasing the money supply may well lead to inflation, which in turn will decrease the value of existing taxes and create an unsustainable economic and political situation for any government that keeps increasing its sovereign debt without attention to maintaining both its tax base and the purchasing power of its currency. Economic historians, therefore, find it useful to explore past episodes of unusually large expansions of sovereign debt to see what were the economic consequences when taxes were raised or not—and when the money supply was increased or not.

The Big Bang of State Finance in 1720

The earliest examples of efforts by central governments to find new ways to deal with the apparent limitations on their ability to finance renewed war expenditures are found in early modern Europe, especially the Mississippi Bubble in France (1719–1720) and the South Sea Bubble in England (1720). The publicity that accompanied both these episodes—which led to spectacular increases in nominal asset values of shares in the Mississippi company traded in Paris, and then with a brief lag, in the nominal values of shares in the South Sea Company traded in London—provided material for political purposes ever since, precisely because both experiments crashed spectacularly by the autumn of 1720.

Historians were the first to highlight the policy implications for modern government from these two episodes, Edgar Faure (1977) for France and Peter Dickson (1967) for England. Each work stimulated deeper analysis of the underlying processes by economists struck by the modernity of the arguments used at the time to support the respective innovations, work that continues as more sources of data are found, transcribed, and analysed.¹ Briefly, these studies find that excess money supply drove the Mississippi bubble in France, with adverse long-run consequences for the financial development of France and its royal government. John Law's fiscal and monetary reforms having failed, French financing of future wars of the eighteenth century (including the American War of Independence!) followed its seventeenth century pattern of episodic surges of direct taxes followed by government defaults on its outstanding debt.² The South Sea bubble was not financed by either new taxes or by excess money supply. When it also collapsed, the government's monopoly bank, the Bank of England, was able to increase its control over the value of the British pound sterling and to aid the further financing of the subsequent wars of the eighteenth century, exception made for the American War of Independence, of course!

The various experiments of the leading mercantile powers in Europe in finding ways to finance their recurring wars can be seen as efforts to test the limits of the trilemma for open macroeconomic policy—the difficulty of

¹ For France, especially the work of Antoin E. Murphy (1986, 1997, 2018) and Francois Velde (2009); for England, especially the work of Larry Neal (1990, 2012) and Anne Murphy (2009).

² John Law was the son of a Scottish goldsmith banker, who witnessed the early stages of monetary and fiscal reform in London in the 1690s, when the Bank of England was founded. He spent his adult life advising other governments how to reform their finances. His efforts culminated in France, where the regent ruling that country in 1715 turned to Law for advice. His system, based on creating a royal bank and replacing government debt with shares in a company with monopoly rights over all foreign trade, most taxes and the mints, briefly enjoyed spectacular success. The system eventually collapsed and Law had to flee France for his life. See Murphy (1997) and Neal (2012).

maintaining simultaneously the policy goals of fixed exchange rates, independent monetary policy, and open capital markets (Neal 2000). The defining test came in 1797 when the Bank of England suspended convertibility of its bank notes into silver or gold in an effort to assert monetary independence. Nevertheless, it did maintain open capital markets, if only to encourage flight capital from Napoleon's conquests on the European continent. To the surprise of policymakers and economists of the time, including David Ricardo, the experiment, which lasted until the gradual resumption of the gold standard in 1819–1821, proved incredibly successful. Just why that was the case began research that continues today (Antipa 2016; Hotson 2017) in order to determine the underlying causes of success of the British experiment, with an effort to apply the lessons of history to current policy issues.

Key to that success was careful management by both the monetary and fiscal authorities to maintain the credibility of stable prices for British sovereign debt, while issuing huge new amounts to finance the repeated wars against Napoleon. Their efforts were constantly tested in the open access and well-publicised London Stock Exchange, formally created in 1801. Prices of British sovereign debt also mimicked the gold price of the floating British pound (Antipa 2016), which provided independent confirmation that “basis risk”, the measure of credibility of a financial asset to maintain its value as collateral, was responsive to interventions by the British authorities (Hotson 2017).

First Mover Advantage?

Meanwhile, Alexander Hamilton's system established in the United States in 1791 had burdened that fledgling nation with an unprecedented amount of sovereign debt, the result of assuming the debts accumulated by individual states during the previous decades.³ But he also established a public bank, the First Bank of the United States, to help manage the payment of interest and redemption of the federal debt. Most important, he unified the external tariff system of the country and maintained an independent body of Federal officers to collect the customs revenues at each port of the United States (something never done by the European Union, by the way). He also established branch offices of the First Bank throughout the individual states. Putting the new US dollar on the same bi-metallic basis as the prevalent coin used for foreign transactions, the Mexican peseta, Hamilton ensured that the US Treasury thereafter could monitor constantly the “basis risk” of its currency and of its sovereign debt.

³Alexander Hamilton was one of the architects of the US constitution, and was responsible for its financial policy for a large part of George Washington's presidency.

The conclusion of the Napoleonic Wars saw widespread attempts to imitate the successful British and American examples, leading to many similar experiments conducted in the management of sovereign debt by a wider and wider variety of countries over the course of the nineteenth century. Successive issues of sovereign debt were duly listed on stock exchanges in London, Paris, Amsterdam, and in the major cities of Germany, providing economic historians today material for testing many variants of experiments made by monetary and fiscal authorities to stabilise, or exploit to the limit, the values of their sovereign debts. Former colonies of the Spanish empire were the first to attempt the felicitous example of Alexander Hamilton's system in the United States, experiments that varied in outcome depending on local arrangements (Marichal 1989), but were followed later by Greece, then parts of the Ottoman Empire, and Egypt (Tunçer 2015).

The multitude of experiments with state financing by issuing sovereign debt, and the subsequent efforts to manage its basis risk, have all been faithfully recorded in the price lists maintained by stock exchanges world-wide. These have been digitised and transcribed into computer data files by modern economic historians. Examples include the *European State Finance Database* and the *Global Price and Income History Group*.⁴ These datasets, and others like them, are constantly expanding for use by economists motivated to learn how sovereign bonds may be made credible for use as collateral in today's global financial markets. In particular, some of these experiments with restoring the credibility of sovereign bonds that default—especially for Greece, Egypt, and the Ottoman Empire—could be useful for designing a programme to restore the credibility of the sovereign bonds of Greece, Italy, Portugal, and Spain after their collapse in 2010 (Esteves and Tunçer 2016).

How Did It All Begin?

Indeed, the history of sovereign debts and state finance goes back to the early history of precisely those countries now at the centre of difficulties in management of the sovereign bonds of the Eurozone. Spain's history of repeated bankruptcies in the sixteenth century has been the subject of study by two competing teams of financial historians, one focused on the re-financing of short-term debt of the king by conversions into long-term bonds (Drelichman and Voth 2014), and the other focused on the re-financing of long-term debt

⁴For the *European State Finance Database*, see: <http://www.esfdb.org>. For the *Global Price and Income History Group*, see: <http://gpih.ucdavis.edu>.

of the cities of Castile pledged to the king (Alvarez-Nogal and Chamley 2014). Both teams of scholars highlight the role of excise taxes collected by municipal authorities of the cities within the kingdom of Castile for guaranteeing the stability of the long-term sovereign debt (*juros*) issued jointly by them for the benefit of the king. They part ways, however, on whether the price stability of those bonds was sustainable in the long run, given the increasing political resistance of the political authorities of the Castilian cities to the pressures of the Spanish monarch, who was determined to maintain control over the first truly global empire. Drelichman and Voth believe yes, the *juros* would have been sustainable if it had not been for the repeated military defeats suffered by Philip II, especially the loss of the Spanish Armada in 1588.

The Drelichman-Voth argument touts the cleverness of the Genoese bankers who were willing, and able, to take on more short-term debt in the form of *asientos* that committed various royal revenues, especially silver imports from the Americas, to the Genoese as guarantees. Further, if the revenues did not materialise for whatever reason, the Genoese could cease making the loan and cover their previous remittances by selling securities the king had given them as collateral, the *juros de resguardo*. In contrast, Alvarez-Nogal and Chamley argue that all this was based on the long-term willingness of the city-states of Castile to keep raising their city taxes to service expanded issues of *juros*. The argument between the two teams became overheated to the extent of a vituperative exchange published in the *Economic History Review* of August 2016, with each team claiming it had been grievously misrepresented by the other (Alvarez-Nogal and Chamley 2016; Drelichman and Voth 2016). Both sides, however, acknowledge the validity of the data collected by Drelichman and Voth.⁵ The exchange, while informative on many levels about the intricacies of the constraints on early sovereign debt issued by an early conglomerate empire without a central tax or monetary system, manages to overlook one innovation by the Genoese bankers that proved to resonate during the financial crisis of 2007–2008.

This was to leverage that collateral of *juros de resguardo* given to the Genoese as security to be sold only if the taxes from the *asiento* failed to appear. In fact, however, the Genoese leveraged their loans by selling to third parties the *juros de resguardo* immediately, gambling that if they had to return the bond eventually, they would have profited in the meantime from the further investments made with the proceeds of re-hypothecating the bonds initially. And so the re-hypothecation of sovereign bonds used for “repo” loans that led to the calamities of the Great Financial Crisis of 2007–2008, it turns out, had been

⁵The Drelichman-Voth data on Castile’s Fiscal Position, 1566–1596, are available to download at: <https://economics.ubc.ca/faculty-and-staff/mauricio-drelichman/>.

initiated already in the sixteenth century! Leveraging in this manner proved to have its limits, of course, when taken to new levels by Philip IV in the course of his attempts to finance the recapture of the Dutch Republic during the Thirty Years' War. But it did prove sustainable for the prosperity of the Genoese banking families well into the nineteenth century.

Moreover, one of the secrets of success for the sovereign debt of Great Britain, once consolidated into the Three Per Cent Consolidated bonds (Consols) in 1752, was that they could be re-hypothecated at will by Dutch merchant bankers in Amsterdam. The successive experiments pursued by the leading capitalist countries thereafter can be followed in Neal (2015).

Monitoring Government Commitments

So, how can economics educators and their students tell whether the rants of politicians against government debt are merited, or whether the promises of other politicians to expand government debt are credible? Briefly, try to observe now the same variables that economic historians analyse when they look at past experiments.⁶ Are the prices of the bonds, both long term and short term, issued by the government remaining stable and in line with the prices of similar bonds issued by other governments? Is the value of the unit of account for the state debt holding steady against the currencies of major trading partners? These are the market indicators of the government's commitment to sound fiscal policy and sound monetary policy. As one policy advisor in the United States famously remarked, 'In my next life, I want to come back as the bond market!' Economic historians do have, however, an important advantage over observers of current events—the benefit of hindsight!

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14

Health and Development

Vellore Arthi

Like development economics, economic history is a field with a discipline-wide remit. Although the work in these fields reaches into all corners of broader economic inquiry, both development and economic history are fundamentally concerned with understanding how to raise living standards and foster economic growth. Naturally, then, the two fields share many common themes—chief among them, the causes and consequences of human capital accumulation.¹

Issues related to human capital formation are of particular importance because interventions on this front can have a direct impact on living standards and individual wellbeing, while also contributing to wider patterns of growth, for instance, through changes in labour productivity and family size. Here, historical evidence can be especially valuable to understanding how human capital is formed, and how it contributes to development. This is because by turning to history, we can often exploit setting-specific features that may not always be available in a modern empirical context—for instance, long time horizons over which to examine long-run and intergenerational effects; large-scale, public-access matched microdata with which to study individuals and cohorts over time; or even specific natural experiments to aid in identification. In this chapter, the focus is on what economic history can tell us about one dimension of human capital in particular: health.

¹ Broadly defined, this encompasses both education and health.

Development Priorities, History, and Health Capital Formation

The United Nations' Sustainable Development Goals (SDGs), adopted in 2015, set out 17 development priorities to guide the efforts of the international development community in the coming decades. At least four of these—namely those focused on health promotion, hunger eradication, poverty alleviation, and access to clean water—are directly linked to issues of individual-level and population health. Yet others, such as those concerning climate change, clean energy, and gender equity, also have implications for global health. On these contemporary policy questions, and the academic debates underlying them, new work in economic history has much to offer.

A recent and extensive review by Cutler et al. (2006) makes these parallels explicit. In it, the authors outline the relative importance of nutrition, public health measures, medical care, and other key factors in the historical decline of mortality over the nineteenth century and compare these patterns with the factors that have been crucial to raising life expectancy and reducing mortality in modern developing countries. In both settings, they suggest that the bulk of the credit for these improvements lies with macro public health interventions (i.e., large-scale public works projects, including sanitation infrastructure, mass vaccination, and milk pasteurisation). These in turn require state capacity and political will, 'neither of which is an automatic consequence of rising incomes' (p. 116).

It is a problem, then, that many low-income settings lack a robust public health infrastructure and that access to quality health care can be highly unequal. In these settings, measures that implement known health technologies—and those that extend access to underserved populations—can have a large and beneficial impact on health. Here, evidence on specific historical public health interventions can be especially illustrative. Below, I provide a brief discussion of this evidence as it relates to three key areas of contemporary policy focus.

Water, Sanitation, and Hygiene

Systematically improving water, sanitation, and hygiene is an objective which requires considerable political will to achieve, but in which coordinated efforts can yield especially high returns. For instance, Cutler and Miller (2005) emphasise the importance of disinfection and sanitation efforts alongside investments in water supply infrastructure in the US in the twentieth century.

Extending this finding, Alsan and Goldin (2018) document complementarities between clean water and sewerage infrastructure in late nineteenth- and early twentieth-century Boston and suggest that piecemeal sanitation interventions will be less successful than multidimensional ones in reducing infant and child mortality. What is more, interventions on this front can have positive consequences not only for survival in the short term but also for long-term health and human capital acquisition (Beach et al. 2016). This is because the eradication of waterborne disease raises the returns to human capital investment, not only by raising life expectancy but also by reducing the chronic disease burden—the latter of which is important both because childhood health and education are thought to be complements in the production of adult human capital (Bleakley 2007) and because early-childhood diarrhoea is associated with poorer cognitive function.

Maternal and Child Health

Because waterborne diseases are a major cause of both infant mortality and ill health in children under five years of age, sanitation measures such as those discussed earlier go a long way in addressing children's wellbeing. However, there are a number of other interventions that have been shown to improve children's short- and long-run health outcomes.² Chief among these are large-scale vaccination campaigns and other medical innovations to fight infectious disease.

For instance, following the advent of the smallpox vaccine in the early 1800s, Ager et al. (2018) find evidence that infant mortality in Sweden fell, and that these mortality reductions were accompanied by a decline in gross fertility attributable to changes in the relative costs of child quality versus quantity. Apart from immediate changes in infant survival and childhood morbidity, these findings are important to questions of human capital formation and economic development because both net reductions in fertility and higher stocks of health capital would likely serve to raise subsequent investments in health and other forms of human capital. Illustrating this point, tuberculosis vaccination campaigns in 1940s Norway raised educational attainment in adulthood by narrowing early-life health inequalities (Bütikofer and Salvanes 2015).

² Although the focus here is on large-scale public health interventions, there is also a robust historical literature on child nutrition and stunting. See, for example, Steckel (1995) on stature and Feyrer et al. (2017) on micronutrient supplementation.

Similarly, the introduction of cost-effective medicines such as sulfa drugs in the 1930s, used in the treatment of infectious diseases including pneumonia, spurred human capital acquisition. Consistent with the growing literature on the importance of early-life health to long-term health and labour market outcomes, Bhalotra and Venkataramani (2015) find that the drug's introduction in the US had a positive impact on the later-life wellbeing of cohorts exposed to the intervention in utero. These effects were driven by reductions in maternal morbidity and mortality, factors which can also serve to raise the educational attainment of women. This is because a reduction in women's mortality risk serves to increase their longevity and, so, raise the returns to other investments in human capital (Jayachandran and Lleras-Muney 2009).

Because of large disparities in access to pre- and peri-natal care—for instance, along race, class, and rural-urban lines—interventions that extend access to underserved populations can also have high returns.³ For instance, with the introduction in the 1960s of the public insurance programme, Medicaid, low-income Americans gained access to more and better medical care. This resulted in sharp reductions in infant mortality rates, concentrated amongst non-whites and attributable to improved care in the first few hours after birth (Goodman-Bacon 2018). Moehling and Thomasson (2014) document a similar effect of 1920s-era programmes offering postnatal home nurse visits.

Climate and the Environment

In light of growing concerns over environmental pollution in rapidly developing economies, and the impact of climate change on health and food security more broadly, a long-run perspective drawing on the experience of early industrial economies is especially valuable.

Extreme weather can have direct effects on health, as well as indirect ones—for instance, through poor agricultural yields. These effects can be immediate, as in the mortality documented as a result of high-temperature days in the early twentieth-century US (Barreca et al. 2016), or longer-term, as in the later-life disability outcomes found as a result of early-life exposure to the droughts and dust storms of the 1930s US (Arthi 2018). However, this adverse relationship has not been static. Barreca et al. (2016) show that the

³ Conversely, social exclusion and discrimination in access to health care can lead to mistrust in the medical establishment, discouraging patients to seek treatment, and reinforcing existing health inequalities (Alsan and Wanamaker 2018).

widespread diffusion of residential cooling technologies explains much of the precipitous decline in hot-weather mortality in the US from the 1960s onwards. Meanwhile, Burgess and Donaldson (2010) find that by promoting openness to trade and market integration, the expansion of railway infrastructure in colonial India served to mute the mortality response to weather-related agricultural shocks. These findings have particular modern resonance in the face of both globalisation and climate change.

Although industrialisation helped to mitigate some of the impact of the environment on health, in many cases it introduced new environmental hazards. A feature of many early urban settings, lead-contaminated drinking water has been implicated in a range of adverse outcomes, including higher rates of infant mortality (Clay et al. 2014) and the perpetuation of socioeconomic inequality through the subversion of cognitive skill-formation in already disadvantaged populations (Ferrie et al. 2015). Meanwhile, air pollution due to industrial activity has been linked to infant mortality—although at low levels of baseline development, this effect may be partially offset by the economic gains due to increased industrial activity (Clay et al. 2016). Of relevance to today's developing-country cities, Hanlon (2018) offers the hopeful view that because pollution exacerbates respiratory illness, public health measures to address infectious disease can also help reduce the health impact of air pollution and vice-versa.

Health and Economic Development

This chapter would be remiss without at least a brief discussion of the role of health in economic development. Here, we can see the impact of many of the health interventions discussed earlier made manifest in broader patterns of growth, through changes in human capital investment behaviour, labour supply, worker productivity, and demographic change. At the intersection of these issues, for instance, studies on the diffusion of oral contraceptives show how access to new medical technology allowed women the reproductive freedom to defer childbearing, reduce marital fertility, invest in human capital, and remain in the workforce (Bailey 2010; Goldin and Katz 2002). Such micro-level changes can have large macro consequences, with human capital acquisition accelerating a process of fertility transition that itself can also impact living standards and growth (Fernihough 2017).

Similarly, reductions in the human disease burden can raise labour productivity and return to schooling (Bleakley 2007) and lower the costs of

child quality (Bleakley and Lange 2009).⁴ Although the accompanying rises in longevity can spur further human capital investment (Jayachandran and Lleras-Muney 2009), these longevity changes may have relatively little impact on welfare through the lifetime “horizon” channel (Bleakley 2018) and can also lead to net population growth that attenuates any per capita income gains from reduced fertility and increased productivity (Acemoglu and Johnson 2007). Nevertheless, these findings should not be ‘construed to minimize the value of health through other mechanisms’ (Bleakley 2018: 40) or to ‘imply that improved health has not been a great benefit to less developed nations’ (Acemoglu and Johnson 2007: 929). Rather, they emphasise that development is multidimensional, encompassing questions of economic performance and growth as well as of wellbeing and human development and that it is a complex process where general equilibrium effects are an important part of the story.

What Can a Historical Perspective Contribute?

One important area for future research in the area of health and human development is to build up evidence on the health production function. This agenda, discussed in greater detail in works such as Almond and Currie (2011), is one to which economic history is especially well positioned to contribute, in large part because of the long-run view that historical settings and data afford. Indeed, recent historical work in this vein has provided some of the first empirical evidence on the capacity for remediating adverse early-life health shocks (Arthi 2018).

Likewise, with the rising availability of comprehensive, individually identified, and privacy-unrestricted historical microdatasets, historical work is well placed to address another theme of growing relevance to development economists: the causes and consequences of inequalities in health.⁵ Here, new initiatives such as the University of Michigan’s Longitudinal, Intergenerational Family Electronic Micro-Database (LIFE-M) project, which links individual-level vital records to census and other administrative data for the nineteenth- and twentieth-century US, will enable an

⁴Reductions in the disease burden may also raise household labour supply in rural settings where intra-household labour complementarities, common in agriculture, amplify the income losses resulting from individual illness shocks (Arthi and Fenske 2016).

⁵See, for example, Ferrie et al. (2015), Aizer et al. (2016), Eriksson et al. (2017) and Arthi et al. (2018), who use linking to generate large-scale historical panel microdata for the study of spatial, socioeconomic, and racial disparities in health.

unprecedented scale and richness in the data available to study patterns in health and demography.⁶

Finally, another emerging area for further study concerns mental health, substance abuse, obesity, and other non-communicable diseases that are prominent in the latter stages of the epidemiological transition. Here again, historical natural experiments can be an asset, providing laboratories to study the evolution of health over the development process.⁷

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15

Education and Human Capital

Sascha O. Becker

Education is generally thought to have many benefits. In modern economies, more educated people typically earn more (see Card 1999). But education has many further, non-pecuniary benefits. Oreopoulos and Salvanes (2011) document that more educated people live healthier lives, are less likely to ever be divorced/separated, are more future-oriented, less likely to have children while teenagers and less likely to ever be arrested. It is fair to say that economists usually focus on the pecuniary.

The reason why education might have pecuniary payoffs is explained by human capital theory. The key idea of this theory is that education is an investment which yields higher labour-market earnings because it increases productivity (Becker 1967). But did education have similar payoffs before modern labour markets developed? Was education beneficial in bringing about the Industrial Revolution (IR)? And even if we show correlations between education and income or other measures of economic development, is there a causal effect of education?

To answer the question of causality, one has to understand what drives differences in education. All of these questions are a matter of ongoing academic debates. While many have argued that education did not empirically matter a lot before 1900, others attribute an important role to human capital in the transition to modern growth (see, e.g., Galor 2005).

Drivers of Education

Before the rise of the state, education was acquired in the family, on the job, or by private providers, including the church. In fact, religion played an important role in fostering education before modern times. Jews, arguably the most educated group worldwide, have a long history of emphasising the importance of literacy. Botticini and Eckstein (2005, 2007) suggest a human capital interpretation of Jewish history, where the ultimate root of Jewish economic prosperity as merchants lies in a centuries-old Judaic rule that required male Jews to be able to read the Torah in the synagogue and to teach the reading of the Torah to their sons.

Becker and Woessmann (2009) make a similar argument for Protestants. Following the Protestant Reformation in 1517, instruction in reading the Bible generated the human capital crucial to economic prosperity. Using county-level data from late nineteenth-century Prussia, and exploiting the initial concentric dispersion of the Reformation to establish causality from Protestantism to education, they find that Protestantism indeed led to better education (and to higher economic prosperity). Interestingly, Protestantism also favoured education for girls. Becker and Woessmann (2008) show that the gender gap in education was smaller in Protestant counties compared to Catholic counties.

Using Swiss data, Boppart et al. (2013) confirm a persistent positive effect of Protestantism on several education indicators, in particular in areas with conservative milieus, and Boppart et al. (2014) show that the effect of Protestantism was particularly large for reading skills, but also existed in other subjects.

Education and the British Industrial Revolution

Given the importance of education in modern times, it is quite likely that education also played a crucial role in bringing about the IR. Interestingly, research on the role of education during the IR is far from clear-cut on this issue. The first phase of the IR is often characterised as one of skill-saving technological change and minimal educational requirements. For a long time, the literature held the view that education unimportant. Mitch (1993: 307) concludes his seminal review by stating that 'education was not a major contributing factor to England's economic growth during the Industrial Revolution', an argument that applies in particular to formal education and to the first phase of the British IR. Many scholars came to similar conclusions.

As Mokyr (1990: 240) famously sums up, ‘if England led the rest of the world in the IR, it was despite, not because of, her formal education system’. Instead, other factors are considered to be more important. Countless reasons have been advanced for England’s technological leadership, ranging from property rights, geography, culture, the biological spread of values, fertility limitation, capital deepening, imperial expansion and a unique structure of wages and energy prices, up to historical accidents and even pure chance.

However, more recently, the role of education during the British IR has been re-assessed. Kelly et al. (2014) point to the importance of human capital (broadly defined) and the quality of the British labour force on the eve of the IR. They show that in terms of both physical quality and mechanical skills, British workers around 1750 were at a much higher level than their continental counterparts. de Pleijt (2018) shows that, in England, human capital facilitated pre-industrial economic development. Madsen and Murtin (2017) take a very long-run view of the role of education in British economic growth since 1270. They show that education has been the most important driver of income growth during the period 1270–2010, followed by knowledge stock and fixed capital, while institutions have not been robust determinants of growth. The contribution of education has been equally important before and after the first IR. All of these papers shed new light on the role of education for long-run economic development.

Education in Follower Nations

Britain’s IR was soon followed by other countries. Nelson and Phelps (1966: 69) point to an important distinction between leader and follower nations, ‘but probably education is especially important to those functions requiring adaptation to change. Here it is necessary to learn to follow and to understand new technological developments’. Becker et al. (2012) attempt to do exactly that. They employ a unique school enrolment and factory employment database linking 334 counties from pre-industrial 1816 to two industrial phases in 1849 and 1882. They find that basic education is significantly associated with non-textile industrialisation in both phases of the Prussian IR. Specifically, they find that the degree of industrialisation in nineteenth-century Prussia would have been lower by one-third had all counties in Prussia only achieved the education level of the least educated county.

Similar results have been found by Sandberg (1979), who argues that human capital was a leading factor in late nineteenth-century Swedish catch-up. Interestingly, Sandberg also points out that Sweden was what he

calls an ‘impoverished sophisticate’ before this catch-up process: a country that had ‘a stock of human capital wildly disproportionate to its very low income level’. So basic literacy is not automatically linked with economic development, but once the circumstances improved, Sweden was ready for take-off.

A number of studies have employed country-level data to explore this question. Based on a cross section of 16 countries, O’Rourke and Williamson (1996) conclude that schooling mattered for catch-up growth in 1870–1913, but only modestly. Using an annual dataset of educational attainment for 21 industrialised countries over the period from 1870 to 2009, Madsen et al. (2017) finds further evidence for the leader-follower notion proposed by Nelson and Phelps (1966). His results show that changes in educational attainment and the interaction between education and the distance to the frontier (either the UK or later the US), as predicted by Schumpeterian growth theory, have been influential for productivity growth over the past 140 years.

Cinnirella and Streb (2017) analyse the role of human capital and innovation in economic development, using Prussian data. They show that, during the nineteenth century, the quality of basic education was associated with both workers’ productivity and firms’ R&D processes. They conclude that their findings support the notion that the accumulation of basic human capital was crucial for the transition to modern economic growth, both via direct effects on worker productivity, but also by fostering innovative capacity. Education of the average population thus seems to play a role in helping follower nations adopt the industrialisation and increase innovation.

Upper-Tail Human Capital

A lot of the literature bases its findings on education or literacy as skill measures of the *average* worker. The non-results in some contexts might be explained by this focus on average skills. Baten and van Zanden (2008) propose per capita book production as a proxy for advanced literacy skills and assess this relative to other measures. In contrast to other metrics which reflect basic skills, such as numeracy, the ability to sign documents with a full name, or literacy, book production proxies reflect more advanced capabilities. Baten and van Zanden (2008) find evidence consistent with the central mechanism of endogenous growth theory by showing book production can explain income levels and therefore economic development.

Mokyr (2005) stresses the importance of this “density in the upper tail”, by which he implies scientifically savvy engineers and entrepreneurs at the top of the skill distribution. Meisenzahl and Mokyr (2012) highlight the role of technical competence, arguably one form of human capital, as a key factor in Britain’s economic leadership. Looking at France, Squicciarini and Voigtländer (2015) show that upper-tail human capital, proxied by the historical presence of knowledge elites, mattered for industrialisation in France. They use city-level subscriptions to the famous *Encyclopédie* in mid-eighteenth-century France and show that subscriber density is a strong predictor of city growth after the onset of French industrialisation. Dittmar and Meisenzahl (2017) document that German cities that formalised public goods provision via church ordinances in the 1500s began differentially producing and attracting upper-tail human capital and grew to be significantly larger in the long run.

But economic development not only benefits from a better-educated native population, immigration is another contributing factor. In 1685, religiously persecuted French Huguenots, well-educated Protestants, settled in Brandenburg-Prussia and compensated for population losses due to plagues during the Thirty Years’ War. Many of them worked there in textile production. Hornung (2014) shows that textile manufacturers in the Prussian cities where Huguenots had settled were more productive 100 years later. This historical incident documents that the immigration of highly-qualified people can sustainably improve technological development and economic productivity and in fact did so in the Prussian heartland, well before the IR.

Education and the Demographic Transition

The historical evolution from stagnation to modern economic growth has also been closely linked to the demographic transition in which the number of children per family declined significantly (Galor 2011). In the pre-industrial world described by Thomas Malthus in the late eighteenth century, economic progress merely increased fertility, so that per capita income stagnated at the subsistence level.

The demographic transition which led the economy from stagnation to growth was in turn promoted by the rising education of the population. For one, a family could “afford” fewer children for a given budget if they invest more in each child’s education. This inverse relationship between the number of children and education per child (the so-called “child quantity-quality trade-off”) can be found already before the demographic transition. Becker et al. (2010, 2012) show that causation between fertility and education runs both

ways. Furthermore, education in 1849 predicts the fertility transition in 1880–1905. Education matters not only in the context of the child quantity-quality trade-off, but for an additional reason: better-educated mothers *chose* to have fewer children (Becker et al. 2013).

Measuring Education in Historical Data

Measurement is a challenge, also when it comes to measuring education and human capital in history. Various measures of education and human capital have been used in the literature, some available at the individual level, some at an aggregate level, for instance, at the city-level or country level. Enrolment in primary and secondary schools (e.g. Becker and Woessmann 2008) and in universities (e.g. Becker and Woessmann 2008; Cantoni et al. 2017) are measures of investment human capital. Somewhat similar to issues in modern-day developing countries, enrolment data come with the caveat that students did not necessarily attend school all-year round, so enrolment data are an imperfect proxy for what is actually learned in school.

Basic proxies for educational opportunities in a location are the existence of schools and the number of teachers, capturing the *supply* of schools and teachers. Of course, having a school building and a teacher is neither necessary nor sufficient for children to learn something. It can also be interesting to look at school enrolment when controlling for the supply of schools to get a sense of the *demand* for schooling (e.g. Becker and Woessmann 2008).

It is typically harder to produce measures of educational achievement. One starting point can be data on number students with a degree (see Cantoni et al. 2017). Data on occupation of adults is often seen as reflecting the human capital they acquired during formal schooling or on-the-job training.

Literacy rates capture which share of the population has achieved the ability to read and write. For instance, in one census, in 1871, the Prussian Statistical Office collected data on literacy for everyone of age ten and above. In the Prussian case, this gives a binary measure: literate yes or no. A commonly used proxy for literacy is the ability to sign one's name on official documents, such as marriage certificates, which can often be obtained from registries or church records.

Data on numeracy skills is equally desirable. A'Hearn et al. (2009) point out that age data frequently display excess frequencies at attractive numbers, such as multiples of five. They use this “age heaping” to measure cognitive ability in quantitative reasoning, or “numeracy”. They demonstrate a robust correlation of literacy and numeracy, where both can be observed. Blum and

Krauss (2018), in an in-depth study of age heaping, use the accuracy of individual age statements (in cases where also independently kept records are available) to shed light on the reliability of age heaping indicators. Their results suggest that the commonly used binary indicator measuring age heaping is a valuable proxy for numerical skills and occupational background in a population. But it is important to ascertain exactly who is reporting age in sources used to derive population age heaping measures (Blum et al. 2017).

There is sometimes richer data for military recruits. Boppart et al. (2014) have nineteenth-century data from Switzerland allowing them to measure cognitive skills by conscripts' marks in the pedagogical examinations on reading, essay writing, numeracy and Swiss history. While this example shows that even test scores can be measured in historical data, other indicators that are considered to be relevant in economic development, such as IQ data (e.g. Ram 2007) are not available for the past.

Finally, education in the upper part of the skill distribution has recently received a lot of attention. As discussed, this can be measured in various ways: book production per capita (e.g. Baten and van Zanden 2008), enrolment in schools above and beyond compulsory schooling age, and in universities and measures of "famous people" (Dittmar and Meisenzahl 2017) or subscribers to encyclopaedias (Squicciarini and Voigtländer 2015).

Some Debates and Open Questions

One ongoing debate in the literature is the relative importance of institutions and human capital. Famously, Acemoglu et al. (2001, 2002) argue that settler mortality and population density in 1500 predict institutional quality and the level of economic development today and from this infer an effect of institutional quality on economic development. However, Glaeser et al. (2004) and Easterly and Levine (2016) argue that Europeans who settled in the New World may have brought with them not so much their institutions, but rather themselves—that is, their human capital. This view, however, is partly debated by Acemoglu et al. (2014). They reconcile their own earlier work and that of Glaeser et al. (2004) by showing estimates that treat both institutions and human capital as endogenous. They instrument institutions by settler mortality (as in their earlier work) and human capital by Christian missionary activity. These estimates suggest that both human capital *and* institutions matter for long-run growth.

Also in the European context is that it is quite hard to tell apart the effects of human capital from those of institutions because schools, or schooling laws, are often part of the mosaic of institutional reforms. For instance, during Reformation times in the early sixteenth century, Protestant cities

introduced city-level laws and ordinances which regulated many aspects of city life, including schools. Dittmar and Meisenzahl (2017) show that Protestant cities with such city-level laws attracted more upper-tail human capital than Protestant cities without such laws, and they grew faster overall. Similarly, the Prussian reforms in the early nineteenth century (“Stein-Hardenberg Reforms”) were wide-ranging and again included reforms to the education sector.

It seems fair to argue that both “good institutions” and human capital support economic development, even more so when they both go hand-in-hand. But there is promise in understanding further historical experiences, where piecemeal reforms might have brought just one but not the other, and from those experiments about the relative contributions of education and institutions for economic development. Another debate that was highlighted earlier concerns the times and circumstances under which kind of human capital matters and which human capital: basic education for some, basic education for all, education beyond literacy, university education, on-the-job training or formal education?

It is also important to point out that many data treasures still remain to be used. Research activity on specific countries is largely driven by individual or collective efforts to digitise data from various sources. The flurry of research on US economic history, the UK and—to some extent—Prussia and Germany more broadly speaking, seems to be partly the result of electronic availability of (digitised) census and other data records. Despite this, it seems that a lot of hidden treasures remain to be explored in the US, UK and Germany, but also in other large European countries, such as France, Spain and Italy. There is no doubt that researchers interested in the role of education and human capital in the history of many of these countries are going to find more data to explore further exciting questions.

Why the Historical Perspective Matters

Understanding the role of education in economic history is relevant for various reasons. Going back in history helps to understand “where it all began”: what were the drivers of human capital acquisition. The consequences of educational reforms and educational investments are equally important. While modern-day studies of effects of education are naturally bound to look at relatively short-run outcomes, economic history informs us about the long-run effects of education. It is fascinating and worrying at the same time to see that

regional differences in education that arose centuries ago may still explain some part of regional income differences we see today.

It is also interesting to note that the way public education was set up often reflected the social fabric of the time. Prussia's education system, considered a leading reform model in the nineteenth century, was set up as a three-tier system, essentially because there were three (perceived) social classes. That split into *Hauptschule*, *Realschule* and *Gymnasium*, invented two centuries ago, has survived to this day, showing the persistence of education over the long run.

What is clear is that human capital played an important role in economic development also in centuries past and not only in our modern economy. While education often was acquired for non-economic reasons, for example, for religious reasons, it did have economic payoffs and mattered for the transition to modern growth.

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16

Famine and Disease

Guido Alfani and Cormac Ó Gráda

The infrequency of severe mortality crises and, more generally, the low prevalence of famine and disease are characteristics of modern industrial and post-industrial societies. Understanding mortality crises is an important part of understanding some fundamental aspects of preindustrial economies. Understanding the processes leading to their decline and the associated improvements in living standards and life expectancy—what Robert Fogel (2004) called the ‘escape from hunger and premature death’—is a precondition for knowing what is needed to prevent the re-emergence of widespread famine and lethal infectious disease. So this is a field in which the contribution of economic historians is vital and, given that the stakes are so high, one in which economists would do well to carefully consider the past, before making assumptions about the future.

Famine

The prevalence of famine across the world declined dramatically during the nineteenth and twentieth centuries, due to unprecedented increases in economic and social development (Fogel 2004; Ó Gráda 2009). Famines are now a rarity and threaten only the poorest countries of the world. Indeed, modern famines tend to be associated with wars and civil strife, which still put the lives of millions at risk (FAO 2017). Hence, most analyses of recent large-scale famines treat them as “man-made”, that is, the products of inefficient

distribution, uneven entitlement to food or war: an approach linked in particular to Amartya Sen (1981). Before him, though, Adam Smith had already discussed famines as the consequence of human actions in *The Wealth of Nations*, claiming that in Europe they had never arisen ‘from any other cause but the violence of government attempting, by improper means, to remedy the inconveniences of dearth’ (Smith [1776] 1976: 526).

Smith’s history was poor but he introduced the analytically crucial distinction between “famine” and simple “dearth”, or scarcity of food. Only under specific circumstances did dearths, which were quite common, develop into famines. Soon, however, Thomas Malthus’s claim that famine was ‘the last, the most dreadful resource of *nature*’ (1798, chap. 7, our italics) replaced Smith’s focus on human agency. According to Malthus, famines contributed to solve the demographic unbalance resulting periodically from the spontaneous tendency of population to grow faster than any possible improvement in agricultural productivity.

The tension between the production of food, underlined by Malthus and his followers, and its distribution, on which Sen and Smith focused, shaped the discussion about the causes of famine. The debate had its political side, since it questioned the behaviour of ex-colonial powers (see, e.g., Sen’s interpretation on the Great Bengal Famine of 1942–1944 as the consequence of British colonial wartime policies) and also pitted supporters of free markets against those more favourable to public intervention. But it is not a question of either/or: famines can result from deficits of either production or entitlement. This is recognised in a recently proposed definition of famine: ‘a shortage of food or purchasing power that leads directly to excess mortality from starvation or hunger-induced diseases’ (Ó Gráda 2009: 4). This definition has the double practical advantage of clarifying what we should look for in terms of famine outcomes (famine is defined as a killing event) and of providing a synthesis of the two different views on the causes of famines (“man-made” and “natural”).

This being said, the historical link between famines and production shortfalls due to adverse weather shocks at times of high population pressure on resources is hard to deny. In preindustrial Europe only from the eighteenth century on did that link loosen, with the role of human agency looming larger thereafter. A recently reconstructed comparative chronology of European famines (Fig. 16.1) provides further support for the view that the probability of a famine occurring was much higher in periods of relatively high population density.

The available data also point to three particularly severe European “super-famines”. The Great Famine of 1315–1317 was the worst food crisis of the

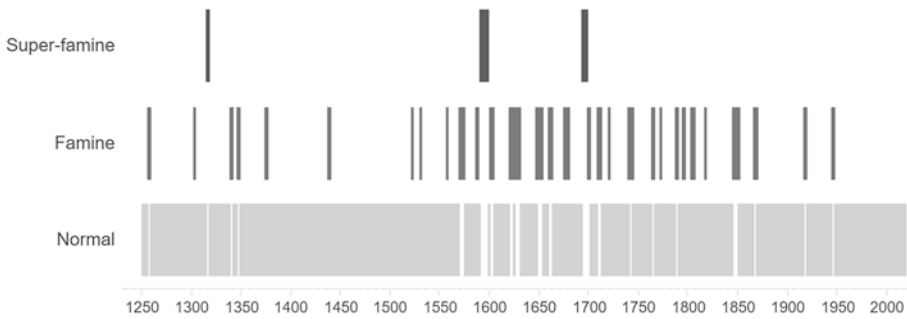


Fig. 16.1 Timing of European famines, 1250–2017. Source: Alfani and Ó Gráda (2017)

late Middle Ages (although it spared Italy and other parts of south Europe), causing huge human losses (Jordan 1996; Slavin 2014). In southern England, the second of our super-famines (1590–1598) was also the last one, while in Italy it was possibly the worst famine on record (Alfani 2013). Finally, the famine of 1693–1697 proved particularly severe in France, where it killed up to 1.5 million people. But the regions most affected were in northern Europe: in 1696–1697, famine may have killed one in four and one in five of the population of present-day Estonia and Latvia (see the relevant chapters in Alfani and Ó Gráda 2017).

In terms of excess mortality these medieval and early modern super-famines dwarf some more familiar eighteenth-century events, such as the Great Winter famine of 1708–1709 and the “Year without a Summer” of 1816 (Luterbacher and Pfister 2015). The impact of the Great Frost of 1740–1743 (Post 1985) was also generally “light” by comparison, except in Ireland (Dickson 1997). The Great Irish Famine of 1846–1850 and the Great Finnish Famine of 1867–1868 (Curran et al. 2015) were two disastrous late “outliers” in the history of European famines.

Beyond this general assessment, the in-depth analysis of specific famines has proved to be a fruitful way of exploring the inner workings of specific historical economies, from the early modern period (Alfani 2013) to the nineteenth century and beyond. In this respect, the Great Irish Famine, well documented in official sources, a lively contemporary press, business and landed estate records, and folklore, is of particular interest. Such sources have informed numerous economic studies of Irish poverty on the eve of that famine and of its demographic toll, starting with Mokyr (1983). The latest of these, a cross-sectional analysis *à la* Mokyr, confirms the role of Malthusian forces as reflected in the ratio of quality-adjusted land to popula-

tion, but also leaves room for institutional factors (Fernihough and Ó Gráda 2018).

The disastrous harvest failures that were the proximate cause of the Great Irish Famine were no ordinary crop failure; they were an ecological disaster. Given the extreme reliance on the potato in Ireland—unequaled anywhere else before or since—that *some* at the margins should perish when it virtually disappeared for several years in a row was inevitable. But most historians agree that dogma—an alignment of political economy, providentialism, fiscal rectitude and prejudice—magnified excess mortality. About one million died of starvation and disease; the emigration of another million or so prevented an even higher death toll (Mokyr 1983; Ó Gráda 2015). Private charity was unequal to the gigantic challenge. Public charity, such as it was, was grossly inadequate, poorly designed and unfairly funded. And, crucially, it was virtually withdrawn when the horror was still unfolding.

The Great Famine brought Ireland's long history of famines to a cataclysmic end. Europe suffered its last famines a century later, during and immediately after World War II. The Great Bengal Famine of 1943–1944 was also ultimately a war-induced famine, though issues of culpability and food availability are still disputed (Sen 1981; Ó Gráda 2009). Some other notorious twentieth-century famines, such as those ravaging Ukraine in 1931–1933, Moldova in 1946–1947, and China in 1959–1961 are blamed exclusively on Soviet and Maoist ideology, respectively, although the issue is controversial (Sen 1981; Ó Gráda 2009; Curran et al. 2015; Alfani and Ó Gráda 2017).

Today no one should die of famine anywhere, and fortunately few do. None of the “extensive shortfalls” identified by the Food and Agricultural Organization over the past decade resulted in a famine, because of a combination of adequate early warnings, economic and societal resilience and the globalisation of disaster relief. It is only famines caused by war—think Ethiopia in 1985–1986 or Somalia in 1993 and 2011–2012—or by dysfunctional politics—think North Korea—that still put the lives of millions at risk.

Disease

As with famine, the prevalence of lethal disease has also declined over time—at least if we refer to mortality from acute infectious diseases, which have been progressively replaced by degenerative diseases or by diseases typi-

cal of older ages as the main cause of death. That decline was dramatic in developing economies during the nineteenth and twentieth centuries, as can be seen in Table 16.1 referring to England and Wales. However, in poor countries today, infectious diseases still account for about 40 per cent of all deaths.

Infectious diseases could cause large-scale mortality crises. During the nineteenth century cholera, originating in India, caused six global pandemics. Europe was first affected during the second pandemic (1829–1851), with hundreds of thousands of deaths across the continent. The so-called Spanish influenza of 1918–1919 was even more murderous, and may have been the worst pandemic in the history of humankind, killing between 50 and 100 million people worldwide (Johnson and Mueller 2002). The huge death toll resulted from the sustained demographic growth that had occurred during the nineteenth century, and from the ability of the disease to infect a very sizeable part (as much as one-third) of the world population. The ability of influenza viruses to spread so quickly and efficiently is the main reason why the possibility of the appearance of a new lethal strain—especially the “avian” variant—is currently considered a major global health threat.

In terms of mortality *rates* (the percentage of the overall population killed) the worst mortality crises of recorded history were caused by plague. Plague also probably had the most pronounced economic consequences. The return of plague to Europe in 1347 and thereafter, after about six centuries of absence, was a momentous event, not only for the resulting mortality—the first wave

Table 16.1 Distribution of causes of death, 1850–2012 (%)

Causes	England and Wales			High-income countries	Low-income countries
	1850	1900	1939	2012	2012
Infectious diseases	44.7	35.8	14.5	6.0	38.6
<i>Not respiratory</i>	26.2	18.2	3.7	2.6	28.2
<i>Respiratory</i>	18.5	17.6	10.8	3.4	10.4
Maternal conditions	0.9	0.8	0.4	0.02	1.7
Neonatal conditions	6.0	3.7	3.7	0.34	9.3
Non-communicable	44.8	56.1	76.5	87.3	40.3
Injuries	3.6	3.6	4.9	6.4	10.1
Total deaths	368,995	587,830	498,968	11,671,361	5,696,969
Life expectancy	43	46	64	79	62

of the Black Death killed between one-quarter and one-half of the population of Europe and the Mediterranean (Alfani and Murphy 2017: 316)—but also for its vast consequences for human history, including economic history. While an earlier historiography underlined the positive economic consequences of the Black Death in the long run, such as efficiency gains through the re-organisation of agrarian production (Herlihy 1997), recent research has focused more on how plague may have favoured the rise of Europe. Diamond (1997) suggests that plague, together with other pathogens, helped Europeans to conquer the Americas by acting as a sort of biological weapon. In the context of the Great Divergence debate, Clark has argued that the Black Death and subsequent epidemics set Western Europe—unlike the most advanced parts of Asia, which were less affected by plague—on a path of quicker economic development by contributing to the creation of a “high-income” Malthusian equilibrium resulting from a “high-mortality” demographic regime. The paradoxical outcome was that plague, by reducing the life span of Europeans, led an improvement in their living standards (Clark 2007: 99–102; Alfani and Murphy 2017: 330). Indeed, there is evidence that real wages increased significantly in the wake of the Black Death (Pamuk 2007; Campbell 2010).

Moreover, in specific settings—from sparsely populated areas of Europe, such as Ireland and Spain, to the Nile basin where, by contrast, the capillary irrigation system depended upon high population density for its maintenance—the Black Death had long-term negative, not positive, consequences because it destroyed pre-existing equilibria without offering opportunities for gains in economic efficiency (Álvarez Nogal and Prados de la Escosura 2013; Borsch 2015; Alfani and Murphy 2017: 331–332). In Eastern Europe, it has long been claimed that the Black Death led to impoverishment for the peasantry through a “second serfdom”, although not all agree on this point (Domar 1970; Dyer 1998: 111). Long-term negative consequences have also been blamed on later plagues, in particular those affecting Italy and other parts of southern Europe during the seventeenth century, which deepened the relative decline of these areas in the context of the Little Divergence between North and South (Alfani 2013; Alfani and Percoco 2018). This highlights the need to pay attention to the context when attempting to provide an assessment of the economic consequences of plagues or of any other severe mortality crisis: hence the importance of adopting a genuinely historical-economic perspective.

Economic historians have explored many other possible implications of the Black Death, for example, the deep causes of the concomitant persecution of Jews (Voigtländer and Voth 2012). But perhaps the aspect currently

commanding most attention is the re-distributive consequences of the Black Death, as this seems to have been the only event capable, during the entire 1300–1800 period, of significantly reducing inequality (Alfani and Ammannati 2017; Alfani and Murphy 2017: 332–334).

Some Lessons for the Future

The progressive disappearance of famine from Europe is clearly connected to economic development. Generally speaking, the same holds for infectious diseases. Malthus himself listed plagues and other epidemics among nature’s “positive checks” on overpopulation—but we now know that the factors leading to the progressive disappearance of plague from Europe, a process which began in the second half of the seventeenth century (London’s last plague outbreak dates from 1665–1666: Cummins et al. 2016), are more complex. Although improvements in public health, hygiene and living standards almost certainly played a role, some of the other apparent changes in the epidemiology of plague remain somewhat mysterious and may have entailed the appearance of new pathogen strains. Pathogens mutate continuously, often in unforeseen directions: that is why the alert about the appearance of lethal strains of influenza viruses is so high today. Furthermore, new lethal human pathogens appear every few decades (think HIV or Ebola). Even *Yersinia pestis* (the bacillus responsible for plague) is far from having been eradicated. On the contrary, it is currently endemic in most continents (Africa, Asia and the Americas) and has to be considered a ‘re-emerging infectious disease’ (Ziegler 2015: 260–263).

The ability of potentially lethal infectious diseases to develop antibiotic resistance means that they continue to be a threat. On the one hand, economic history offers some consolation: most of the gains in life expectancy from reduced mortality due to infectious disease predate the “antibiotics revolution” (Ó Gráda 2016). On the other hand, though, the history of microbial threats may imply that our societies and economies are more fragile than we like to think.

We might seek some reassurance from the thought that even if disease is still among us, famines in peacetime have virtually disappeared. But the historical experience of even a rich area like Europe, which experienced several famines during World War II, is a reminder that compromising the equilibrium between population and resources can always have dire consequences (Alfani and Ó Gráda 2018)—something worth bearing in mind in an epoch of rapid climatic change. Gaining better knowledge of past crises is one way

of getting ready to face future ones, which is why the contribution of economic history seems to be particularly important in this area.

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17

Women and Children

Jane Humphries

Feminist historians have long complained that women and children have been written out of mainstream history. “Hidden”, their voices lost, they were not even present during the demographic transition (see Bennett 1993; Mackinnon 1995; Janssens 2007)! Until recently, the same charge could have been levelled against economic history. Women and children’s work and living conditions rarely featured in accounts of economic growth, structural change, and the evolution of the standard of living, which were calibrated by reference to *male* workers, *men’s* wages, and *breadwinner* families. This was in contrast to the attention that had been given to women and children in older, more traditional narratives, and despite the obvious fact that since women and children make up some 60–70 per cent of the population, an economic history which leaves them out is obviously incomplete but also probably incorrect.

This is changing. Women and children have become key players, pulled from the wings of economic history not by the growing volume of empirical evidence on their economic roles, but by the emergence of new grand narratives, in which *serendipitously* they have star parts.

Women, the Workplace, and the Wage

The dramatic increase in women’s involvement in paid work after World War II prompted many labour economists to construct models to explain and predict changes in female participation rates (see Mincer 1962). Economic historians began to look at the history of these developments and thereby to

think about parallels between women's position in poor countries and their historical status. A pioneer whose work drew on both traditions is Claudia Goldin.

Goldin's work on the long-run evolution of women's participation combined a cross-section description of countries at different levels of income, a choice theoretic model of time allocation, and a historical case study of US women. Goldin (1995) appealed to a theoretical model in which as household income levels increased, women's choices would rationally evolve—first a retreat from the labour force and only much later, when their education levels had caught up with men's, reengagement in market work. In support of the downward section of the U-shaped curve relating female participation to income levels, Goldin cited the pioneering work of development economist Esther Boserup (1970) who had argued that the initial structural changes associated with economic development reduced women's participation—an idea that had become a standard of development economics. She generalised confidently: '[A]cross the process of development the adult women's labour force participation is U-shaped' (1995: 62).

Despite its metamorphosis into a stylised fact of development and gender history, the downward section of the U remained sketchily documented and explained. Critics have argued that decline was often the product of a failure to account for the movement of women's work from the subsistence sector to the market, even when commodified women's work was undercounted, points which Goldin herself acknowledged (Goldin 1995: 79). Interested economic historians sought to demonstrate the extent to which women's economic participation was badly recorded in historical documents, including early national censuses, to explain why this was the case and to wrest improved estimates out of an often intransigent historical record (for a survey of this literature and some recent contributions see Humphries and Sarasua 2012). The implications are of huge importance: poor estimation of the female labour force has implications for the validity of the estimated evolution of structural change, of productivity growth, and so of total factor productivity (TFP). But despite these implications there are few studies that make the difficult step from revision of the gender record to insistence on the implications for the metanarrative. Carmen Sarasua's brave paper (2018) suggesting that more careful and sensitive measurement of women's work has dramatic effects on estimates of Spanish structural change and so on understanding of Spain's development story is a pioneer in this emerging tradition.

Goldin's *The Quiet Revolution* (2006) went on to provide an elegant periodisation of the upward section of the U-shaped curve with the Revolution itself (1970s–present) preceded by evolutionary phases in which women

played different roles in the changing US economy, differentiated by marital status, age, and education. While the drivers of change in Goldin's account stem primarily from shifting demand for workers of different kinds, important feedback effects on women's ideas about job prospects, married life, and continuity of work affected behaviour. Key here was the birth control pill which enabled different choices to be made. More generally, demographic changes, of importance in long-run models of economic growth, provided further motive to include women and children's economic activities in any analyses.

As with the focus on women's participation, modern concern about the gender pay gap (GPA) prompted interest in its history. Economic historians adapted the Oaxaca-Blinder decomposition developed to separate that part of the GPA that could be ascribed to the different productivity enhancing characteristics of men and women from a residual identified with discrimination (for the original contribution, see Oaxaca 1973). Goldin again contributed. For the UK, Joyce Burnette's (2008) comparison of male and female pay combined econometric analysis with impressive archival evidence. Burnette's work illustrated the need to adapt models designed for modern labour markets to historical circumstances, for she argued controversially that when work was a physical challenge, as in most historical societies, then male upper-body strength gave them an advantage which in market terms justified much of their higher pay.

Research on the evolution of participation and the GPA also generated studies of gender divisions of labour both within waged labour and between unpaid work and paid work. These themes resonated with both current debates about working women's double shift (with its adverse effects on pay and promotion) and social and cultural historians' investigation of the meaning of gender and its deep roots in institutions, ideas, and consciousness.

Another rich source of crossover studies from development economics to economic history, and one which brought children into focus, originated in Amartya Sen's discovery of 100 million missing women in Indian census data (1992). He explained the ominously unbalanced sex ratios in terms of "son preference": female children received less food and reduced health care and so suffered higher mortality. Theoretical models of household bargaining within which girls and women had less voice for both economic and cultural reasons generated gender-biased allocations of resources and were readily generalised. Higher infant mortality among girls has been difficult to document in the European demographic evidence, but there are suggestions that historically patriarchal structures bore down not on children but teenage girls and older women (McNay et al. 2005). Heights, BMI, literacy, and numeracy have been

used to try to identify discrimination both over time and within sub-groups of the population with some interesting, if piecemeal, results (Horrell and Oxley 2013).

Research on gender is hard because it requires new data and so intensive archival work. Some recent research has risen to this challenge, for example, integrating women into estimates of the sectoral composition of the labour force and providing a wage series for women workers comparable to the well-known and much-used series for men (Humphries and Weisdorf 2015).¹ Recovering children's experiences is even more difficult, though again there have been pioneer studies of children's work and wages based on documentary evidence and framed by theoretical models of labour markets with child labour (Humphries 2010). However, even when uncovered, the evidence is often difficult to interpret and unamenable to cutting edge econometrics, leaving results to be overlooked or even dismissed. It is easier to see women and children as playing bit parts in economic history: less productive and less likely to work, they could be treated as appendages of men who continued to dominate accounts of economic life. Genuine inclusion rested on new theorisations of economic growth.

The Industrious Revolution

In the 1990s, Jan de Vries reinterpreted the economic growth of northwest Europe in terms of an early modern increase in market work, a rise in industriousness, motivated by a desire for new market goods such as tropical groceries and attractive fabrics (de Vries 1995, 2008). De Vries's "Industrious Revolution" was grounded on earlier ideas about seventeenth and eighteenth-century proto-industrialisation, when manufacturing grew based on household units, Smithian divisions of labour, and artisan production techniques. It provided insight into the comparatively early prosperity of Britain and the Dutch Republic, which had moved ahead before industrialisation, creating the "Little Divergence" among European countries. Crucially, women and children were explicitly identified as being located at the forefront of industriousness: for once they had leading roles.

The first wave of industriousness was only part of de Vries's metanarrative, which integrated economic development with changes in the structure and functioning of households. In phase one, from around 1650, women reallocated time from leisure and household production to paid work in order to earn

¹ See <https://www.campop.geog.cam.ac.uk/research/occupations/>.

money to buy the novel consumer goods that trade and conquest were making available. De Vries postulated a preference switch in favour of market consumption reinforced by shifting relative prices. There was an increase in working time and a boom in the participation rates of women and children, which increased the share of manufacturing in both output and employment, and set the scene for capitalist industrialisation. De Vries envisaged a second phase after c.1820, when women retreated back to the home, prompted again by shifting preferences as well as relative prices, and perhaps income effects from rising male wages. In this phase, women preferred cleaner, more comfortable homes and more cared for “higher quality” children. Domestic comfort, only obtainable in families, retained earning children while maternal attention increased human capital formation. De Vries even sketched a third phase post-World War II in which women’s increasing TFP echoed Goldin’s “Quiet Revolution”, but with a downside: two-earner households were often cash strapped and time poor, their children short of attention, and family life impoverished.

In support of his ideas, de Vries referenced qualitative and quantitative evidence on women and children’s work, including neo-Marxist accounts of the intensified exploitation-enabled capitalist industry, as well as a previous literature on early modern consumption. Follow-up studies have devised less or more ingenious reconstructions of time use using wage books, court records, and verb-led approaches, the last mentioned being used particularly to investigate women’s work (Voth 1998; Allen and Weisdorf 2011; Ågren 2017). Accounts of consumption have been enriched by additional studies of probate inventories, retailing, and marketing (Overton et al. 2004). But much work remains to be done particularly on time use and the motives for industriousness. Optimistically, a voluntary sacrifice of leisure and domesticity in favour of novelty consumption, longer working time and intensified household participation might also have been coerced by legal changes, the organisation of labour markets, and the pause in any improvement in men’s real wages in the early nineteenth century (Allen 2009a).

Attractive as the Industrious Revolution was, one main reason for its impact was its resonance with emerging accounts of economic growth that also adopted a longer time horizon and a global perspective. These narratives, exemplified by Broadberry et al.’s recent (2015) survey of British economic growth in an European perspective demonstrated divergent trends in GDP per capita and (male) real wages. Macroeconomic accounting explains such divergence by shifts in factor shares, or in the relative prices of workers’ consumer goods, or in labour supplies per capita (Angeles 2008). The last explanation appears the most likely (Broadberry et al. 2015, 247–278). So variations in the intensity of

work provide the missing pieces of the macro puzzle and as such have become entrenched in the mainstream along with the women and children who bore the brunt of drudgery and graft.

Unified Growth Theory

Another important recent influence on economic historians has come from unified growth theory (UGT). UGT provides a framework adapted to long-run historical analyses since it attempts to build a single framework transition from Malthusian stagnation to modern economic growth while also explaining the Great Divergence and the Demographic Transition (Diebolt and Hauptert 2017). As with the Industrious Revolution, the model is grounded on household decision-making and so inevitably draws women and children into the frame. The driver is again a household switch in preferences but in this case from a large number of “low-quality” children to a small number of “high-quality” children. As it stands, there are many different ideas about what induces parents to invest in the quality of offspring (Galor and Weil 1996; Iyigun and Walsh 2007; Strulik and Weisdorf 2008; Doepke and Tertilt 2009; Largerlöf 2003). Historical testing will not be easy, but as most hypotheses relate to shifting roles for women and children within households, they must feature.

Related theorisations of growth and divergence revisit the knotted interdependence of economics and demographics that inevitably draws women and children into the picture. Building on an earlier empirical observation by Jan Hajnal (1965), De Moor and van Zanden (2010) and Voigtländer and Voth (2013) argued that the high wages that followed the demographic catastrophe of the Black Death were shared by juveniles on traditional service contracts, which involved co-residence with employers. The higher wages earned by these young people caused them to postpone marriage, cementing a distinctive “Northern European Marriage Pattern” (NEMP), which then lowered population pressure and enabled a geographically restricted escape from Malthusian stagnation. Although there is circumstantial and indirect evidence in support of these interlocking hypotheses, which incidentally provided a neat explanation of the Little Divergence, the demographic data needed to settle the case does not exist. Nor is the recent evidence on long-run wages consistent with the argument since workers on annual contracts did not enjoy the boom in wages that was experienced by day labourers. If anything, the incentives were towards early marriage and large numbers of children retained in peasant households. Much remains to be resolved (for more on this debate, see Dennison and Ogilvie 2014; Carmichael et al. 2016).

The High-Wage Economy

Women and children did feature in the classic debate as to whether and over what time frame the British Industrial Revolution with its machine industry and factory production improved workers' living standards. Optimists argued that industrialisation benefitted "peripheral" workers including women and children, a view that reverberated with the Industrious Revolution described above. Pessimists focussed on the drudgery and long hours of mechanised work, and the miserable and unhygienic conditions in unplanned but rapidly growing cities, which bore oppressively on women and children who were the mainstay of factory labour and more likely when not working to be confined at home (for the classic introduction to this debate, see Pinchbeck 1981). However, as soon as the protagonists began to *measure* well-being, attention fixated on the male experience, which was so much more readily documented (e.g. Lindert and Williamson 1983; but see also Feinstein 1998). The standard of living question was reduced to trends in male wages which excluded any serious discussion of the sourcing of family incomes, an omission rectified only by the attention to women's and children's contributions as necessitated in the Industrious Revolution (Horrell and Humphries 1990, 1995).

The latest round in the standard of living debate has been triggered by Robert Allen's (2009b) influential account of the origins of the Industrial Revolution. Allen identified high wages as prompting the substitution of capital for labour and so propelling Britain onto a superior growth trajectory. Since he included the spinning jenny as one of the three macro-inventions of the era (alongside the steam engine and smelting with coal), the role of children in particular demands attention.

For empirical support, Allen relied on evidence compiled earlier by Muldrew (2012) and a one-off observation from Charles Feinstein which turned out to be crucial in demonstrating the run-up of wages prior to the wave of mid-eighteenth-century textile inventions. Although all authors (particularly Muldrew who was the pioneer here) are to be commended for attention to a feminised occupation and its terms and conditions, the evidence again is insubstantial, relying particularly on the views of opinionated and interested social commentators. Evidence on spinners' *actual* earnings tells a different story for it was poorly remunerated even by the standards of women's work and there is no spike on the eve of the invention of the jenny, water frame, or mule (Humphries and Schneider 2018). An alternative interpretation reaches back to Berg's (1985) *Age of Manufactures*. It suggests that mechanisation by narrowing the productivity differential between adults and children, as explained in Basu and Van's influential (1998) model of labour markets with child labour, created the opportunity to make super profits by employing

children, a prospect that was enhanced by the demographic and labour market conditions of early industrial Britain (Humphries 2010, 2013). Allen (2015) sees this as merely one channel by which high wages (of adult males) might have induced mechanisation. Perhaps this channel was uppermost in the minds of men who invented, employed, and made their profits in the bounded reality of the factory districts of Great Britain.

Providing Historical Context

In a world where glass ceilings still exist, where child labour remains endemic, and where #metoo has struck such a public chord, understanding the origins and evolution of gender discrimination and child exploitation remains a top priority. Recent debates provide exciting and fresh material through which to study these issues in their economic and historical context.

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18

Slavery and Discrimination

Richard H. Steckel

This chapter argues modern-day discrimination against African Americans had roots in the racism that surrounded slavery in the United States. Once used to justify slavery, this racism persisted long after the Civil War was over and may have been reinforced by the arrival of a new, more-cognitively capable generation of African Americans near the turn of the century, which stimulated a backlash that included the savage practices of lynching and Jim Crow.

Types of Discrimination

Let us begin, however, by exploring the meaning of discrimination. In general, it refers to distinctions made in favour or against a person based on the group, class, or category to which the person belongs (Fishbein 2002). The basis for these distinctions varies widely, ranging from religion, gender, and age to sexual orientation, disability, race or colour, and nationality. People may harbour prejudices for a long list of reasons including combinations of the above, such as age and race, or religion and nationality. For example, someone may particularly dislike Catholics from Poland or blacks from Central Africa.

Discrimination matters because it often has consequences. Without knowing much about them as individuals, people may vote against, or otherwise refuse to support, candidates because they dislike the class or group from which they hail. With little knowledge, they attach to individuals their

general feelings about the group. These attitudes might be expressed along a continuum from extreme knee-jerk hatred, to indifference, to automatic welcoming acceptance.

It is an interesting question why people behave this way, particularly since the practice seems to have been widespread over time and across space and cultures (Fishbein 2002). One may speculate that long ago, when people lived primarily in tribes or other small communities, they feared “outsiders” or anyone who was “different”, perhaps because they brought disease or conflict. Why be nice or welcoming to outsiders if it sometimes brought harm and often little benefit? Of course, the growth of trade began to change these benefits and gradually outsiders came to be accepted, but suspicion continued in isolated settlements.

The Record for African Americans

Whatever the explanations, it was certainly the case that widespread discrimination (and much worse, including considerable violence) against African Americans existed in the United States following the end of the Civil War in 1865 (Tolnay and Beck 1995). The reasons differ, however, from most of those cited above for discrimination in other times and places. For one thing, African Americans lived in close proximity to whites under slavery and for decades following (Logan and Parman 2017). They were neighbours and some lived in close quarters to whites as cooks, servants, maids, butlers, and so forth. They were not “outsiders” to be feared for introducing contagion or bringing conquest. They had the same nationality as whites but of course had fewer political freedoms. Religious beliefs were also similar to the extent that Christianity reached slaves; both groups were largely Protestants. Nor were they competitors, eligible for the same positions in civil society except for a brief moment under Reconstruction.

It can be said that African Americans, as freed slaves, knew their place, and if they acted otherwise, rebukes were sudden and harsh. Legal slavery may have ended but other instruments of control took its place to maintain the traditional social order. The generation that “knew its place” was born under slavery and the institution inculcated deference to whites. Southern society punished African Americans if they became “uppity” or socially conspicuous.

As absurd as it sounds to us, southern apologists even went so far as to suggest that slavery had a civilising effect on African Americans, teaching them

beneficial habits of discipline, respect, and morality (Hunt 1867). An additional benefit was that the peculiar institution, depending upon the slave owner, sometimes exposed the “heathen” to Christianity.

Childhood Nutrition and Racism

In my view, most southern whites viewed slaves as cognitively deficient individuals who lacked social skills and civic aptitude. Although efficient at producing field crops (Fogel and Engerman 1974), the slaves they observed in the fields had endured extreme nutritional deprivation *in utero* and as young children (Steckel 1986a, b). Today it is well known that such deprivation permanently stunts cognitive development (Berkman et al. 2002; Heineck 2009). Sadly, the practice was profitable even though planters owned all the future income of their slaves; any investments in their health and physical development were recoverable, unlike training by firms in modern workers who are mobile.

How is this known? On average, young slave children fell below the first percentile of modern height standards, a condition that places them below the stature of modern-day children from the slums of Lagos, Nigeria. Studies of children from poor developing countries indicates that such stunting is caused by severe protein-calorie malnutrition, often accompanied by a harsh disease environment (Young et al. 1998). Knowing the price of pork, its protein content, and the value of an inch of height (Margo and Steckel 1982) determined in slave markets, one may calculate the rate of return on an investment in adequate protein that would have brought the young slaves up to modern height standards. Under reasonable assumptions about the protein deficits in their diets, this investment has a poor, and even negative, rate of return. It is even lower if one includes the higher childcare costs of supervising well-nourished children (Steckel [forthcoming](#)).

In my mind, the social order changed dramatically when the generation born *after* slavery reached maturity and sought its place in the sun. Some relief from malnutrition under slavery arrived for African Americans born after the Civil War when parents had freedom to determine the breastfeeding regimen and diets of their children. This change led to a structural break in the net nutrition of young children, which enhanced the cognitive potential of the cohort born after the Civil War. This is indicated by the amount of taxable wealth held by African Americans in 1900 and 1910 in Georgia; those born after 1865 owned about four times as much as those born prior to 1865, other things being equal (Steckel [forthcoming](#)).

Consequences

The new generation arrived on the scene with improved cognitive abilities that challenged whites in the economic and social realms. They began to accumulate property, which was a threat to whites (Duster 1970). It is my thesis that the southern white power structure began a campaign to intimidate African Americans using lynching and Jim Crow laws that was reinforced by pernicious racism (Woodward 1974). In fact, it was probably shocking to southern whites, once comfortable in their perceptions of cognitive inferiority, to discover that some blacks were outdistancing whites in wealth ownership. As stated by African-American activist Ida Wells, such uppity-achieving blacks had to be taught a lesson.

The “lessons” continued long after the early twentieth century. Even though lynching gradually diminished, expenditures on black schools remained low, segregation persisted in public spaces, and absurd tests of “literacy” blocked voting by blacks.

Adverse conditions persisted until well after World War II, up to the point when Great Society legislation began to reverse formal discrimination. Significant progress was made by the Civil Rights Act of 1964, which outlawed discrimination in hotels, motels, theatres, restaurants, and housing. The Voting Rights Act of 1965 had two important provisions: one that provided nationwide protection for voting rights and another giving special protections in states that historically restricted voting rights of minorities through devices such as bogus literacy tests. Although discrimination is on the wane in the United States, it is difficult to change attitudes that limit social mobility in other ways.

Research and Teaching Possibilities

How might research be extended on the topic of discrimination and slavery? One could study, for example, conditions that African Americans experienced while travelling. Due to segregation in housing and hotels, blacks had to make careful plans to stay in private homes or certain hotels. There was something entitled *The Negro-Motorist Green Book*—first published in 1936, and updated thereafter—which listed private homes and businesses that would reliably serve African Americans on the road. Presumably some African Americans recorded their travel plans and travails in diaries and letters while on journeys across America. It would be an interesting project to locate and summarise

these sources for insights into difficulties that African Americans faced prior to the Civil Rights revolution. Perhaps it could be connected with a family history project.

The ideas in this chapter could easily be used in teaching. One approach would be to begin by asking students to write down their own experiences, or those of friends, regarding discrimination. It is challenging to teach this subject to students if they have had little personal experience with discrimination. Then contrast the experiences of students with those known to have afflicted blacks during the nineteenth and twentieth centuries. Quite likely the harsh realities of Jim Crow and lynching are unknown to them on a personal basis.

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19

Crime and Violence

Rowena Gray

Crime is a relatively recent concept, a construction of an era that focuses more on the legality of a person's actions than their morality, as the focus may have been centuries ago. The economics of crime has emerged as an area of study in the period since the Industrial Revolution where violence has been on a long-run decline¹ and global wealth and income have been rising. The latter has meant that there is simply more to steal or expropriate, which has stimulated a proliferation of new laws and property rights which need to be enforced and policed and led to larger governments and police forces charged with protecting citizens and enforcing the state's will. In a world where life expectancy is extended, children are valued in their quality rather than their quantity, leisure time is more available (in short societies with more middle-class values and riches), and limiting social problems is an important function of government.

Economists have come late to the game in terms of addressing questions of why crime occurs, who commits crimes and how to reduce it. Interest in this subfield was sparked by Gary Becker's seminal work of 1968 in which he developed an economic model of the determinants of crime. He modelled individuals as considering work in both the legitimate and illegitimate sectors and theorised that they will choose whichever sector has the highest

¹ Clark (2007: 160) describes how homicides in England had already fallen substantially from the thirteenth century to the early modern period, and violence has continued a general downward trend since then.

expected returns, given the risk of apprehension and harshness of punishment. In general, this model performs well when estimated on property crimes. But the economic model of crime performs poorly when trying to explain violent crimes, such as homicides, assaults and rape, where there is more scope for sociological explanations.² One aspect which economic theory struggles to explain is that most crime is committed by men in their late teens and twenties.

Overcoming the Data Challenges

Economic historians have only recently become interested in crime. Initially, data on prisoners and convicts were utilised as a source in studies of heights of the general population, which proxy for nutrition and living standards in the absence of series on incomes.³ The prisoners themselves were uninteresting; they were a means to an end. Bodenhorn et al. (2012) were among the first to utilise this information to analyse the economics of crime. They showed that shorter individuals entered the illegal labour market at an earlier age than their taller counterparts, possibly because they faced worse prospects in the legal labour market.

The dearth of economic history work on crime is perhaps because of a lack of “clean” and comprehensive data sources for the era prior to the US Federal Bureau of Investigation’s Uniform Crime Reports, which were collected from 1930 onwards and are the main source of information for modern studies of the US. Historical research to date has mostly used prison or court records, and criminologists such as Eric Monkkonen (1981) painstakingly compiled arrest data to detail police activity for the urban USA, from sources ranging from police reports to newspaper accounts of criminal activities. New data sources are beginning to be identified, digitised and exploited. Ancestry.com now has records from a number of prisons, including Sing Sing and San Quentin. The US Census also documented the stock of individuals in prisons at each census date, and now 100 per cent databases for most years up to 1940 are available (for use of these data in a study of crime and education, see Campaniello et al. 2016). Gray and Peri (2018) have collected data from annual police reports, at the city level. A remaining challenge for all historical data is that crime is usually measured as a rate, per 100,000 of the population. Historical

² Kelly (2000) summarises the economic, strain and social disorganisation theories of crime in his context of analysing the impact of inequality on American crime rates. He finds that inequality had no effect on property crime, but did increase violent crime, lending weight to the strain and social disorganisation theories.

³ Bodenhorn et al. (2017) discusses and uses many prisoner samples used in this literature.

population data at high frequency can be difficult to obtain and different sources often contain conflicting numbers.

The journal *Business History* ran a special issue on white collar crime in 2018, highlighting case studies as a source of insights into topics in historical crime. Such detailed case analyses also show how the definition of crime has evolved, which is useful for putting the current environment in its historical context. The introductory essay by Berghoff and Spiekermann (2018) discusses how the phrase “white collar crime” was coined in 1939 by the sociologist Edwin Hardin Sutherland. This new type of crime only came to light as information became more easily transmitted and companies became larger, with more scope for malfeasance that could now be legislated against, and as government expanded its regulatory role from the 1930s onwards. Similarly, studies of police forces (such as Lane 1967) show how they have evolved in their functions from nineteenth-century welfare providers, moral guardians and unrest preventers to crime fighters using sophisticated techniques to prevent and detect a wider variety of crimes.

Policy Experiments of History

An early area of interest for economists was in the effects of making products illegal, such as alcohol during 1930s Prohibition in the USA. These types of policies are not uncommon throughout the world and are implemented based on the belief that violent crime is fuelled by substance abuse. This is somewhat born out in studies such as Wong (1995), who argued that decreased alcohol consumption contributed to the decline in crimes in nineteenth-century England, and Mehlum et al. (2006), who look at weather-induced changes in economic activity to show that violent crimes increased in times of prosperity which coincided, they argue, with low beer prices. Jeffrey Miron and Emily Owens are most commonly associated with prohibition studies. In theory, there is a tension between decreased substance consumption—as Dills and Miron (2004) documented for alcohol prohibition—which might be expected to reduce violence, and the emergence of black market activity, which likely increases violence. Miron (1999) prepared time series data on the USA's homicide rate per 100,000 people for the years 1900 to 1995 and found a positive association between that and expenditures on enforcement of drug and alcohol prohibition. Owens (2011) re-examines this question using state-level variation in prohibition laws which existed before and after Federal prohibition was enacted, from 1920 to 1933. She found that, once demographics are controlled for, prohibition did not increase homicides and concludes that

urbanisation was the key driver of murders before 1940. Jacks et al. (2017) has developed county-level measures of the intensity of prohibition in the years after the 1933 federal repeal. This type of approach might be extended in the future to revisit this question with finer crime data on a broader set of crime outcomes.

An expanding topic for economic historians concerns the impact of immigration on crime. There is a huge role here for economic history to inform the current debate on this topic, which is raging across the developed world in the wake of the recent refugee crisis and relative unpopularity of labour migrants among developed country voters. Countries like the US maintained an open border policy until almost the 1920s and experienced huge inflows of migrants, bringing the share of foreign-born among the working age population very close to current levels. Bodenhorn, Moehling and Piehl have been the main researchers in this area to date. Moehling and Piehl (2009, 2014) were the first to explore differences in incarceration rates for immigrants and natives, describing an initially elevated level for immigrants before a convergence in criminality by 1930—which can mostly be explained by the changing demographics of immigrants, as older groups are less likely to commit crimes. Their work with Bodenhorn in 2010 looked at incarceration rates by nativity for antebellum Pennsylvania. They document again a convergence in behaviour for immigrants—the Irish were the main group at this time and their legal labour market possibilities may have risen over time. These studies have generally used prison data, which measure the stock of individuals in prison at a given time and do not always provide information on the crimes for which they were incarcerated. This will be a growing area of research as other sources are exploited, such as police reports and arrest and commitment records, that detail criminal histories. There remains no clear consensus among economists using modern data on this subject, with recent contributions offering opposing results.⁴

There are many policy and natural experiments in history that can be exploited. An important question in the crime literature is how income and business cycle shocks affect crime. Wong (1995) and Mehlum et al. (2006), mentioned above, provide insights on this question, focusing on the UK and Germany, respectively. Bignon et al. (2015) find that the phylloxera crisis in the French wine industry in the late 1800s led to increased property crimes but reduced violent crimes in regions that were more intensively affected. Fishback et al. (2010) use the policy experiment of the New Deal legislation

⁴See, for example, Chalfin (2014) who finds that immigrants to the USA do not increase crime, and Spenkuch (2014) who finds the opposite.

in the 1930s to evaluate how keeping household income high in times of distress affected social outcomes including crime. They found that both work relief and welfare payments during the decade reduced property crimes, with the biggest benefits coming from the work replacement side, because people were occupied rather than committing crimes.

Recent newly discovered data sources at a microlevel are facilitating in-depth analyses of bias at various levels of policing and the court system, which are of critical concern today. These data may be difficult to come by today, but are available historically—but only after some time effort by the researcher. Bodenhorn (2009) used information on two Pennsylvania courts from 1819 to 1876 to look at the determinants of sentencing. He found that, surprisingly, immigrants tended to receive lighter sentences (the Irish in particular received shorter average sentences, controlling for other factors), and that the wealthier classes received the harshest sentences. This is interpreted as characteristic of the age of the common man, when most cases went to a trial in front of a jury of your peers and the notion of fairness across social groups was paramount. Vickers (2016) constructed data from trial records for England and Wales for 1870–1910 and explores class-based differences in conviction and sentencing. He finds that the early years are characterised by higher status individuals receiving higher sentences for property crimes, but the reverse for violent offences. The property crime effect dissipates by 1910, but for violent offences the result endures. The analysis of these issues prior to the twentieth century is very useful, because since 1900 plea bargaining has become so much more common, meaning that modern analyses of bias among judges and juries may itself be biased by selection into jury trials.

A final new area of focus for economic historians is on the effects of the environment on crime, which follows burgeoning interest in this issue for those using modern data. Feigenbaum and Muller (2016) suggest that cities' use of lead pipework significantly increased homicide rates for the years 1921–1936. It is generally accepted that heavy metals, such as lead, may alter brain functions and behaviour, providing a mechanism for this result. This is the first paper to look at this historically, and with only one outcome (homicide), so more work may be done to explore the broader crime effects in this era.

Summing Up

Teachers and students of economic history may be interested in how historical episodes can inform the current debates about the roles of demographics, business cycles and inequality as drivers of crime rates, and how policy

experiments such as immigration restriction or prohibition of alcohol or drugs affect crime rates. With the cost of data collection decreasing all the time, new sources can facilitate further in-depth analyses of these questions, and the economic historian has the advantage of not having to worry about privacy concerns, ethics committees, or the resistance of law enforcement agencies in reporting their current performance. I expect to see an explosion of historical crime studies and a broadening of topics, using data at both the city/county level, as well as the individual level, making use of criminal careers to examine questions such as what makes a criminal and what causes recidivism.

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20

Business Ownership and Organisation

Michael Aldous

The relationship between the different ways in which business is owned and organised, and the effects these choices have on the performance of firms and economies, is widely debated in economics, management, finance, law, business and economic history. Optimising the form of ownership and organisation can increase the scale and scope of operations, whilst improving productivity, efficiency and the capacity for innovation. The role of the corporation, or the joint-stock company, has been central to these debates.

In theory, the benefits of incorporation run as follows. The corporation is underpinned by five basic legal characteristics—separate legal personality, limited liability, transferable stock, delegated management and investor ownership (Kraakman et al. 2004). In particular, limited liability and transferable stock incentivise individual investors, their risk diversified amongst multiple shareholders and protected by liabilities limited to the extent of their shareholding. This allows corporations to form deeper capital reserves and retain them for longer periods of time, as investors can sell their shares rather than take their capital out of the firm. A separate legal personality, deeper capital and longer investment horizons allow corporations to invest in fixed assets and undertake activities such as R&D on a greater scale than other forms of ownership, such as the partnership.

This has provoked three main areas of study in economics: (1) identifying the legal and institutional conditions required to enable efficient incorporation, (2) quantifying the effects of incorporation on firm and economic performance and (3) examining how corporations can be effectively managed. In particular, the focus has been on how to address the principal-agent problems

created by the separation of owners and managers inherent in the corporate ownership structure. The interest in performance and institutions has encouraged historical study to provide longitudinal data and understand the context in which institutions have emerged and evolved.

Corporations in Economic History

The dominant historical narrative describing the role and importance of the corporation shows that from the mid-nineteenth century, institutional developments in the UK and the US completed the legal formation of the corporation, significantly reducing costs and barriers to incorporation. This led to a rapid proliferation in the number of corporations in the late nineteenth century. They dominated emerging asset-intensive industries, which required extensive capital investments such as railways, oil, chemicals and automobiles (Schmitz 1993). This period also saw experimentation with the organisation and management of firms, as new hierarchical structures such as the multidivisional form were used to allow managers to efficiently integrate and control larger organisations (Chandler 1977). By the early twentieth century, firms of unprecedented scale and scope, predominantly organised as corporations, became the dominant form of business organisation in industrial economies.

While providing some support for the notion that the corporation was a key enabler in the transformation of industrial economies, historical study has raised a number of questions that challenge and nuance the contemporary study of firms. Questions related to timing, causality and the long-run effects on development have led economic historians to rethink the debates around corporate law and institutions, the effects on the performance and management of the corporations. These continue to be highly active research areas which address both historical and contemporary audiences. This chapter outlines three key debates surrounding the corporation from the perspective of economic history and relates them to wider discussions in economics.

Institutions and the Law

The evolution of the corporation as a distinct legal entity was a historical process and economic historians have sought to identify when, where, and why it occurred—particularly seeking to establish the emergence of the “modern corporation”. This was initially identified as occurring in the US in the early twentieth century, as legal changes secured protection for investors and

growing capital markets encouraged tens of thousands of Americans to buy shares (Berle and Means 1968). Individual shareholders were now part of highly diffuse bodies of ownership, which meant control of the firm was vested in salaried managers as individual shareholders could no longer exercise legal ownership rights to dictate the running of the firm. Economists are especially interested in the corporate governance issues raised by this extreme separation of ownership from control, discussed later in this chapter, but it also provoked examination of why conditions in the US had been particularly favourable to this process.

The dominant thesis has focused on the historical common law origins of the US legal system, which is claimed to be more flexible and favourable towards commerce, as opposed to the restrictive civil law systems found in France and Germany (La Porta et al. 1998). Known as the “law and finance school”, this approach has been widely used to claim causation between commercial law and economic development, with comparative research revealing that nations and regions lacking the institutions to shape flexible commercial law perform relatively worse (Kuran 2012).

Yet, the timing and location of the divorce between owners and managers has been reassessed, with extensive evidence showing that these features were already present in Britain in the late nineteenth century (Foreman-Peck and Hannah 2012). Similarly, the legal origins thesis has been roundly attacked, predominantly on the grounds that, although it pertains to be a historically grounded argument, empirical historical studies do not bare out clear causation between common law and commercial development (Musacchio and Turner 2013).

These findings have spurred further efforts to examine the legal origins and evolution of the corporation, investigating antecedents dating from late medieval roots. Research shows a long and convoluted process driven by competing interests, much of which occurred outside of the US and Britain, in advanced financial markets such as the Netherlands (Gelderblom and Jonker 2004; Harris 2000). Accepting that there is no defined trajectory to form the “right” institutions opens many possibilities for further research to understand the emergence and effects of institutional arrangements on commercial activity.

Owners, Managers, Principals and Agents

The separation of owners and managers wrought by the corporate ownership structure created distinct challenges of governance. The critical question being: how can the owners, or principals, ensure that their managers, or

agents, act in their interests? These problems are exacerbated as firms increase in size, increasing threats of opportunism and moral hazard, while complicating the coordination and the alignment of interests. In economics, the challenges of these principal-agent problems have been extensively theorised (Jensen and Meckling 1976; Alchian and Demsetz 1972).

This body of theory has been adopted by economic historians to explain the historical evolution of corporations. In particular, it has been used to examine the growth of early corporations, such as the chartered trading companies including the East India Company and Hudson's Bay Company (Carlos and Nicholas 1990). Due to the geographically dispersed nature of their activities, the owners of these firms needed to find ways to align the interests of their distant managers. Innovations in the structures of reporting, monitoring and incentives enabled these firms to reduce the costs of agents, allowing them to expand their operations in the eighteenth century and open up international markets (Hejeebu 2005; O'Leary et al. 2002). Similarly, innovations in monitoring and incentives in the early twentieth century allowed more effective control of giant industrial firms (Chandler 1990).

Yet, research drawing on agency cost theory has shown that the corporation failed in certain industries due to the inherent organisational problems caused by the separation of owners and managers (Hilt 2006). Historical study has shown that business forms, such as the cooperative and partnership, have governance features that resolve agency problems more efficiently than the corporation. In industries where monitoring of agents and customers was difficult, the corporations often failed as the incentive structure of the partnership and cooperative bonded distant agents and monitored customers more efficiently (Guinnane 2001; Henriksen 1999; Aldous 2015). The historical analysis of principal-agent problems has done much to reveal the importance of other forms of business ownership and offers great scope for further investigation to assess which forms of ownership and governance matter in addressing different challenges.

Do Corporations Matter?

The argument that large, hierarchically integrated firms, organised as corporations, are the optimal form of business organisation was strongly linked to the rapid economic growth of the US across the twentieth century (Chandler 1977). Dramatic increases in productivity, enabled by integrated corporations, were correlated to rising GDP and other measures of living standards (Broadberry 1997). Similar to the law and finance argument, these claims were strengthened by comparative research between nations, where a lack of

widespread incorporation and large vertically integrated firms was seen as a key factor in limiting productivity and economic growth. In particular, the growth of the US was contrasted with the economic stagnation of the UK in the early twentieth century, and presented as a function of British entrepreneurs' failure to widely adopt the corporate form (Lazonick 1992).

The importance of the corporation as an engine of economic growth saw pronouncements such as this from Micklethwait and Wooldridge (2005: xv), 'the most important organisation in the world is the [joint-stock] company: the basis of the prosperity of the west and the best hope for the future of the rest of the world', become commonplace. However, others are less sure; as Jensen (2000: 3) noted, 'in spite of this relative success it is clear from the evidence of the last twenty five years that the corporation has failed in many ways as an organising device'. These competing claims are widely debated in economic history, with both theoretical and empirical studies challenging the importance and dominance of the corporation.

In particular, studies have questioned the preference for the corporate form when entrepreneurs could select from a menu of alternative organisational forms. By the early twentieth century, a range of legal forms of business ownership were available in the leading industrial nations. Quantifying the choice of different forms over time has revealed a preference for privately owned companies in the UK, France and Germany (Guinane et al. 2007). This has motivated the question: why would entrepreneurs make the mistake of choosing another form of organisation when incorporation was freely available (Lamoreaux and Rosenthal 2006)?

Further, the extent to which the corporation did "win the competition" has been questioned. Increasingly, the corporation is regarded as critical for industries with intense fixed-asset investments and high costs of hold up but less important in other sectors (Hannah 1999). Indeed, as industrial structures changed across the twentieth century, the rationale for the widespread use of the corporation has become less clear. Further historical study will help to contextualise the role of the corporation, both at the level of individual industries and national economies, improving our understanding of where and when incorporation is effective.

The Importance of History

These debates have opened a rich seam of opportunity for both historians and economists. In economic history there is no consensus on a historically or theoretically optimal firm type and debate continues to rage around the role

and importance of the corporation. Why different business forms emerge, proliferate or fail, and the effect this has on performance and economic development, remain open questions. Similarly, historical study has raised questions about how institutional arrangements affect commercial law and therefore which institutions matter in enabling entrepreneurs to respond successfully to the challenges they face.

Conversely much current research in economics continues to focus on modelling optimal structures and systems to resolve principal-agent problems within corporations. Little interest has been spent in considering the importance of other forms of business ownership in addressing economic challenges or assessing the impact that use of different forms of ownership has on industry and economic performance. Economic history has fundamentally challenged and advanced the debates around business organisation. It offers the opportunity to improve understanding of the rationale by which entrepreneurs make choices on firm ownership and to assess the impact of their decisions.

Historical research on the corporation has drawn on a wide range of theories and methodologies. Detailed industry and case studies (e.g., Carlos and Nicholas 1990) are extensively used to identify how firms and institutions have functioned. To capture the evolution and effects of processes more effectively, longitudinal (e.g., Chandler 1974) and comparative studies (e.g., La Porta et al. 1998) have been employed, whilst efforts to quantify scale and change draw on a range of descriptive statistics (e.g., Guinnane et al. 2007; Aldous 2015). Interest in the causal relationships between choice of organisation form and subsequent effect on economic performance has seen the use of more formalised modelling (e.g., Lamoreaux and Rosenthal 2006) and econometric methods (e.g., Hilt 2006). This plurality of methods offers economists and historians interesting opportunities for collaboration.

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21

Competition and Collusion

Alexander Donges

The preservation of competitive markets is a major cornerstone of modern economic policy. Cartels and anti-competitive practices are restricted, and competition authorities control mergers to avoid the excessive accumulation of market power. Their main goals: the protection of consumer interests and the creation or maintenance of a level-playing field between companies. Competition among firms reduces allocation inefficiency, but policymakers also have to take production efficiency into account, since firms with high market shares might be able to produce goods at lower costs by realising economies of scale (see Motta 2004, for an overview).

This trade-off is no new finding. In his seminal studies, Joseph Schumpeter already assumed a negative effect of competition on innovation and progress (Schumpeter 1934). This perspective was influenced by the experience of the Second Industrial Revolution, when industrial concentration grew rapidly and large corporations dominated in emerging industries such as electrical engineering or the chemical sector (Chandler 1994). These companies were at the spearhead of technological change, even though market concentration was high. Today, a similar development can be observed in the information technology sector. The view that a certain degree of market power might be conducive to economic progress is supported by the findings of Aghion et al. (2005) that suggest a non-linear relation between competition and innovation.

Economic history may give us a better understanding of how market structures evolve over time and to what extent competition affects innovation and growth in the long run. Moreover, a look into the past is useful to study the

behaviour of firms at the micro-level, in particular when no modern data are available. Data on modern cartels, for example, are difficult to obtain, since cartels and other forms of anti-competitive practices are prohibited in all major industrial countries and firms try their best to keep any illicit agreements secret. Most illegal cartels are not uncovered by the competition authorities until one of the cartel members deviates from the cartel agreement to take advantage of a leniency programme. As a result, we only have information about failed cartels, but not about successful cartels that remain secret. Moreover, it is uncommon that illegal cartels collect data or documents as they could be used as items of evidence before court. To understand why cartels arise and how they work, one may therefore focus on cartels that operated in periods before the introduction of effective competition laws (see Levenstein and Suslow 2006, for examples). In this context, it must be emphasised that the restriction of anti-competitive practices is a rather modern concept, as the following overview shows.

Anti-competitive Institutions in Early Modern Europe

The restriction of competition is no specific characteristic of the industrialised age. In Europe, anti-competitive institutions were widespread for many centuries. Guilds emerged in the medieval period to regulate production and local markets, and powerful trade federations, such as the Hanseatic League, were able to restrict the exchange of goods at the supra-regional level. Among economic historians, there is a vigorous debate about the economic effects of guilds on the economy. While some scholars point out that guilds ensured high product quality and education standards, others highlight the negative effects resulting from the restriction of competition (see Ogilvie 2014, for an overview). The creation of market entry barriers, and a high degree of regulation in favour of the guild members' interests, lowered economic incentives for innovation. Mokyr (1992) and other scholars provide evidence that guilds impeded technical change by preventing the use of modern, labour-saving technologies. However, along with the rise of factory-based industries, the influence of guilds diminished. This process was fastest in England; guilds persisted longer in Continental Europe. However, at the beginning of the nineteenth century, guilds disappeared in most European states as a consequence of the civil and economic liberalisation after the French Revolution (e.g. Acemoglu et al. 2011).

The Emergence of Cartels in the Nineteenth Century

The process of economic liberalisation created an environment more conducive to competition, but the voluntary formation of cartels was not yet restricted. In fact, in the second half of the nineteenth century, formal and informal cartel agreements became increasingly popular. Cartels were not only used to regulate prices and production quotas but also to standardise products or to share and protect technology. In the 1880s, cartelisation accelerated in reaction to decreasing prices, in particular in the United States (see LeClair 2011, for an overview).

Like in most parts of Europe, cartel agreements were legal in the United States, but it was not possible to enforce cartel agreements in courts. The lack of legal protection made it difficult to form stable cartels, since it was harder to punish cartel members if they violated agreements. As a result, the *trust* became one of the most popular models of industrial concentration in the United States (see Bittlingmayer 1985, for the process of concentration). In contrast to modern mergers, firms that entered a trust could maintain a certain degree of independence, even though they were administered by trustees to ensure a common price and production policy of all trust companies. Controlling prices and production was the main objective behind trusts, while mergers typically also focus on cost-cutting rationalisation measures or economies of scope. The most prominent one is the Standard Oil Trust, which was created by John D. Rockefeller to control the North American oil industry. Granitz and Klein (1996) illustrate Rockefeller's predatory, anti-competitive strategies that aimed at excluding existing rivals from the market and setting high entry barriers for potential competitors.

Competition Policy in the United States

Trusts became increasingly powerful, but in contrast to Europe, the anti-trust movement, which was substantially pushed by farmers who had been hurt by excessive prices, was able to impel the United States Congress to pass laws aiming at the re-establishment of fair competition and the punishment of anti-competitive business practices (see Peritz 2000, for an overview of early competition policy in the United States). In the literature, the starting point of modern competition policy is typically associated with the Sherman Act of 1890, even though it was not the first anti-trust law (Ulen 1980). The aim of the Sherman Act was

to re-establish competition, to avoid excessive market concentration, and to protect consumers and small businesses. In this regard, the Sherman Act set the ground for a policy framework that spread across Europe after the Second World War and that persists until today.

Under the Sherman Act, the government was able to act against abusive market concentration, but it took several years to restrict market power substantially, since the trusts first circumvented the rules by changing their organisational structure. Initially, the courts followed a narrow interpretation of the Sherman Act. While cartels and cartel-like organisations such as trusts were prohibited, full mergers were still allowed. According to Bittlingmayer (1985), this kind of “legal loophole” triggered the first great “merger wave” at the end of the nineteenth century (see O’Brien 1988, for another view on the merger wave). The trusts were transformed into holding companies by fully incorporating former trust members. This merger wave created large, horizontally integrated holdings with high market shares.

However, Theodore Roosevelt pushed the administration to act against these holding companies. In 1904, the Supreme Court decided that the Sherman Act could also be applied to break up holding companies, which was interpreted as a landmark decision in favour of a more restrictive competition policy. As a result, holding companies with excessive market power were broken up. Rockefeller’s Standard Oil Company of New Jersey, for example, was split in 1911.

Moreover, competition policy was strengthened under the Clayton Act of 1914, which explicitly aimed to restrict horizontal mergers, and create a separate competition authority: the Federal Trade Commission. Further competition acts were passed during the interwar period and after the Second World War, which gradually expanded the power of competition policy, for example, by introducing a merger control that also considers vertical concentration.

The European Perspective on Cartels

While the Sherman Act marked a major policy shift towards an active and effective competition policy in the United States, Europe remained in general cartel-friendly (see Gerber 2001 for an overview). Until the post-Second World War period, the European governments took hardly any legal actions against the accumulation of excessive market power. Powerful business lobbies acting in favour of a weak competition policy may provide a potential explanation for this fundamental difference, but, from the European perspective, there were also economic advantages. Webb (1982), for example, provides

evidence that price cartels were considered as effective means to avoid ruinous competition and to reduce price fluctuations in the iron and steel industry. Such measures stabilised the economy in business cycle downturns and avoided the massive layoff of workers.

Competition Policy in the United Kingdom

In the United Kingdom, there was no specific cartel legislation before the Second World War. The common law doctrine of the restraint of trade only restricted the private enforceability of anti-competitive agreements, but it was not applied to impede the creation of cartels and monopolies (Scott 2012). Thus, market concentration and cartelisation increased over time (Levy 2018), and the share of cartelised production remained high until the post-Second World War period (Broadberry and Crafts 2001).

A major policy shift did not occur until 1948, when the Monopolies and Restrictive Practices (Inquiry and Control) Act was passed (see Scott 2012, for an overview). The Act of 1948 did not restrict cartels per se, as the Sherman Act did in the United States. It only set a legal framework for the monitoring of cartels in order to identify agreements that operated against the public interest but did not specify any consequences. As a result, there was no significant reduction of cartelisation so that the actual success remained limited. Yet, the Act of 1948 was important since it paved the way for a gradual improvement of competition policy in the United Kingdom. Worth mentioning is, for example, the 1956 Restrictive Trade Practices Act, which required the registration of specific cartel agreements. Based on data from this cartel register, Symeonidis (2002) analyses the effect of cartel policy on competition. This study shows in an exemplary way how economists may take advantage of historical cartel data.

Competition Policy in Germany

Perhaps the most prominent example of a highly cartelised economy is Germany in the late nineteenth and early twentieth century. German cartels were able to widen their influence and cartelisation increased massively at the end of the nineteenth century. The degree of legal protection provides a potential explanation for this development. In 1897, the Imperial Court of Justice decided that cartel agreements had to be treated in the same way as other contracts under the German civil law. Consequently, it was possible to form strong cartels that could legally punish their members if they violated or circumvented cartel agreements.

Legal protection does not only explain the high number of cartels in Germany but also the way in which cartels were organised (see Fear 2008, for an overview). The *syndicate* is the most prominent type of cartel that emerged during this period. Syndicates were organised as non-listed joint-stock companies or companies with limited liability, which were owned by the cartel members. In contrast to weaker cartel types, syndicates were not only used to regulate prices and production quotas but also to centrally organise the distribution of cartelised goods. These higher-order cartels were widespread in the commodity sectors and attracted most attention in economic history. Hughes and Barbezat (1996) and Webb (1980), for example, studied German iron and steel cartels, and Burhop and Lübbers (2009) analysed the effect of cartelisation on productivity in the coal mining industry.

After the First World War, German policymakers discussed the regulation of cartels (see Feldenkirchen 1992, for an overview). The political debate was both influenced by the hyperinflation and the conflict between big business and labour unions about large-scale socialisations, and it ultimately led to the German Cartel Decree of 1923. However, this law was only a meagre compromise, since cartels remained legal, and even the abuse control was not clearly specified. After the stabilisation of the German currency in 1924, there was no political will anymore to regulate cartels. The fact that the number of cartels increased after 1923 supports the view that the German Cartel Decree did not restrict cartelisation.

In West Germany, a major policy shift did occur after the Second World War, when the United States enforced a strict ban of cartels in those areas that were occupied by the Western Allies. The ban of cartels was also incorporated in the Law Against the Restriction of Competition, which was passed by the Parliament of the Federal Republic of Germany in 1957. Even though exceptional rules were maintained, which made it possible to get an approval for the creation of cartels if certain conditions were fulfilled, this law marks the starting point of active competition policy in Germany. Other European countries followed a similar path by gradually adopting a competition policy influenced by the US role model.

Perspectives Beyond Economic History

Analysing the evolution of competition policy and its effects on market structures and business strategies opens interesting perspectives beyond economic and business history. While the legal background is already well studied for

most countries, there is still a lack of quantitative research, in particular with regard to the economic consequences of competition policy shifts. The studies of Bittlingmayer (1985) and Symeonidis (2002) provide useful examples that show how historical data can be used to analyse the effects of policy changes. Economic history can help us to get a better understanding of the inner life of cartels. Genesove and Mullin (2001), for example, analyse cartel strategies based on detailed notes from meetings of a sugar-refining cartel in the United States. From the perspective of industrial organisation, one could also raise general questions about the effectiveness of cartel agreements, since a high number of mergers also occurred in cartel-friendly countries such as the United Kingdom and Germany (see Kling 2006, for the pre-1914 German merger wave). By using historical data to analyse the firm's decision to participate in cartels and mergers—to some extent a trade-off—one could find results that allow for a better understanding of market structures in general. In this regard, economic history might provide new insights that are relevant for empirical research on industrial organisation. Furthermore, there are links to the finance literature that analyses mergers and merger waves (see Martynova and Renneboog 2008, for an overview).

Data Sources

The use of historical data can push empirical research on cartels, mergers, and competition policy. In this regard, economic history can be linked with research in other fields such as industrial organisation, law and economics, and finance. The use of meaningful historical data can provide real added value to empirical research in industrial organisation or finance, but its collection is typically not an easy task. Official cartel registers, as used by Symeonidis (2002), are one possible source of data. Fellman and Shanahan (2015) provide an overview of countries where cartel registers were put in place in a specific time period. Data from cartel registers are particularly useful for cross-industry analysis. By contrast, a within-industry analysis of competition could require more detailed information than is available in a cartel register. Additional data sources include archival records from public authorities (e.g. irregular industry surveys), business associations (e.g. minutes of meeting), and companies (e.g. internal memoranda about competitors). Apart from archival records, one may also take advantage of handbooks that include firm-level information, such as financial data of joint-stock companies.

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22

Human Resources and Incentive Contracts

Andrew Seltzer

Personnel economics is the study of contracts between workers and firms. It covers various aspects of the employment relationship, including hiring, training, job assignments, and compensation. At the heart of most employment contracts is the well-known agency problem (Jensen and Meckling 1976). The principal (the employer) hires an agent (the worker) to perform a series of tasks. However, the principal and agent share neither the same underlying objectives nor information sets. Typically, employers are assumed to be risk-neutral profit maximisers, whereas employees maximise a utility function containing some combination of compensation and effort level, which is assumed to create disutility.¹ Employees possess greater knowledge of their own abilities and their actions undertaken on the job than do employers, resulting in the problems of adverse selection (hidden information) and moral hazard (hidden action). The fundamental problem of personnel economics is to design contracts to overcome these problems, in other words, to use hiring, pay, promotion, monitoring, and dismissal practices to align the interests of workers with those of the firm.

In this chapter, I examine the underlying issues in personnel economics in theory and in history. I outline the main approaches to resolving the problems of adverse selection and moral hazard. I examine how economic historians have compiled data from government records and company archives to

¹The assumptions of risk neutrality and profit maximisation are for the sake of modelling convenience. Many of the underlying conclusions of personnel economics fundamentally do not rely on these assumptions. For example, the nature of firm/worker contracts is much the same in non-profit organisations.

examine hiring, wages, job assignments, promotion, and other personnel practices at a variety of medium-sized to large firms. Finally, I examine the role of historical analysis in both research and teaching personnel economics. In many cases, the available historical data are much richer and more comprehensive than contemporary data, which are often treated as proprietary and are therefore very difficult to access.

Core Topics in Personnel Economics

The hidden information (adverse selection) problem is particularly important in hiring decisions. Hiring the “right” workers is important to firms facing high fixed per-worker costs. However, hiring the right workers is inherently difficult. Workers vary along a number of different attributes, such as underlying ability, motivation, loyalty, honesty, labour force attachment, and so on. Many of these attributes are difficult to observe prior to commencing employment. In some cases, important worker attributes will depend on the (*ex ante* unknown) quality of the worker/firm match. Moreover, “low-quality” prospective workers have no incentive to reveal their true “type”.

Because hiring is both important and inherently difficult, firms use a variety of strategies to overcome the asymmetric information problem and screen the applicant pool. These include using formal credentials or test results as screening mechanisms, even when these do not directly affect individuals’ ability to perform a job (Spence 1973; Weiss 1995); use of referrals from existing employees (Montgomery 1991); and designing contracts to encourage *self-selection* by the “right type” of prospective employees (Lazear 2000).

The hidden action (moral hazard) problem is particularly important for employment relationships after a worker has been hired. Workers make many day-to-day decisions about their jobs. Employers have imperfect information about these decisions and often cannot observe whether they are *ex ante* optimal. Even if they fully observe outcomes, *ex post* they may not be able to infer whether a worker’s actions were optimal because external environmental factors also affect outcomes.

There is a huge literature addressing the moral hazard problem. One strand of this literature considers ways to tie wages to output, such that the contract becomes self-enforcing and there is less need for firms to monitor effort (Lazear 2000; Seiler 1984). Another strand considers a mix of rewards for good performance and dismissal for shirking, where shirking is caught with some probability determined by existent monitoring technology. Efficiency wage models assume that workers receive above-market wages,

thus the penalty for shirking is the possibility of facing lower wages or unemployment upon dismissal (Shapiro and Stiglitz 1984). Deferred compensation models assume that younger workers are paid below-market wages and older workers are paid above-market wages, and thus dismissal results in lower lifetime earnings (Lazear 1979). Tournament models focus on the long-term prospect of promotion as an incentive to supply effort (Lazear and Rosen 1981; Rosen 1986).

A general theme of this literature is that there is no single solution for all personnel issues. Industry and worker characteristics, external labour markets, technology, and broader institutional context all matter to the optimal design of personnel policies. History inherently matters to the design of optimal contracts between firms and workers simply because specific context is important.^a

Personnel Economics and Economic History

Like many fields of economics, mathematical theories of personnel preceded empirical work.² These theoretical models consider specific aspects of personnel practices but do not look at incentives within firms in a holistic manner. In practice, firms face a variety of contracting issues which may differ considerably across workers, based on their roles or skill levels. Most firms use multiple contractual mechanisms that are adjusted to different types of workers. While these models provide useful and plausible insights, empirical evidence examining a range of hiring, wage, and promotion practices in medium- to large-sized contemporary firms dates back only to the 1990s (Baker et al. 1994a, b).

A fundamental problem facing empirical work is the availability of suitable data. Firms generally regard recent employment records to be confidential and proprietary, and thus relatively few firms have shared data with academic researchers. Most of the firms which have allowed access have restricted the information which can be disclosed and thus limited researchers' ability to examine the relationship between institutional characteristics and personnel practices (Baker et al. 1994a, b). Other firms have been more forthcoming with information, but these firms tend to be small and to employ a very specialised and idiosyncratic group of workers (Lazear 2000; Bandiera et al. 2005, 2011). Matched Employer-Employee (MME) records collected by a

² There does exist a much older descriptive literature in human resources which examines worker/firm relationships and internal labour markets (see Taylor 1911; Doeringer and Piore 1971).

growing number of countries provide a second source of contemporary personnel data. These records have the advantages of providing both worker and firm identifiers, following workers and firms over time, providing very large samples, and having low degrees of measurement error. However, MME records are often limited in the amount of information collected and thus in their ability to describe personnel practices.

Because contemporary data are inherently limited, historical research has accounted for a large share of empirical research in human resources. Historical studies have several advantages over contemporary studies. Firms generally consider historical data to be less proprietary or confidential than contemporary data, and thus researchers have been able to gain access to employment records of a large number of employers. Secondly, there exist a variety of records from archives and government publications containing personnel data. Often scholarly business histories have been published for firms which are willing to release these records, and thus it is possible to provide considerable background and context. Third, many of the available historical datasets cover very long periods of time, enabling researchers to examine long-term compensation strategies and incentives over entire careers.

Because of these advantages to working with historical records, a growing number of historical personnel datasets have been digitised and studied by scholars. In one of the earliest and most unusual examples, Brunt (1950) examines papyrus manuscripts containing information about the pay and pensions of Roman legions. Other examples are far more recent and are based on wage books and other administrative records that have been released in government publications or kept in company archives. The list below is not intended to be comprehensive but rather illustrative of the range of available historical personnel records that have been studied by economic historians.

Large trading companies were among the first private companies to keep detailed records. Rei (2014) uses records of the Dutch East India Company from the century to examine the career histories of Company “servants”. Banks, by their very nature, need to keep extensive records of their business transactions, and so the personnel records have survived for numerous banks. Boot (1991) examines salaries at the Bank of Scotland in the eighteenth and nineteenth centuries. In a series of papers, I have used nineteenth and twentieth century records from Australian and British banks, primarily the Union Bank of Australia and William Deacons’ Bank (Manchester), to examine a variety of practices, including hiring, promotion, wages, and separations (Seltzer and Merrett 2000; Seltzer and Simons 2001; Seltzer 2011, 2013). The William Deacons’ Bank records contain data on female employees from 1915, and I have used these records to examine female employment and wage, and

the impact of female employment on male workers. Blue collar employment has been the subject of fewer studies; nevertheless, there are studies of several employers. Railways are the largest and most bureaucratic blue collar employers, and studies have been undertaken covering railways in several countries including Canada (Mackinnon 1996), the United Kingdom (Howlett 2004), and Australia (Sammartino 2002). Ford Motors is another large blue collar employer which has released its personnel records. Raff and Summers (1987) and Raff (1988) examine Ford's use of efficiency wages and reasons for implementing the US\$ 5 working day, respectively. Maloney and Whatley (1995) and Foote et al. (2003) examine differences in wages and other employment practices for white and African-American workers.

Future Research Directions

There exist numerous untapped company and government records, many of which will contain records about various aspects of human resources and personnel practices. The source materials that have been used to date were frequently selected simply based on comparative ease of use, for example, due to manageable numbers of observations, ease of access to records, and the extent to which records have been catalogued. As more company archives are fully catalogued (and, increasingly, digitised) the opportunity for additional study increases.

The availability of yet-unused data from both previously studied and yet to be studied employers opens up the opportunity to explore new research directions in personnel economics. To take an example from my own research, the banking records that I have inputted and analysed end in 1941 in the case of Williams Deacons, and effectively end in the 1950s in the case of the Union Bank. Later records for these firms exist, but have yet to be coded or examined. The later period witnessed important changes in both technology and in the supply of clerical workers. From the 1970s, ATMs increasingly replaced bank tellers. Computer technology made monitoring workers easier and less costly, reducing the number of back office clerical staff needed. Women increasingly remained in the labour market after marriage and overall education levels increased, creating a "thicker" market for clerical labour. Simple economic theory suggests these changes must have fundamentally altered the labour market for bank workers; however, to the best of my knowledge there exist no studies of how personnel practices changed in response to these underlying changes in the labour market. The broad pattern of changing product markets, labour markets, and technologies influencing contract design is likely to apply across many other industries.

Teaching Personnel Economics with History

Personnel economics is now taught in many universities, either as a separate course or as part of labour economics or managerial economics courses. My own experience is that students are able to relate much more directly to quantitative case studies covering a specific firm operating in a well-defined context than to either theoretical models or empirical studies covering a broad sample of workers. Context matters and is one of the comparative advantages of economic history.

To take a specific example, I have long used my own work on personnel practices at the Union Bank of Australia in the late nineteenth and early twentieth centuries as a seminar reading in my undergraduate personnel economics course (Seltzer and Merrett 2000). In the article, employment records covering the complete careers of about 1700 “officers of the bank” who were employed at the bank in 1887 or entered between 1888 and 1900 are used to identify personnel practices. Briefly, most new hires were juniors straight out of secondary school, although the Bank also hired some experienced workers, most of whom had worked for overseas affiliates. Juniors were paid according to a formal salary scale for seven years and an informal scale for approximately another 15 years. The formal salary scale was very similar to those for civil servants, insurance clerks, and railway clerks, suggesting an integrated white collar labour market. Juniors tended to move between branches every few years, and those who moved more were more likely to later be promoted. Promotion was slow, and working up to the level of manager took at least 10 years for even the most capable juniors. Further advancement, to a manager of a larger branch or the inspection staff, also was a slow process. Promotion, particularly to the level of branch manager, was associated with above-normal pay increases. Employees who were never promoted to branch manager typically spent the last approximately 20 years of their career working in the back office of one of the larger branches. All employees received annual pay increases, and a substantial proportion of total pay increase over a career occurred after 20 or more years on the job. Pensions were generous; an employee working until age 60 would typically retire on up to half of their final salary. Careers tended to be long. Although about half of all hires left within seven years, a majority of those who remained seven years stayed until retiring on a pension.

Taken together these practices can be thought of as a way of mitigating adverse selection and moral hazard in an environment characterised by imperfect information; costly and imperfect monitoring; and extreme exposure to

theft, malfeasance, or employee incompetence. They provide an example of how firms use practices such as deferred compensation or promotion tournaments. Students develop a deeper understanding of these models and the underlying principal-agent relationship in the context of this sort of econometric case study than they do by studying only the models themselves.

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23

Global Divergence and Economic Change

Jared Rubin

Understanding the “Great Divergence” between Western Europe (and its offshoots) vis-à-vis the rest of the world is one of the great tasks of economic history. By the mid-nineteenth century, Western Europe was clearly far ahead of the rest of the world economically and technologically. It was the first to experience the fruits of the Industrial Revolution, along with its offshoots such as the USA and Canada. This explosion in wealth occurred while the economies of the rest of the world remained relatively stagnant, which, to be fair, was the condition of nearly *all* economies in the world prior to industrialisation. Some parts of the rest of the world have since caught up with Western Europe (e.g., Japan, Singapore, and South Korea), but many parts of the world have not. The Great Divergence is still with us today.

The questions asked by economic historians generally boil down to “Why Western Europe?”, “Why England (the first country to industrialise)?”, or “Why not some other part of the world, such as China, India, or the Middle East?”. The answers to these questions are far from obvious. Indeed, from the perspective of, say, the year 1000 C.E., it was highly unlikely that the modern economy would first emerge in Western Europe. At that time, Western Europe was a relative backwater. Eastern Europe under the Byzantine Empire, the Middle East under the Abbasid Empire, and China under the Song Dynasty were almost certainly much wealthier and more technologically advanced than even the most well-off parts of Western Europe (save, perhaps, Spain,

which was ruled by the Muslim Caliphate of Cordoba at the time).¹ What happened in the late medieval period (c. 1000–1500) and the early modern period (c. 1500–1750) that set Western Europe off from the rest of the world? Economic historians have proposed numerous theories addressing this question. This chapter overviews three of the primary themes that run through this literature: warfare, institutions, and culture. There are numerous other themes in the literature—geography, demography, relative prices, trade patterns, and colonisation—which also likely played some role in the divergence. I confine this chapter to the three sets of explanations I find to be the most convincing. At the conclusion of this chapter, I leave the interested readers with citations for key works on the other important explanations.

Warfare

The first set of arguments overviewed in this chapter relate to *warfare*. Prior to the nineteenth century, European states were in a near constant state of war with each other. Yet, the relationship between warfare and economic success is not an obvious one. Warfare is clearly destructive, in terms of lives, capital lost, and opportunity cost; expenditure on just about anything else would often be better. However, this literature spells out numerous unintended, positive, long-run consequences of persistent European warfare. The most famous statement in this literature is from Charles Tilly (1975: 42): ‘War made the state, and the state made war’. The idea, which Tilly (1990) develops much more thoroughly, is that wars require money, which requires the capacity to tax (i.e., fiscal capacity).²

Solving this problem is difficult—people do not like paying taxes—and it requires aligning the incentives of all of the relevant players (rulers, tax collectors, and tax payers). This may require providing public goods or certain protections for the economic elite in return for tax revenue. For instance, Stasavage (2011) shows that small medieval European city-states became dominant economic and political players because the merchant elite had a vested interest in maintaining the credit worthiness of the state, which largely required them to

¹ For theories focusing on the divergence between China and Europe, see Pomeranz (2000) and Rosenthal and Wong (2011). Indeed, the term Great Divergence comes from the title of Pomeranz’s book. On the Middle East and Europe, see Kuran (2011) and Rubin (2017). To my knowledge, there is no comparative account employing economic theory on the long-run economic divergence between Western Europe and Byzantium.

² Recent works in the fiscal capacity literature are summarised nicely by Johnson and Koyama (2017) and Dincecco (2014).

fund the state's coffers in times of need. Moreover, the “property rights protection in return for taxation” political model encouraged capital accumulation, as those accumulating capital could be reasonably assured that the state would not extract their riches.

This was not the case in more predatory states like Imperial China or the Ottoman Empire, where confiscation by rulers in times of fiscal emergency were common (Karaman and Pamuk 2013; Ma and Rubin 2017). Such capital accumulation, it is argued, helped set Europe apart from the rest of the world on the eve of industrialisation. Hoffman (2015) presents a modified version of the warfare thesis (building on Kennedy 1987), arguing that Europe's constant warfare encouraged investment in military technology to a greater extent than elsewhere in the world. Once gunpowder was introduced to Europe, these technological advances gave Europeans an immense upper hand in colonising the rest of the world. From that point, it is not much of a leap to make an argument about economic divergence based on colonisation, military might, and political power (which is not a leap Hoffman makes one way or the other).

Institutions

A related set of explanations focuses on a society's *institutions*: those political, religious, economic, legal, and social aspects of society that incentivise individuals to act in certain ways. This class of explanations is most commonly associated with Douglass North (1981, 1990), who famously defined institutions as the “rules of the game” that structure how economic agents act and interact with each other. In North's view (also advanced in North and Weingast 1989; and North et al. 2009), England's political institutions set the stage for economic development, following the Glorious Revolution of 1688. After 1688, England established institutions based on the rule of (constitutional) law, which constrained the arbitrary whims of rulers, provided the Crown with a credible commitment to abide by its promises, and applied the law similarly to all property holders. North et al. (2009) call this latter transition one to an “open access” society, while Acemoglu and Robinson (2012) argue for a similar role for “inclusive” institutions (i.e., ones that give some degree of power to all affected parties).

Avner Greif (2006) pushes this line of thinking further, providing a general framework for understanding institutions, why they evolve (or stagnate) over time, and the role of culture and historical circumstances in this process. Greif's framework sheds light on why some historical institutions are

self-enforcing and tend to persist over time, while others undermine themselves as outside economic conditions change. This framework is largely applied to explain the rise of impersonal exchange in medieval Europe (i.e., exchange with unknown parties, one of the essential features of the modern economy), and why exchange remained largely personal in the Middle East. Kuran (2011), Iyigun (2015), and Rubin (2017) also address the divergence between Western Europe and the Middle East. Kuran (2011) argues that certain aspects of Islamic law (related to partnerships, inheritance, public goods (*waqf*), and more) were suitable for the pre-modern economy but tended to persist even as economic conditions changed that made these aspects of the law obsolete. This ultimately resulted in stagnation, as Islamic law was not dynamic enough to address the needs of the modern economy. Iyigun (2015) focuses on the “one true God” aspect of monotheism, arguing that this set the stage for existential crises between Europe and the Middle East, which played out in their histories of conflict. Rubin (2017) argues that Muslim religious authorities played a greater role in legitimising political rule in the Middle East than Christian religious authorities did in Western Europe. This meant that in Europe, economic agents were more likely to replace religious agents at the political bargaining table, with the result being laws and policies more favourable to commerce. In short, all of the “institutional” theories suggest that there is something about how European political, economic, religious, and/or legal institutions evolved that set the stage for the modern economy to emerge there and not elsewhere.

Culture

Finally, some economists have put forth explanations that rely heavily on some aspect of European *culture* to explain the divergence. Recent theories are more nuanced than the Eurocentric theories proposed by Weber (1905 [2002]) or Landes (1998), who tend to look for either European cultural traits associated with hard work or non-European cultural traits associated with conservatism. Among the most nuanced of the more recent theories is the one proposed in a series of books by Joel Mokyr (2002, 2009, 2016). Mokyr ties certain aspects of European culture to the increased rate of technological progress that occurred in Europe beginning in the early modern period (and accelerating in the industrial period). This is a good place to start with a cultural argument, since the Industrial Revolution was inherently a technological revolution, and understanding Europe’s rapid acceleration in technological progress is of first order importance. Mokyr (2002, 2009)

highlights that new ways of thinking, as associated with the seventeenth- and eighteenth-century “Industrial Enlightenment”, were more practical in nature, encouraging experimentation by a large group of English tinkers towards more efficient techniques.

Mokyr (2016) takes this argument one step further, suggesting that Baconian and Newtonian ideas regarding practical applications of scientific principles and techniques created a “culture of growth” that was bolstered by international, intra-European networks of upper-tail human capital individuals, who kept in regular correspondence. Mokyr argues that Europe’s fractionalisation played a key role in the spread of ideas, as it shielded new ideas from the coercive power of the state. He suggests that this could be why early modern China, which was largely unified under the Ming and Qing, never had an Industrial Enlightenment despite having many preconditions similar to those found in England. Mokyr’s insights are consistent with those put forward in McCloskey’s “Bourgeois Trilogy” (2006, 2010, 2016), which argues that a key step on the path to modern economic success was the spread of “bourgeois values”—acceptance of mercantile and productive activity as a means not just to wealth, but social prestige. As these values spread, the way people talked about commercial activity changed with it, placing it in a much more positive light than in most other parts of the world. This could explain, for instance, why the ancient Romans never had an Industrial Revolution, as manual labour and trade were near the lowest of the low-prestige occupations in Roman society.

Conclusion

These three classes of arguments—warfare, institutions, and culture—are hardly incompatible with each other. Indeed, some complement each other in important ways. For instance, the best cultural arguments often tend to work via institutions, while the best institutional arguments (such as Greif’s) recognise the role that culture plays in shaping institutions. Likewise, a necessary component of arguments based on warfare is that Europe was more fractionalised than the rest of the world (and thus there were plenty of European states to fight with), but this is also an important aspect of Mokyr’s (2016) cultural argument. Of course, some of the arguments are substitutes for each other, too. And these are hardly the only arguments related to the great divergence; other classes of arguments focus on geography (e.g., Diamond 1997; Pomeranz 2000), demography (e.g., van Zanden 2009; Clark 2007), relative prices (e.g., Allen 2011; Rosenthal and Wong 2011),

trade patterns (e.g., Findlay and O'Rourke 2007), and colonisation (e.g., Acemoglu et al. 2001; Nunn 2008; Michalopoulos and Papaioannou 2016).

While the debate between the various theories can at times be heated, this is for good reason. We know that, at some point, the modern economy arose in Western Europe and with it the keys to economic success. Accessing those keys is essential for the alleviation of human suffering that persists in those parts of the world that have not yet caught up. Economic historians have come a long way in finding those keys, but understanding which of the keys are prime movers and which are tertiary (or endogenous to other explanations) remains a challenge. These literatures suggest that there is probably no silver bullet (i.e., sufficient condition) for economic success, but there may be a number of necessary conditions which, when combined, portend economic growth.

These debates are clearly important beyond the realm of economic history. Most importantly, development economists have gained much from incorporating such “big think” arguments into their frameworks. Now more than ever, undergraduate and graduate courses in development economics include books and papers cited in this chapter in their syllabi. Likewise, development economists are much more likely to ask questions that address the key insights of the big think literature.

Although I have primarily cited books in this short review, the importance ascribed to articles on this topic by the broader community of top economists is clear via revealed preference. Almost all of the best articles are in top 5–10 journals in economics, a feat which would have been unthinkable a mere two decades ago. This means, of course, that new scholars interested in entering these debates will potentially find a large audience should they ask interesting and unexplored questions. And indeed, there are many such questions. The most fertile ground probably lies in providing a better understanding of why the modern economy did not first arise in areas such as the Middle East, South Asia, or China. Although the current literature provides some insights, there is a wealth of data in each of these regions that remains untouched. Collecting and analysing these data remain the lowest hanging fruit in the great divergence debate.

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24

Industrial Revolution and British Exceptionalism

Christopher L. Colvin and Alexandra M. de Pleijt

The British Industrial Revolution is probably the most important event of the last 10,000 years. Without the step-change in economic output that it brought, the world would look very different today. We would live much shorter and less healthy lives. There would be far fewer of us as a species! Social structures and power relationships would probably be more unequal. Most of us would be working in agriculture. There would have been no proliferation of Great Cities with Great Universities. The globe would be more local. We would have less variety, and far fewer goods and services to enjoy.

Understanding the causes of Britain's Industrial Revolution has always been, and will always remain, the economic history profession's "Holy Grail". Indeed, each new generation of economic historians attempts to answer the question: why did the Industrial Revolution take place when it did, and where it did? And each of these generations tries to bring something new to the table: new hypotheses, new data, and new methodologies.¹ Occasionally, a new consensus emerges. But such moments are fleeting—as soon as one arrives, it is destroyed. Although we now know much more about the British Industrial Revolution, in many ways we appear to be no closer to answering "The Question" than we were when the field of economic history first emerged.

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¹ Often, these new things turn out to be quite old things, addressed in the literature many times before!

In what follows, we review some of the (more recent) highlights of this literature in order that you, the reader, are up to date with where the debate is currently at. We focus on the timing and location questions separately. We then discuss the more fundamental difficulties of ascertaining the reason “Why Britain Was First”. As economists, we view economic changes as having necessary and sufficient conditions. However, we fear that we may never know with any level of certainty which conditions were necessary for Britain to emerge as the world’s first industrial nation. And it is highly likely that none of these conditions were by themselves sufficient. While this may be quite frustrating for someone used to cleanly identifying cause from effect, this should not deter us from looking into the past. After all, just because The Answer to The Question is complicated does not mean we should ignore it!

Timing the Industrial Revolution

The first issue relates to the timing of the Industrial Revolution. Can we explain economic growth by looking at the eighteenth century alone, or is a much longer time span required? An earlier generation of economic scholarship pushed the Industrial Revolution from the eighteenth century into the nineteenth. The so-called Crafts-Harley view, restated in Crafts and Harley (1992), holds that Britain grew much more slowly than we once thought, with productivity improvements contributing to growth only from the second quarter of the nineteenth century. But Crafts and Harley simultaneously pointed towards the British economy in the mid-1700s as being more prosperous than others held out to be the case. Clearly, a longer time span is therefore required to understand issues of timing.

Indeed, a substantial body of evidence now points to the fact that there was a divergence in levels of economic performance within Europe between the late-medieval period and the early nineteenth century. Following the Europe-wide increase in wages in the fourteenth century in the wake of the Black Death, Northwestern Europe showed more-or-less stable real wages at this permanently higher level. Meanwhile, real wages on the rest of the continent went down in the long run (Allen 2001). A similar divergence between the North Sea area and the rest of the continent is evident from recent estimates of per capita gross domestic product (GDP). On the continent, per capita GDP stagnated (Spain) or declined (Italy), whereas Holland and England showed a lot of economic progress—they were significantly richer in 1750 than in 1500 (Bolt and van Zanden 2014). With respect to Britain, the

recent figures on per capita GDP by Broadberry et al. (2015) show that incomes tripled between 1270 and 1800.

This “Little Divergence” (1500–1800) between the North Sea Region and the rest of Europe is highly relevant for the debate about the “Great Divergence”—it is not Europe as a whole that diverged from the rest of the Eurasian continent but only the Northwestern part of it. It is also important for understanding the roots of the Industrial Revolution, which was to some extent a continuation of trends going back to the late-medieval period. New evidence for an early Industrial Revolution comes from Kelly and O’Gráda (2016). They consider changes in the value of watches (one of the important sectors of the British economy) and document that sustained innovation had already started by the early eighteenth century. This view, however, is not universally accepted. For instance, Clark (2001) has shown that pre-industrial income in Britain was as high on average as in 1800, thereby suggesting that the causes of the Industrial Revolution must be sought later, from the late eighteenth century. Clearly, there is no consensus on timing.

Reasons for British Exceptionalism

The second issue relates to the specific causes of Britain’s Industrial Revolution. As we mentioned in the introduction, there is no bigger question in economic history, and there are numerous explanations advanced by economic historians. We attempt to divide these into seven broad categories.

Institutions and Parliament

An influential body of literature argues that it was the specific political economy of Western Europe, and in particular the balance of power between sovereigns and societal interests represented in parliaments, that created the right institutional conditions for Britain’s specific growth pattern. Roughly two versions of this hypothesis can be distinguished. The first one stresses the Glorious Revolution as the watershed between “absolutism” and some form of “parliamentary” government and sees this event as the main cause of the Industrial Revolution of the eighteenth century (North and Weingast 1989; Acemoglu and Robinson 2012). The other one argues that the institutions that resurfaced in 1688 have a much longer history: forms of power sharing between the Prince and his (organised) subjects go back to the medieval period and are rooted in the feudal power structures of that period (Van Zanden et al. 2012).

The idea in both strands of this literature is that the sovereign had to be constrained to protect the property rights of citizens. Property rights were most secure in systems with a strong parliament. This translated into, for example, lower interest rates at the capital market. This view is contested, however. Clark (1996), for example, has argued that there is little evidence of significant insecurity among private owners before 1688.

Ideas, Beliefs and Useful Knowledge

Mokyr (2002, 2009) does not deny the importance of efficient institutions for economic growth but contends that this alone would not be enough for an economic take-off in Britain. He argues that the Industrial Revolution was the result of an interaction between favourable institutions and the arrival of a new set of ideas and beliefs. The “Scientific Revolution” of the seventeenth century produced “useful knowledge”, such as mathematics, which laid the foundation for “Industrial Enlightenment”. This Industrial Enlightenment, which he defines as ‘the application of scientific and experimental methods to study of technology’ (Mokyr 2009: 29), connected the Scientific Revolution to the technological innovations of the Industrial Revolution. In other words, the take-off of England depended on what people knew and believed, which in turn affected their economic behaviour. In an allied explanation, McCloskey (2016) points towards ideas of “Bourgeois dignity and equality”, which emerged among the urban middle classes, and gave a reason for ordinary people to innovate.

Demand and Factor Prices

Allen (2009) argues that institutions favourable to growth cannot have caused the Industrial Revolution because property rights in England were as secure as in France and China. Neither does Allen believe that the Scientific Revolution and the Industrial Enlightenment were of key importance.² Both were Europe-wide phenomena that did not distinguish England from the continent. Therefore, instead of focussing on the supply of technologies as Mokyr (2002, 2009) does, Allen shifts focus to explaining the demand for technology: that is, why did Britain invent the technologies it did? According to Allen (2009), Britain invented the ‘steam engine, the water frame, the spinning jenny and

² More specifically, according to Allen, Industrial Enlightenment would have caused a single increase in productivity and not sustained growth in productivity levels of workers.

the coke blast furnace' because entrepreneurs were induced to implement labour-saving technologies since 'labour was expensive and coal was cheap' (p. 2). It was not profitable to invent the Industrial Revolution in France because labour was cheap and energy was expensive. This view, which was beginning to emerge as a consensus explanation, has come under recent scrutiny in the work of Stephenson (2018), who has found evidence that the London wage estimates used by Allen may be inflated; the take-home pay of the British builders that underpin Allen's wage series may have been no better than their French counterparts.

Consumer Goods and Household Work

De Vries (1994) argues that there was an "Industrious Revolution" in England that preceded the Industrial Revolution, and the former was a necessary condition for the latter. His idea is that household labour increased per year somewhere in the seventeenth century. Direct evidence for this hypothesis is hard to come by, however. Voth (2001) found an ingenious solution: descriptions of working life taken from witness statements in court records. His data confirm de Vries's hypothesis: more days per week worked and longer working hours per day. However, he argues this change occurred in the late eighteenth rather than the seventeenth century. De Vries (2008) puts increased consumer demand at the centre of his explanation for increased labour, which he links to the appearance of new consumer goods imported from the colonies.

Female Agency and Human Capital

Van Zanden (2009) links the take-off of the Industrial Revolution to the institutional developments in the North Sea Region between 900 and 1500. Almost all European countries experienced economic growth between 900 and 1300, but it was only the counties bordering the North Sea that managed to sustain these high levels during the period of the Little Divergence. The Low Countries and England were different from the rest of the continent because of the favourable characteristics of a "European Marriage Pattern". The region enjoyed a high degree of female agency, the outcome of two core institutions: consensus-based marriage and neo-locality of the household (de Moor and van Zanden 2010). This resulted in a high age of marriage for women, a high percentage of singles and a low share of complex households—all favourable to an emerging commercial environment and investments in

human capital formation (via the lowering of fertility rates). Human capital formation subsequently led to economic growth (Baten and van Zanden 2008). But while studies of human capital in the British Isles have not revealed literacy levels there to differ from other European countries, Kelly et al. (2014) stress a dimension of human capital that did set British workers apart: their higher average physical condition. The importance of human capital as a driver of modern growth is stressed by Unified Growth Theory, which is originally developed by Galor and Moav (2002) and summarised in Galor (2011).

Finance and War

The exact role of the financial system in Britain's Industrial Revolution remains contested. Neal (1990) proposed that financial institutions imported from the Netherlands by William III, and then improved in London during a "Financial Revolution", put Britain on a divergent trajectory already a century before industrialisation. But using the records of eighteenth-century private bankers, such as C. Hoare & Co., Temin and Voth (2013) argue that this Financial Revolution led to an explosion of public debt at the expense of private credit markets. This resulted in markedly slower growth in the English economy—the state crowded out private investment. One logical implication of this idea is that we should instead look towards the expenditure of the state as an explanation for Britain's Industrial Revolution: warfare with Revolutionary France. O'Brien (2017) points to this war leading to a victory that provided the British economy with a more efficient state, navy and merchant marine that enabled Britain to retain the gains from trade and servicing the global economy, all at the expense of European rivals.

Trade, Empire and Slavery

Which brings us to explanations concerning trade, empire and its associated institutions, such as slavery. Allen (2003) and Acemoglu et al. (2005) empirically documented the impact of the growth of overseas, especially transatlantic, trade on pre-industrial economic growth. A more recent literature, calling itself the "New History of Capitalism", stresses the role of the slave economy in the US South in fuelling Britain's industrialisation by making cotton imports cheap. However, this literature largely ignores the work of economic historians (see Hilt 2017). For a more economic treatment of the role of empire, economists need look no further than Findlay and O'Rourke (2009), who argue that Britain's ability to specialise in manufacturing points to the

gains from becoming an open economy. Specialisation and reliance on food imports resulted in Britain gaining a comparative advantage as a centre for world distribution of all primary goods, not just from its own colonies.

Geography and Coal

A final body of literature is linked to geographic endowments, notably the role of coal at the centre of the Industrial Revolution. This is an old view that has been revived through the influential work of Pomeranz (2001), who argued that a “Great Divergence” between Europe and China occurred because of the abundance of coal in the former. Fernihough and O’Rourke (2014) take this hypothesis further to empirically test for the relationship between coal abundance and economic development in Europe between 1300 and 1800. Exploiting exogenous variation in proximity to rock-strata from the Carboniferous era as an instrument for coalfield locations, they find that coal had a strong effect on the growth of European city populations from 1750 to 1900. But while explanations involving coal are very good at locating early industrialisation to the north of England, they are not very good at locating the Industrial Revolution to a specific place within Europe as a whole. Many countries had coal, but only Britain industrialised when it did.

Problems and Solutions

Thanks to the recent research by Broadberry et al. (2015) and others, we now have good annual estimates of per capita GDP, often going back to the medieval period. These allow us to analyse underlying trends much more systematically than the “older” research that usually resulted in a number of benchmark estimates (e.g., Maddison 2001). We can now really observe *when* breaks in growth occurred and what long-term trends in performance are.

Although we have a much more detailed picture of economic growth in the past, unravelling the causes of economic growth and industrialisation remains challenging. In particular, there are severe identification problems. Not only are historical data harder to come by and/or prone to measurement error, but there are also issues related to reverse causality. Many of the proposed causes of the British Industrial Revolution are endogenous to the growth process. For example, relatively successful economies such as Britain in the eighteenth century might have had higher levels of international trade as rich countries were able to afford those higher levels. Recently, therefore,

economic historians have shifted to the use of natural or quasi-natural experiments, which provide exogenous variation to disentangle the causes of (pre-industrial) economic growth and industrialisation. However, with respect to why the Industrial Revolution happened in Britain as it did, there is, of course, no credible counterfactual.

Taken individually, each of the theories we have introduced in this chapter suggest that there must be one single cause of the British Industrial Revolution. This is partly the result of the way in which social scientists argue; one “new” contribution to this literature has to highlight one “new” explanation at the expense of everything else. However, it is likely that no scholar of the Industrial Revolution truly believes in their monocausal explanation. What is much more probable, but is hard to present in one single academic contribution, is that there is a multicausal explanation for British exceptionalism that mixes necessary and sufficient conditions for industrialisation and growth more generally. And then adds to them a bit of luck (cf. Crafts 1977). Indeed, we think that there is great scope for new research that moves away from monocausality to find ways to run horse races between these alternative theories. One problem such work would face, however, is that some of the theories seem to us to be rather complex and very hard—if not impossible—to quantify or test explicitly. Perhaps you can prove us wrong!

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25

Innovation and Technical Change

Gerben Bakker

Imagine that by 1820 a cat-borne toxoplasma broken loose during the Napoleonic wars has infected everyone's brain, making people unable to design anything new or improved. All innovation comes to a halt. The next two centuries meander forward without railways, the telegraph, Corliss steam engines, safety pins, machine guns, barbed wire, weather forecasts, asbestos cement, the electric chair, cigarettes, the Taylor system, paper clips, motor cars, aeroplanes, cinemas, plastic, fertilisers, tanks, mustard gas, radio, DDT, supermarkets, Zyklon-B, the interstate highway system, antibiotics, atom bombs, television, the pill, the internet, computer viruses, killer drones, bitcoin or revenge pornography.

Where such a counterfactual world would be now is unanswerable, but we may get some idea from actual total factor productivity (TFP) growth, the rate at which outputs have grown faster than inputs and thus the rate at which the economy has become more efficient.¹ A standard input bundle was three to four times as efficient in 2003 as in 1820 (Table 25.1), implying that many developed countries need an economy three to four times their present size to provide the same outputs with 1820 technology. Moreover, with the inefficient coal-fed steam engines of the Industrial Revolution, the greenhouse effect would be at least an order of magnitude larger.

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¹ The output that can be achieved by a standard composite unit of inputs (capital and labour weighted by their income shares) is called total factor productivity (TFP).

Table 25.1 Contribution of capital intensity and TFP growth to labour productivity growth in Britain, the United States and Japan, 1820–2016

		Level in 2016 (initial year = 100)			Market size in 2016 (initial year = 100)	
		Y/L	K/L	TFP	Actual	K/L only
Britain	1820–2016	2,224	706	315	4,298	1,365
USA	1820–2016	3,498	891	392	84,630	21,562
Japan	1890–2016	3,885	894	435	7,379	1,698

Source: Maddison (2007); and <http://stats.oecd.org/>

Notes:

Y/L refers to the output per hour worked. K/L refers to the contribution of the increase in K/L (capital per unit of labour) to the final-year level, i.e., it reflects the growth in the capital/labour ratio times the capital income share

TFP is “refined” TFP rather than “crude” TFP; it is net of increases in labour quality
“K/L only” refers to what the level could have been if there was no TFP growth but K/L growth was what it was

This thought experiment reflects probably every single issue that surfaces in the economic history of innovation, including what we mean with the “same” output, what TFP growth represents and what fraction of us would be alive today in the counterfactual scenario. Nevertheless, the experiment does show the crucial importance of innovation for almost everything in our modern economy. This chapter discusses three broad categories: conceptual approaches, direct approaches and indirect approaches to study the economic history of innovation.

Conceptual Approaches

Joseph Schumpeter (1942) defined innovation broadly, distinguishing product, process, supply, market and organisational innovations. He saw innovation as a pioneer entrepreneur breaking an existing equilibrium, and once that equilibrium had been restored at a new, more efficient level, another entrepreneur might upset it again through an inherently unbalanced process of incessant “creative destruction”. Like walking, this process might look continuous in the long run and with the aggregation over many industries.

One way to conceptualise this process is the production possibility frontier (PPF), originally introduced by Gottfried von Haberler (1930), one of Schumpeter’s contemporaries. One could qualitatively categorise innovations by the way they caused an outward shift in the PPF. For example, in spectator entertainment, one could argue that the nineteenth-century deregulations led to a theatre boom and scale increase that caused an outward shift in the PPF,

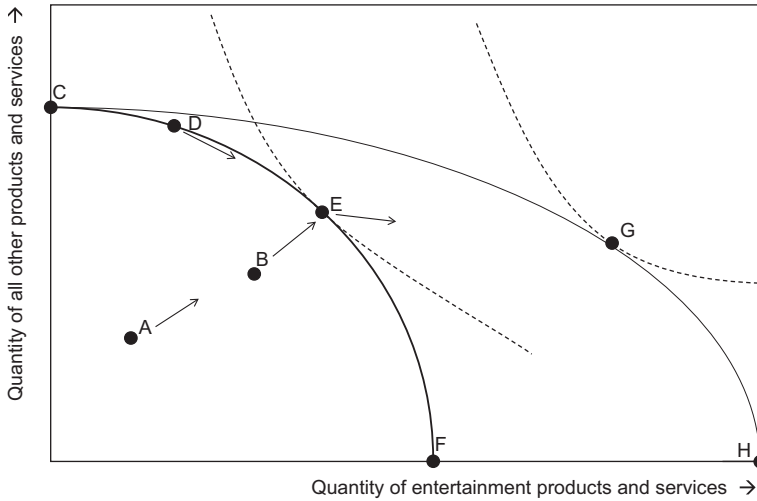


Fig. 25.1 Hypothetical production possibility frontier for entertainment and all other products and services

Table 25.2 Qualitative analysis of successive shifts in the production possibility frontier for raw news during the nineteenth century

Exogenous technological change	First-order outward shift of PPF related to news	Second-order outward shift of PPF related to news
Good and cheap telescopes	Printing press (15th c.) Postal pigeons (18th c.)	Trading news Local news agencies
Electricity	Optical telegraph (1790s)	International news agencies
Magnetism	Electric telegraph (1830s)	Global news agencies
Literacy		

Source: Bakker (2015b)

Note: PPF = Production possibility frontier (see text)

and that later steel frames, high-capacity theatres, railroads and centralised booking with the telegraph led to a further shift, and then later motion pictures caused yet another shift (Bakker 2015a). Figure 25.1 depicts this process. Within the PPF labelled CF, moving from A to B yields more efficiency with existing technology. At E, an outward shift in the PPF is required—such as that from CF to CH—to increase efficiency further, perhaps enabled by the invention of motion pictures. Table 25.2 illustrates such a process for a different industry, the trade in news. Joel Mokyr (1990) observed that nineteenth-century innovations were often an ingenious combination of existing technologies.

Historians have developed many other conceptual ways to look at innovation. Thomas Hughes (1991) argued that large technological systems were like the advancing frontline of an army. Inventors tried to discover a “reverse

salient”, a shark’s tooth in the frontline that holds up the entire army, and then defined the critical problem that caused it. Thomas Edison, for example, reading all patent applications, discovered that short-lived filaments held the advance of electric lighting back, translated this into the critical problem of designing a thermostatic device that intermittently turns an overheated filament off. When that failed, he turned it into the critical problem of discovering a high-resistance, durable filament. Likewise, the reverse salient of inadequate overland transmission capacity, which held back electricity distribution in the late nineteenth century, led inventors to the critical problem of using high voltages with adequate insulation. When several inventors focused on the same reverse salient, this sometimes led to similar inventions occurring simultaneously, a phenomenon often seen in history.

Another way to conceptualise the dynamics of innovation has been “unbalanced growth” (Hirschman 1958). This approach argues that development is often uneven. Rapidly developing sectors, such as railways, electricity or information and communication technology (ICT), have many “backward” and “forward linkages”. Railways, for example, needed a lot of steel, wood and coal as well as a telegraph system, and also had many forward linkages to users such as farmers, traders, commuters, armies, and so on. In other words, an outward PPF shift in one industry may cause another elsewhere.

Direct Approaches

Economists have theorised that market growth increases the propensity for innovation because the fixed cost for given efficiency increases remains the same, while the marginal benefit increases (Schmookler 1966; Romer 1986; Grossman and Helpman 1994). The USA’s market in 2016, for example, was over 800 times the market of 1820 (Table 25.1). The endogenous growth models give high importance to R&D employment. Economic historians, however, have found that the models cannot explain when and where the Industrial Revolution emerged all that well but could potentially explain why British TFP growth initially was so low compared to later periods (Crafts 1995; Harley 2003).

A related debate is on the effect of R&D inputs on TFP growth, crucial in endogenous growth models. TFP growth is much wider than narrowly defined R&D-led innovation and includes things such as organisational innovations, improvements in productive and allocative efficiency, learning-by-doing and many more known and unknown factors as well as measurement errors. Historical studies show that the effect of R&D on TFP is not zero, but that it is certainly not a one-to-one relationship. In the USA’s interwar manufacturing

industries, the relation was weak, and an industry such as chemicals, enjoying 30 to 40 per cent of all R&D inputs in manufacturing, accounted for only 5 to 7 per cent of TFP growth (Bakker et al. 2019). For the postwar period, the Bureau of Labor Statistics reports that only between 0.1 and 0.2 percentage points of the 1.2 per cent per annum TFP growth between 1948 and 1982 can be explained by R&D (Sveikauskas 1986). Meanwhile, Corrado et al. (2006) show how capitalising R&D and other intangibles reduces the USA's TFP growth from 0.48 to 0.41 per cent per annum between 1973 and 1995 and from 1.42 to 1.08 per cent per annum between 1995 and 2003, leaving a lot of TFP growth to be explained.

Another debate is about neutral versus biased technical change. Allen (2009) argued that British innovation was biased towards labour-saving technology that used liberal amounts of capital and energy because Britain had high wages and cheap energy. Allen argues that a “macro-invention”, like the spinning jenny, emerged in the 1760s for this reason, while Crafts (2011) calculated that if so, the question remains why the spinning jenny was not introduced a century earlier. Allen also argued that biased technical change using macro-inventions explains why the European continent and China did not develop the jenny.² Wages were too low in Europe and energy costs too high in China. Abramovitz and David (2001) similarly suggested that, in the nineteenth century, technical change in the USA was capital-using, with a low elasticity of substitution between capital and labour. If these assumptions are correct, re-estimated TFP growth would have been several times higher (Crafts 2009).

An unsolved mystery in the 1980s was why computers were ‘everywhere except in the productivity statistics’, as Robert Solow put it. Economic historians pointed to a large gap between invention and a measurable productivity impact for “General Purpose Technologies” (GPTs) such as steam (80 years) and electricity (40 years). GPTs are technologies that initially have much scope for improvement and eventually become widely used, with many uses and many complementarities (Lipsev et al. 1998). When, eventually, TFP growth boomed in the 1990s, about 30 years after the adoption of ICT, economic historians found this early rather than late.

A debated issue is whether GPTs cause spillovers, that is, whether buyers get more output than is already reflected in the price, for example, when electricity allows the use of cheaper single-storey factories (Devine 1983). For the 1920s, Bakker et al. (2019), using David's (1991) method, show that electricity TFP spillovers explain at most a quarter of TFP growth in US manufacturing, and that for all four “great inventions” of the Second Industrial Revolution (electricity,

² For a different view on the British Industrial Revolution focusing on scientific knowledge, craftsmanship and institutions, see Mokyr (2009a).

chemicals, the petrol engine and communications; see Gordon 2016), the share was about one third between 1929 and 1941. They also show that, without spillovers, great invention sectors explain at most 38 per cent of the USA's TFP growth between 1899 and 1941, compared to 67 per cent for the modernised sectors during the British Industrial Revolution; the USA's TFP growth had many different sources and would still have been high without the great inventions.

Within industries, economic historians have studied path dependence, how one situation depends on a preceding state or how industries get on a certain path over time. Within innovation diffusion, the QWERTY keyboard is an iconic case. David (1985) argued that better alternatives were not adopted because of a quasi-irreversible lock-in, with most typists qwerty-trained and machines having a qwerty layout—a conclusion debated by Liebowitz and Margolis (1995). Likewise, Cowan (1990) found that light-water nuclear reactors, adopted for submarines, only became optimal for civilian use because prior experience increased their safety. Starting from scratch, better alternatives would have been available. In a classic study on hybrid corn diffusion in the USA in the 1930s and 1940s, Griliches (1957) showed that slow adoption, which initially looked inefficient, was explained by varying profitability across states, and the slow development of suitable hybrids for the South.

Another path-dependence topic is why some industries are highly concentrated across countries, with just a few supplying most of the market in each country, while others are not. Sutton (1998) showed how this empirical regularity could be explained by an industry's changing pattern of technology and tastes that determined how “cheap” it was for R&D to reach a given quality level, and what the willingness-to-pay was for such a level. When R&D was cheap and products vertically differentiated, often, firms escalated sunk outlays on R&D and captured large market shares. Using this theory, Bakker (2005) shows how a short “escalation phase” in the 1910s—a “quality race”—determined the market structure of the motion picture industry for a century afterwards. Industry studies by Sutton and others (e.g., Van der Hallen 2007 on interwar Belgian beer) show similar results.

Another form of path dependence is the agglomeration of industrial districts that benefit from labour market pooling, external economies of scale and knowledge spillovers. “Jacobs externalities” reflect co-location benefits of different industries, only achieved for a few outsize metropolises, while “Marshall externalities” reflect the more common within-industry agglomeration benefits. Scranton (1997) provided a comprehensive historical analysis of innovation in some of these districts in the USA. Somewhat related is “collective invention”, in which firms release information so they benefit from competitors' innovations based on it—discussed by Allen (1983) for the steel industry and Nuvolari (2004) for mine pumping steam engines.

Sutton (2012) related the industrial district question to the globalisation question, why not all developed countries' production moved to emerging markets, where labour was much cheaper, even when corrected for education and skill levels. Putting it simply, Sutton showed that some firms can only offer price-quality combinations that fall outside a specified window because some qualities cannot be sold at any price, and because raw material costs put a floor under what price emerging markets can offer, even with zero factor costs, this price might not be low enough to compensate for lower quality. Bakker (2004) makes a similar point for the international competition in film production.

At the micro level, economic historians sometimes study cases of inventions or inventors, for example, to establish who was first. One case can falsify an entire chronology, which makes case studies very powerful. If one asserts, for example, that R&D projects before 1850 were not carried out on a twentieth-century scale, then one case study would be enough to refute such a notion (Bakker 2013). Sometimes economic history case studies also explore a phenomenon in innovation, such as the iconic spinning jenny and QWERTY cases mentioned earlier, or the building of liberty ships showing that taking into account capital increases and declining quality halved observed learning-by-doing effects (Thompson 2001).

Indirect Approaches

A more indirect way to study innovation is to focus on institutions that have been developed to stimulate it. Patents form a much-studied one in economic history. Mokyr (2009b) argues that during the Industrial Revolution, applying for a patent was a gamble, and that patents could also be used to block innovation, but that they still may have worked as an incentive for inventors who clung to the belief their odds were better than they really were (see also MacLeod 1988; Bottomley 2014). Alternatives existed, including first-mover advantages, an honour system, and trade secrets. Lamoreaux and Sokoloff (1999) argue that in the nineteenth century the US patent system facilitated a reasonably efficient market for inventions, where especially city-based independent inventors could easily trade by using patents, specialised journals and intermediaries.

However, Moser (2005) shows that at major industrial exhibitions only 10–15 per cent of inventions were patented. Likewise, studies on prizes, such as Brunt et al. (2012), show that targeted innovation awards provided another institution that could motivate inventors, and direct their efforts while ensuring inventions were made public. Hall et al. (2014) observe that firms often used a mix of formal and informal protection of intellectual property, and that

patents and trade secrets may not reflect underlying innovation all that well. Similarly, Alexopoulos and Cohen (2011) find that the number of publications about key technologies provides a much better innovation indicator than the number of patents. Moreover, the use of patents, trade secrets and alternatives might be industry specific.

Organisational innovations were also important. Mowery and Rosenberg (1989) document the boom in corporate R&D labs in the USA in the early twentieth century. Nicholas (2010) shows how many early R&D labs mainly facilitated knowledge absorption, such as buying in patents or taking over technology-intensive firms. Gradually, the labs started to develop more in-house inventions. Yet some degree of complementarity between labs and independents remained, and by 1930 independent inventors still accounted for over half of U.S. patents and more than three times the number granted to firms with R&D labs. Nicholas (2003) shows how the threat of creative destruction could make those corporations more innovative, and how they became skilled in convincing shareholders of the value of their patents and R&D outlays. Chandler (1962) highlights the modern, multidivisional business enterprise (MBE) that emerged since the late nineteenth century as an important organisational innovation, enabled by many new technologies. Field (1987) characterises the MBE as a capital- rather than labour-saving innovation, arguing the latter had been exaggerated. Using bibliometric data, Alexopoulos and Tombe (2012) show that new management techniques introduced in the USA since the 1920s significantly boosted TFP growth.

Many other institutions could potentially stimulate innovation. The essays in Lamoreaux and Sokoloff (2007) discuss several when they give an overview of the financing of innovation since the nineteenth century. After 1945, universities and government labs became increasingly entangled with business in a symbiosis in which they bore the cost of discovery and failures, allowing private firms to pick the cherries. The public institutions incurred the fixed costs of expensive facilities or star scientists, which the firms then could rent at marginal cost. Venture capital funds bet on entire industries that were undergoing some seismic change and initially invested mainly in business-to-business or business-to-government activities. Many other institutions became important for venture capital, including stock exchanges that allowed flotation of loss-making ventures such as the NASDAQ since 1981. A large literature exists on venture capital (see Bakker 2013).

Conclusion

This chapter has given a somewhat eclectic peek at innovation and technical change in economic history. An interested reader wanting more could do worse than to start with Edgerton (1996) and then read Mokyr (2002).

Economists might also want to consult the essays in the classic Nelson (1962), many of which discuss the dynamics of innovation and stand at the origins of a long literature.

The opportunity of economic history is to add the “when?” question to concepts and theories, combined with the cliometric question “how much?”, sometimes with a “where” added, to compare places that can reflect different whens in economic models. The meeting of the when, where and how much questions often leads to the cliometric fireworks that bring abstract theories down to the peculiarities of time and space and changes over time. For a theory such as endogenous growth theory, the question then becomes when and where it could be applicable, to what degree, and by how much it could explain technical change.

Likewise, economic history is also able to give a rough feel of the order of magnitude of innovation, by providing a reasonably objective comparator through history. Bakker (2013), for example, compares the size of R&D projects over two centuries. And in the debate whether the Second Industrial Revolution dominated TFP growth, Bakker et al. (2019) compare it with the First Industrial Revolution, showing that modernised sectors were much more dominant during the First. Likewise, the time lag in ICT’s productivity impact has been compared with that of other GPTs, such as steam and electricity. In short, history can provide a yardstick for many technological developments.

As a result of the above, cliometrics has transformed grand qualitative historical narratives and endless debates, for example, about the Industrial Revolution, the railways, electricity, patents and the quality of life during industrialisation. It “hacked” them through temporalisation, localisation and quantification. It might not quite be the predictive “psychohistory” as Isaac Asimov imagined it in the *Foundation* trilogy—after all it deals with the past rather than the future—but it might as yet be the closest thing we have to it.

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26

Culture and Religion

Christopher L. Colvin

Max Weber (1904/1920) placed the economic analysis of societal ideas and beliefs at the centre of his new “science of society” when he linked the cultural traits of Calvinists with the origins of modern capitalism. However, it is only since the early 1990s, when Avner Greif started to frame his work on medieval trade networks in terms of societal beliefs, that culture has become a more mainstream subject of economic study. It is very noticeable that this new-found interest has been instigated and driven by economic historians. This chapter explores why this may be the case, and how economists can learn from this recent economic history scholarship.

What do economists mean by culture? Peter Temin (1997) describes culture as ‘distinctive attitudes and actions that differentiate groups of people’ (p. 268). He argues culture is the result of—and is itself expressed through—religion, language, institutions, and history. And he notes cultural attitudes tend to persist but can change over time. Indeed, Luigi Guiso et al. (2006) stress this persistence in their version of culture: ‘those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation’ (p. 23). Meanwhile, Joel Mokyr (2016) regards culture as something ‘entirely of the mind, which can differ from individual to individual’ (p. 9). He contrasts culture with institutions, which he frames as ‘socially determined conditional incentives and consequences to actions’.

What exactly are the persistent attitudes/beliefs/values which constitute culture? Boris Gershman (2017) describes how social scientists have adopted a “culture in examples” approach to identification and measurement, focusing on ‘patience, or long-term orientation, work ethic, risk preferences, concern for relative standing or social status more generally, fairness considerations and other types of interdependent preferences’ (p. 2). While economists studying the modern world can use survey data to systematically analyse such examples and exploit differences across societies (see, e.g. Colvin and McCracken 2017), economic historians are not so lucky; the people we wish to survey are usually dead. Economic historians must instead use archival data to obtain proxies for culture—such as the proximity to, or membership of, religious organisations—or conduct microstudies of culture in a particular individual or society at a particular juncture in their history, where cultural attitudes may be revealed through behaviour.

Given the complexities in definition and measurement, why should economists care about culture? And how can economic historians contribute to the economic study of culture? Economists should care because culture really does matter. While it may not function in the way originally proposed by Weber, various cultural traits have been linked empirically to the institutions which govern society and facilitate economic and financial development. Religion in particular has been shown to influence the location and timing of industrialisation. Economic historians can contribute to, or even lead, this research agenda in two ways. Firstly, they can uncover quantitative and qualitative evidence on new historical case studies in which culture has directly affected economic outcomes, both contemporaneous to the case under study and across the long run of history. Secondly, economic historians have at their disposal questions, approaches, and methodologies which economists rarely use but nevertheless contribute directly to improving our understanding of economic processes and trends.

Cultural Attitudes, Industrial Progress, and Institutional Change

Arguably, the most important goal of economic history remains explaining where, when, and how mankind managed to escape the squalor and banality of everyday life which was the norm for most people up until the Industrial Revolution. This question has traditionally been reduced to accounting for the location and timing of the industrialisation of production and exchange, and culture is one of the many competing explanations advanced by eco-

conomic historians. If the Industrial Revolution started in Britain, is there some uniquely British cultural attitude which helps to account for the adoption of the new technologies and production methods which took place there first?

While the existence or merit of a “British culture” may surprise modern readers, recent scholarship by both Gregory Clark and Deirdre McCloskey points exactly in this direction. For Clark (2007), the principal cultural facet which permitted Britons to escape poverty was materialism, an attitude which was most prevalent among Britain’s wealthy. He finds quantitative evidence these wealthy were more successful at procreation than other groups, their offspring were downwardly mobile, and consequently able to spread their values across all strata of British society. McCloskey criticises Clark for failing to address the ideological change which underpinned this materialism. Summarised most succinctly in McCloskey (2016), the attitude responsible for Britain’s success was, according to her thesis, “egalitarian liberalism”, which gave the necessary dignity to both the bourgeoisie and their employees to pursue their personal interests. The source of this new culture: a rhetorical change which replaced mercantilist attitudes. Meanwhile, Joel Mokyr (2016), that other giant of Industrial Revolution scholarship, has advanced a new culture thesis: the relevant cultural attribute was ‘the attitude towards nature and the willingness and ability to harness it to human material need’ (p. 14).

While material bourgeois nature-exploiting cultural attitudes can no doubt affect the behaviour and preferences of (groups of) individuals, these attitudes must translate across to the structure of the institutions that govern society before they can affect change. Nobel Prize-winning economic historian Douglass North framed institutions as the ‘humanly devised constraints that structure political, economic and social interaction’ (North 1991: 97). For North, institutions emerge through a process of repeated interaction to provide the necessary incentive structure to facilitate exchange. Culture does not feature here, at least not explicitly. But by pushing North’s framework a little further, culture can be hypothesised to directly influence both institutions and the organisations that rely on them. Indeed, when Temin (1997) contrasts Anglo-Saxon individualism with Japanese collective culture, he argues cultural preferences shape the nature of production and the organisation of business. Cooperation rather than competition leads to individuals trading off short-term gains for long-run stability; the institutions that govern Japanese society are a product of Japanese culture.

But the culture-institutions nexus is complex; the direction of causality is difficult to ascertain (see Alesina and Giulliano 2015). One way in which economic historians are uniquely able to disentangle cause from effect is to exploit the linear nature of history; cause cannot occur in time after effect. In

a series of articles about the networks of the Maghrebi Jews in the eleventh century, Greif (e.g. 1994) shows how the specific cultural beliefs of Muslim Mediterranean society—namely mutual responsibility and a fundamental duty not only to practise good but to ensure others do too—help to explain why some societal groups can be more economically successful than others. Collective cultural beliefs originating in the Muslim world were transmitted across borders within Jewish social networks by Jewish traders, who then established informal institutions that enforced these beliefs—institutions which enabled this group to foster and further trade across long distances.

Culture can also have a scarring effect on institutions and organisations. In their study of the long-run impact of the African slave trade, Nathan Nunn and Leonard Wantchekon (2011) find persistent cultural norms of mistrust originating from slave trade raids—norms which affect African legal and political institutions—persist to this day. Meanwhile, by linking the persecution of Jews in areas affected by the Black Death with violence against Jews in the interwar period, Nico Voigtländer and Hans-Joachim Voth (2012) show how localised demographic events can shape that region's culture, that this culture can persist for hundreds of years, and this culture can eventually negatively shape institutions that govern an entire society. It is prudent to conclude here with Mokyr (2016): the best way of thinking about the culture-institutions nexus is to acknowledge that the two coevolve, 'much like a species and its environment' (p. 10).

Religious Revolution, Human Capital, and Club Goods

Religion is either one manifestation of culture or itself shapes that culture. Either way, religion counts among the most profound and important facets of a society's cultural identity. If religion shapes our culture, then religion defines our attitudes towards one another and the institutions which have developed to govern our social interactions. If culture is something more fundamental, more innate, then its very nature means it can only be studied academically by looking at its various manifestations, including religion.

The main area of research on the economics of religion remains the Weber-inspired forensic examination of the impact of the Protestant Reformation. Weber (1904/1920) attributes modernity to uniquely Protestant attitudes towards work, thrift, and self-improvement. His ethic hypothesis concerns the direct content of Protestant teachings—or, more precisely, Calvinist teachings. He argues their behavioural ethic stems from their unique dogma of

predestination, which encourages worldly activity as a means of gaining the self-confidence to be counted among the elect. A new generation of quantitative economic historians has now re-evaluated Weber's largely discredited thesis, this time using systematic evidence from censuses and tax records and by adopting methodologies which isolate causal mechanisms (see Becker et al. 2016). These scholars find little evidence of a unique Protestant ethic. Instead, they find something else: they link Protestantism with progress through attitudes towards education. The work of Sascha Becker and Ludger Woessmann (2010) stands out in particular. Using disaggregated data pertaining to nineteenth-century Germany and adopting an instrumental variable approach, these labour economists link the success of Protestantism with literacy. Protestant culture is not unique in this respect: Maristella Botticini and Zvi Eckstein (2012) link the professional occupational specialisation of the Jewish people that occurred from the late Roman period with human capital development due to Rabbinic Judaism's emphasis on the Written Torah. And Mohamed Saleh (2015) finds similar patterns of human capital differences when comparing Coptic Christian minorities with Muslim majorities in nineteenth-century Egypt.

Understanding the causes and consequences of competition is a staple topic for all economists and represents another lens through which religion has been analysed. Indeed, Adam Smith (1776) theorised competition between small religious sects ensured institutional stability. Weber also focused on religious sects in his lesser-known work on religion in North America (Weber 1904). After observing the unique way in which American businessmen interacted in rural communities, he postulated sect membership acted as a signal of trustworthiness which facilitated economic exchange. Protestant sects were organised by their members rather than commanded by a hierarchy, and so members themselves vetted and enforced behavioural standards. Weber argued American culture was itself a product of this religious organisation.

Smith and Weber's work on sects has been extended more recently using rational choice theory. Larry Iannaccone (1992) frames religious groups as "club goods", providers of public goods or services that are excludable but non-rivalrous. This framework has allowed others to analyse the costs and benefits of membership of religious groups in history: Ran Abramitzky (2011) analyses the emergence and long-run success of the Israeli Kibbutz system, while my own work (Colvin 2017) does the same for religiously affiliated banking organisations in the Netherlands in the early twentieth century. There is great potential for this more microeconomic scholarship to improve our understanding of the formation and performance of social or business

organisations, and it is my opinion the intersection of rational choice theory with business history has scope for generating new and interesting historical case studies involving culture and religion.

The Bottom Line

The systematic study of culture and religion has not typically been of interest to economists. Eric Jones (1995) labels this lack of interest “Cultural Nullity”, where culture is assumed to adjust to economic realities. The opposite position, termed “Cultural Fixity” and the approach taken by anthropologists and sociologists, instead ascribes all economic realities to some prevailing culture. The field of economic history has recently come to lie somewhere between these two extremes, exploring the possibility that culture and economy coevolve. In the same way an earlier generation of economic historians embraced the role of institutions in explaining historical trends and phenomena, a newer generation is now embracing culture too. It is incumbent upon economists in other fields to follow their lead.

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27

Agriculture and Rural Development

Paul R. Sharp

Agriculture plays a central role in economic history for at least two reasons. First, around 12,000 years ago the Neolithic Revolution transformed human society perhaps more than any other event: with the invention of agriculture permanent settlements could form and population began to grow. Second, since the Neolithic Revolution, the vast majority of human activity has been primarily concerned with agriculture. In fact, it is only within the last couple of decades that the rural population of the world was overtaken by that of the cities. Thus, any economist working with economic history needs to take agriculture seriously.

Agriculture and Comparative Development

In terms of thinking about the very long run, there have been a number of studies which attribute a key role to agriculture in determining the relative development of different parts of the world. The most famous hypothesis is that of Diamond (1997) who argued that Eurasia enjoyed a number of environmental advantages, including plants and animals available for domestication and its East-West orientation, which made it easier for agricultural innovations to spread after the Neolithic Revolution. The technological and other advantages which followed meant that Europeans came to enjoy economic and later political dominance over the world. Other authors have considered other genetic and cultural factors, some of which are due to agriculture (see the survey by Spolaore and Wacziarg 2013).

Moving forward to more modern times, early economists also focused on the importance of the land: understandably, of course, given the background against which they were writing. Thus, for example, during the Enlightenment, the French Physiocrats believed that the wealth of nations derived exclusively from the value of “land agriculture”. Influentially, both at the time and today, Malthus argued that limitations to the amount of land would mean that economic growth would ultimately be choked off as population expanded and food became scarce. His theory forms the backbone of our understanding of the modest or zero levels of economic growth in the world before around 1800, and even in present day developing countries, for example, among practitioners of Unified Growth Theory (see, e.g. Galor 2005). The Malthusian interpretation of history has been debated by many economic historians, who often prefer to argue for what they see as a more gradual movement to modern economic growth. For example, Persson was a long-standing opponent of Malthus (Persson and Sharp 2015). Much of his critique relies on calculations made by economic historians of productivity increases in agriculture, with estimates of total factor productivity (TFP) growth in agriculture of up to 0.1 or 0.2 per cent per year in pre-industrial times if resources were efficiently exploited (which required the possibility to trade or the presence of large urban centres), as well as more recent reconstructions of historical GDP/capita. He explains how land is not necessarily a limiting resource in the Malthusian sense. Crop ratios (the number of crops per year per unit of land) increased from 0.05 in primitive agriculture to close to one per year in Europe and higher for other regions producing rice. Moreover, yields per unit of land also increased, for example, through the use of manure as fertiliser, or the introduction of clover, even before soil chemistry gave a scientific understanding of why this worked. In fact, even today only between 80 and 90 per cent of all cultivable land is now used (Federico 2005).

Agriculture and Technological Change

Economists have recently focused on measuring the impact of specific examples of technological change in agriculture. Two examples will suffice. First, the introduction of the potato to the Old World from the Americas has been found to explain a large proportion, around one quarter, of the population and urbanisation increases of the eighteenth and nineteenth centuries (Nunn and Qian 2011). Second, the introduction of the heavy plough to Europe in the Middle Ages allowed farmers to make more efficient use of heavy clay soils, also leading to greater rates of urbanisation in these areas (Andersen

et al. 2016), although this was not without costs, since it also seems that societies which made more use of the plough, since it required physical strength to employ, also developed gender norms less favourable to women (Alesina et al. 2013).

The Malthusian model suggests, however, that productivity increases in agriculture will simply be matched by population increases, leading to greater population density but moderate or no improvement in standards of living—hence the use of population density or urbanisation rates in the aforementioned studies. Urbanisation, which is often easier to measure than population density before modern censuses, also presents a useful proxy for the productivity of agriculture, since a larger fraction of people living in cities can be taken to imply that agriculture is generating a surplus which is able to feed the urban population—although this might be due to imported food from other surplus regions rather than domestic productivity.

An Active or a Passive Role for Agriculture?

This discussion about urbanisation gives the impression that a decline in the share of the population in agriculture is fundamental to the process of development. In fact, much of the debate about agriculture and development does indeed give it a rather passive role. For example, the two-sector classical growth model by Lewis (1954) simply sees agriculture as a sector where labour is employed very inefficiently and can be moved into more dynamic economic activities without affecting agricultural production. Indeed, somewhat based on the experiences of developed countries, the consensus in the 1950s was that agriculture should shrink for agriculture to develop. This was because the demand elasticity for agricultural produce is below one, traditional agriculture does not use its resources efficiently, and besides, the sectors with the greatest potential for productivity gains lay outside agriculture. With this way of thinking, agriculture simply had to decline gracefully—and in the meantime produce the food needed for industry and services to thrive, save so as to allow investment elsewhere, provide domestic markets for manufactures, and generate export income so that modern technology can be imported (Johnston and Mellor 1961). Moreover, specialisation in agriculture might be dangerous if the terms of trade turned against primary product producers, as happened after the Second World War and was noted by Prebisch (1950) and Singer (1950)—see also the discussion in Williamson (2011).

Recent work has disputed this pessimistic point of view, however. Overton (1996) describes the Agricultural Revolution which took place in England

after 1750, both preceding and alongside the Industrial Revolution. Olmstead and Rhode (2008) present a story of an extremely dynamic American agriculture for the two centuries prior to the Second World War. Lampe and Sharp (2018) document the remarkable transformation of Danish agriculture from an absolutist, quasi-feudal system in the 1700s to a leading agricultural exporter by the end of the nineteenth century. Common to the massive productivity increases which these countries experienced were a string of biological and technological innovations, and, for example, improved use of the land through better crop rotation systems—all of which built upon an increasingly enlightened or scientific approach to agriculture, including accurate book-keeping, the foundation of agricultural societies, agricultural schools and extension services, and scholarly debate in agricultural journals. These innovations in turn laid a solid scientific basis for the subsequent Green and Genetic Revolutions. Thus, from around 1960, international research centres and national research programmes helped develop many new varieties of crops and big productivity gains, although these have been uneven across crops and regions (Evenson and Gollin 2003).

Institutions and the Spread of Agricultural Innovations

The context within which agricultural innovations spread to new countries differs of course hugely over time and space. While the Green Revolution was the result in part of a conscious international effort to spread knowledge to developing countries, Lampe and Sharp (2018) describe how an elite group of enterprising landowners helped to spread proto-modern dairying into Denmark, laying the foundation upon which subsequent advances would be made, and ultimately seeing its spread beyond the realm of the large estates to the peasantry, who founded cooperative creameries. These empowered the peasantry but disempowered women, who were not welcome to work at the butter factories, despite their traditional role in dairying. At the same time, the process of taking food production out of the farmhouse and into factories began to blur the distinction between agriculture and industry, and modern employment classifications would put industrial dairying within the manufacturing sector.

Land inequality is often also seen as a barrier to progress within agriculture, with the literature usually explaining it based on geographic and political factors—for example, it is higher where soils support “plantation-friendly” crops

like sugar or cotton—the so-called Engerman-Sokoloff thesis (see Engerman and Sokolof 2000). Many developed countries today had to go through extensive land reform, abolishing institutions such as serfdom, share cropping, the open field system, and common land—although developing countries today often struggle with many of the issues that European countries had to deal with centuries ago. Other nominally communist countries, such as China struggle with reforming their collective agriculture.

Cooperatives, both producer cooperatives such as the aforementioned butter factories, and cooperative banks are often seen as the answer to the issues confronting poor peasant farmers, both, for example, in terms of giving access to credit to make the necessary investments, allowing peasants to enjoy economies of scale and giving better opportunities for marketing their product. Attempts to impose them from above through government regulation have, however, proved rather unsuccessful, and modern development economists (see, e.g. Birchall 2003) look more to enabling peasants to allow the sort of bottom up process seen, for example, in Denmark.

Outside the establishment of cooperatives, governments have regulated agriculture for centuries. Before the First World War, complex systems of import tariffs and export subsidies sought to maintain prices, but these were gradually liberalised, with predictable effects on market integration (see, e.g. O'Rourke and Williamson 2001). However, in the interwar period, and especially after the Second World War, new tariffs were joined by national support programmes, offering subsidies, governing prices, and buying up produce. Few believe that these help increase the productivity of farmers, and US and EU agricultural support surely has the unfortunate consequence that developing countries with large agricultural sectors do not have the opportunity to enjoy the benefits of exporting to rich markets—although China in particular seems to be offering new opportunities.

Avenues for Future Research

The economic history of agriculture is a large topic, and this chapter only touches briefly on a selection of interesting research that has recently been carried out in this area. The above discussion will hopefully, apart from providing an overview of some of the paths already trodden in the literature, also provide inspiration for new work on agriculture and economic history. In particular, it seems that we can learn a lot from history when trying to answer the challenges of agriculture in the twenty-first century.

Future research might therefore seek to understand how agriculture in once poor but now rich countries was improved, the role of policymakers or others in society had in facilitating this, and how this improvement contributed in turn to economic development. Economic history is surely filled with illustrative examples, beyond those which have already been studied, which can help guide our thinking about the situation of developing countries today. Moreover, a comparative approach looking at the differences and similarities across countries and regions might also yield interesting findings.

One issue which certainly deserves more focus in the historical literature, and is a massive issue for agriculture today, is the impact on the environment. Agriculture has generally become more intensive, with arable agriculture placing greater burdens on the soil. Although this has been offset with, for example, pesticides and fertilisers, these themselves present additional challenges. Moreover, as countries have become richer, so too have they demanded more animal products. Livestock itself also needs to be fed from the land, and beef and dairy production in particular has recently been associated with climate change due to the methane the cattle produce. The intensification of agriculture is something which has been going on for centuries, and a longer perspective on the current issues might be extremely valuable.

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28

Environment and Natural Resources

Eoin McLaughlin

The popular iconography of the landscape of the Industrial Revolution tends to show smoke billowing from chimneys in urban factories, thus highlighting two indistinguishable aspects of industrialisation: coal and pollution. These scenes of dirty cities and towns associated with our economic past have not been relegated to the dustbin of history; modern development in cities in India and China are reminiscent of these earlier images of industry. The study of environment and natural resources is, therefore, a relevant aspect of economic history and one with increasing contemporary resonance.

The past decade has seen increased research activity in both the fields of economic history and environmental economics (Kelly and Bruestle 2011; Rath and Wohlrabe 2016). Contemporary developments, such as economic recessions and climate change, are key drivers of research activity in both fields. The focus of this chapter is on the overlap of both sub-fields, *environmental economic history*, and outlines key contributions and research gaps in this area. The traditional research interests of environmental economics have primarily related to environmental policy instruments, cost-benefit of pollution control, non-market valuation and natural resource economics (renewable and non-renewable resources). More recently, there has been a shift in interest towards topics such as climate change and sustainable development. Also, following wider trends in economic research (Hamermesh 2013), environmental economics has shifted from a theoretical focus towards more applied empirical research (Kube et al. 2017).

Certain topics in environmental economics are clearly not examinable using historical data—for example, stated preference methods for eliciting willingness to pay for environmental goods and services requires the use of surveys of living subjects. However, in other areas there is certainly scope for the input of historical research—for example, revealed preferences for environmental goods and services could be elicited using historical data on house prices, with the identification of environmental attributes such as distance from sources of pollution.

Elsewhere, there have been calls for greater integration of the sub-fields of environmental and development economics, dubbed “envirodevonomics” (Greenstone and Jack 2015). As many developing countries are experiencing environmental problems similar to what was previously undergone in developed countries (e.g. water and air pollution), historical perspective can be very useful as both a guide and for policy recommendations. However, developing countries are still underrepresented in many lines of research, for example, Alló and Loureiro (2014) find that the majority of studies on the preferences towards climate change policies were carried out in the US (52 per cent), followed by Europe (34 per cent), Asia (9 per cent) and Oceania (5 per cent). Along these lines, I would like to echo Greenstone and Jack (2015) and call for greater integration of environmental and economic history.

The focus of this chapter is on several key research themes that are cross-cutting along history and environmental lines; these include the resource curse, sustainable development, the “Environmental Kuznets Curve” (EKC) and climate change. The chapter finishes by highlighting areas of future research. As much of the research in environmental economics is dealing with uncertain future problems, insights from the past can help provide some illumination to guide us along this dark, uncertain path.

Resource Curse

Auty’s (1993) study of development in mining economies coined the term “resource curse”, namely that countries with an abundance of natural resources have tended to exhibit weak economic growth. A glut of empirical studies have shown such a relationship, most notably Sachs and Warner’s (1995, 2001) finding that resource dependence had a significantly negative impact on economic growth over the period 1970–1989. This approach has been widely cited and the literature has taken both cross-country and in-country studies. An example of the latter is James and Aadlan’s (2011) study of US counties over the period 1980 and 1995 which finds evidence of a resource curse. This literature has also been widely criticised, for example,

Brunnschweiler and Bulte (2008) highlight the fact that the measures of resource abundance used in the literature are actually more akin to measures of resource dependence and that resource abundance positively affects growth. More recent approaches to the resource curse have adopted more sophisticated empirical strategies. Smith (2015) is an example in this vein, focusing on the impact of resource discovery in a longitudinal setting, as opposed to the cross-sectional settings used in earlier studies, and finds no evidence of a resource curse effect on economic growth.

Moreover, the resource curse is counter-intuitive when we think of the historical record. The Industrial Revolution is synonymous with coal mining in Britain for one. The resource curse also does not sit with the experience of countries that have developed in the past and continue to develop using natural resource wealth, such as the USA and Australia (Wright and Czelusta 2004, 2007). The use of the historical record leads to a more nuanced perspective. Barbier (2011) is a thorough scoping of the historical record to assess why countries do not succumb to the “curse of natural resources”. Natural resources need not be considered a curse, but the development of linkages with other sectors of the economy and institutions to incentivise and facilitate such linkages are important facets in managing natural resource rents.

Sustainable Development

The United Nation’s Sustainable Development Goals (SDGs), introduced in 2016 and planned to run until 2030, cover 17 areas with the main objective of ending poverty in all its forms and promoting “sustained, inclusive and sustainable economic growth”. How to measure the sustainability of economic growth and development is clearly an important question, therefore. The economics of sustainable development focuses on the links between changes in comprehensive/inclusive wealth and future well-being (consumption). The central argument of the theoretical approaches to this question is that countries who fully utilise their gifts of nature are those that reinvest in other forms of capital. This has led to both the World Bank and the UN publishing reports of the sustainability prospects of countries. However, many of the theoretical models are set in infinite time and are future looking.

The core contribution of economic history here is to assess the empirical foundations of these theoretical models using the historical record by taking the vantage point of a hypothetical finance minister in 1800 or 1900 and looking 100 years into the future (i.e., within sample test of the model predictions), as opposed to a finance minister today looking into the future

with no way to test the predictions of the model. The earliest studies made empirical estimates that were approximates of the theoretical predictions. These were used to show whether a country was on a sustainable (positive) or unsustainable (negative) path (Hamilton and Clemens 1999). Studies have used historical data collected by the World Bank to test the Genuine Savings indicator. Ferreira et al. (2008) find weak evidence to support genuine savings as a predictor of future well-being. This methodology for testing genuine savings was followed by Greasley et al. (2014) and Hanley et al. (2016) who look at the historical record of Great Britain (1750–2000), US and Germany (1870–2000). Here they do find evidence in support of genuine savings as a predictor of future well-being. The key contribution of the historical record highlights aspects of the development process overlooked in the current World Bank/UN framework, particularly the importance of technological progress.

Environmental Kuznets Curve

Does economic development come at the expense of environmental quality, or is there a tendency for pollution to fall as society attains a certain level of income? The EKC, named after Simon Kuznet's findings of an "inverse U" between income inequality and economic development, is a literature that studies the link between environmental indicators (e.g. water pollution, air pollution, deforestation, biodiversity) and economic development (e.g. GDP per capita) (Dinda 2004). Sophisticated econometric methods have been applied to the study of the EKC using both modern and historical data. The use of either cross-sections or panels assumes that the income levels of separate nation states at a point in time are representative of stages of development. However, a more nuanced picture can be observed by tracing the historical development of individual countries and their corresponding pollution levels.

EKC studies using data from the more recent past, such as Grossman and Krueger (1995), look at a host of environmental indicators (urban air quality, oxygen regimes in river basis, faecal contamination of river basins, contamination of rivers by heavy metals) at different levels of aggregation. However, conducting similar research using longer run data is constrained by the lack of consistent recorded observation of environmental quality over time. Instead researchers have focused on a series of indicative pollutants, such as carbon dioxide (CO₂) and sulphur dioxide (SO₂) emissions, that have been estimated historically. Historical EKC studies have focused on country-specific and panel analyses using either SO₂ or CO₂ as the pollutant variable. Markandya

et al. (2006), in a panel of 12 European countries over the period 1870–2001, test for an EKC relationship between SO_2 and GDP; these authors find evidence of an EKC and suggest that this may be linked to environmental policy.

More recent contributions to the EKC literature have addressed the endogeneity between income and environmental degradation. Firstly, Lin and Liscow (2013) argue that there is a simultaneity bias in the EKC; increasing GDP may increase pollution, but pollution may harm health and thereby reduce GDP. Also, the estimated EKC relationship may suffer from omitted variable bias if an omitted third variable jointly causes both economic growth and environmental degradation.

Using debt service and age dependency as instruments, Lin and Liscow (2013) find evidence for EKC for 11 water pollutants over the period 1979–1999. Elsewhere, Sephton and Mann (2016) model UK CO_2 (1830–2003) and SO_2 (1850–2002) emissions using multivariate adaptive regression splines to estimate EKC relationship. They find strong evidence of an EKC and evidence of turning points for CO_2 in 1966 and SO_2 in 1967 that coincide with the passing of the Clean Air Act in 1956.

Climate Change

Climate change has spurred a large body of research. Within the economics literature, the unsettled debates primarily relate to the choice of discount rate and whether we cut emissions now or wait and cut (e.g. Stern versus Nordhaus), but these are normative questions that recourse to the historical record cannot settle. Although, it might be helpful to use the historical record to think about appropriate and realistic discount rates (see Gollier 2012 and the long-run data on interest rates in Homer and Sylla 2005). A growing body of literature focuses on the linkages between weather and economic outcomes in order to understand the economic consequences of potential climate change (see Dell et al. 2014 for a review). The importance of history is evident here as weather is determined in short horizons, but to analyse climate researchers need to focus on longer time horizons (decades, centuries, etc.). A recent example of this comes from Bleakley and Hong (2017), who study the economic impact of US weather patterns over 140 years.

Where history can be particularly relevant is in examining the impacts of natural disasters and mitigation efforts, and to get a better understanding of how past societies have adapted to changes in climate. Kahn's (2006) study of the frequency and mortality resulting from natural disasters sought to address

whether richer countries experience fewer natural disaster shocks and what factors helped mitigate natural disaster shocks. The relevance of this study to climate change is that that climate models predict future sea level rises and increases in the number of floods, therefore support for climate change policy depends on who benefits from this mitigation and where the impact of climate change will fall. The main findings were that high- and low-income countries were equally likely to experience a natural disaster but that the death tolls were lower in developed countries. The reasons for this were attributed to income levels (e.g. better early warning signals), institutions (e.g. democracy and lower inequality) and geography (i.e. Africa experienced fewer natural disasters).

Economists have used history as a quasi-laboratory to test the short-, medium- and long-run impact of environmental shocks. One such environmental shock is the American Dust Bowl that led to permanent soil erosion in the 1930s with some counties more affected than others. Hornbeck's (2012) use of contemporary data enables us to get a better insight into how people might adapt to climate disasters. In the short-term, the Dust Bowl led to changes in agricultural production to types more suited to the new environment although there were a number of constraints to adjustment including the lack of access to credit. Hornbeck found that one of the largest economic adjustments was through migration from high erosion counties relative to counties with lower erosion.

Future Research?

Greenstone and Gayer (2009) advocate for the increased application of quasi-experimental and experimental methods to help get a better understanding of key issues in environmental economics and to move away from associational findings towards more robust causal relationships. This call has led to greater use of experimental methods such as randomised control trials and choice experiments. But it has also led to several studies using historical events, such as recessions and historical policies, as natural experiments. This has been very prevalent in studies looking at the impact of pollution concentration on health outcomes such as birth weights, life expectancy and morbidity. Part of the difficulty in establishing causal relationships comes from the fact that pollution is not randomly assigned and there may be other confounding variables limiting statistical inference. Also, adult populations have the ability to move (residential sorting) and the lifetime exposure of adults to pollutants is unknown. Lastly, there may be "harvesting", that is, those that are already sick may be the ones who die.

Chay and Greenstone (2003) is a landmark study in this vintage. They exploit spatial variation in particulates arising from the 1981 to 1982 recession in the US. Their main finding is that there were 2500 fewer infant deaths in areas that experienced a fall in total suspended particulates compared with areas that experienced no such falls. Another example of this approach comes from Chen et al. (2013), who study the effect of the Hai River policy on health outcomes in China. The Hai River policy is a historical scheme implemented by the Chinese government from 1950 to 1980s and gave free coal for winter heating to residents north of the Hai River, while at the same time exploiting the *hukou* system of household registration that restricted mobility. Using a discontinuity design, Chen et al. (2013) find a discontinuity in both total suspended particulates and life expectancy at the Hai River, attributing the lower life expectancy to the worse air quality north of the river. There is clearly scope for more work of this type for other historical contexts.

Conclusion

Economic history and environmental economics can learn from each other and can also be useful in the classroom. How the economic environmental dynamic has evolved over time can offer valuable insights for environmental policy. Hanley et al.'s (2011) environmental economics textbook uses historical examples of exogenous (changes in climate led to reduced yields and affected land settlement in Neolithic Scotland) and endogenous (changes in how land was irrigated for cotton irrigation in nineteenth-century Egypt) co-evolution of the economic environmental system. Likewise, an understanding of the environment can be insightful for our understanding of economic history. Siegler's (2016) textbook on US economic history contains chapters explicitly relating to environmental issues such as externalities and illustrates the importance of natural capital in US economic development. History can also provide useful real-world case studies of private solutions to environmental problems (Dingle, 1982; Desrochers, 2008) and some examples of managing common pool resources, such as Leuck's (2002) excellent study of the extermination and conservation of American Bison. Here a nice counterpoint could be made with Steckel and Prince's (2001) study of Native American anthropometrics, as their main food source was bison.

In terms of future research, in their call for envirodevonomics, Greenstone and Jack (2015) identified one big question: 'why is environmental quality so poor in developing countries?' The answers they suggest are a greater willingness to pay for consumption over environment, a higher marginal costs of environmental improvement, political economy distortions of the policy

process, or market failures (e.g. information asymmetry) distort people's true willingness to pay. This big question, and related sub-questions, could also be asked of the historical record: why was environmental quality so poor during industrialisation, what were the health burdens of poor environmental quality and what were the implications for productivity and growth? These are questions that have driven past research in economic history (e.g. Ferrie and Troesken 2008) and are still relevant today.

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Part III

Eras, Regions and Contexts in Economic History

Haiku by Stephen T. Ziliak

A labour market
without consent or contract —
Culture's fixity

“Buffalo Soldier” —
Economic history
taught by Bob Marley



29

Economic Prehistory

Eva Rosenstock

Prehistory encompasses the period from the origins of human culture to the advent of written records. Hence, its duration varies from region to region of the world. Prehistory starts with the appearance of first human groups, about two million years before present (bp) in Africa and Eurasia, 40,000 bp in Australia and 15,000 bp in the Americas. It ends as early as the fourth and third millennia BC with the onset of cuneiform and hieroglyphic writing in the civilisations in the Near East, or as late as the eighteenth century AD on Easter Island with the first records of European merchants. Lacking any or relevant written sources, prehistory is recorded only by material remains, usually to be excavated from the soil—such as burials, dwellings, bones, potsherds and stone tools as well as inhering biogeochemical traits. These remains require careful analysis and interpretation by drawing analogies from similar findings in historic periods, or well-documented ethnographical contexts, before conclusions can be made or non-experts in this field can use this information in further studies (Renfrew and Bahn 2012).

Near Eastern and European prehistory is broadly divided into the Palaeolithic, Neolithic and Metal Ages. While the Palaeolithic—roughly coinciding with the quaternary glaciation up to about 10,000 BC—is characterised by a foraging mode of subsistence. Neolithic subsistence was mainly based on plant cultivation and animal husbandry. It developed with the Holocene warming from 10,000 BC in the Near East and subsequently spread into Europe from about 6500 BC onwards. As Europe had initially adapted to Holocene environmental conditions with developed foraging strategies, a

Mesolithic period lasting up to the advent of farming is interposed here. Extractive copper-based smelting metallurgy attested in Southeastern Europe and the Near East from about 5000 BC, and amended by tin alloying to Bronze from about 3000 BC onwards, constitute the Copper and Bronze Ages. As parts of Europe did not adopt or embrace these early metallurgies, the term Neolithic is used to describe the period up to 2000 BC in some regions. At about 1000 BC, however, iron mining and smelting were quickly adopted throughout the Near East and Europe and form a convenient upper end of the time treated in this chapter, as this date roughly coincides with the onset of written records in Greece and hence the beginning of historical Antiquity (Vandkilde 2006; Snell 2007; Milisauskas 2011).

What Can Prehistory Do for Economists?

Prehistory witnessed some of the most important developments that still constitute the economic basis of our modern life. These are, among others: (1) the transition from foraging to farming as the basic mode of subsistence; (2) intensification and extensification strategies of resource use; and, in some regions of the Near East and Europe, (3) the origins of surplus production, craft specialisation, division of labour, inequality, commodification, premonetary currency, barter and trade. A basic knowledge of the prehistoric economy is hence essential for economists to understand enormous time depth of these developments, from which there was at some point, maybe even in the Neolithic, no way back due to path-dependency. A forager lifestyle would not have enabled us to sustain almost eight billion people, no matter what proponents such as Jared Diamond (1997) or Yuval Harari (2014) say about the bad effects of Neolithisation. They often use arguments strongly influenced by economists, such as Oded Galor, who seek the origins in global inequality as deeply rooted in Palaeolithic human evolution and Neolithic economic development (e.g. Ashraf and Galor 2013; Galor and Özak 2016). Such research, however, often shuts off later prehistoric and historic developments, as well as underlying environmental determinants, all which should also be controlled for when conducting analyses.

Hunter-Gatherer Economies

Human groups until the end of the last glacial maximum (at about 18,000 bp) are commonly interpreted as “simple hunter-gatherer societies”. Judging from modern analogies, such societies are characterised by a high degree of

mobility, low amount of personal property and non-existent to at best small surplus production, resulting in a strongly egalitarian social structure and almost no territoriality. Group sizes likely were kept below 50 people by cultural instruments, such as long birth spacing acting as preventative Malthusian checks to keep populations within the carrying capacity of their ice age environment.

For the period of warming after the last glacial maximum, however, archaeologists largely agree that “complex hunter-gatherer societies” emerged with a high degree of sedentariness, larger communities with up to several hundreds of inhabitants, personal property including land, a significant amount of surplus production and storage, as well as some degree of social stratification (Harle 1999). After all, Holocene plant and aquatic resources were more predictable than ice age game and more abundant than ice age plant food. Near Eastern Epipalaeolithic communities increasingly used wild cereals from about 18,000 bp onwards by means of permanent grinding installations. And from the tenth millennium BC onwards, Mesolithic groups in Europe intensively exploited, and probably even managed, hazelnuts in inland sites, as well as fish and molluscs around the Mediterranean and Baltic seas, occasionally resulting in impressive middens—dumps for domestic waste (Gamble 1999; Bailey 2008).

Farming Economies

This trend to intensified resource use continued in the so-called Fertile Crescent, where in the tenth millennium BC wheat and barley show first morphological signs of domestication such as larger grains and a tougher rachis. This transition from management to cultivation marks the start of the Neolithic in the Near East. By about 8000 BC, pulses such as chickpeas, peas, lentils and *vicia* beans—as well as goat, sheep, pig and cattle—complete the spectrum of domesticates. Hence, the Near East is to date the earliest and best researched among other, and most likely independent, centres of Neolithisation in China (based on millet, rice and pigs) and Mesoamerica (based on squash, maize and *phaseolous* beans) (Bellwood 2005; Simmons 2007; Fig. 29.1). Farming transitions are a main field of application of anthropometric approaches to prehistory: using a method initially developed in economic history, such approaches seek to find out whether farming transitions meant a decline or rise in human welfare as reflected in stature (e.g. Angel 1984; Mummert et al. 2011; Boix and Rosenbluth 2014; Larsen 2014; Rosenstock 2014). However, rather than a simple before-after scenario,

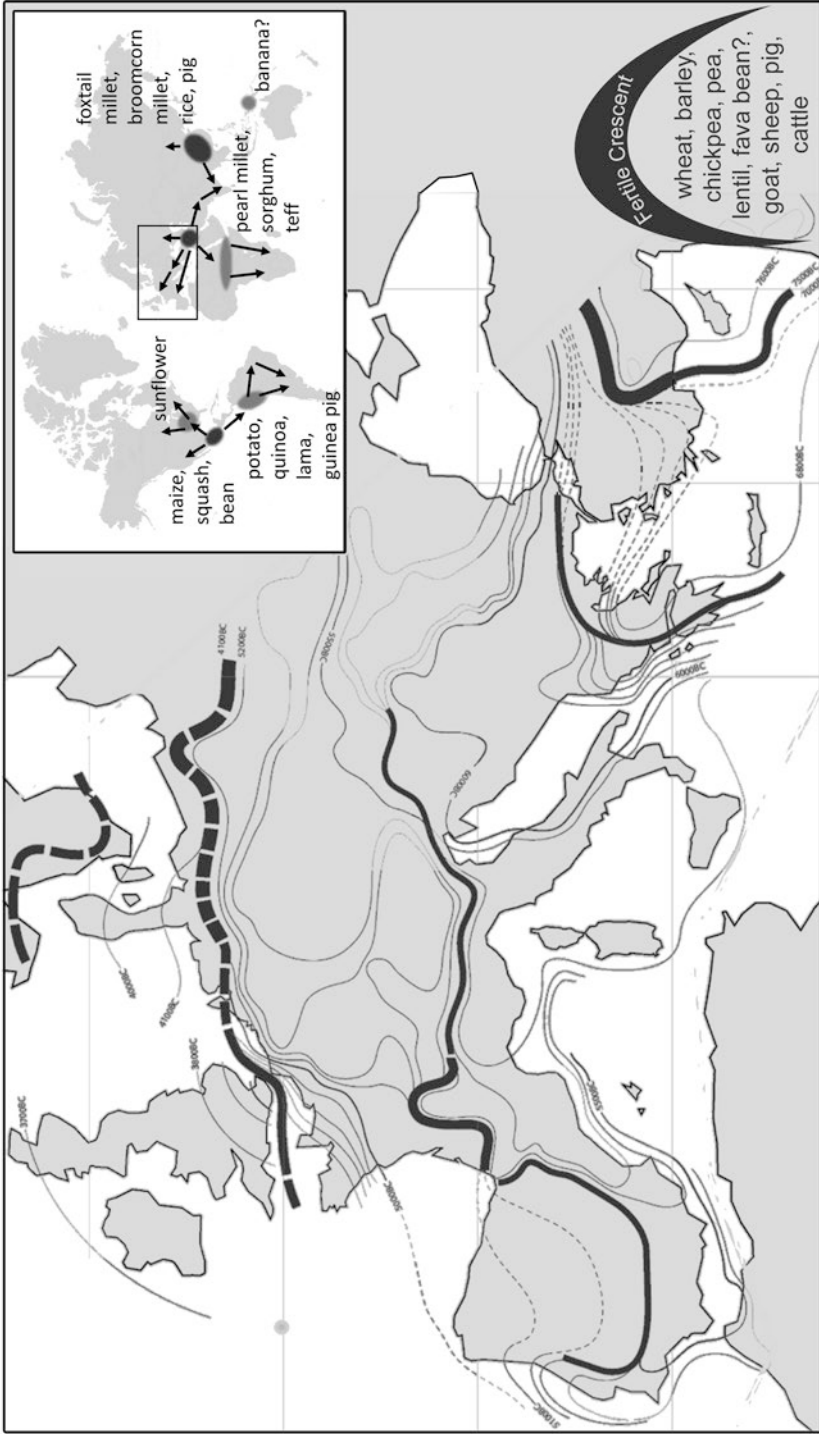


Fig. 29.1 Main map: Spread of farming out of its Near Eastern homeland into Europe. After Rasse (2014). Inset: Autochthonous farming centres ca. 10,000–6000 BC (dark grey) and later Holocene centres ca. 5000–3000 BC (light grey). - Adapted from Bellwood (2005)

farming transitions are long and complicated processes—also including occasional returns to foraging.

In the Near East, it took probably until about 7000 BC to integrate plant cultivation and animal husbandry into an intensive farming system. There are hints that around this date, the focus of economic production shifted from the entire community to individual households in at least parts of the Near East (Düring 2011, 122–125). Probably these two traits enabled the fission of settlements necessary for the Neolithic to spread out of its homeland and into Europe between about 6500 and 3000 BC. This brought an entirely new socio-economic system, previously unknown plant and animal species, as well as novel foodstuffs like soured milk and cheese to the local foragers of Europe. Hence, this Secondary Neolithisation by migration and acculturation was probably much more of a Neolithic Revolution—a term coined by Marxist archaeologist V. G. Childe (1935) as an allusion to the Industrial Revolution—than the gradual and autochthonous Primary Neolithisation in the Near East.

As garden-based agriculture likely involving a lot of human workforce with hoes or digging sticks, the Early Neolithic of Europe until about 4500 BC was largely confined to very fertile soils like the loess—silt-sized sediment that is formed by the accumulation of wind-blown dust—of Central Europe, despite initially fast-growing population densities. The glacial moraines of the circum-Alpine regions and the Northern European plains, however, remained the domain of foragers until after the middle of the fifth millennium BC, when new developments, mostly towards extensification, in farming took place (Bogaard 2004; Fowler et al. 2015). Cattle-driven carts and wagons in the fourth millennium (Fansa and Burmeister 2004) allowed for expedient cultivation—also of poorer soils. It is important to note, however, that these developments occurred at a time when other regions already developed metallurgy and were termed Copper or even Bronze Age.

Craft Specialisation and Trade

Shortly after 5000 BC, the first evidence for copper surface mining and smelting appears in Southeastern Europe and the Near East (Roberts et al. 2009; Rosenstock et al. 2016), regions in which the following two millennia are termed the Copper Age (also known as the Chalcolithic or Eneolithic). The labour investments and skills needed to produce copper implements probably mark the beginning of craft specialisation and the division of labour. Moreover, the fungibility of the material, the standardised shapes it was cast into, as well as the occurrence of copper adzes and axes in hoards, suggest that

they represent early commodities or even commodity money. According to classical economics, to which most prehistoric archaeologists unconsciously subscribe, craft specialisation and commodification require a certain degree of surplus production and inequality visible in storage in the Near East or in the Gini coefficients of grave goods in Europe (e.g. Windler et al. 2013).

Around 4000 BC, attempts at alloying copper with first arsenic and then tin led to a regular production of Bronze in the third and second millennia BC. As a very rare metal, tin was imported from areas peripheral to the known world, such as the Hindukush and the British Isles, in transactions that were presumably more formalised than previous assumed barter exchange. Non-utilitarian items of standardised weight, such as neck-rings often found in hoards with more than 100 items, have therefore been interpreted as premonetary currency. Somewhat later in the second millennium, copper ingots of about 30 kg weight suggest that they were equivalent to a head of cattle as suggested by their peculiar ox hide shape and what first millennium accounts tell us about the *talent* as a unit of measurement. The need for tin might have been the motor behind the close network of—probably elitist—contacts visible in the material record of the Near East and Europe in the second millennium, before the rise of iron as a ubiquitous raw material accompanied its breakdown around 1000 BC at the dawn of Antiquity (Bailey 2000; Vandkilde 2006; Snell 2007).

What Can Economists Do for Prehistory?

As the examples presented here have shown, prehistorians often involve economic reasoning when interpreting their findings. However, economic terms and concepts are often used in an incorrect, biased or unreflected way. Prehistoric archaeologists tend to call only farming communities “food-producing”, whereas in economic understanding, also a basket of gathered wild berries constitutes a product. When discussing the origins of money, metallism views prevail and may blind us for early forms of fiat money. And the question whether and from what time on we can speak of trade and markets largely depends on the definition of these terms (Windler 2013). Finally, the *homo economicus* is often an implicit premise when prehistoric decision-making is explained (Kerig 2013). These are only three examples, but they call for a closer cooperation between economists and prehistoric archaeologist and open up fields for further research.

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30

The World Wars

Jari Eloranta

The world wars were the most destructive conflicts in human history, both in terms of the economic cost and the human misery they inflicted. It is important to understand the origins, impact, and outcomes of these conflicts, since they did not occur before or after the twentieth century. Thus, one of the questions we should examine is why the twentieth century? And whether the twentieth century was therefore a dismal century for mankind? To start with the latter question, we can definitely say that the twentieth century was not solely a period of misery, since the century featured rapid economic and technological growth, an increase in the number of democracies, and massive advances in living standards globally. Moreover, while the destruction brought on by the world wars suggests that the period was one of extreme violence, scholars have pointed out a global pattern of declining warfare and violence even in the twentieth century. And, in fact in comparison with previous centuries, the twentieth century in Europe was less violent (Field 2003; Pinker 2011).

In general, we should look at the world wars to address the following questions: (1) why did these wars develop into such brutal conflicts and assume the most extreme characteristics of total war? (2) how have their impact and outcomes been measured, and what do those results tell us? and, (3) what were the economic and societal conditions before and after the wars, or how long did those effects linger? Here we are discussing both Europe and the wider world, although the European theatres of war were often crucial in the context

of these conflicts. Furthermore, a lot of the newer research into these wars rely on quantitative methods, which enable us to assess the various dimensions of the war in a new way. A lot of this literature has emerged in the last 20 years or so, and especially the centennial of World War I has inspired a lot of new research (on the myths surrounding the war, see Harrison 2016). The reading list at the end of this chapter highlights some of that research.

Economic History of Warfare and Defence Economics

The study of wars is one of the key issues for most social sciences, including history and economics. In economics, a separate sub-field called defence economics is dedicated to the study of military spending as well as the causes and impact of wars, both from the supply- and demand-side perspectives. Typically, though, the contributions of defence economists are focused on current-day conflicts, and even studies on the Cold War period appear fairly sporadically. Other fields that discussed the economic impact of conflicts include sociology—such as Charles Tilly (1992), who argued that warfare forced European monarchs to give up some of their power in return for funding for their wars—as well as conflict and peace sciences in the field of political science, which are focused mostly on analysing the dynamics and causes of conflicts, often based on long-run quantitative data (see e.g. Dincecco and Prado 2012; Dincecco and Onorato 2016).

Defence economics offers us tools with which to analyse the costs and impact of conflicts. For example, they can highlight the negative impact of military spending and wars on the supply side (including crowding out effects, political distortion, destruction of human and fixed capital, etc.), along with the positive demand-side implications (increased government spending and investment, recruitment of women and minorities to work in war production during the world wars, increased government efficiency in handling crises, positive technological spillovers like the radar and nuclear power, and so on). Typically, the economic growth impact of military spending even in peacetime is slightly negative, let alone during massive conflicts like the world wars (Sandler and Hartley 1995, 2007).

There are many studies by political and military historians as well as economic historians that have also looked at long-run economic impact of wars and military spending. One of the most interesting new books is by Philip Hoffman (2015), in which he outlines how European states gained supremacy in the last 500 years due to military competition, or in “tournaments” as he

models these interactions. Hoffman's model links the high probability that European rulers would go to war to the high value of the victor's prize, similarity of resources, military technology, and the ability to mobilise those resources (absence of a hegemon is crucial). Thus, Hoffman's four conditions for Europeans' path towards global dominance include frequent war, high military spending, adoption and advancement of gunpowder technology, and relative lack of obstacles to military innovations. Europeans enjoyed low fixed costs for going to war, distances were small, variable costs for mobilisation were low, and there was a merchant base that helped with the financing of conflicts. For Hoffman, the analysis of military competition is the key to understanding how certain societies thrived and other did not.

Another example of using economic theory to analyse military history is a book by Jurgen Brauer and Hubert van Tuyll (2008). One of the issues they tackle is the location and layout of medieval and early modern castles. They employ the concept of opportunity costs and sunk costs to explain what were often inefficient fortifications, since castles were often expanded outward, especially after the fourteenth century, as a response to the emergence of gunpowder weaponry. Moreover, they argue that diminishing marginal returns set in for bigger castles, although more remains to be studied in this respect. Similar analysis could be applied to modern fortifications and certain types of military investments, which embody sunk costs.

Economic historians have also provided new perspectives into the analysis and the efficacy of economic warfare, which can range from fairly benign policy measures and pressure to outright warfare. Lance Davis and Stanley Engerman (2006) have studied one particular form of economic warfare, naval blockades, spanning several centuries. They also emphasise both the costs and challenges of sustaining a successful blockade. For example, during the Napoleonic Wars, the legalities of blockades were rather unclear, especially the issue of neutrality. The success of a blockade, as they point out, is often difficult to assess as well. Periods of actual warfare, even blockades, can bring substantial opportunities, as well as disruptions, for trade. Similar lessons apply to modern forms of economic warfare too, including sanctions, which have been ineffective in effecting the changes that those states that imposed them would have wished for.

Similar to the Napoleonic Wars, the world wars were massive global conflicts, which can be classified as "Total Wars" (Bell 2008). Such conflicts have included mobilising entire populations to fight for them both at the front lines and in the domestic economy, efforts to hurt the enemy's economy by any means necessary, and the acceptance of high civilian casualties. Moreover, as Kevin O'Rourke (2006) has argued, such conflicts also typically harm the

economic growth of the participants the most. His results show that Great Britain was the least affected of the belligerents, whereas France and the United States suffered more. The welfare losses were around 5–6 per cent of the GDP for the United States, which could be classified as substantial.

Mobilisation, Impact, and Demobilisation

We know much more about the scale and scope of the world wars than we did 40 years ago—when Alan Milward wrote his landmark contribution to the economics of World War II (Milward 1979). With the introduction of two edited volumes by Mark Harrison and Stephen Broadberry, namely Harrison (2000) and Broadberry and Harrison (2005), the extent of the mobilisation and the impact of the world wars are fairly clear. First, the economic damages from World War I amass to several hundreds of billions of US dollars (in real terms), whereas the damages from World War II were likely 5–6 times higher. This includes losses of human life, destruction of capital, wasted resources, and so on. Second, these conflicts were the costliest wars in human history, regardless of the indicator we use to measure that. Third, these wars turned into much lengthier conflicts than their participants expected, which forced them to mobilise farther and farther. Fourth, the mobilisation of richer and more democratic countries (like the United States) was the most extensive and effective, which gave the Allies an edge also in the battlefield as the constraints of total war become more apparent. Fifth, the dislocation caused by the wars for individuals and nations was immense, the demobilisation from the conflicts was difficult, and the economies tended to be prone to fiscal, social, and political instability afterwards.

The extent of the mobilisation, and the impact of the world wars, are described in Table 30.1. Most losses in World War I were incurred in the brutal trench warfare of the Western Front, and the most losses in World War II occurred in the Eastern Front—with the key battles like Stalingrad and Kursk. Civilian losses were less pronounced in World War I, given the more stationary nature of the conflict. In World War II, the extensive aerial bombings, various forms of genocide, and the more mobile and global nature of the conflict meant much higher civilian casualties, especially towards the end of the war. As Niall Ferguson has discussed, the “bang for the buck”, that is, increase in the ability to kill per dollar, increased rapidly in the twentieth century as a result of the world wars. The ultimate step in this direction was, of course, the invention of the atomic bomb, which was used for the first time in Hiroshima and Nagasaki in August 1945.

Table 30.1 Resource mobilisation by the Great Powers in World War I and World War II

Country and years in the war	Average military burden	Average defence share	Military personnel as perc. of pop.	Battle deaths as perc. of pop.
<i>France</i>				
1914–1918	43	77	11	3.5
1939–1945	–	–	4.2	0.5
<i>Germany</i>				
1914–1918	–	91	7.3	2.7
1939–1945	50	–	6.4	4.4
<i>Russia/Soviet Union</i>				
1914–1917	–	–	4.3	1.4
1939–1945	44	48	3.3	4.4
<i>UK</i>				
1914–1918	22	49	7.3	2.0
1939–1945	45	69	6.2	0.9
<i>USA</i>				
1917–1918	7.0	47	1.7	0.1
1941–1945	32	71	5.5	0.3

Source: See Eloranta (2016) for details

On aggregate, both world wars were won by the side with superior economic resources. As Ferguson (1998) has argued, Germany did not mismanage its resources and thus lose the war as a consequence. Rather, the Allies had a massive advantage in terms of total GDP, population, military personnel, armaments production, and food supply throughout the conflict; a situation that became even more pronounced when the United States finally entered the war on their side. In November 1914, the Allies had 793 million people under their control compared to 151 million for the Central Powers. By the end of the war, the Allies controlled 1272 million in terms of population (or 70 percent of world total), whereas the Central Powers' total was still under 200 million. The Allies had a massive advantage in population, territory, and GDP throughout the war; this advantage became even more disproportionate as the war went on. Moreover, even though the Central Powers initially did quite well with the limited resources they had, the Allies were able to mobilise their far superior resources better both at the home front and to the front lines. Their more democratic institutions supported the demands of the Total War effort better than their authoritarian counterparts. Therefore, the richer countries mobilised more men and materiel for the war, and their war industries proved quite capable of adapting to fulfil the needs of the war machine. Moreover, having a large peasant population turned out to be a hindrance for the production of food under wartime constraints. As Avner Offer (1989) has argued, food (or the lack of it) played a crucial part in Germany's collapse.

In World War II, the initial phase until 1942 favoured the Axis as far as strategic and economic potential was concerned. After that, the demands of Total War set in, with the United States and the USSR joining the Allies and turning the tide. For example, in 1943, the Allied total GDP was 2223 billion international dollars (in 1990 prices), whereas the Axis accounted for only 895 billion. Also, the impact of World War II was much more profound for the economies of the participants. For example, at the height of World War I the UK incurred a military burden of about 27 per cent, whereas the military burden level consistently held throughout World War II was over 50 per cent.

Finally, the war had an immediate and long-term impact on the economic development of these economies. In Germany, rationing began even before the war began. Furthermore, the cost of living steadily increased for the average family, particularly for food, and the dismantling of the German state and economy following the war left little doubt of the destructive power that World War II had on the economies of Europe. Even victors, such as the UK, experienced major losses. Based on physical capital, the British lost 18.6 per cent of their pre-war wealth. This left many concerned that the state planned to continue nationalising industries (though that fear never materialised). In the end, every nation in Europe felt the economic pain of war.

The demobilisation from the world wars was quite difficult, although more so after World War I. The belligerents in World War II were better prepared for releasing soldiers back into the society. The men who fought displaced again the various minorities and women in the work force. The bigger consequences of the world wars pertained to their macroeconomic impact. For example, World War I was followed by an imperfect peace agreement and ineffective international efforts to limit military spending, immediate exogenous shocks that hindered economic recovery, a poorly conceived return to the Gold Standard, and fragile financial and monetary system. Both world wars were part of the disrupted globalisation and decline in world trade. The legacies of war debts, border adjustments, slow implosion of colonial rule, and overall political instability were hard to resolve. The situation after World War II was different insofar that the winners of the conflict actively engaged in rebuilding the countries that lost and built up international institutions to facilitate cooperation among states, albeit after 1945 within the confines of the emerging Cold War rivalries.

Tips for the Classroom

The topic of warfare is suited for interdisciplinary discussions in the classroom, given the very nature of the problem. Moreover, the world wars are good cases to study, since they have been the biggest conflicts in human history and they offer plenty of topics for both theoretical and empirical discourse. One can discuss, for example, the effects and impact of mobilisation on the supply and demand side or how to assess the damage arising from these wars, both in the short and the long run. I would also suggest that discussions of the world wars in the classroom can effectively be linked to the events of the wars, for example, how resources ultimately were decisive in the war (tactical versus strategic dimensions). Moreover, I would also recommend the study of the world wars in case you wish to analyse the evolution of the government's role in the economy, and how the labour markets were shaped by the inclusion of minorities in the workforce.

Reading List

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31

Western Europe

Matthias Blum

We all stand on the shoulders of giants, a fact of academic life one has to subserviently admit when writing a chapter on practically any aspect of the economic history of Western Europe. A great deal of the literature in economics and economic history has been dedicated to this world region and there are good reasons for that. I will use a very convenient definition of Western Europe for the sake of this book: it is the counterpart of the corresponding chapter on Eastern Europe. The focus within this loosely defined region is naturally on a set of countries that most notably left an imprint on Europe and the world: former colonial powers and exporters of people, because Europeans have had an influence on large parts of the world through these two means.

Landes (1998) notes that in the year 1000 no one had anticipated that Europe, including its “offshoots”, would form the wealthiest and most powerful part of the world. Around this phenomenon, a debate of the reasons for this “reversal of fortunes” has evolved. There are various approaches aiming at addressing this question. There are the big theories that focus on European peculiarities, such as its geography and natural resources (Diamond 1998); others have emphasised that this reversal reflects changes in the intellectual and philosophical renewal during the eighteenth century (Mokyr 2010), or the institutional setup resulting from colonialism and international trade (Acemoglu et al. 2002, 2005). Europeans successfully exported their people, ideas, languages and institutions and this has changed

the world irreversibly, often at the expense of native populations. Moreover, Europe, or England to be more precise, was the centre of one of the defining moments of human history: the Industrial Revolution. The growth of its economies, and its international interdependence, led to dependent countries participating in Europe's economic, diplomatic and military adventures.

The result of Europe's economic success has also brought about certain infrastructural advantages. Europeans started relatively early to document economic activities, storing historical materials in archives. This has enabled us to trace European economic history better, and further back in time. Also, Europe has many relatively well-funded universities, allowing us to fall back on a constant stream of talented young researchers and funding to allow them to conduct research projects, many of which naturally relate to European matters.

The Giants

Europe's economic, financial and business history comprises of more important events and phenomena than could be possibly summarised in a single chapter. I will therefore dedicate a large share of this chapter to refer to a selection of other "giants" work and recommend how to "dip" into their work. It is important to mention that a good deal of these works are written from a macroeconomic perspective, that is, describing a country's historical development path, including a discussion of the important factors. Some works take a regional perspective when discussing development in Western Europe, emphasising that industrial development depends on local realities (Pollard 1981), while other works are question-led (e.g. 'Why Ireland Starved', Mokyr 1983). The narrative in each of the suggested readings depends partially on the perspective taken by their authors; new scholars should be aware of the methodological and epistemological choices of their predecessors. In addition to the selection provided below, most "general" economic histories discuss both Europe and its impact on the world. The diligent reader will notice that often multiple volumes were written on the same country. This should be considered a stroke of luck since more than one perspective is available, often enriched with new evidence, arguments and reflections.

A good starting point to develop a solid foundation is Broadberry and O'Rourke's (2010a, b) collection of summary chapters. This publication provides insights into Europe's growth and business cycles, sectoral developments, the role of human capital, demography, institutions, war, trade and

European integration. This work comes in two volumes, allowing the reader to study European economic history separately for the periods 1700–1870 and 1870 to the present. Other valuable (text) books include Persson and Sharp (2015), who cover the period 600 to the present, Di Vittorio (2006), covering the period since the late medieval period, and Fogel (2004), who discusses Europe in the mirror of other world regions.

Moreover, there are a series of publications on specialist topics on the economic history of Europe. Lains and Pinilla (2008), for example, provide an overview of agriculture and economic development in Europe since 1870; Landes (2003) famously highlights the importance of technological change in Europe since 1750 to the present; Kindleberger (1984) is an important classic on the financial history of Western Europe; while Klemann and Kudryashov (2013) provide insights into the economic history of Nazi-occupied Europe. Other publications cover medieval Europe (Pounds 2014), the Greco-Roman world (Scheidel et al. 2007), women and gender (Wiesner 2000) and the period 1648–1815 (Winks and Kaiser 2004). More recent decades in European economic history are discussed in Urwin (2014) and Schulze (2014).

A series of data compilations may be valuable to the interested econometrician, such as Mitchell (2007), who provides annual series on population and demographics, labour force, sectoral developments, trade, transportation and communication, prices, education and national accounts. Flora's (1983) data handbook provides quantitative information about western European states, their economies and societies between 1815 and 1975. In addition to GDP growth figures and income distribution, this volume provides state revenues and expenses, government and military personnel, families, urban living as well as trade union activity. Vast demographic information about Europe's societies can be found in Rothenbacher (2002). This volume includes statistics on population, territories, marriages, households and family life for the period 1845–1945. Needless to mention that all of these sources can be complemented with more recent national statistics, census data and secondary data from an array of relevant studies that are published by governments and NGOs.

Probably the best-researched part of Western Europe are the British Isles. The sheer consequences of the Industrial Revolution are an ongoing motivation for researchers to dedicate time and resources to investigate its economic history. Among the many “giants” who have worked on Britain, I would like to suggest a selected number of recent publications. Mokyr (2010) writes about the Age of Enlightenment and its importance for the intellectual and philosophical development that formed the basis for Europe's economic

success. Mathias (2013) and Floud et al. (2014) provide an account of Britain's economic history after 1700. These authors cover not only the Industrial Revolution and its aftermath but also provide a prelude. This is an important feature of their works as the circumstances under which the Industrial Revolution developed are illustrated.

Also very well researched is the island of Ireland and a plethora of contributions exist on virtually any aspect of Irish economic and social history. Most notably these include the Great Irish Famine (Mokyr 1985; Ó Gráda and Eiriksson 2006), agriculture (Kennedy and Solar 2007), migration (Guinnane 2015), religion and colonialism (Kennedy 1996). Others have published more general and integrative studies of Irish economic history, also for the twentieth century (Ó Gráda 1997; Daly 1981; Bielenberg and Ryan 2012). All of these volumes that constitute landmarks of British and Irish scholarship are great readings, but they are rarely updated. This shortcoming is addressed by regular updates on British and Irish economic history, published annually by *The Economic History Review* (see Hale et al. 2017 for a recent example).

As for the European Continent, Caron (2014) presents an economic history of modern France since 1815, covering the usual issues necessary to explain economic growth, such as agriculture, institutions, finances, trade and industrialisation. To go further back in time, Horn (2015) discusses the economic history of early modern France, covering matters such as the role of guilds, regulation, proto-industrialisation, commerce, the role of ethnic minorities, innovation and entrepreneurship, as well as the impact of the French Revolution. Similarly, Scribner (1996), Ogilvie (1996) and Scribner and Ogilvie (2003) provide an overview in three volumes of the economic and social history of Germany from the medieval era to the present. Borchardt (1991) illustrates Germany's role of being a latecomer to modern industrialisation until the Nazi's seizure of power. For any interested part in German business and financial history, edited volumes by Berghoff et al. (2013) and Fohlin (2007) account for Germany's rise to power are of interest.

Several volumes on Italy allow us to study its economic history since the fall of the Roman Empire (Luzzatto 1961), between 1850 and 1918 (Toniolo 1990), 1860–1990 (Zamagni 1993), and from its unification to the First World War (Fenoaltea 2011). Similar volumes exist for Spain during 1650–1800 (Sarrión 1996) and 1850–2015 (Prados de la Escosura 2017), the Netherlands during the twentieth century (Van Zanden 2005), as well as the Netherlands' financial history (t Hart et al. 1997).

The Quest for a Contribution

In light of this impressive body of literature, an important question is how a novice to the field could possibly make a novel contribution. Common strategies involve the acquisition of a new data source, the use of a new method in order to reanalyse existing information, the application of an existing research strategy to a new historical setting or a combination of these approaches. Data work is probably the most popular of these strategies, which appears to explain why researchers create new variables and whole datasets on an almost constant basis, helping to open up new areas for novel research.

Recently published articles in the field make for an encouraging perusal in this regard. Dimico et al. (2017), for example, discovered a nineteenth-century survey on the activities of mafia organisations on the Italian island of Sicily, digitised the information this survey contains and combined it with existing information to explore the origins of this form of organised crime. Larger projects involving the creation of completely new datasets, or expanding existing datasets, may require the scope of a PhD project, since conventional Bachelor's or Master's dissertations are limited in scale and scope. An exemplary case of such a large endeavour is the Maddison Project, a collaboration of scholars aiming to develop and improve existing historical GDP series in a global perspective (Bolt et al. 2018). Needless to mention, any gap in this dataset represents a potential contribution. Also, successful projects can be used as a template to lift “data treasures” and develop new projects. Even in intensely researched countries—such as the UK and Germany—archives hold many unutilised information sources that are worth investigating. As a rule of thumb, the more has been written on a country's economic history, the less scope for new projects remains, but the easier it is to link new information with existing ones—a classic trade-off that needs to be considered when planning a research project on any aspect of the economic history of Western Europe.

While new qualitative and quantitative evidence is always welcome among economic historians, more appealing still is a novel *intellectual* contribution, combining evidence, methods and ideas. While new data and empirical results often speak for themselves, the *interpretation* of empirical results and novel ideas may have the potential to be controversial. Let us take the following, somewhat daring idea to illustrate this point: Western European economic history naturally revolves around events and developments that are relevant for Europe's economic and political success. So why not focus on Western European features which have *not* prevented Europe to succeed? These

features are usually not in the spotlight since determinants for success are more attractive to investigate in a relatively successful part of the world. Take the example of ethnic diversity. A large body of empirical literature suggests that linguistic fractionalisation is associated with lower economic growth. That said, a large portion of this literature relates to sub-Saharan Africa and aims to explain the lack of sustained growth in that world region (see Alesina et al. 2016, for a recent contribution). Sub-Saharan Africa seems to be the ideal world region to conduct a research project since colonialism resulted in near-randomly drawn borders that left different ethnic groups within the same nation state while cutting through traditional settlement areas and pre-colonial societies. The outcome of such a project seems to be easy to predict as an allegedly detrimental factor to economic development is prevalent in a developing region (see Dimico 2017 for a challenge to this thesis).

In contrast, looking for detrimental effects of ethnic diversity on Europe's economic development seems not an obvious choice in light of Europe's economic success, but this certainly does not prove the absence of such an effect. In many respects, modern European borders are also a product of chance and, when looking through the lens of an anthropologist, were not exclusively (in some cases not at all!) drawn based on ethnic identity or linguistic similarity. Many European borders cut through linguistically similar settlement areas; some European countries even consist of linguistically and religiously diverse populations, yet fare very well in terms of per capita income and living standards. Historical circumstances led to the integration of some linguistically diverse territories into one nation state while other events resulted in division. Historical migration created ethnic minorities across the continent and many such minority groups have maintained their "otherness" until the present day.

Europe has seen all sorts of violent conflicts, including religiously motivated wars and genocide, but in contrast to sub-Saharan Africa, ethnic diversity, and the tension it may cause, is not typically considered a barrier to Western Europe's economic development. So what is so special about diversity in Western Europe? Is it the fact that Europeans have not inherited colonial institutions as Nunn (2007, 2008) and Acemoglu et al. (2001) describe them? Or is it the formation of nation states in the nineteenth century that formed new supra-ethnic identities? Or is it Europe's economic success that has helped to limit unrest?

How is this relevant to a new scholar interested in Western European economic history? Europe and other parts of the world have similarities but also many unique features that distinguish them. Ethnic diversity is such a similarity (although not as pronounced as in many African settings). Economic analyses, repeatedly applied to different settings, may help to disentangle the

determinants of ethnic diversity. Assume the effect of diversity on development observed in two otherwise identical empirical studies is different in sub-Saharan Africa and Europe, then (all else equal) the difference in these effects must be caused by an unobserved factor. Replication studies are quite common in disciplines other than economics, but this does not mean that combining theories, methods and settings in a novel way cannot provide vital clues and research ideas in economics too. Why not use Europe as the testing ground for theories and methods usually used in non-European settings? Other “context” chapters in this volume burst with historical experiences, ideologies, policy interventions and other natural experiments. Are there any European resemblances or parallels? Let them be your inspiration for research on European economic history.

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32

Central and Eastern Europe

Peter Foldvari

Providing an insightful answer to what Central and Eastern Europe (CEE) actually is, is not straightforward. Berend (1986) distinguishes three sub-regions within CEE: the area which is usually referred to as Eastern Central Europe comprises the countries sharing the Carpathian Basin and the Polish plains; the second is the Balkan; and finally what he refers to as Russian Eastern Europe. Szűcs (1983) on the other hand uses a more historical definition of Eastern Europe: the region between the Western part of the Roman (Frankish) Empire and the Byzantine Empire that was influenced by both as attested by the division between Catholicism and Orthodoxy, Latin letters versus Cyrillic alphabet or in the relation between state and citizens.

The three regions of CEE were all subject to some dominant external influence that can serve as possible explanation for their specific social and political characteristics. In the Balkans, this influence is identified with the Ottoman Turkish rule lasting half a millennium, while for Russia and its neighbours it is usually the Mongolian invasion and the Byzantine influence that comes into mind. For Eastern Central Europe, the set of external factors are more heterogeneous: about half of the area, previously under Roman and Frankish rule, had the legacy of Roman Law and Catholic Christian institutions and ideals. The rest of Eastern Central Europe had to import these institutions and state forming principles from the West during the last centuries of the Early Middle Ages. But by the time these newly formed Catholic kingdoms adopted Western-type feudalism, arguably only superficially, it was already beyond its zenith in Western Europe.

Whether the economic and social development of the CEE region is considered as a delayed development or a unique path mainly depends on how much we believe that the local societies were capable of adopting Western European institutions. Some initial developments—such as the relatively fast conversion to Christianity and the successful entry into the commercial circulation of Europe during the thirteenth to fifteenth centuries—confirm the delayed development argument (Szűcs 1983; Topolski 1981; Wandycz 2001). But the low degree of urbanisation with an ethnically fragmented (mostly foreign) and weak citizenry, the ultimate failure of local political elites to establish a firm base for centralised monarchies and nation states points out the contradictory nature of this medieval transition. Between the sixteenth to the eighteenth century, the previously strong medieval states of Eastern Central Europe lost their independence to their more developed or militarily stronger neighbours.¹

Another view is represented by Wallerstein's (1976) world system theory that enjoyed great popularity in Eastern Europe during the 1970s and 1980s. Wallerstein claims that the Western European markets integrated all parts of the world in the course of the sixteenth to eighteenth centuries, which led to an uneven interregional exchange of resources. The demand for agricultural goods and raw materials in the increasingly urbanised core area of the new world economy (Western Europe and later North America) allowed the nobility of the periphery to acquire the economic strength needed to counteract and revert any social and political movements that could have weakened feudalism (Szelenyi 2004).

The First Round of Modernisation

With some simplification, one could consider the last 200 years of the CEE region as a series of failed or partially successful attempts to catch up with Western Europe in terms of economic performance and well-being. Most historians would agree that, whatever the underlying reasons are, CEE could not keep up with Western Europe in terms of population growth and urbanisation until the second half of the nineteenth century. While the westernmost parts of the region—such as Austria, the German states east of the Elba river, Bohemia and to a much lesser extent Western Hungary—had already undergone a small-scale industrialisation by the 1850s, it was the

¹ On the importance of pre-industrial urbanisation see De Long and Schleifer (1993) and Bosker et al. (2013).

regaining of political independence and the formation of nation states that triggered the first, partially successful convergence period of the region. Yet, while Western Europe had many centuries to develop from the beginnings of capitalism in thirteenth-century Italy to the Industrial Revolution of the nineteenth century, in CEE this all had to happen in the course of a few decades. This also meant that old feudal institutions, such as nobility and serfdom, occasionally coexisted with an emerging class of industrial and agricultural wage workers and entrepreneurs leading to what is often referred to by Eastern European historians as a “congested society” (Berend 2003).

The idea that the CEE countries follow a special path has been most effectively popularised by Gerschenkron (1962). His seminal essay on economic backwardness identified certain typical characteristics of modernisation in peripheral countries ranging from Germany to Japan. First, unlike the early capitalist countries like Britain or the Netherlands, these countries lacked financial markets (stock exchange, bonds market) that could have financed industrialisation directly. Consequently, investment banks as intermediaries played a prominent role. But the success of banks in financing industrialisation was conditional of the existence of a middle class wide enough to produce enough savings (France, Germany, Austria and Bohemia). Where the population did not have adequate savings for the banks to collect (Hungary, Romania, Russia), the state had to take a more active role in industrialisation and engage in infrastructural investments, as demonstrated by the increasing budget deficit in these countries in the second half of the nineteenth century. Yet, as Gerschenkron notes, even though in Eastern Central European countries industrialisation became self-sustaining after a while, an active role by the state did not encourage individual business activities and proved a lasting phenomenon.

But did the region succeed in catching up to Western Europe? Convergence requires that the per capital national income in CEE countries grew on average faster than in the core European countries. Katus (1970) estimated the national income in Hungary increased by 2.5–3.0 per cent per annum in the 1870–1900 period, which translates to roughly 1.4–1.5 per cent per year in per capita terms (Bolt and van Zanden 2014). The economic growth was of similar magnitude in other CEE countries, namely in Romania (1.35 per cent) and Poland (1.56 per cent). This growth exceeded per capita economic growth in the United Kingdom (1.10 per cent), France (1.38 per cent) in the same period and was roughly comparable to Germany’s 1.56 per cent per year growth.

What can explain this apparent success? The first to note is that these countries underwent major institutional changes in the period. The abolition of

serfdom with the preserved dominance of large estates freed up a large pool of cheap unskilled labour. This was paired with new economic laws including private property laws, legalisation of selling land, corporate laws (Prussia 1843, Austria and all German states 1860) that reduced the risks of individual investors (Pistor et al. 2003). Furthermore, the development of the financial sector, paired with a relative abundance of foreign capital resulted in an easy access to capital. In line with the Solow model (1956), these favourable changes shifted the maximum attainable income level for these countries and economic growth started, albeit due to the diminishing returns to factors of production, this growth must have been temporary. This phenomenon is basically what is referred to as “economic miracle”, a phase of fast but transitory economic growth.

Whether the period of convergence could have continued remains unclear, with the shock of World War I disrupting the process of catch up, but the economic growth did not seem to have slowed down significantly prior to 1910 in the region. Altogether, there seems a consensus among economic historians that if there was a real chance for lasting and perhaps even complete convergence between the two halves of Europe, then it was during this period.

State-Socialism: An Exogenous Shock or a Stage in Development?

The reader may be surprised why the interwar period is not discussed in this text. Besides limitations in space, my motivation to skip this roughly 25-year-long period is that the few years of economic revival in the 1920s was quickly cut short by the Great Depression and a global tendency of isolation. Yet, the interwar period offers a glimpse into the post-World War II future of the region: an increase of government intervention, ranging from strict control of labour unions to state-owned firms and cooperatives gaining monopoly over the trade of agricultural goods.

Perhaps the most fundamental question is whether we should consider state-socialism as an exogenous factor, in other words an unfortunate accident in Eastern European history or rather classify it as a natural stage in the attempt of the region to catch up with Western Europe. Marx saw the communist revolution as the logical consequence of the historical development in the most developed countries and would likely not have imagined Russia to become the first officially communist country in the world. The historical dominance of the state in economic development and the weakness of civil

society resulted in low resistance to the state-socialist transformation after 1945. Hence, even though state-socialism and a single-party political system was forced upon the nations of CEE by Soviet troops, the fundamental ingredients, namely statism and central planning, fell on fertile soil in the region.

Another precedent of the post-war state-socialist period was the Stalinist industrialisation of the USSR in the 1930s, serving as a model for all Eastern European communist regimes after 1945. The quick industrialisation of the Soviet Union was then considered as obvious evidence of the viability or even the superiority of central planning. This view, however, was contested heavily from the 1970s, when the apparent difficulties of CEE economies clearly showed how inefficiently state-socialist economies allocated their scarce resources. This critical view is forcefully represented by Rosefelde (1996) who argues that the initial estimates of the Soviet economic growth were strongly biased upward and the industrialisation only achieved the impoverishment of the people. This assessment was supported by new data published by Khanin during the late 1980s that illustrated the highly inflated nature of the official soviet economic statistics (Harrison 1993). While officially national income grew 89.5 times in the USSR in the 1928–1987 period, Khanin showed that the increase was just 6.9-fold. Yet, not all consider state-socialism a complete failure. Allen (2003) claims that even with a moderate growth of aggregate consumption well-being still increased in the 1928–1940 period due to urbanisation and an apparent increase in life expectancy. Indeed, the great degree of social mobility (due to the destruction or demotion of the former elite) could temporarily make the impression of significant improvements in welfare, even in the absence of any production efficiency gains.

As for CEE countries, the rapid industrialisation and social mobility of the 1950s was quickly replaced by a slowdown of economic growth, partly as a result of the mismatch between market forces of demand and supply and partly due to the imperfect information available to the planner. Additionally, the existence of a soft budget constraint, due to the willingness of the owner (the state) to move in and bail out firms that were operating at loss, removed all motivation for pursuing economic efficiency (Kornai 1986). As a result, after an initial fast growth, which can be attributed to the post-World War II reconstruction, state-socialist countries could not sustain the initial pace of investments and their reforms in mass education did not translate into better economic performance in retrospect (Vonyo 2017b). This creates the impression that socialism was viable only in the short run and was destined to fail ultimately (Szelenyi and Szelenyi 1994).

But socialism had a more profound effect that is likely to affect the prospects of economic convergence and political integration of the region into the European core. Alesina and Fuchs-Schuendeln (2007) use survey data on former East- and West-German citizens to look for differences in the attitude towards government and social policies. They find that former East-German citizens favour state intervention and redistribution significantly more than their western counterparts and these differences may last for decades. Another consequence of socialism is shown by Boenisch and Schneider (2013) who also make use of the East-West German division as a natural experiment. Their results indicate that former East-German citizens accumulated more informal and less formal social capital than West-Germans. If their finding can be generalised, we can expect reduced prospects of civil society that could serve as a check on governments in these countries, leading to strong centralisation tendencies in the coming decades.

Conclusion

What makes the CEE region especially interesting to a social scientist, including economists, is that while it shares many common characteristics with Western Europe, it is still significantly poorer and less developed. While the history of CEE cannot technically be considered as a natural experiment, it can still be very useful to understand the source of economic success and failure. This is especially attractive in the light of the New Institutional Economics which stresses the role of informal institutions in development (see North 1990; Landes 1998; Williamson 2000). The CEE region offer plenty of possibilities for the interested scholar to measure such institutional characteristics, without the need to control for large climatic, biological or geographic differences like in Africa or Asia.

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33

Sub-Saharan Africa

Alexander Moradi

In 2000, sub-Saharan Africa was the poorest region in the world in terms of GDP per capita—and the home of the majority of the world's poor (Collier 2007). It is challenging to explain the lack of convergence between the economies of the African continent and the rest of the world (Collier and Gunning 1999). Structural change and urbanisation have not lifted national incomes, as people moving to the cities did not end up in the modern, high productivity, high wage sector but rather in the low productivity, low wage, informal sector instead (McMillan and Rodrik 2011). Lack of human capital may be a constraint, but the vast investments in schooling and expansion in formal education since the 1950s have again not made a big difference to incomes (Pritchett 2001). Economists also argued that market failures are at play; for example, that the lack of investments is caused by a shortage in national savings and foreign exchange reserves (not by a lack of opportunities and low returns to investments). However, foreign aid, intended to lift capital constraints, has not measurably increased incomes (Arndt et al. 2010).

Economic history can contribute to our understanding of why Africa is poor. It can shed light on fundamental impediments of economic growth. By taking a long-run perspective, we can reveal long-run trends, and keeping track of the dynamics allows us to identify which factors are able to overrun those trends temporarily or even break them permanently. On this basis, we can make projections about the future.

Geography, Institutions and Historical Legacies

In the last two decades, a new economic literature has emerged turning its attention to fundamental impediments of development: geography and institutions. Bloom and Sachs (1998) argued that Africa's poverty is rooted in her extraordinarily disadvantageous geography. The tropical climate results in a high-disease burden. Tropical diseases such as malaria, yellow fever and worm infestations put a significant strain on labour productivity and human capital formation. The poor soils negatively affect agricultural productivity. The large landmass, few navigable rivers and population concentrated inland result in high trade costs.

Another strand of literature emphasises institutions that are rooted in Africa's very past. Acemoglu et al. (2001) pointed to extractive, anti-growth institutions that colonial powers created and that persisted post-independence. Nunn (2008) showed that the slave trade between 1400 and 1900 is associated with lower GDP per capita today, emphasising in later works mechanisms such as a lack of trust (Nunn and Wantchekon 2011) or a population concentration in historically safe but rugged places that are not conducive for an open, modern economy (Nunn and Puga 2012). Other examples in this literature include Gennaioli and Rainer (2007) and Michalopoulos and Papaioannou (2013), who point to pre-colonial institutions, showing that delayed state formation left countries and ethnic groups poorer today. Alsan (2015), meanwhile, emphasises that pre-colonial political centralisation was constrained by the Tsetse fly. This fly inhabited back then large parts of Africa, transmitting trypanosomiasis, a parasite causing sleeping sickness in humans and nagana in livestock, thereby hampering African agricultural productivity, lowering population densities and the tax base that limited the capacity to finance a central state. This extremely innovative economics literature, excellently reviewed in Michalopoulos and Papaioannou (2018), often uses state-of-the-art econometric techniques to identify causal effects.

Compression of History

With the emphasis on historical legacies and long-term development, economists seem to have entered the very realms of economic history. However, approaches differ considerably with respect to methodology as well as focus. The economics literature tends to link some feature in the distant past to development outcomes today. Austin (2008) called this the 'compression of

history': there is so much history in between the past and today that the identity of actors, categories, and relationships turn out to be less stable than implicitly assumed. We cannot simply assume that external validity holds unanimously across time. This may limit insights and lead to factually incorrect interpretations of results.

For example, Austin (2008) criticised in how Acemoglu et al. (2001) interpreted 400 years of the pre-colonial era (c. 1500–1900) as if it was genuine colonial rule. He argued that it is just plain wrong that Europeans could simply impose anti-growth institutions without territorial control, which happened only at the end of the nineteenth century. Austin (2008) also pointed out that in Ghana and Nigeria, indigenous property rights systems proved supportive of the cash-crop revolutions of the early twentieth century. It is precisely the conditions under which time-invariant variables change their effect that are interesting. Econometricians would call these time-variant interactions. They cannot be studied in a cross-section.

The slave trade is another example. While there is a negative correlation between African slave exports and GDP per capita in 2000, there is surprisingly no such correlation when using GDP figures from 1950. Certainly, there are many ways to reconcile this seeming paradox. One explanation is measurement error in the GDP estimates—the typical reflex of data analysts. However, one may also argue that the slave trade started to affect economic growth only after independence, when various ethnic groups within African societies struggled for their role under the new system. But by a toss of the argument, this would imply that colonial regimes may have been successful in switching off distrust across ethnicities, which in turn many historians would deny. Another explanation may be that the causal effect of the slave trade has not been properly identified and that omitted variable bias is still at work: slave exports are positively correlated with trade and those countries that were relatively rich due to trade were hit hardest by the structural adjustment programmes of the 1980s. In any case, taking time seriously can open new avenues of scholarly enquiry.

To be fair, the economist's toolbox does include techniques on how to treat time more seriously. One of the standard identification strategies does precisely this: choosing a suitable comparison group and test whether pre-treatment trends run parallel and divert only after the treatment (when groups were exposed to the treatment). The economics literature, however, struggles in the African context where written historical records are fairly scarce and easily accessible data is lacking. We simply do not have a quantitative database of institutions, GDP, and many other measures between 1400 and 1950.

Measuring and Decompressing History

African economic history went extinct in the 1980s. However, in the last 10 years, a new generation of scholars revived the field. A data revolution, encompassing unprecedented advances in data collection, transcription and collaboration, is at the forefront of the research agenda tackling the lack of pre-independence quantitative information (Fourie 2016).

Jerven (2013), battering economists about their use of poor GDP estimates for African economies, is proposing his own estimates for colonial times (Jerven 2014). Moradi (2008, 2009) and Cogneau and Rouanet (2011) collected and analysed heights of thousands of Africans, showing that living standards in Ghana, Kenya and Cote d'Ivoire improved during colonial times and until the 1970s. Frankema and van Waijenburg (2012) have compiled African urban real wages, showing that unskilled real wages in Africa were rising significantly under colonial rule and were well above subsistence by 1960, exceeding levels of other developing regions, again challenging the idea of sub-Saharan Africa having always been the poorest region in the world and its economic development stagnant during the twentieth century. Meier zu Selhausen et al. (2017) uncovered more than 100,000 marriage registers across Africa, shedding light on a dynamic upward social mobility among Christian Africans, which was helped by a change in occupational structure during modernisation. There are also substantial research efforts to better understand the role of the state during colonial times (Frankema 2011; Huillery 2009; Cogneau et al. 2018). New estimates of African population growth are now available (Frankema and Jerven 2014; Manning 2010). The *African Commodity Trade Database* (ACTD) has made exports and imports statistics public, showing that African trade was expanding until the 1970s (Frankema et al. 2018b). A new database of commodity prices is in the making. There is more data on climate, health, education, taxes and so on that can be compiled from colonial yearbooks and departmental reports—digitised and easily accessible from the British Online Archives (2018).

The field is now organised around the *African Economic History Network* (AEHN), an interdisciplinary scholarly community consisting of economists and historians aiming to explore and interpret the past and what different interpretations can tell us about the present and project for the future. To promote the teaching of African economic history in African universities and elsewhere, the network published an open access textbook—*The History of African Development* (Frankema et al. 2018a). Currently, 14 chapters describe and explain various important aspects of historical African development trajectories aimed to better understand the current situation.

Predictions for the Future

After the lost decade of the 1980s, growth is recurring in several African countries in the twenty-first century. Using *The Economist's* labels, Africa has turned from “the hopeless continent” in 2000 to “rising” and “aspiring” in 2011 and 2013, respectively. How shall we judge this growth? Can this growth be sustained?

To answer such questions, the economics literature that emphasises historical legacies in economic fundamentals, but “compresses” history, is of little informative value. If external validity holds across time, persistence means precisely that. African countries cannot change their geography or history. The past is past. In a strict causal sense extractive institutions, slave trade and delayed state formation will continue to drag economic development and let differentials across space more or less remain. Trends, changing contexts and influences cannot be detected when time-warping through history. Still, a long-run perspective can be helpful to more clearly see (changing) fundamentals and contexts that have an impact on African development.

The heavy investments in infrastructure that we currently observe in Africa are not without precedent. In the early twentieth century, colonial regimes heavily invested in railroads, and for much of the same reasons as (Chinese) investors do today: access to markets and mineral resources. Jedwab and Moradi (2016) have shown that the construction of colonial railroads dramatically reduced transportation costs, which in turn increased the profitability of cocoa production, which then expanded and triggered urbanisation patterns in Ghana. Similar effects are demonstrated for the rest of Africa. Because Jedwab and Moradi (2016) studied subsequent waves of infrastructure investments throughout the twentieth century, they were able to demonstrate a decreasing marginal product of infrastructure investments; every new wave of investments (the interwar, the post-World War II railroads and the post-independence roads) showed less and less of an effect on the urban equilibrium. The reduction in transportation costs was lower, but also the problem of where cities should be located was solved by the early railroads. Hence, taking this insight and the trend would suggest that today's infrastructure investments may not lead to the large economic transformations that people hope for.

Africa's economic development today is fuelled by an expansion of trade and an improvement in the terms of trade (ToT), that is, Africans get more for their exports. A historical perspective provides three helpful observations. First, African commodity trade was expanding over much of the twentieth

century but contracted significantly in the 1980s and 1990s (Frankema and van Waijenburg 2018). Second, African economies proved vulnerable to the frequent shocks to export prices. Third, improvements in the ToT may take longer swings, but overall, the historical account of the twentieth century has largely confirmed the Prebisch-Singer hypothesis—that primary commodity producers lose in the long run. Nowadays, China's increased demand and supply on the world market stage is driving much of Africa's ToT (Kaplinsky 2006). But it is unclear whether the upward trend—or, indeed, the present level—will be maintained. Moreover, trade is reinforcing the old patterns dictated by Africa's comparative advantages in agriculture and mineral resources. In fact, this makes it more difficult to climb the next stages in the diffusion of industrialisation (Allen 2011).

Many African countries now enjoy a period of relative political stability, more democratic institutions and a reduction in armed conflicts as compared to previous decades. Bates et al. (2007) compared this to what Latin America experienced post-independence about 150 years earlier. They found striking similarities, hinting that nations and societies struggle to agree on a new order for about a half-century. Many African countries indeed began to adopt more pro-growth policies that also implied a marked change in the distribution of rents. Those parallels would suggest that Africa might be entering a period of economic growth.

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34

South Asia

Tirthankar Roy

There can be two ways of studying the economic history of a large and diverse region like South Asia.¹ One of these starts from the premise that this is a geographically diverse land, home to an unequal and hierarchical society, ethnically very mixed, and a land that has never been politically integrated until the late nineteenth century. For all these reasons, a single story about “India” or “South Asia” cannot exist. We should instead study how the big forces that shaped material life in the long run—population growth, technological change, globalisation, or imperialism—changed the pattern of inequality within the region, by affecting parts of society while leaving others almost untouched. These effects were often contradictory. For example, the British Empire in South Asia (1858–1947) achieved a political unification, but its effects on the economy was very mixed. The Empire helped private enterprise in trade and industry much more than it helped peasant agriculture. As a schoolboy in India in the 1970s, we were told repeatedly that Indian history represented “unity in diversity”. I now know that this slogan was a nationalistic project rather than a real description of the country. The reality was deep and persistent inequality. One aim of an economic history of South Asia, then, is to explain inequality within the region.

The second way to study South Asia is to start from the premise that, no matter the diversity, we can still find an average to represent the whole region. Let us assume, as Angus Maddison (2006) suggested, that per capita income

¹ I confine to the three large mainland nations, India, Pakistan, and Bangladesh, whose combined territory roughly corresponds to that of India under British colonial rule, including the princely states.

represents regional conditions well enough.² If it does, then we can compare the average income of South Asia with average income in other world regions like Europe, North America, and China. When average incomes are arranged in a graph, we should observe that South Asia fell behind the world at some point between 1820 and 1970. Answering the question why South Asia fell behind solves an enduring puzzle at the heart of world economic history—Why some countries grow rich before others do. In this second approach, the aim of an economic history of South Asia should be to explain the origin of world inequality. It should illustrate the “fall” of the non-West, as a counterpart to the theories that explain the “rise” of the West.

These two ways of doing South Asia do not co-exist happily. One of these insists that there is no one story for the whole region and the other one insists that there is, and the story is that South Asia stayed poor and fell behind. If we are faithful to regional history, that is, recognise differentiation as a central feature of the region, it becomes difficult to start with a story of world inequality that flattens these differences into one average picture. The tension is absent when historians study specific subregions or livelihood groups. Such region-bound studies without global ambitions pose no problems but attract little attention outside the region. The tension returns when we try to integrate South Asia with world history, where the big debates are. I will call these two ways of doing the economic history the micro and macro, or the regional and the global.

Four Stages in the Economic History of South Asia

A brief history of the field will show that it moved back and forth between these two modes of doing economic history. The evolution of the field happened in four stages (these were not sequential and overlapped). Economic history of India was formally born around 1900, with an ideological debate about the effects of British colonial rule (1858–1947). The most influential works were written to show that colonialism underdeveloped India by integrating the Indian economy with the British economy through trade, migration, investment, and remittance (see, especially, Dutt 1908; Naoroji 1901). Free trade destroyed artisan industry, immigration of European officers and

² Research on per capita income in South Asia has moved on since the work of Maddison. A state-of-the-art paper in this literature is Broadberry et al. (2015).

personnel involved remittance abroad, and interest on public debt was a drain on savings. Since the British Empire was keen to foster openness within the vast area that it ruled over, and gave no part of the empire a choice in the matter, this was also a criticism of nineteenth-century economic globalisation. The process helped the economy of Britain and damaged the economy of India, the argument went. The counterfactual was that without colonialism and globalisation, India, already advanced enough, would industrialise as fast as Britain did. The emerging nationalist movement believed in that counterfactual and insisted that India, when it gained freedom, would need to protect its economy from trade and foreign investment.

When the field moved into the university after 1947, it had less reason to stay political. A new generation of historians turned to the archives, turned empirical and moved to the region-bound microstudies model. There were debates about periodisation in Indian history, the suitability of European categories such as feudalism, and about the Mughal state. These were evidence-based and region-bound debates, in which historians took the lead. In 1982/1983, *The Cambridge Economic History of India* was published. The books built upon three decades of solid archival scholarship and displayed little regard for political positions.

A third turning point occurred in the 1980s, when Marxist historians criticised such apolitical region-bound writings, and tried to reconnect India with world history. They wanted to show that Europe's modernisation impoverished the third world, by erecting an exploitative economic system that operated on a world scale. They drew ideas from the nationalist reading of India. Although the Marxist brand of world history retreated in the 1990s, before it did, there was a steady fall in publications originating in South Asia, as polemics overshadowed evidence-based research.

The trend reversed after 2000. The roots of the reversal were located outside South Asia, in new tendencies within world economic history. One of these roots was the institutionalist reinterpretation of "the rise of the West" offered by Douglass North and others (North and Thomas 1973). A second root was the availability of cross-country historical income data. New developments in the theory of growth rekindled interest in that old and half-forgotten question, why do some countries grow rich and others remain poor? And finally, in the 2000s, historians like Kenneth Pomeranz (2000) questioned the idea that the rise of the West had owed to exceptional cultural and institutional traits of Europe. The exchange that followed became known as the "Divergence Debate".

In the wake of the divergence debate, Britain's economic history has been retold. So has South Asia's. Exciting new stories have been told in the last ten

years about why South Asia fell behind since the nineteenth century. Jeffrey Williamson (2011) believes that the specialisation pattern that emerged during the nineteenth-century globalisation was damaging for some countries in the world and fits India into that story. Roman Studer (2016) rehabilitates an idea going back to Adam Smith that trade costs matter to the possibilities of market exchange, showing that geographical barriers to trade acting via transportation costs were greater in India than in some European regions. Prasannan Parthasarathi (2011) suggests that India and Europe were similar in scientific and technological capability before 1800, but whereas early modern Europe had states that came to the aid of industry, Indian states did not. Others used international data sets on per capita income to test causal models that tried to explain differences in average incomes with institutional factors. India entered some of these works as well.

Fitting South Asia into World History

On closer look, one might find the new scholarship more innovative than credible. Williamson, for example, struggles to fit India into his model of the world; Studer's evidence comes from a part of the Deccan Plateau; Parthasarathi's case, according to critics, is semi-inferential. Causal models testing the effect of institutional differences between countries assume an identity between one country and one institutional situation, which is an unrealistic assumption for India.

Specific criticisms apart, there are two fundamental problems with fitting South Asia into the Divergence Debate. First, South Asia has been growing much faster than the world since the 1980s and converging to the world average income. The theoretical models used to explain why the world became more unequal from 1820 to 1970 fail to explain the recent emergence of South Asia in an easy or direct way. If the world market had been bad in the nineteenth century, why is it so good now? If institutional quality explains divergence in the nineteenth century, when and how did a reversal happen? In fact, a reversal never happened; most measures of institutional quality still place South Asia at the bottom of the lists. If models that predict divergence do not predict convergence in an easy way, they should be treated as unreliable theories of history.

Second, the inequality debate encourages the student of world history to ask the wrong question about South Asia, why it stayed poor and fell behind Europe. Whether South Asia became more like Europe, or less like it, depends on where we look—whether we look at Bombay's textile factories or at peasants

growing millets in the arid Deccan Plateau. Their experiences were fundamentally dissimilar. Agricultural productivity was one of the smallest in the arid zones and changed little during colonial rule. These areas suffered repeated and devastating famines in the last quarter of the nineteenth century. But the picture was vastly different when we move to the port cities like Bombay and Calcutta that led an Asian surge in trade and industrialisation. The volume of long-distance trade in India grew from roughly one million tons in 1840 to 160 million in 1940. As profits in trade were reinvested, India led the contemporary developing world in two leading industries of the Industrial Revolution, cotton textiles, and iron and steel.

Agriculture did not do well. But then there are no instances of an Agricultural Revolution in the arid tropics without substantial state investment and subsidisation. Capitalism did well, however, thanks not only to freedom of trade, but crucially, to free movements of capital, labour, and knowhow. These attractions concentrated in the port city with a long tradition in trading in the Indian Ocean. By starting with the falling-behind question, divergence historians missed the central paradox of Indian economic history: the coexistence of robust capitalism and stagnant agriculture.

All this is an argument to step back from the international inequality problem and refocus on the region. It is an argument against seeing regions as raw material in what historians pejoratively call “grand narratives” of world history. But what concrete projects, questions, and lessons await us when we look more closely within the region? Let me list four.

Where Do We Go from Here?

One key question is, why do world-changing forces like globalisation and colonialism cause inequality within a region? Trying to answer this question means not only exploring differences that emerged within South Asia as it took part in nineteenth-century globalisation but also making history a more effective tool to explain the present times, when again rapid economic growth has entailed growing inequality within South Asia.

A second question is, how does geography matter to the prospects of economic growth? In 1970, the Caribbean economist and later a winner of the Nobel Prize in economics, W. Arthur Lewis, published an edited book called *Tropical Development* (1970). At this time, the western world represented the only model of economic modernisation available to the world, and region-specific experiences that did not resemble that trajectory were treated as deficient. The Lewis volume recognised that pathways of change varied and that the tropical

world, because of its resource endowment and political heritage, followed a different pathway.

Japanese writers have gone further than others in explaining varieties of industrialisation. They suggest that Asian industrialisation, unlike the one that emerged in Britain and Western Europe in the nineteenth century, developed in the backdrop of an impoverished countryside and a cosmopolitan and advanced maritime capitalism concentrated in the port cities. As Kaoru Sugihara (2019) explains in a series of works, British industrialisation used a lot of fossil fuel and capital, whereas monsoon Asia needed a different pathway to industrialise, one less energy dependent, more trade dependent, and more labour intensive.

A third question comes from the need to refocus attention on capitalism, that is, on the people who set up firms and industries in the modern times. This is a question about business history—What obstacles did they encounter in starting a business, and how did they overcome these? Business historians the world over study how firms, entrepreneurs, communities, and organisations adapt to the environment or what happens to corporate governance when companies are run by families and small groups. For a large “emerging” region like India, business historians clearly need to do more. In emerging economies in general, industrialisation and commercial growth happened in the backdrop of relatively high interest rates, poor institutions, and a shortage of skills. Why did modern enterprise grow at all in a region where capital, institutions, and skills were scarce and expensive? We can ask this question for the present times, and we can ask this for the 1800s. History matters to the answer in both.

A final set of points concerns method. In postcolonial nations like India, interpretations of history tend to be influenced by a counterfactual: that without colonial rule, vastly better prospects lay ahead of the region. The burden of this counterfactual is forever present, for being a patriotic Indian means believing that India was always great but for foreign invasion. Counterfactuals like these can lead to a selective use of evidence and even a devaluation of evidence-based research altogether. This is a type of history that one should avoid doing at all costs.

The message of this essay was that economic history should be region bound and evidence based, but of course, it should also try to link the region with world history. The Divergence Debate is not a good way to do that, I suggest. A better approach is to compare similar geographies. I have suggested elsewhere that in much of the arid tropical zone, water is a scarcer resource than land and capital, and this made for a certain distinctness in the economic history of the tropics (Roy 2007). There is a shadow of Lewis and Sugihara in

this hypothesis. The hypothesis is a global one but draws connecting lines differently from the divergence debate. It calls for a comparative history linking one resource-poor region with another, rather than bunching all the poorer countries of the world into one basket, into the opposite of the rich western world. That agenda, thankfully, has been in decline for some time.

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35

East Asia

Stephen L. Morgan

The economic history of East Asia's re-emergence as a major driver of the global economy challenges our view on the sources of growth and continued growth in the region.¹ During the second half of the twentieth century, world economic activities shifted, slowly at first, from the trans-Atlantic economies to the East Asia economies. In 2016, East Asia accounted for 24 per cent of global GDP (World Bank 2017). This "reversal of fortune" from 6 per cent of global GDP in the 1940s is still short of the 39 per cent share held in the early nineteenth century (Maddison 2007). Demographic factors and human capital were major sources of this growth, which in turn was facilitated by the social capital embedded in the networks of business that underpin these economies. These two dimensions of East Asia's past are the focus of this chapter.

Economic Re-emergence in the Late Twentieth Century

Japan was first to recover from the wartime destruction, becoming a manufacturing powerhouse between 1955 and 1972 (Vestal 1993). Next followed the "Four Tigers", the newly industrialising economies (NIEs) of Taiwan, South Korea, Hong Kong and Singapore, and later the second-tier NIEs in

¹ East Asia is defined here as China, Hong Kong and Macau, Japan, North and South Korea, Mongolia, and Taiwan. Broader definitions include Singapore, Malaysia, Indonesia and Thailand.

Southeast Asia, Malaysia, Thailand and Indonesia (Chowdhury and Islam 1993). Together they were an “Asian Miracle”, their success framed as market- and export-oriented development enabled through positive government intervention, that among many things, fostered human capital (World Bank 1993). The NIEs were joined with Japan in production networks during the 1980s, which linked firms into a web of dispersed manufacturing (Hatch and Yamamura 1996).

In the 1980s, East Asia’s largest country, China, abandoned the autarky of the Maoist planned economy to embrace markets, re-engaged with the world and became the final assembler in many product-manufacturing networks (Brandt and Rawski 2008; Brandt et al. 2014; Timmer et al. 2014). Its transformation looms large in recent East Asian economic history. Its size swamps the data from the others. It has also reshaped the writing of world economic history as many attempt to explain the re-emergence and account for the earlier “failure” of China to industrialise (Brandt et al. 2014; von Glahn 2016; Vries 2015).

Demography, Ageing and Economic Growth

East Asia is populous. Today the region confronts a shrinking workforce and ageing population on a scale we have not seen before in economic history. For the past 70 years, though, their large and once young population was a source of economic growth. But past economic history most often viewed East Asia’s large populations as an obstacle to development. The neo-Malthusian view was that abundant and cheap labour, combined with scarce capital, blocked technical progress, which in China was portrayed as a “high-level equilibrium trap” (Elvin 1973). China faced the constraints of high population, scarce arable land, insufficient capital and near static technology.² Meanwhile, in Europe and especially England, scarce and expensive labour was seen as the impetus for technological innovation and the adoption of the labour-saving applications that ushered in the Industrial Revolution (Allen 2011; Mokyr 2004). Europe is also argued to have had better growth-supporting institutions, which safeguarded property rights and promoted investment in capital-intensive technologies (North 1990).

Kaoru Sugihara proposed a contrarian East Asian “labour-intensive path” for industrialisation (Sugihara 2013). He argued that Meiji Japan from the mid-nineteenth century was able to build on the accumulated skills and

²Not all agreed that technical change was static (Li 1998).

knowledge of the pre-industrial era, which combined with imported technologies made the most appropriate adaptive use of the available resources, technology and institutional factors. The abundant labour of Asia, when combined with relatively high levels of human capital, was therefore far from an obstacle to modern economic growth.

A big component of East Asia's economic growth over the past few decades was the one-off gain from the population transition and "population dividend" (Bloom and Williamson 1998). The expanding workforce of young workers drove down the total dependency ratio (working to non-working population), which raised labour productivity, aggregate GDP and per capita income. China's growth since the 1970s illustrates this well. Following the population spurt after the famine of the Great Leap Forward (1958–1960), fertility began to decline quickly, even before the one-child policy was introduced in 1980 (Zhao et al. 2018). Each year until the 2000s, the new workforce entrants were not only young but possessed higher human capital; they were increasingly better educated, healthier and more productive (Heckman 2005; Liu et al. 2008). The total dependency ratio fell rapidly from 124 per cent in 1970 to a low 49 per cent in 2015 (see Fig. 35.1). Along with the inflow of foreign capital from the 1980s, China's labour productivity (and living standards) soared.

In 2010, the demographic dividend came to an end when the workforce peaked. China's workforce is now shrinking and the population ageing. By 2060, the total dependency ratio is forecast to rise to 97 per cent and the old

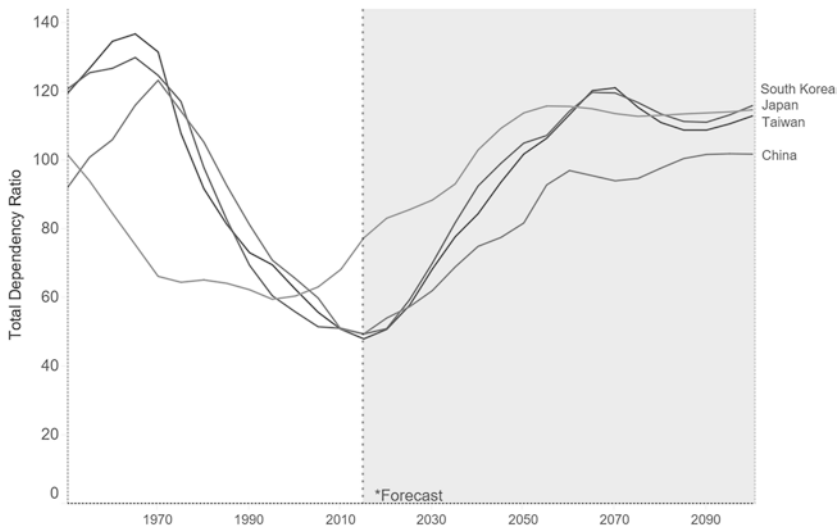


Fig. 35.1 Total dependency ratio for four East Asian economies. Source: UN (2015, 2017)

age dependency ratio (>64/20–64 years) will reach 60.2 per cent (UN 2017). The median age has increased from 19.3 years in 1970 to 35.2 years in 2010 and will reach 48.0 years in 2050 (UN 2017). China, Japan, Korea and Taiwan by the mid-century will be among the world's oldest populations. Their populations will also be smaller. Japan's total population is already shrinking and China's will begin to shrink by the 2030s.

The demographic shift in East Asia is unlike anything economists and economic historians have seen in the past. Its significance is plain if we think of economic growth in simple terms as the sum of population growth and the growth in productivity. A major driver of past growth is heading in the wrong direction, which makes sustaining growth and living standards a lot harder for East Asia. Past population declines had stemmed from calamitous events, such as war and disease, which often had short-term positive effects on wages and the standard of living. For East Asia today the question is how are they—all middle- or high-income countries—to sustain growth and living standards with a contracting workforce, a declining population and one that is also ageing? A shrinking population and static growth would raise GDP per capita, statistically, but how any individual might experience that is uncertain when ageing will increase both the state and private burden for health care and pensions (Zhao et al. 2018).

Economic history suggests these constraints will incentivise efforts to find solutions. Innovation is the panacea that China's government touts to catch up with the high-income economies. But raising productivity in China will require a different approach to how economy, politics and society is organised, one which allows for a more open and inclusive society (Acemoglu and Robinson 2012; North et al. 2012). No middle-income economy has previously made the transition to an advanced economy without political liberalisation (Shambaugh 2016). The former middle-income economies such as Taiwan and South Korea were successful in the presence of an expanding economy and the population dividend. This is not the case for China today, the world's largest economy (in purchasing power parity terms). An intriguing question is whether China will be the first middle-income country to become an advanced economy and yet remain an authoritarian and non-inclusive society.

Networked Societies, Firms and Economies

Economic, political and social life in East Asia revolves around particularistic personal social networks. These informal institutions structure everyday life in profound ways and influence the organisation and practices of firms and the

conduct of entrepreneurs. Business and trade networks have been central to the economic history of East Asia, whether the focus is within the country or between countries (Cochran 2000; Ng 1983; von Glahn 2013a). Finding a sea route to East Asia spurred the Age of Discovery and ushered in the early globalisation following the voyages to the Americas and the rounding of the Cape into the Indian Ocean (Finley and O'Rourke 2007). Europeans disrupted intra-Asian trade in many ways, their guns and aggression changing trade dynamics and restructuring polities. The flow of silver from the Americas—via Europe and the Indian Ocean and from Acapulco across the Pacific—fuelled the commercial and urban renewal in Ming China, 1368–1644, and the even more so in Qing China, 1644–1911 (von Glahn 2013b). Although Japan engaged in trading silver with China, the Tokugawa Shogunate (1603–1867) curtailed international trade, which makes Japan a “natural experiment” in the effect of trade opening after 1868 (Bernhofen and Brown 2005). However, the role of firm and personal networks, historically and economically, is an under-researched topic.

Asian firms are networked firms. Formation of their ties in supply chain and subcontracting networks differ from those western firms create, though this is obscured by the prevalence of the distributed or “fragmented” production chains that have marked the rise of East and Southeast Asia as pivots of the global economy (Timmer et al. 2014). Automobiles, electronic and electrical products, and even garments and toys, are joined in complex networks where discrete processes take place in separate countries. Once the labour-intensive final assembler, who contributed little value added, Asian firms are narrowing the gap with the frontier economies.

In describing the networked East Asian firms, authors have invoked terms like alliance capitalism, bamboo networks, *guanxi* (connection) capitalism, network capitalism and so on (Gerlach 1992; Weidenbaum and Hughes 1996; Hamilton 2006). Their intent is to emphasise the importance of connections between firms. Connections and network ties are important for European and the American firms too. The strategic management literature has for the past two decades explored how ‘industries can no longer be meaningfully analyzed without considering the strategic networks that bind’ (Gulati et al. 2000). In East Asia, the management, organisation and business practices of the firm are especially bound up with particularistic social networks. These reach into the fabric of community and personal life. This is as true of the Chinese family firm as it is of the private conglomerate or the large state entities in contemporary China (Redding 1990; Lin and Milhaupt 2013; Zhao and Morgan 2016). The challenge for economists, economic historians

and management researchers is not in mapping the ties but measuring their “content”: what is communicated, transferred or influenced through the ties? These networks increase information and trust among members, reduce asymmetric information with positive effects on coordination and monitoring, allow the capture and sharing of resources and capabilities and enable firms to influence political actors. The “social logic” of these networks embedded in everyday business practices “enhance economic rationality” in ways similar to law in western societies (Chung and Hamilton 2001). But whatever the logic of the networks, the testing of their effect empirically in the present or the past is a challenge.

That East Asia has been so successful economically despite often ineffective and inefficient institutions is simply a wonder. Difference in state capacity, for example, explains a lot about China falling behind not only Europe but also falling behind Japan before the twentieth century (Sng and Moriguchi 2014; Francks 2016). It also explains the differences in timing of post-1945 development of East Asia. While China’s party state may take credit for the transformation, Allen observed (2011: 144) that institutions are so imperfect that the question we need ask is ‘why has its [China’s] mediocre market institutions worked as well as they have?’ Markets and private business in China have succeeded on the basis of deep-rooted informal institutions, the social networks that tie, which I suggest provide an informal governance mechanism to reduce uncertainty and which substitute for ineffective formal institutions and mitigate adverse state actions.

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Australasia

Les Oxley

Geographically, Australasia includes Australia, the island of New Guinea, some eastern islands of the Indonesian archipelago and the Pacific islands of New Zealand, Vanuatu, the Solomon Islands and New Caledonia. Politically and more commonly, however, Australasia refers just to Australia and New Zealand. The origins of Australia and New Zealand were as British Dominions—semi-autonomous polities—until the Statute of Westminster in 1931 confirmed on all British Dominions their full legislative independence.

Socially, economically and politically, Australia and New Zealand share similar historical characteristics having European, typically British Settler, origins. The original economic drivers, and ultimately migration patterns, came initially from the harvesting of the seas (whales and seals), followed by the discovery of gold and other minerals, first in Australia, 1851, followed by New Zealand in the 1860s. Australia's settler origins also have the unique contribution derived from British penal transportation initiated with the first convicts transported to Australia in 1788. Those in New Zealand are connected more closely to the Crown via the 1840 Treaty of Waitangi and the large-scale settlement practices see (Hawke 1985).

The common view of both Dominions, however, is their transformation of the land into an economic resource via natural resource extraction, agriculture and particularly pastoralism. This was facilitated, by the end of the nineteenth century, with the widespread use of refrigeration.

Australia

The Federation of Australia in 1901 acted as a watershed in the transformation of Australia as an economy and society. From around 1820 to 1901, Australian outputs were led by wool and gold, although other natural resources also contributed to economic development. Vamplew (1987) argues that the primary sector contributed 31 per cent of Australia's GDP per capita, leading to internationally leading levels of average incomes. However, this early period was not led by export growth, where only about 2 per cent of GDP was exports in 1820 (Greasley 2015: 152). By 1850, however, wool accounted for approximately 94 per cent of New South Wales's exports, with exports overall now around 25 per cent of GDP. However, the 1850s were defined by the growth of pastoralism, with the rapid growth of the colonial population only exceeded by those of the flocks of sheep. The growth of population pushed the farming frontiers ever outwards, with the demands for fine merino wool and beef leading to a population of around 3.1 million sheep and 640,000 cattle in 1838. The first pastoral boom ended in the 1840s and was followed by a transformation of agriculture to wheat, maize and potatoes. Gold may have led to Australia's economy growing 300 per cent in the 1850s; minerals and mining more generally did not become a dominant and sustainable force until the 1890s with the expansion of mining, facilitated by the opening of new lands. This was against the background of a fivefold increase of sheep and cattle stocks between 1850 and 1890.

The 1890s was the beginning of an almost fifty-year period of slow economic growth, which first had local origins in the bust in property prices. It was not until the 1960s that Australian levels of real GDP per capita exceeded those of the pre-1890s (see Greasley and Oxley 1998). In contrast, manufacturing's contribution to economic growth and development in Australia peaked at 30 per cent of the GDP in the 1960s, later to languish at around 10 per cent by the new millennium.

New Zealand

New Zealand's early economic development was based upon an abundance of land and natural resources relative to labour and capital. In the period 1840–1890, economic activity centred on the South Island, and on the export of wool and gold. Thereafter, the economic frontier shifted northwards, stimulated by the export of dairy products and frozen meat. Refrigeration and

dairying technology developments from the 1880s were partly responsible, but bringing more of the richer, wetter lands of the North Island into productive use also depended upon the construction of a transport infrastructure and on the pacification of the Māori. By 1913, dairy products and frozen meat exports exceeded the value of wool exports and were twice as great in 1920.

Like most primary producers, New Zealand was hit hard by collapsing export prices at the onset of the Great Depression, but her recovery from the slump was both early and unusually fast. New Zealand's real GDP declined 16.6 per cent between 1929 and 1931, but by 1938, New Zealand's real GDP stood 46.3 per cent above its 1929 level. Over the same period, real GDP growth in Australia, Denmark and the UK ranged between 18 and 22 per cent and was 10 per cent in Argentina. In 1938, New Zealand had, in Maddison's (1992) estimation, the world's highest GDP per capita, adjusted for purchasing power parity, with a level 3–5 per cent above that of Switzerland and the USA and 8–13 per cent above that of Australia and the UK. If one considers GDP and real wages to be relatively narrow and imprecise measures of well-being, one can turn to Crafts's historical Human Development Index (HDI) (see Crafts 1997), which places New Zealand and Australia, respectively first and second in 1913 (with the UK and USA respectively sixth and eighth). By 1950, New Zealand remained top, however, by 1975, it had fallen to fifth and then sixth in 1999.

Similarities and Differences

Both are ex-British Dominions; both have had gold rushes; both have pasts where the pastoral sector has been the dominant sector; both countries have lead the world in terms of living standards and both no longer do so. So where are the differences?

Indigenous Populations

Australia's indigenous people, "Aboriginals" and "Torres Straits Islanders", have lived traditional nomadic lives for thousands of years. Currently, however, they have little if any political power and suffer some of the worst health outcomes with suicide being double the rate of non-Aboriginals. Colonial migrants did not attempt to integrate the Aborigines and the Australian Constitution makes no references to them.

In contrast, New Zealand's indigenous peoples, the Māori, are currently an effective political group. The differences stem in part from the origins of Māori-European first contacts, where trade was an important feature, but crucially the signing of the Treaty of Waitangi on 6 February 1840, introduced an agreement between Māori Chiefs and the British Crown, which still forms the basis of the relationship and powers of the two. The basis of the Treaty was a broad statement on the principles by which the British and the Māori would found a nation and build a government in New Zealand. The Māori gave the Crown an exclusive right to buy lands they wished to sell, and, in return, were guaranteed full rights of ownership of their lands, forests, fisheries and other possessions. The Māori were given the rights and privileges of British subjects.

In reality, the outcomes were far less agreeable, with significant confiscation of Māori lands leaving them with the poorer, less fertile areas. The New Zealand Land Wars (1845–1872) between the Māori and the New Zealand government were in the main about different views on the value and use of land but also about the actual social and political roles for Māori, post the Treaty. The Māori population of around 90,000–100,000 in 1840 declined to 60,000 in 1860 (approximately the same size as the growing European population) and then to 39,000 at the first Māori Census in 1896. By contrast, in 1896, the European population was around 673,000. Some of this decline came from the new diseases introduced by settlers and no doubt the effects of land confiscation and the effects of the Land Wars.

Although Māori and Pacific Island peoples in New Zealand suffer some of the health and well-being disadvantages of Australian Aboriginals, economic, social and political differences between these two indigenous groups today could not be starker.

Land Use and Land Ownership

The 1880s saw considerable political debate in New Zealand on land policy, particularly as a route to promoting more intensive settlement (see Greasley and Oxley 2005: 26–30, for more details). This led to expansions in the farm frontier north in New Zealand supported by the subdivision of the “great estates” in the south, which broke the land congestion. Land monopoly was associated in New Zealand with estates in excess of 10,000 acres, but these accounted for only 3.5 million acres of occupied land in 1910, compared to 7.8 million acres in 1892. In contrast, the number of land holdings in New Zealand almost doubled from 43,777 to 84,076 in the period 1891–1921, and their average size fell. Small and medium-sized farms dominated these,

with 44.1 per cent of holdings being in the 100–1000 acre range. The move to closer settlement had important implications for rural land market activity and farm productivity. For a more detailed discussion on this issue, see Greasley and Oxley (2009).

One contemporary analyst (Condliffe 1936) noted that New Zealand might have developed on the South American model, characterised by extensive pastoral farming closely connected to the large-scale industrial processing of foodstuffs without such institutional reforms including those related to land reform (Greasley and Oxley 2009: 329).

In contrast, Australian agriculture was based on extensive farming, more akin to the US mid and southwest model. Dairying in Victoria was markedly different to the North Island of New Zealand (Greasley 2015: 169). The heady heights of pastoralism's 25 per cent contribution to GDP found in Australia during the mid-late nineteenth century showed a decline to 10 per cent for the new century up to World War I, replaced by mining and manufacturing.

Immigration

Both Australia and New Zealand faced, as settler economies, an initial scarcity of labour. In the case of Australia, the origins of the nation as a penal colony also introduced a particular male gender balance into the population as it grew (see e.g. Seltzer 2015). The sheer size of the Australian landmass, however, meant that throughout the nineteenth century Australia was very sparsely populated even by the standards of Canada and the USA (see Seltzer p. 179, Table 8.1).

New Zealand migrants were typically self-funded and were often whole families. Although the English dominated in terms of numbers, Scotland provided more migrants as a proportion of their homeland representativeness. State subsidies for migrants occurred for a relatively short period of time in the 1870s in New Zealand and were reintroduced, for a while, after World War I. Between 1947 and 1975, a total of 77,000 men, women and children arrived from Great Britain, under the assisted passage scheme, with smaller numbers coming from the Netherlands and other European countries. In New Zealand, by the mid-1960s, natural increases in population far outweighed net migration, which was by that time less than one-tenth of the total population growth. An exception to this trend in New Zealand came only in 2002–2003.

There has been a long history of trans-Tasman migration flows. The 1880s slowing of growth in New Zealand saw perhaps the first major outflow of New Zealanders to Australia. However, this was short-lived, with the onset of

the 1890s depression in Victoria seeing a reversal. This pattern of trans-Tasman flows was to become a feature of the twentieth century, with the 1960s and 1970s and later 1990s being a particularly intense period of outflows to Australia, resulting again from growth slowdowns in New Zealand.

Phillips et al. (2010: 15) present data on net overseas migration (NOM) for Australia from 1901 and the pattern may surprise some in terms of the number of times NOM was actually negative. This reflects the point earlier regarding trans-Tasman, business cycle-related, migration. According to the Australian Department of Immigration and Citizenship, the contribution of immigrants from all parts of the world to Australian society, culture and prosperity has been an important factor in “shaping the nation”. Between 1945 and 2010, approximately seven million permanent migrants have settled in Australia, adding to the 700,000 that settled between 1905 and 1945 (Phillips et al. 2010). Compared to New Zealand, Australia has tended to receive migrants from a much wider range of countries, particularly from Mediterranean countries and Asia, although the contribution from Asia has grown dramatically in New Zealand recently.

Resource Curse or Boon?

Natural resource and land abundance is sometimes seen as a curse in terms of economic growth due to, for example, unfavourable effects related to exchange rate appreciation (the Dutch Disease), domestic inflation or crowding out of education in resource-rich countries. Australia, the most land-abundant country by 2006 (Greasley and Madsen 2010: 315), and, perhaps to a lesser extent, New Zealand certainly fit into the “natural resource and land-abundance” categorisation—the issue here is whether this effectively became a curse and, if not, why?

In New Zealand, issues to do with the ownership of land (discussed earlier in this chapter) led to continuous increases in the intensiveness of land use, increasing productivity in, for example, the dairy-related industries. Knowledge spillovers were led from the mining and latterly pastoral sector to other areas of the economy with patenting per capita some of the highest in the world.

Turning to Australia, Greasley and Madsen (2010) consider the balance of boon/curse in 16 OECD countries, concentrating particularly on Australia. They conclude that mineral resources stimulated knowledge creation in Australia, but that land abundance alone does not (see their Table 7). Interestingly, they also highlight that the mining and resource sector is much

less important than land abundance with idiosyncratic bursts in the mid-nineteenth and early twentieth centuries and the 1960s, driven in part by commodity price changes fuelled by the growth of China in the late twentieth century, and, up to the new millennium at least, increases in productivity in the sector. Minerals did produce positive productivity externalities in Australia but not in a ‘singularly exceptional way’ (Greasley and Madsen 2010: 322). The mineral-GDP ratio has, in fact, been more important in the European countries than Australia and New Zealand, and hence, the traditional route via crowding out manufacturing has been less important, especially in New Zealand where that sector was and is relatively small compared to other OECD countries.

New Zealand, it seemed, “dodged the resource curse bullet” in part by maintaining a large pastoral-based sector as it developed into the twentieth century, with positive knowledge spillovers in productivity coming from knowledge creation in both the minerals and pastoral sectors. The underdevelopment of a manufacturing sector relative to other OECD countries meant that any Dutch Disease effects would be small. Furthermore, the minerals sector’s contribution declined over time compared to Australia.

In Australia, land-abundance effects dominate mineral-based effects and, given the former, has more curse than boon characteristics. Linking land abundance with low agglomeration (due to a large rural sector), then Australia seems to be less able to reap the rewards associated with the new economic geography that sees positive externalities coming from the growth of cities (Greasley and Madsen 2010). Although not covered by Greasley and Madsen, the lack of knowledge spillover and agglomeration effects could be equally applied to New Zealand. For Australia, resource is a curse *and* a boon (see also Greasley et al. 2017).

Tracing the rise and subsequent fall of Australia and New Zealand from the top of the economic growth league table at the beginning of the twentieth century to their current status of “middle of the economic growth pack” remains a fascinating area for future investigation with many as yet unanswered questions. For example, in gauging the relative scale and effects of agglomeration and knowledge spillovers in the two countries over the twentieth century. Similarly, Greasley and Madsen’s (2010) excellent work on the resource curse or boon would benefit from comparisons that include New Zealand. Finally, recent work by Greasley (2015) and Qasim et al. (2018) has highlighted some new approaches to measuring sustainable development, using Australia and New Zealand as pioneering cases.¹ However, opportuni-

¹ Countries where the approach has been applied other than New Zealand and Australia include Britain, the USA and Germany, see Greasley et al. (2014) and Hanley et al. (2016).

ties to apply this approach to other countries promises some interesting and fascinating results where, in this case, the results from Australasia are “leading the pack” in terms of new knowledge and conclusions relating to sustainable economic development in the long run.

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37

North America

Price V. Fishback

North America is a large continent endowed with large amounts of fertile soil and natural resources. The economic histories of the USA, Canada, and Mexico offer contrasting examples that can illustrate multiple themes in world economic history. The trend lines in Fig. 37.1 show that the USA and Canada have ranked among the richest nations in the world for the past two and one-half centuries. Mexico ranked relatively high in the late 1700s but has grown much more slowly since then. It now ranks in the middle of the world distribution with a current per capita GDP of roughly one-third of the level of its northern neighbours (Fig. 37.1). As is the case with all comparisons of economic development, there are multiple reasons for the difference in the development of these countries.¹ The discussion here focuses on differences in the institutional structures of the economies.

Over the last several decades the literature on worldwide economic development has increasingly recognised the importance of institutions to economic growth. Douglass North (1981, 1990) received the Nobel Prize in 1993 for his work in this area. A large narrative and quantitative literature in the social sciences finds that per capita incomes tend to be substantially higher in countries that have the following features. They protect individual freedom, define and enforce property rights with a broad distribution of property

¹ For discussions of a broad range of topics in American economic history that discusses the research over the last decade and predictions of where the research will be going in those areas, see the volume edited by Cain et al. (2018). For descriptions of the role of government, see Fishback et al. (2007).

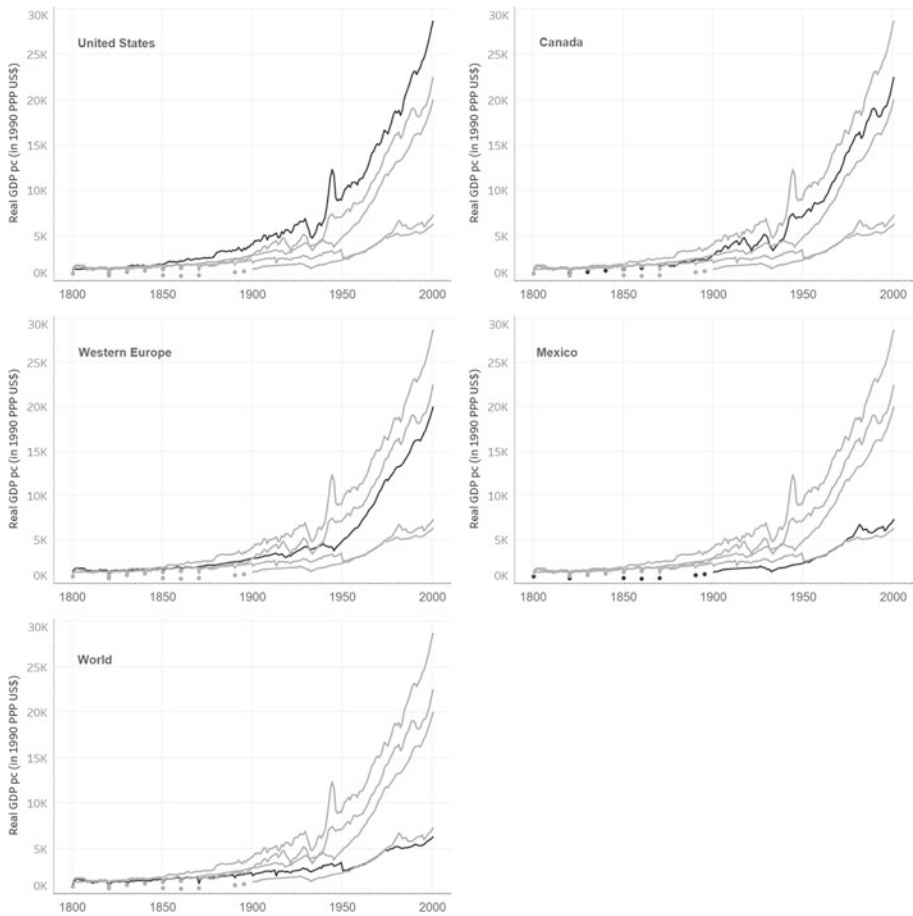


Fig. 37.1 Real GDP per capita in 1990 PPP dollars. Source: Bolt et al. (2015)

among the populace, and provide access to ex ante unbiased courts. They do not erect internal barriers to movement of goods, services, and people. They have long offered broad-based access to education, and they promote democratic political structures that lead to peaceful transfers of power (e.g., see Acemoglu and Robinson 2012; Barro and Sala-i-Martin 2003; Goldin and Katz 2008; Knack and Keefer 1995). Douglass North et al. (2009) suggest that these institutions signal the presence of *open access orders* that allow large numbers of people and organisations to participate freely and effectively in economic activity. The vast majority of countries over time have been *limited access orders* in which a relatively small number of elites control access to the political and economic levers of power in the economy.

A rough idea of the differences can be shown with indices of democracy and economic freedom. The USA and Canada have long ranked at the maximum of the Polity 2 scale of democracy (see Fig. 37.2). In the early 1800s, the US score rose from 4 to the maximum of 10 and has stayed there since. This score is too high for the American South for most of the period, because the slaves were not freed until the 1860s, and blacks were disenfranchised until the 1960s in many parts of the South. After gaining independence in 1867, Canada was well above the average scores in Western Europe and the rest of the world and joined the USA at the maximum by 1920. When Mexico gained its independence from Spain circa 1820, it too started above the rest of the world but descended to a score of minus 9 near the autocracy minimum of minus 10 during the dictatorship of Porfirio Diaz after 1876. The score bounced to zero during the Mexican Revolution of the 1910s, but fell back to minus 6 from 1930 to 1976 as the Partido Revolucionario Institucional (PRI) dominated the political structure of the country. Since then, it has risen to 8 in the 2000s, as the political parties have become more competitive. On the Economic Freedom Index developed by the Heritage Foundation and Wall Street Journal since 1995, Canada and the USA are considered mostly free, Mexico is considered moderately free, and the world average sits on the borderline between moderately free and mostly unfree.² More detailed narrative descriptions of Mexico's development offer similar pictures of Mexican political economy (Bortz and Haber 2002; Haber et al. 2003; Diaz-Cayeros 2013).

Economic historians have suggested two broad reasons for why the institutions developed so differently within North America: resource endowments and the colonial government structures imported from the home country. The initial endowments of resources when Europeans first settled led each area to specialise in different exports in the international economy (McCusker and Menard 1991). Stan Engerman and Ken Sokoloff (2012) argue that these "staples" were associated with economic structures that ranged from slavery to small farm holdings, which in turn influenced the income distribution and the political structures that developed. North (1966) made a similar argument about staples and regional differences in development in the USA through 1860. Coercion in gold and silver mining in Mexico and slavery in the production of sugar, tobacco, rice, and later cotton were associated with highly unequal income distributions and political economies. Meanwhile, the colonial economies based on food production, trade activity, and export of

² See <https://www.heritage.org/index/visualize>.

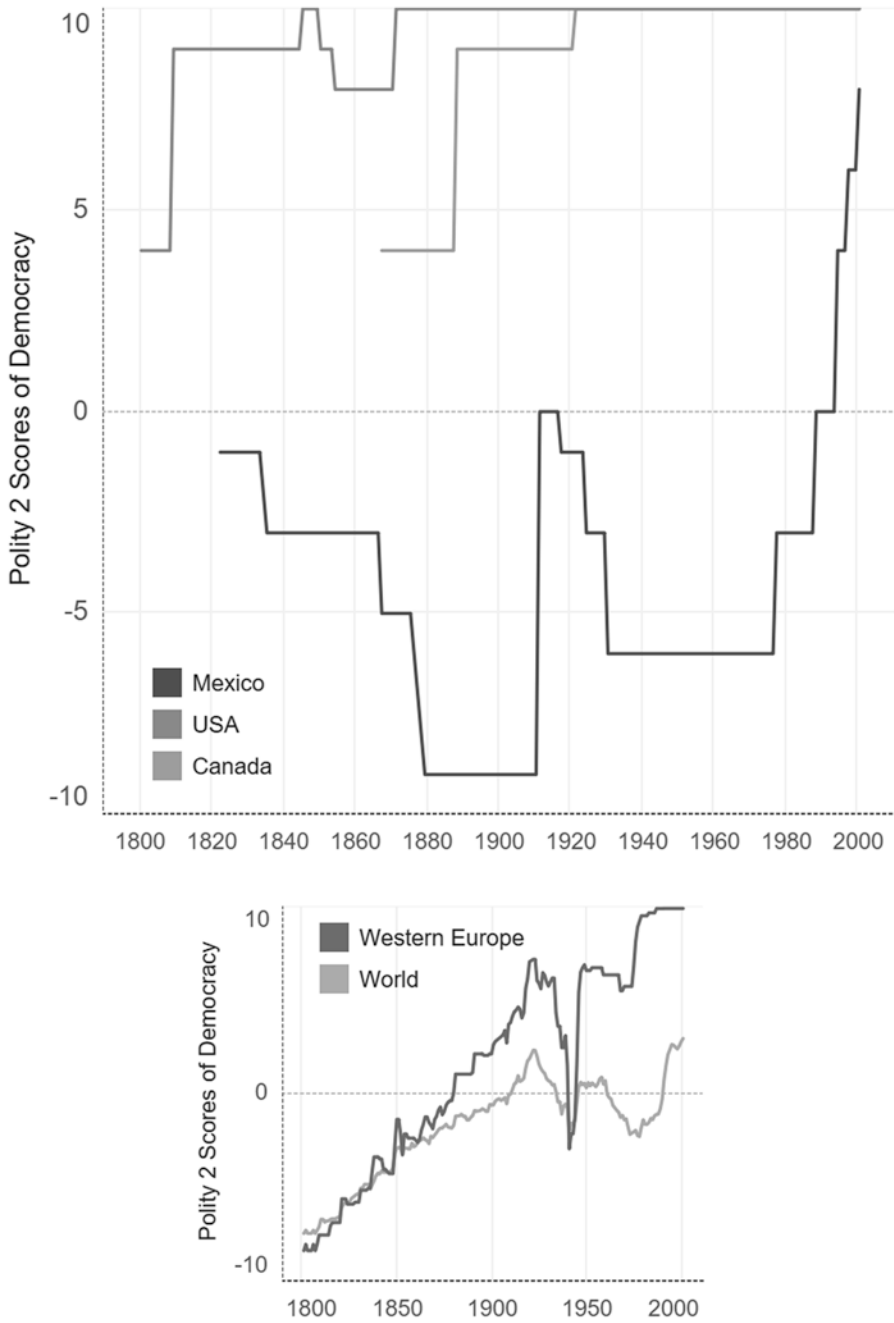


Fig. 37.2 Polity 2 scores of democracy. Source: Marshall et al. (2015)

fish, timber, and furs tended to have more equal income distributions and political economies more conducive to growth.

When North et al. (2000) compared the USA and Latin America, they argued that differences in how Spain and England organised their colonies led to differences in institutions after independence that put Mexico at a disadvantage. The Spanish organisation in Mexico developed a 'system of privilege based on personal and corporate connections with the Crown', while the English developed a more decentralised system in which land ownership was based on rules enforced by courts and colonists had representative legislatures (p. 52). Upon gaining independence, the USA and Canada could build on a representative system with individual freedom and rule of law, while Mexican independence led to a continuing struggle between interests seeking to regain the monopolistic control they had under the Spanish Crown (p. 53). Both sets of scholars agree that both endowments and institutions are important, and somehow related to one another. They and other scholars differ on how much weight to give to these and other factors as explanations.

The Mexican Experience

When the Spanish began arriving in Mexico in 1513, they gained control in large part because most of the local population died when they came into contact with European diseases. Relatively few Spaniards migrated to the new world. Instead, the Spanish Crown sent over administrators to oversee the mining of gold and silver by the indigenous population. This elite essentially ran the colony to their own benefit and that of the Spanish Crown, which led to a highly uneven income distribution. By the late 1700s, the Spanish monarch had relaxed some of the restrictions, but the local bureaucracy still focused on its ties to Spain while maintaining substantial control by forbidding the vast majority from bearing arms, and maintaining tight control over mining and the ports.

The War of Independence with Spain from 1810 to 1821 created new opportunities for open access by giving a large majority of males the right to vote, but a period of political instability followed. The initial monarchy was displaced by a republic, which then was dominated by Santa Anna until 1854. A revolt in Texas in 1835 and an American invasion in 1848 caused Mexico to cede roughly half its land to the USA. Reformers overthrew Santa Anna in 1854, Benito Juarez became president, and a new constitution in 1857 introduced separation of church and state and equality before the law. A Civil War

over the reforms soon followed, and the losers invited France to invade and establish a new monarchy under Maximilian. When France withdrew support, Maximilian was dethroned and executed in 1867, and Benito Juarez returned as President. Within a few years of Juarez's death, military leader Porfirio Diaz took control in 1876 and established an autocratic central government that kept the peace until the Mexican Revolution began in 1910. Under Diaz, the economy grew but was run by a powerful and relatively small elite with close family ties and joint memberships on corporate boards, while Diaz's military kept a tight rein on social protest (Coatsworth 1978; Haber et al. 2003).

The Mexican Revolution in the 1910s promoted land reform, universal education, workers' rights, and social protections. Mexico seemed to be on the path to an open access order, but the PRI developed a new ruling coalition that incorporated labour unions and peasant organisations and controlled the central government from 1929 to 2000. The PRI government expropriated the oil fields and developed state enterprises in telecom, railroads, steel, utilities, sugar mills, paper and airlines—and limited imports to protect them. After a debt crisis in the early 1980s the government began to privatise the industries, improve the rule of law, and signed the North American Free Trade Agreement (Diaz-Cayeros 2013). Mexico is now recognised as a democracy, and its measures of economic freedom have improved.

The USA and Canada

After 1600, settlers from England, France, and the Netherlands began establishing colonies along the Atlantic coastline and further inland along larger rivers. Disputes with the indigenous tribes typically ended with peace accords or the tribes moving westwards, a pattern that continued for nearly three centuries. The settlers tended to be more independent of their home monarchies than in Spanish colonies. A significant number came as part of corporations or as groups seeking safe havens where they could practise their religion. Under English rule, the colonists had their own legislatures and often made economic decisions that were limited only by laxly enforced parliamentary requirements that exports come through England. Property was distributed by rule much more than through political contacts. When England sought to tighten enforcement and increase colonial taxes to cover defence costs, a tit-for-tat series of protests and rounds of regulations escalated into the American Revolution. By 1781, the American colonists gained independence despite the apathy or opposition of over half of the colonial population and only a handful of military victories.

Building on the existing English institutions, the USA restructured its government in 1787 by writing a new constitution that came out of the debates, negotiations, and compromises of a convention and then ratification decisions by the states. The document was influenced by a complex combination of the economic interests and ideologies of the convention participants and of the constituents in the states (McGuire 2003). When combined with the Bill of Rights, it established a wide range of individual rights and freedoms, including protection of private property, patents and copyrights, free speech, freedom of the press, freedom of religion, the right to sign contracts without government interference, a representative democracy, the right to trial by a jury of one's peers, and a series of other individual rights too numerous to mention here. The document gave the national government the right to collect some taxes, to engage in foreign policy, and removed barriers to mobility of goods and people across states.

Many of the ideas and the institutions supported in the constitution had been developing in England and in other countries during the Enlightenment, but they came to full fruition in North America. The Canadians had a number of these rights under British control, and their rights expanded along with the rights of the English as they made progress towards independence with Confederation in 1867. Many countries have written similar constitutions, but world history shows that the document alone is not enough. The key to its success in the USA and later Canada was the commitment of people and governments to follow the strictures of the document in their policies and in the decisions made by courts, presidents, governors, mayors, legislators, and administrators. A reading of debates in the 1790s shows how easily US officials might have backtracked on the constitutional rights. George Washington as the first American president led the way to peaceful transitions to new leaders by stepping down after two terms. The majority successfully defended the new rights, and Chief Justice John Marshall on the Supreme Court prevented attempts by governments to interfere with private contracts and to limit trade of goods and services across state lines.

The general trend has been to expand the voting rights of the populace. Property requirements for voting were eventually eliminated. Popular votes for senators and a variety of other reforms were established in the Progressive Era of the early 1900s. Women obtained voting rights in various states in the late 1800s and early 1900s, and for the national government in 1920. Slavery was eliminated in the 1860s and eventually the Jim Crow laws that had disenfranchised blacks after slavery were struck down in the 1950s and 1960s (Fishback et al. 2007).

The consequences of the maltreatment of slaves and Native Americans help illustrate the importance of the institutions described here. These groups were not provided the same protections and institutions available to the rest of the society, and their economic welfare suffered. Slaves lacked rights to self-determination (Fogel 1994). After they were freed during the Civil War, many state and local governments failed to enforce their property rights and protect their freedoms, while segregation based on laws and social norms limited their opportunities for advancement (Higgs 1977). Meanwhile, white settlers encroached on the lands where Native Americans had long lived. After numerous skirmishes and treaties, the tribes were given or forced onto reservations, where their property rights were subject to paternal control by government officials. Even within the reservations boundaries, Native Americans lost access to many productive resources as a consequence of the way the property rights were administered (Carlson 1983). Large numbers of people in these groups have made impressive gains in income and status in overcoming these obstacles, while others still struggle to overcome the legacies of these policies. As a result, the average incomes for these groups still lag behind the overall average for the USA in census statistics.

Concluding Remarks

The study of North America offers a wide range of teaching and research opportunities. Lectures on the economics of property rights, institutions, and economic success can be built around the analysis described here. It can easily be expanded to comparisons with the rest of Latin America and the world. When teaching about the modern economy, historical examples show how the decisions on similar issues in the past influence the debates over such issues as tariffs, the minimum wage, antitrust, macroeconomic policy, and the impact on society of technological changes. The historical examples also have the advantage of showing the long-range impacts of those past decisions.

The opportunities to research the long- and short-range changes in economic activity have increased with the development of a wide range of new data sources in North America. The Integrated Public Use Microdata Samples (IPUMS, <http://www.ipums.org>) now has 100 per cent samples from a variety of census years that help in linking people across time. The Census and other government agencies have been setting up Restricted Access Data Centers around the country that permit scholars to link people and firms over extended periods of time even up to the current day. In addition, economic historians are providing access to new datasets on economic policies and a wide range of

economic activities that can be combined with these resources. One example is a dataset from my own research with Shawn Kantor and others on Roosevelt's New Deal in the 1930s (Fishback 2017). In developing research on past events, avoid reinventing the wheel by learning about the research already done by economic historians on the topic. They typically have investigated the institutional context in which people were making decisions with an economist's eye. Their work can serve as a building block or complement to new research that incorporates additional sources and techniques.

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38

Latin America

Leonardo Weller

Latin America is part of the Americas. This tautological statement is often overlooked. Like the USA and Canada, Latin American countries were colonies of Western European Empires. Eurasian diseases vanished pre-Columbian civilisations, and, as a result, a high rate of natural resources to labour has characterised the whole continent since colonial times. In spite of these similar European backgrounds and factor endowments, however, Latin America is several times poorer than its northern neighbours.

Latin America's underdevelopment becomes an intriguing puzzle if one considers long-term trends. Figure 38.1 shows that GDP per capita was fairly homogeneous across the Americas when new countries gained independence from Spain and Portugal in the 1820s. It was during the nineteenth century that Latin America lagged behind vis-à-vis the USA. The gap started to narrow around the turn of the twentieth century, when the continent experienced a commodity export boom. The state-led and inward-looking industrialisation of the post-war period promoted some catching up. Yet that model of growth was unsustainable, and the continent fell behind once again after the 1980s, which is unsurprisingly known as the "lost decade".

Variations across countries are nevertheless noteworthy. Brazil's relatively stable monarchy (1822–1889) stagnated in the nineteenth century and so did politically decentralised and violent Colombia. Mexico was similar to the latter until the autocratic regime of Porfirio Díaz (1876–1880, 1884–1911) established peace and progress. It then lagged behind after a revolution toppled that dictator. Industrialisation promoted growth in Brazil and Mexico

in the post-war period, but high inflation and a series of recessions have castigated their economies in the last four decades. The governments of Colombia were less intrusive and more orthodox, and the country did not industrialise as much as other large Latin American economies. Its GDP per capita remained stable vis-à-vis the USA, albeit at low levels (Fig. 38.1).

Argentina, Uruguay, and Chile—the subtropical and temperate countries of the Southern Cone—constitute exceptions to the general trend. Growth attracted European migrants between the mid-nineteenth and the early twentieth century. Chile stagnated during the politically turbulent 1960s and 1970s, after which it adopted a market-friendly and commodity-based model that has been generating growth. Disastrous economic policies and financial crises have made once-wealthy Argentina more similar to the rest of Latin America.

In spite of these differences, Latin America failed to catch up, and inequality is high across the continent—Chile may become an exception to the former but not to the latter. This uniting feature suggests that inequality may be linked to underdevelopment. The rest of this chapter reviews general theories and historical research that shed light on the reasons why Latin

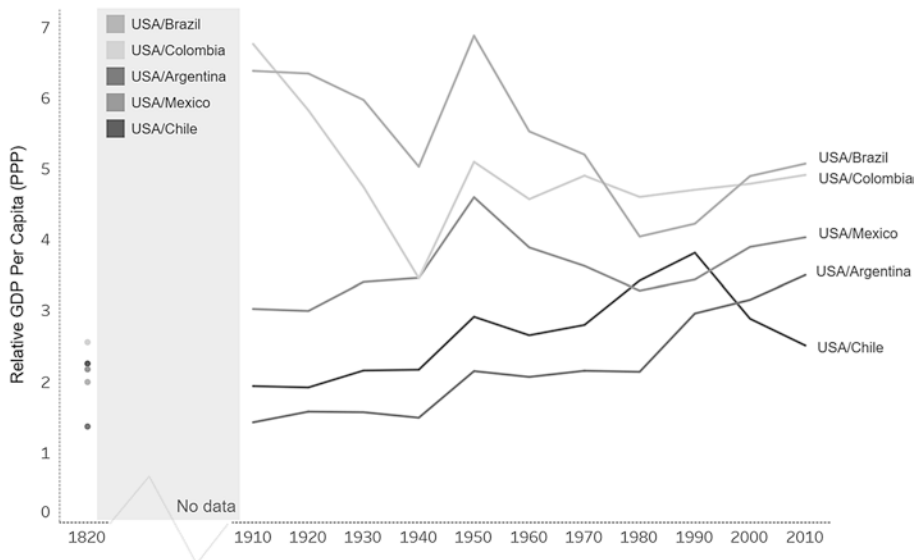


Fig. 38.1 Relative GDP per capita (PPP), USA/selected Latin American countries, 1820–2010. Source: Maddison Project

America lagged behind. Instead of providing a definitive answer, it presents a roadmap that includes different explanations and evidence on this question.

General Theories

The reasons why Latin America failed to develop as much as the USA is not only relevant for the continent; it is also important in explaining the US success in forging ahead. Unsurprisingly, US-based as well as Latin American scholars have studied the causes of the continent's underdevelopment.

In his seminal work, North (1990) proposes that the institutions Latin America inherited from its colonial masters were less efficient in protecting property rights and stimulating the expansion of markets than the Anglo-Saxon institutions Britain introduced in the USA. In a chapter that applies the New Institutional Economics (NIE) to Latin America, North et al. (2000) argue that authoritarian and centralised Iberian institutions generated political instability after independence. The end of Spain's repressive rule unlocked a process of intermittent wars and revolutions that explains why the continent's economies performed poorly in the nineteenth century.

Institutions are also key in the theories that compose the Dependency School, which Prebisch (1962) and Furtado (1970) devised under the support of the UN's Economic Commission for Latin America and the Caribbean. The recent book by Bértola and Ocampo (2013) confirms that this tradition is still influential in the continent. The Dependency School proposes that Iberian institutions created unequal societies designed to export commodities, from which the local elite generated the foreign exchange it needed to import luxury goods from Europe and the USA. The supply of commodity exports is price inelastic and the demand is income inelastic, resulting—so the argument goes—in falling terms of trade and impoverishing growth. The continent had to export increasing quantities of goods to satisfy the elite's appetite for expensive imports. Differently from the NIE's emphasis on the rule of law, the *dependentistas* present foreign trade and inequality as the causes of poverty. In common, both traditions stress the long-term adverse consequences of colonial institutions.

Engerman and Sokoloff (1994, 2002) also draw attention to the importance of inequality, but they stress the role of domestic rather than foreign markets in shaping Latin America's underdevelopment. The authors provide an economic explanation for inequality: gains from scale in mining and tropical crops imposed barriers to entry that concentrated income. Inequality persisted after colonial times, limiting markets for mass consumption and the

potential for industrialisation. Matters were different in the US Northeast and Midwest: temperate crops, such as wheat, enabled the appearance of inclusive societies and the development of large domestic markets.

The influential work of Acemoglu and Robinson (2001, 2006) and Acemoglu et al. (2002) also stresses the adverse consequences of inequality, tracing it back to colonial times. They argue that colonisers introduced extractive institutions such as slavery and the *mita* (roughly speaking, the servitude of indigenous peoples) that generated but also concentrated wealth. Inequality prevented the state from protecting the property rights of the masses, unleashing a vicious cycle of exclusion and poverty that still persists in current times.

What Does Historical Research Tell Us?

Historical research suggests that Latin America's past is more complex and nuanced than assumed in the broad theories described earlier. The work by many economic historians challenges the causality between the continent's colonial—and presumably extractive—institutions and its long-term underdevelopment. However, the debate on political institutions is so far inconclusive and NIE's interpretation of cronyism seems to be pertinent. In contrast, the Dependency School's emphasis on terms of trade finds little support in history.

Colonial Institutions and Inequality

Historians have produced strong evidence that colonial Latin America was less unequal than generally thought. Based on the comparison of real wages, heights, and GDP per capita, González and Montero (2010) found that today's Mexico, Colombia, Bolivia, and Venezuela were, together with the USA, the less unequal regions of the world between the mid-eighteenth and the early nineteenth centuries. Coastsworth (2012) shows a combination of estimates of Gini coefficients on wealth that indicate that inequality was roughly at the same level across the Americas around the turn of the nineteenth century. Johnson and Frank (2006) discovered evidence that Rio de Janeiro and Buenos Aires were as unequal as the major cities in the northern side of the Atlantic in the first half of the nineteenth century. Prados de la Escosura (2007a, b) sums up this literature when he asserts that inequality started to increase well after the continent's independence. Latin America was unequal by today's standards but so was the USA and most of the world in the early modern era.

A number of researchers have come up with explanations for the surprising finding that Latin America was not the world's inequality champion centuries ago. Arroyo et al. (2012) point out that real wages in once populous Viceroyalties of Nueva España and Peru were similar to those in continental Europe because high death rates dramatically reduced labour supply after the Spanish *Conquista*. According to Garavaglia and Marchena (2005), most miners in the Andes and today's Mexico were free paid workers. Their results contest the established view that the *mita* was widespread across Spanish America. Based on a series of regional census, Klein and Luna (2009) demonstrate that the vast majority of people living in early nineteenth-century Brazil were neither slaves nor slave owners; what is more, the latter most often had up to two and very rarely more than ten captives. In short, the large mines and plantations crowded with coerced natives and slaved Africans—a picture one gets when reading Engerman and Sokoloff and Acemoglu and Robinson—were more the exception than the rule.

Why Is Inequality So High?

If inequality was not so high in colonial times, the question then is when and why Latin America became so particularly unequal. Williamson (2009) proposes that the globalisation of goods and labour that took place from the 1870s to 1914 concentrated income. Following a Heckscher-Ohlin approach, he claims that a dynamic commodity exporting sector raised the rents from land while immigration compressed real wages. The empirical study by Bértola et al. (2010) confirms that income concentration increased from an already high level in the Southern Cone during that period.

Besides the globalisation of the turn of the twentieth century, Thorp (1998) proposes that Latin America became more unequal due to the industrialisation of the post-war period. Altmir (1996) asserts that the continent's high concentration of human capital raised income inequality because industrialisation increased the demand for skilled vis-à-vis unskilled labour. Meanwhile, Colistete (2007) presents an additional explanation for the case of 1950s Brazil: political polarisation compromised the bargaining power of unions, which failed to raise real wages as much as labour productivity.

Political Institutions and Cronyism

Latin Americanists have disputed NIE's very core thesis on the institutional comparison of Britain and Iberia. The work by Irigoien and Grafe (2008) and

Hespanha (2001) proposes that the Spanish and Portuguese fiscal institutions were not substantially more autocratic and centralised than their British equivalent. Authorities legislated, taxed, and spent across the Iberian empires to attend local interests, bargaining with the crowns for informal although substantial autonomy. In a response to Irigoien and Grafe (2008), Summerhill (2008) accepts that the Spanish king was less powerful than new institutionalists had assumed. However, he points out that this does not change the conclusion that Spanish institutions did a relatively poor job in enforcing contracts and promoting markets.

Latin America has been plagued by coups and dictatorships since independence. The region became more peaceful and states gained capacity in the late nineteenth century, but stability and growth most often came together with authoritarian rule. Haber et al. (2004) claim that dictators distributed rents to compensate for the institutional risk their regimes created. The Porfirian Mexico is presented as an emblematic example of such crony capitalism systems. Haber (1992) studies subsidies that favoured Mexican entrepreneurs who invested in uncompetitive industries. Along the same lines, Maurer (2002) shows that Porfirio Díaz handed the government's finances to Banamex, a semi-official bank, which in turn financed railways and the fiscal deficit. Nevertheless, Ludlow and Marichal (1998), Passananti (2007), and Weller (2015) found evidence that Díaz and his officials avoided handing stable sources of rents to foreigners. The government negotiated with international bankers and creditors in order to safeguard the interests of the state and stopped distributing rents as soon as it acquired a reputation in world financial markets.

Trade and Industrialisation

The Dependency School's emphasis on the negative effects of foreign trade performs more poorly to historical scrutiny than NIE's interpretation of political institutions. Prados de la Escosura (2009) found that rising terms of trade promoted some growth between the 1820s and the 1870s. Growth was uneven, and results would have been better had the continent avoided intermittent wars. However, Latin America grew once it got rid of the colonial monopolies to join international markets. After independence, the continent fell behind in relation to the USA but not to the rest of the world. Williamson and Bértola (2003) draws attention to the fact that trade delivered growth in the first globalisation. Interestingly, terms of trade ceased to increase and assumed a more erratic pattern in that period.

Dominant among policymakers in the post-war period, the Dependency School gave academic support to the implementation of protectionist and pro-industrialisation policies. These initiatives were expected to lift Latin America from its adverse position in the global economy, delivering development in the long run. Love (2005) reminds critics that the *dependentistas* advocated the formation of a continental trading block, an initiative that never fully materialised. Lack of scale was a problem that scholars envisaged but officials failed to solve. Yet it is unclear that the continent's industrialisation would have delivered sustainable growth in a counterfactual involving regional integration. Taylor (1998) shows evidence that, in comparison to egalitarian East Asia, the pro-industry policies implemented in Latin America promoted more market distortion and less growth. The subsidies governments distributed in Latin America attended the interest of the elites, which grew rich under limited competition. Here again, cronyism seems to be a key cause of failure.

Conclusion

Historical scholarship suggests that Latin America's colonial institutions and factor endowments may not have been as important in blocking the continent's development in the long run as assumed by most broad theories on the topic. The modern local elites bear more responsibility for today's poverty and social exclusion than colonial masters or the global division of labour. Inequality is crucial, but it seems to be a more recent phenomenon than it is widely thought. Tracing the origins of underdevelopment back 500 years is historically problematic. Oddly enough, the work of economic historians indicates that the recent past instead of the remote history tells why Latin America is the poor part of the Americas.

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Part IV

Methods and Techniques in Economic History

Haiku by Stephen T. Ziliak

Search all that you dare
for downloadable data
in sterilised form

Significance test:
A rookie's part of the real
Guinnessometrics



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Impact and Communication

Judy Z. Stephenson

What good is a good idea if no one ever hears it? Impact, in a scholarly context, is defined by the difference your research makes to the theory and practice of your academic discipline, and others. In short, it is about who sees, reads, cites, and uses your research. In a wider societal sense, impact is demonstrable in the way that ideas contribute to or affect wider understanding and debate of the topic. The idea—that quality research is well specified *and well communicated*—is at the core of how higher education and funding bodies evaluate research.

Economic history is a growing and increasingly broad field which attracts social, cultural, political, environmental, financial, economic, business, and other specialist historians as well as economists and econometricians. It is, by its very nature, interdisciplinary. Its potential impact, therefore, is broad, but because it deals with some of the “big questions” that are always at the forefront of social science and political economy, the potential wider impact—academic or societal—is enormous. There is a fine tradition, in both economics and history, of assuming that because your research is academically lauded, eventually—through some alchemy of institutions, journals, conferences, and networks—it will find its deserved place in the mainstream conversation. These days that assumption does not hold. Impact is determined by *how you* communicate, both in pursuit of your research goals and in possession of them. This chapter briefly explores how to communicate your research as a means of gaining “impact” for it, through using a simple framework based on the practices of the communication industry: communication objectives, target audiences, and messaging hierarchies for your work.

History Counts

Since the late nineteenth century, much activity of the discipline has been to chart economic development over time. This has been done in both the narrative or descriptive sense, and the statistical or quantitative sense. Key contributions that garnered academic impact contributed by “counting things” to create a long-run database of economic development and an accompanying narrative, and this is still the case for many emerging economies.

The advent of the cliometric revolution from the 1960s created a new kind of scholarly impact—related to *method*—highlighting the strikingly different stories that statistical and econometric analysis could produce compared to traditional narrative approaches. For some years, it was feared by many economic historians that all future impact in the discipline would be built on “quant”, or cliometric, approaches, and that there would be a decline of the historian in economic history. These fears were in spite of the evidence, however. For the scholarship which garners *societal impact*, off the bookshelves of Barnes and Noble or Waterstones, and onto the kindle devices of a wider audience, has always been the scholarship of ideas, not numbers. The “Industrious Revolution”, the “Great Divergence”, and the “Enlightenment Economy”, all of which sold hundreds of thousands of books (Pomeranz 2001; Mokyr 2004; Clark 2007; De Vries 2008; Humphries 2010, to name but a few), are complex ideas resting on informed, rigorous quantitative analysis expressed in a powerful historical narrative that gained traction with a broad readership. Their impact is derived from the economic and historical narratives they tell. As an interdisciplinary field, economic history has many similar narratives it can create on many topics. In short, when it comes to communication and impact beyond the academy, history and historical sources count.

One of the major challenges for economists in dealing with the past is to deal with the relative lack of the data they are used to being able to lay their hands on more easily for today’s world. Decisions about identification strategies, proxies, and variables involve getting one’s hands dirty in the broader, interdisciplinary, interrelated, human spaghetti soup of historical factors that will have affected the phenomena you want to study. Data collection and creation are a vital part of continuing analysis of economic progress and behaviour, and robust or plausible results demand sound, rigorous, and often time-consuming historical and social science methods. But data collection also offers a range of communication and impact opportunities.

The places and sources that produce proxies for the weather, environment, wages, business owners, and consumers of the past are the organisations and institutions that have left a legacy around us that attract interest and engage-

ment from their current stakeholders and communities. Research that involves such sources has the potential to engage communities at all levels. Of potentially even more interest and engagement are the institutions and organisations that have vanished, which provoke both imagination and intrigue in the past—something that as any publisher or TV producer will tell, has the capacity to garner significant attention. Even without the help of rare or important historical artefacts or institutions, your sources provide a natural communications plan and way to engage both academic and wider audiences.

Planning Your Communication

How should you think about, plan, and go about communicating your research? It goes without saying that before you communicate, you should know what you want to say—but this does not mean that you should publish the paper before thinking about communication. Before any communication can be carried out, you must know your objectives. Economics tends to prepare researchers well for hypothesis specification and research design. Economists generally know instinctively the important questions to ask, and what the indicators to their answers might be. Well-specified research questions naturally define their communication objectives and opportunities.

If your research question is, for instance, “the extent of female market labour in Qing China”, your statement of intent naturally sets expectations for findings about female labour and about Qing China. The important questions you want to answer in a social science context are likely to be directly relatable to the interest of a wider audience (and if that is not the case, then that is worthy of research in itself). This applies even to questions unrelated to topics as gender and China. “The living standards of dogs in France in the eighteenth century” is a research thesis with some important implicit economic and social themes; dogs as pets and as servants; living standards (human and other, and the relationship between); working and nonworking animal populations; and the construction of consumption in eighteenth-century France. Knowing which question you want to answer is to already know what is communicable or impactful about it. Your research question and your communication objectives are intimately bound together to the extent that thinking about communication can help you hone your question.

But—just because you have a great question and a great source—you cannot assume that the research will communicate itself. Think about your target audience. Your primary audiences are the scholars and researchers in your discipline who care about the topic that you are researching, methodologically, theoretically or historically. Where are they? How do they assemble?

Attend. Ask questions. Participate in debate. What journals do they care about and why? Read them. What do they say about those journals? Do you agree? Present to them. Do not wait for your work to be finished. Presenting and discussing work in progress is a key part of how ideas and research gain currency and impact. It helps your work answer the questions that really matter, and it gives your work a natural context and rationale. Not participating in these activities, which are the backbone of academic and research life, is to deny your work its context.

However, in economic history, there is a high likelihood you have at least two or three target audiences and juggling and managing them may become an integral part of a successful career. This is particularly the case for those, such as business historians, who juggle a number of theoretic social science paradigms before they even think about place and people. Networking beyond your core scholarly group or tribe counts.

A potentially hugely important audience is the historians of the period and place that your research question pertains to. For the female Chinese labour example given earlier, there are many interested audiences of development economists, gender historians, Chinese historians, cultural historians, feminist economists, historians of living standards, household, and consumption, to name but a few. The eighteenth-century dog thesis would also likely find collaborators and interest among sociologists, cultural historians, and veterinary biologists, not to mention the large international group of long duree scholars of the Ancien Regime and eighteenth-century France, and those who calculate consumption baskets for econometric living standards research. Engaging with these groups does not just give you impact to a wider audience, it gives you fresh questions, sources, collaborators, and publishing opportunities, in a world where your economics training is likely to give you a unique perspective and skills set.

To really communicate to both your academic tribes and wider audiences, you need to take this further. Where do your audiences communicate? What are the key scholarly Societies? Blogs? Events? Who are the people who are really making an impact in your field? Are they on twitter? Reddit? Research Papers in Economics (RePEc)? Social Science Research Network (SSRN)? What are the working paper series that everyone uses? Get on the same channels, get into the conversation. You need to publish in the top academic journals—and be part of the associated blogs, tweets, conferences, and comment. Beware! This is a complementary relationship. If you do the academic journals without wider engagement, you risk your work, no matter how important or good, only very slowly attaining the recognition it deserves outside your immediate field. If you tweet all day long but never actually publish in the good journals, you will be briefly part of the conversation but never make any

impact. Also—beware—what sort of people and organisations want to know about such matters? What support do they offer? Why? Is it biased or neutral?

Hierarchy of Ideas

This insight leads to the third part of communication planning—messaging hierarchy or planning. Good communication is a more usually a conversation than a speech. Just as the best theses and the best papers do one very well-defined thing very well, good communication does the same. Pick your audiences and your themes. Think strategically about how you specify paper titles and which journals they go to, about how you comment on others' work to different audiences. Think opportunistically about how you use specific pieces of your research to contribute to conversations or conferences that are relevant to your research. Communication professionals at the highest level use this brief hierarchy of ideas to plan their communications: clear, well-researched objectives; two-way conversations with discreet and well-defined target audiences; planned messages around those audiences; and what happens in the wider world.

Many academic researchers are more comfortable in the library than they are in the chat room. There are resources that can help you write for a wider audience. Utilise the American Economic Association¹ and policy blogs such as Vox and EconTalk.² Follow guidelines from the most influential blogs and publishers, such as the London School of Economics (LSE) Impact Blog and handbook (2011).³ Deirdre McCloskey has written extensively and clearly on the practice of writing and on engagement for economists (1986, 2000, 2018). As you try to take on the challenges of not just practising cutting-edge research but publicising it, be aware that there is one principle that never fails in communication, academia, research, publicity, the media, or wherever they interact. It is, of course, the golden rule. Communication is the most reciprocal of human activities. Be courteous, be kind, be polite, assume good intent, say thank you, and acknowledge the people who gave you time and expertise to help you realise your research goals. In research, as well as communication, *you* are the source that will have the biggest impact.

¹ <https://www.aeaweb.org/about-aea/committees/cswep/mentoring/reading>

² <http://theconversation.com/uk>, <https://voxeu.org>, <https://marginalrevolution.com>, <http://www.econ-talk.org>

³ <http://blogs.lse.ac.uk/impactofsocialsciences/>

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Publishing Economic History

William J. Collins

Publishing research in economic history requires many of the same elements as publishing research in general—a compelling question, clear analysis and description, original and persuasive insights, and efforts to relate the findings to existing and future research. Economic history is now fairly well integrated with economics, as described in Abramitzky (2015) and Margo (2018); there is no obvious border, let alone barrier, between the fields (Collins 2015). Economic history research appears with some regularity in the top general interest journals of economics. Some scholars cross back and forth between publishing on economic history and publishing on more contemporary topics.

There are, however, aspects of publishing economic history research that merit special attention from authors who are not already steeped in economic history's literature and mindset. This chapter describes some of those considerations and challenges. Most of the discussion is from the hypothetical perspective of an economist preparing to publish in an economic history journal for the first time. Many of the same considerations would pertain to efforts to publish economic history research in general interest journals, though I discuss some additional considerations in that case. Oftentimes, simply being aware of the issues is a step towards overcoming them in crafting one's paper. But more often than not, successful publishing in economic history requires a significant investment to become familiar with historical literatures and data sources and to write in a way that engages the journal's audience.

The author thanks Jeremy Atack and the editors for suggestions.

Framing Research for Economic History Journals

Economic historians seek to understand the wealth and poverty of nations and the people within them. Growth and inequality are foremost themes, supported by centuries of research on industrialisation, demographic change, finance, trade, labour markets, and political economy. Economic historians contemplate specific research questions in the context of this bigger picture. A successful economic history paper need not be *about* long-run growth, institutions, inequality, and so on, but it is important that the paper *connects* to the larger story of economic development. In other words, even when focused on a particular time or place, successful economic history papers speak to broader themes, pay attention to historical and institutional context, and provide new and clear evidence in a careful manner. To be sure, many successful economics papers, whether historical or not, have exactly these same characteristics.

Successful writing is always considerate of the intended audience. The author must consider the audience's knowledge, experience, and values, by which I mean the questions audience members care about and the kinds of evidence and rhetoric deemed persuasive. Economic history journals are similar to general interest journals in economics in that they have a broad and diverse audience, albeit not the same audience. Successful papers are motivated accordingly and are accessible and appealing to that audience. The journals that I have in mind here are the ones that I have edited, the *Journal of Economic History* and *Explorations in Economic History*, but similar considerations are pertinent for all economic history outlets.¹

If one is attempting to develop a paper with historical evidence and hoping to publish it in an economic history journal, then in a proximate sense, the key audience members are the journal's editor and referees. But the editor and referees are often thinking about or have internalised the interests and values of the broader economic history profession. Therefore, apart from issues of technical correctness, editors and referees are also concerned with whether a particular paper "fits" the journal in terms of its research question and motivation, its awareness of previous work by economic historians, and how its evidence is marshalled and weighed. Will the journal's readers find the paper interesting and convincing? Will they cite it in their own research? Will it

¹ There are several other journals in economic history (e.g., *Economic History Review*, *European Review of Economic History*, *Cliometrica*, and more). At any point in time, they vary in style, which reflects a combination of the current editors' preferences, the journal's stated mission, and the state of the art in research. Reviewing recent issues of each may provide a (noisy) signal of the differences. That said, there is a great deal of overlap, with common themes and values. It is rare that a paper is "just right" for one journal but a bad fit for the others.

draw new readers to the journal? Does it engage enduring themes in a thoughtful, well-informed way?

Jumping into economic history might sound daunting if one's main area of expertise is in more contemporary settings, but it need not be. Modern economic historians speak the language of economics, including theory and econometrics. Most have been trained in either an economics programme or in a specialised economic history programme that has a strong economics component. Thus, an author who is contemplating submitting a paper to a leading economic history journal can assume a fairly high level of technical competence in the audience, referees, and editors. In addition, many economic historians have deep competence in other subfields of economics. Thus, there is plenty of common ground for conversation and exchange of ideas.

It would be a mistake, however, to suppose that a good economic history paper is just like a good paper in some other subfield of economics but with older data. Not all competent empirical analyses are deemed suitable for an economic history journal. Papers centred on long-ago events can easily fall flat, and papers on fairly recent events can make for perfectly good economic history. Using "old data" as fodder for the application of a new econometric tool does not make a paper suitable for an economic history journal, nor does using old data to address a question of narrow interest to another subfield (in that case, you should submit it to a journal in that subfield). Many such papers are rejected at economic history journals because they fail to motivate the research appropriately and because they emphasise new techniques but do not develop novel and persuasive historical insight. A frequent trait of such papers is a focus on the statistical significance of regression coefficients without much effort to understand their magnitude in historical context.

Framing Research for General Interest Outlets

Publishing economic history in high-quality general interest journals requires framing the paper for an audience of economists. Some economists might not be very interested in history per se, but many are open to learning about economics from historical evidence. To be clear, economic history papers that succeed in general interest journals are not just excellent examples of economic history research—they are often especially well motivated for a wide audience. Whereas framing for an economic history audience might emphasise using economics to better understand the past in the context of the economic history literature, framing for a general interest audience might

emphasise understanding economic forces and theories more generally. Hornbeck (2010), for example, is successfully framed as a paper about property rights and development rather than about agricultural history in a narrower sense.

There is no simple or single recipe for framing such a paper. But there are certain lines of argument that one often sees to motivate economic history research for general audiences. Studies of the Age of Mass Migration, for instance, might emphasise not only its historical importance but also the appeal of studying international migration in an environment free from policy barriers to European immigrants (Abramitzky et al. 2012). The idea is that in the absence of modern policy distortions, we can see economic forces operating more clearly. Studies of history and health might emphasise that a long-run perspective is required to understand the lifelong implications of particular events or policies (e.g., Almond 2006; Goodman-Bacon 2017; Alsan and Wanamaker 2018). Other studies might suggest that historical experience provides useful insight on issues that are highly relevant in developing economies (e.g., Nunn 2009; Donaldson 2018). And yet others find historical examples of exogenous shocks or random assignment that yield compelling insight on important economic questions (e.g., Hanlon 2015; Bleakley and Ferrie 2016). The list could go on, but the pattern is apparent—the authors establish the work's relevance to economics broadly speaking and then carry out exemplary empirical research.

Data in Economic History Research

Many economic historians cut their teeth doing archival work or creating new datasets from other primary sources. Economic history probably attracts scholars who are inclined to enjoy this kind of work, but I think the training and effort reinforce a value system in which creating and evaluating new data are highly esteemed. If you have created a new dataset to address an important question, the effort should be highlighted in the main text, especially if submitting to an economic history journal. In addition to drawing attention to the novelty of the research effort, this emphasis engages economic historians in something they care about deeply—the handling of primary data sources, the interpretation and coding of historical information, and the decisions and trade-offs that are made along the way. It is possible to bog down a paper with too much detail (data appendices are helpful), but it is fair to say that economic historians, like all good empiricists, are curious about the origin, quality, and vulnerabilities of data.

On the other hand, if an author has “merely” downloaded some existing dataset for analysis, the paper can still succeed as an economic history paper. But the author should keep in mind that economic historians will be highly attuned to the quality and quirks of historical data sources; the author must be, too. It is not enough to say that some well-known paper or author used the data previously and that, therefore, it must be reliable for the analysis at hand. For instance, thanks to Ruggles et al. (2017, henceforth IPUMS), it is now easy to download large samples of harmonised US census microdata. Yet every census varied in how it was carried out, in what questions were asked and about whom, and in how answers were originally recorded before being harmonised for the IPUMS. The IPUMS website provides copious documentation and references for researchers; it seems clear enough that users should read it, but many do not. Perhaps a greater danger is that as more historical data have become easily accessible via the Internet without careful curation by the IPUMS team or a similar group, well-intentioned novices might mishandle and misinterpret raw source information. Referees and editors might (or might not) save an author from a mistaken interpretation, but they might also reject the paper for being careless or uninformed.

The key point is simply that economic historians take data quality seriously. That includes knowing the primary sources and their limitations and being upfront about those limitations in writing the paper. Successful publishing in economic history is more likely when authors demonstrate that they have done some serious digging to understand their source material, to improve it where they can, and where they cannot, to be clear about its blind spots and biases. The other side of this coin is that economic historians tend to be sympathetic to those who must work with imperfect data, as long as it is done with care.

History in Economic History

Economic history papers come in many flavours. Some emphasise a combination of economic theory and history in settings where quantitative information is scarce. Others delve into archival sources to illuminate important historical institutions, trends, or events. But many papers in leading economic history journals are similar in structure to papers that one sees elsewhere in applied empirical economics. This familiarity of form might obscure the amount of historical knowledge that goes into producing credible economic history research.

It is important for economists who are writing about economic history to be fully aware of related work in history (i.e., written by historians and likely to be found in books rather than in articles) or in other fields. Writing well about economic history requires finely tuned historical perspective and deep knowledge of the time, place, and sources. This is a challenge for many who come into economic history from a traditional background in economics, where theoretical and econometric skills are built from the start, but where exposure to historical themes and sources is often quite limited. Reading deeply in history can help in everything from choosing an interesting research question, to thinking clearly about the institutional framework that is essential to one's theory, to finding a compelling research design, and to locating and interpreting primary sources and relevant case studies.

The historical literature evolves over time, and economic history research needs to keep pace. Do not pick up the "classic" book on your topic and end your investigation there. On occasion, a referee report from an historian may be amused (or aghast) by a paper's outdated characterisation of a certain historical setting or event, its failure to cite relevant recent research by historians, its oversimplification of history, or its misreading of some sources and ignorance of others. All of this damages the paper's credibility. Sometimes that kind of lapse is repairable, but other times it goes straight to the heart of the paper's interpretations or assumptions. There is no substitute for reading history and delving into primary sources, but it helps to confer with experts and to seek feedback on preliminary research through correspondence, conferences, and seminars long before submission to a journal.

I want to end on an encouraging note for those who are considering writing research in economic history. There is one great advantage to being new to a field—the ability to see everything with fresh eyes and from a new point of view. Progress in any field depends, in part, on newcomers breathing new life into it, making connections that earlier scholars did not. I think economic history is especially welcoming of newcomers, perhaps because it is open to insights from multiple disciplines. If you have ideas to contribute, we are all ears.

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Archival Evidence

Graham Brownlow

Joseph Schumpeter within his *History of Economic Analysis* distinguished between historical and theoretical temperaments. He observed that those historically inclined would “delight in all the colors of historical processes”. In contrast, he suggested theorists would prefer the “neat theorem” and moreover, these two temperaments were not likely to appreciate each other (Schumpeter 1954). Schumpeter’s observation regarding competing temperaments is of continued relevance in the twenty-first century. Most modern economists have a theoretical temperament cultivated (or at least inculcated) by modern graduate school-style training.¹

But there is a tradition within economic history that suggests when it is best practised, it involves both economic methods being applied to serve historical scholarship and the historian’s craft being used to produce better economics (Solow 1985; McCloskey 1987; Mokyr 2005; Brownlow 2010). In this chapter, I outline the value of cultivating a historical temperament in economic research, and I suggest that nurturing archival interpretation is part and parcel of developing such a temperament. I seek to explain why archival evidence, irrespective of the time period covered by the research, is an important (if all too often neglected) part of the empirical toolkit.

Striking the appropriate balance between Schumpeter’s temperaments rather than following a predetermined set of procedures, protocols or algorithm involves scholarly judgement, interpretation and creativity. The old

¹ They often assume that because such an approach is so ingrained that it is the sole natural approach; such an assumption is incorrect however, as there are a number of roads to the past.

quip about economists “looking under the street lights for keys” has some truth to it. Just because a variable is easily measurable—and hence endowed with abundant supporting data sources—does not logically imply that it was the most important historically (and indeed vice versa). Judgement can never be avoided in trying to craft economic history, and the ability to make judgements in turn often rests on the ability to interpret archival evidence in an honest manner. Economic history is far from the only field in which exercising such judgement is necessary; I hope that the lessons of this chapter are relevant to economists more widely.²

There are three main arguments developed in the discussion that follows. A first overarching methodological message is that just as a microscope can be adjusted to provide varying levels of focus, so the reliance on archival evidence, both quantitative and qualitative, can be used to discover more or less historical (empirical) detail. The more that factual historical detail and interpretation is required in examining a particular economic topic, the less the enterprise will rely solely on “stylised facts” (and the more it will depend on historical material as recorded in primary sources). Obtaining such evidence is best acquired via archival “digging” because primary sources allow for particularly solid interpretations. The second overarching message is that the strengths of history and economics are complementary. Regarding issues of contingency and specificity, history is the relatively stronger discipline (albeit one subject to the Humean problem of induction). In contrast, economics is much stronger in dealing with issues of generality, causality and deductive reasoning (Boumans and Davis 2010). A third message is that good practice is more pluralistic than is often assumed; it does not follow that “anything goes” either (Evans 1997; Galenson 2017).

Archival Evidence and the Enterprise of Economic History

The enterprise of economic history is perhaps best understood as taking from both its parent disciplines (economics and history). Just as the best mathematical economics requires the economist to consider mathematical probity and rigour as well as economic relevance, so the best economic history requires

²Robert Shiller’s recent discussion of economic narratives, in which he argues that certain stories may have economic repercussions, is a further example that economists are attempting to provide more realistic (or convincing) explanations of economic life (Shiller 2017). Economic historians can develop Shiller’s insight by judging the factual veracity of influential narratives.

consideration of both causality and contingency.³ Some important earlier works downplayed the role of archival evidence in building secure historical foundations in the production of economic history. For example, neither Tuma's nor McClelland's book-length discussions of cliometric methodology even merit putting "archive" or "primary source" in the indexes of their books (Tuma 1971; McClelland 1975)! Moreover, even a recent survey of economic history fails to distinguish between the analysis of *primary* and *secondary* sources as historical evidence (Galenson 2017).

Archival evidence refers to the original source materials, which historians regard as primary sources. Everything that they and predecessors write about the past is described as a secondary source. Most of what gets into historical textbooks is concerned with secondary sources—that is, how historians formulate problems and reach conclusions and how students and researchers should evaluate their work. Historians will usually prefer sources that are closest in time and space to the events being examined (Tosh and Lang 2006: 61). Tosh notes that the process of creating historical documents is an ongoing one and that the most revealing source is that which was produced with no thought for posterity. In terms of original sources, these could take either qualitative or quantitative form including narratives, memoirs, official papers, newspapers, records of church and state, private papers and diaries.

Moreover, there is no implication that a primary source is reliable or unbiased as such sources can be 'inaccurate, muddled, based on hearsay or intended to mislead' (Tosh and Lang 2006: 61). By way of illustration, British reactions to the French Revolution, no matter how garbled much of it was, explain much about British political history. Likewise, economic historians of the Great Depression would clearly be concerned with the views of the contemporaries as well as actual economic data. Indeed, the Keynesian notion of "animal spirits" suggests that for investment decisions during the 1930s, the distinction was actually blurred.

Historians interrogating primary sources, of whatever form, do not think there is a simple, single, unalterably "true" meaning to such sources. They acknowledge, moreover, that we cannot simply impose any interpretation we wish either. Historians are limited in their interpretation by the language of the original text (Evans 1997). Ultimately, it is no exaggeration to state that academic history does not rest on what is handed down by earlier historians, but on the constant reassessment of the original sources—and this includes scrutinising the sources for distortions.

³This observation is far from an original insight; it was the basis of many an early paper or inaugural lecture covering the methodology of economic history (Heckscher 1929; Harte 1971).

Archives, Counterfactuals and Reconciling Economic and Historical Temperaments

Given the early research programme of cliometrics, many earlier methodological discussions within economic history focused understandably on issues concerning quantification, counterfactuals and model selection. Debates on counterfactuals—as interesting and important as they are—have continued to crowd out discussion on the role of archival material. Galenson's (2017) recent discussion, for example, while it represents a shift away from a focus on counterfactuals, equates any mention of the past based on secondary sources with the use of historical evidence. Economists embrace the counterfactual insight strongly considering “what would have happened if?” statements an uncontroversial applications of microeconomic reasoning or thought experiments (McCloskey 1987). Historians in contrast—with some notable exceptions—have long been unconvinced about the merits of counterfactual (or virtual) history (Evans 1997, 2014; Tosh and Lang 2006).⁴

In order to reconnect economic history with economics successfully, however, it is important to ensure that it is not “crowded out” by work that at first sight appears historical, but which on closer inspection is merely applied economics/econometrics (albeit with older datasets). One way to avoid this crowding out is recognise that “historical” research is defined more by the methods used than the time period covered (Solow 1985). Such an insight is obvious to historians, but it needs to be made to economists. The differences over counterfactuals are not insurmountable—and accordingly the possibility of taking the best from each discipline is made more likely—if we view the differences as ones of temperament or training rather than fundamentally incompatible philosophical world views.

Economic History, Economist's History and Facts

Indeed, sometimes the usefulness of archival research is so taken for granted that it is frequently not taught as a separate university module. Yet when historians from a wide range of ideological outlooks do examine the use of archival evidence more thoroughly, they tend to agree that just as the skilful and diligent use of archival sources is the mark of honest empirical research, so the

⁴For example, Evans disputes that Fogel's work on the railways is counterfactuals at all: he argues that there is no chance or contingency in such an exercise (Evans 2014: 38–39).

selective, unskilled or misleading use of such sources indicates poor historical practice (Evans 1997).

Official governmental records may provide the archival base for certain genres of economic history. For instance, the history of monetary policy or trade will require central bank and civil service primary sources, including diaries, memoirs and minutes, if an insider's account is to be produced. However, not every genre of economic history has the benefit of rich sources of the type that a historian of macroeconomic policy could take for granted. A business could be of economic consequence but merit little governmental interest because there being of limited public policy importance.⁵

The difference over counterfactuals discussed earlier is just the most famous example of a wider difference in outlook between those economic historians trained in economics and those trained as "traditional" rather than "scientific" economic historians. Indeed, Robert Fogel and G.R. Elton, coming to the task of producing economic history from opposite ends of the supposed spectrum, concluded with something of an uneasy methodological truce: both worried about applying assumptions to historical materials that bore little relationship to the economic past. Where they differed was while Fogel considered that (absent the explicit application of microeconomics) historians would descend into erroneous implicit assumptions, Elton argued that some economic models bore little resemblance to the historical record (Fogel and Elton 1983).

However, despite their differences, there was sufficient methodological consensus between them to agree on the vital role of archival evidence in historical research. Fogel and Elton, even though disagreeing about much concerning the writing of history, were forced to agree that the quality of interpretation (an "output" of the historian's craft) could not be separated from the quality of the underlying archival "inputs". They concluded that: 'the quality of an historical interpretation is critically dependent on the quality of the details out of which it is spun' and that what is needed, of course, is a proper balance between evidence and interpretation (Fogel and Elton 1983: 125).

The neglect (or misrepresentation) of the use of primary sources in some of the literature has unfortunate implications. For example, by ignoring archival evidence and focusing on topics with more easily collected numerical sources, important topics that are hard to measure are neglected in favour of topics

⁵Whitehall had very little interest in the humble fish supper, for example. Walton's wonderful book on fish and chip shops in the UK, and the place of such shops within society and economy, relied instead on the sources held at the Leeds headquarters of the National Federation of Fish Friers rather than any abundant holdings in official archives (Walton 1992).

that are based on more easily accessible data. So, for example, rent-seeking (with its associated extractive political institutions) is very difficult to measure (Del Rosal 2011). Economic historians trained in cliometrics may consequently shy away from considering it as an important determinant of economic prosperity.⁶ A narrow focus on easily measurable factors, and corresponding indifference to archival sources, may lead economists to shy away from more fruitful lines of enquiry where primary sources exist but not necessarily in a form that cliometric training can investigate.

There, nevertheless, is an important role for the type of economist history which is closer to an applied economics of the past rather than a historically-rooted investigation of economic processes.⁷ That the models of Rostow or Kaldor fell at the empirical hurdle, even before archival evidence was examined in detail, should provide a cautionary tale for contemporary authors invoking historical explanations of economic growth. While modern economists may be less willing to use the term stylised facts, they remain equally reluctant to use archival material in constructing economic arguments. Acemoglu and Robinson in *Why Nations Fail*, as with Douglass North's work, discuss a wide range of historical cases from around the globe, yet the supporting archival evidence is flimsy at best (North 1991; Brownlow 2010; Acemoglu and Robinson 2012).⁸

Lest anyone think that these comments are intended as a jibe against "mainstream" economics, it is the case that modern heterodox economists are equally willing to build narratives about the economic past without securing archival corroboration. Consider, for instance, Ha-Joon Chang's *Kicking Away the Ladder* (Chang 2002). Chang's analysis argued that interventionism lay at the heart of the development path of developed economies, but he made little reference to the economic history literature in making this assessment, and nor was it based on any original archival work. It is worth speculating that the

⁶While rent-seeking may not always be an important determinant of economic progress, in some cases previously unavailable or neglected primary sources may reveal it as an important part of the institutional and economic environment (Brownlow 2007).

⁷The so-called stylised facts which earlier generations of economists, such as Rostow or Kaldor, used to discuss growth and development should not be confused with the product of more detailed archival-based investigation (Rostow 1960; Kaldor 1966). Notably, as Kuznets (1963) and Crafts (1993) demonstrated, when one considers in more historical detail the actual performance of economies, the stylised facts appear not to be facts at all.

⁸Galenson's assessment that the model of development created by Acemoglu and Robinson is built upon an "extensive analysis of historical evidence" depends upon Galenson ignoring the absence of solid primary source evidence in *Why Nations Fail* (Acemoglu and Robinson 2012; Galenson 2017: 1750). So while *Why Nations Fail* is a fine work with plenty of insight judged as a piece of historically informed economics, the cases discussed are not analysed with the historical depth or detail that historians would expect.

fate which befell the Rostow and Kaldor growth models may one day apply to the models found within *Why Nations Fail* and *Kicking Away the Ladder*.

Examples and Advice

A good recent example of the importance of archival evidence comes from David Mitch's (2016) piece on the role of hiring decisions in creating the "Chicago School". Previously, Milton Friedman's recruitment by the University of Chicago in 1946 was viewed as almost an inevitable component of the creation of Chicago School economics. Mitch's contribution was to look at the archival evidence concerned with Friedman's hiring, and in so doing, he demonstrates that the decision to hire him was far more contingent than the traditional story. Indeed, Mitch shows that Friedman's hiring reflected a compromise between a group led by Frank Knight on the one hand, and those academics, as exemplified by Jacob Marschak, associated with the much more formalistic Cowles Commission—a research institute then based at Chicago.

While it would be fiendishly complicated to think about the counterfactuals (Chicago without Friedman) in any precise way, it is the case that had any of these hires not actually transpired, then it is safe to say that the intellectual direction of Chicago's Economics Department would have been very different. Once again the diligent pursuit of archival evidence reminds us that observed outcomes—in this case Friedman's hiring—created a certain type of research programme at the University of Chicago, but that this outcome in turn depended on a certain series of events.

Concluding the chapter, I impart two pieces of advice that are of most relevance prior to visiting the archive for exploration. Firstly, and most importantly, archival research is a classic case of the value of reconnaissance and learning by doing: hence by far the best way to learn how to conduct archival digging is to visit an archival website before your visit and get an idea of how the catalogue is set up. Once you are there, the employees in the archive will usually be only too willing to help. Word searches within the online catalogues may provide surprising sources of help in getting you started. In my experience starting with the most recent sources and working backward often helps you think about how things turned out in a particular historical episode and what expectations were prior to that.

A second piece of advice is to read widely and look at exemplars of best practice in terms of archival work. One such exemplar is Gregory and Harrison (2005), who surveyed research on the Soviet Union found in the governmental, party and military archives of the Stalin era. They noted that it was only

when the Soviet Union collapsed in 1991 that many archival sources became available. The volume of such sources was amazing, running to hundreds of millions of files. That these primary sources were never intended for publication and research provides an unvarnished range of resources for economic historians. The sources examined by Gregory and Harrison have thrown up a number of specific findings that are of more general relevance for those looking at a range of other topics. Overall, their archival evidence suggests that incentives were omnipresent features of even the least market-oriented situations.

The second example of good archival practice comes from further back in time, narrower in its range of sources and more quantitative in focus. However, as with the previous example, there are more general lessons to be taken. Judy Stephenson (2018) takes issue with how previous economic historians have understood wage bargaining in the early modern period. Until Stephenson's efforts, there had been a discussion of the role of a "high wage economy" in stimulating industrial development. Yet such discussions rested on the veracity of sources being taken for granted. Examining the sources in detail, Stephenson discovered that the supposed wages were no such thing; they instead were the rates charged by building contractors to clients. So the "day wages" included contractor margins.

The net result of Stephenson's diligent archival excavation is that the actual wages actually paid to London building workers were likely to have been well below the previous estimates. Accordingly, a high wage economy should not be used as conceptual starting point in thinking about the Industrial Revolution as no such situation existed. Stephenson's contribution is, however, conceptual as well as empirical: the organisational structure of production was much more important than the previous studies (with much weaker archival support) considered. Again economic historians of other times and places can take much from this example of archival digging. The general message is clear: nurture your historical temperament by getting to the archives as soon as you can.

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Case Studies

Abe de Jong and Hugo van Driel

In economic history research, many studies involve cases. The defining characteristic of a case study is that an individual unit is analysed, while alternative approaches are based on a larger set of units. In economic history, this unit can be, for example, an entrepreneur, a firm, an industry or a country. Cases are studied within a specific period of time and in geographical space. And as the emphasis is on the analysis of the individual unit, the study is typically in-depth and broad in its scope. In economic history, the case study method is well-suited to exploit the richness of archival data sources and also to document historical developments. The opportunities for the use of a case study method in empirical research are manifold and allow for a wide array of applications.

In history, the number of books and articles about historical figures and events is enormous. In particular, the genres of biographies of famous people and of battles and wars are very popular. However, these works do not qualify as case studies per se, because of the descriptive nature and absence of analytical purposes. In the social sciences, case study methods are widely used. The landmark reference on case studies, the book of social scientist Robert K. Yin, *Case Study Research: Design and Methods*, has been cited over 150,000 times according to Google Scholar.¹ Unfortunately, Yin limits the focus of a case study to contemporary phenomena (2014: 2) The aim of this chapter is

¹ The book's editions from the first issue in 1984 to the most recent fifth issue of 2014 have over 151,500 citations in Google Scholar at the time of publication.

to provide an introduction to case studies for the purpose of economic history research.

For economists and business scholars conducting historical research, three types of case studies can be distinguished. The first is an exploratory case study, where the analysis revolves around a unit of study for which source materials are available from archives or secondary sources and the aim is to describe and understand developments across a particular time span. The second type is unique cases. Here, the unit of analysis is studied in a specific time frame, where the setting is unique and of historical importance. Compared to exploratory cases, unique cases cannot be used as a starting point for generalisations. The third type of case study research aims to test theory and the unit of analysis is chosen in order to best allow the testing of hypotheses.

Exploratory Case Studies

We start with two examples to illustrate case studies of an exploratory nature, where the description and understanding of the case are the central aims. Cusumano et al. (1992) present an example of a standardisation battle. In a bibliometric study, De Jong et al. (2017) find that this paper is the most cited article published in a business history field journal when counting citations outside the field of business history, typically as an illustration of theories of innovation and standardisation. Cusumano et al. (1992) present a careful description of the early market for home videocassette recorders and the rivalry between the VHS and Beta formats in the 1970s and 1980s. Although the formats were comparable in quality and technology, effective strategic manoeuvring by the VHS producers made this the global standard. This case study thus analyses the mechanisms generating a certain outcome rather than the correlation of variables.

Another exemplar exploratory case study is Carlos (1992), who studies two early chartered companies in seventeenth-century England: the Hudson's Bay Company and the Royal African Company. Operating in long distance trading with limited communication, these companies faced problems in incentivising their overseas employees. Carlos describes the contract structures of these companies' head offices with their overseas managers. The contracting included fixed salaries, an oath and a bonding arrangement with bonds of six to ten times the annual wage. The bonding was enforced via monitoring of incoming and outgoing ships and correspondence with local agents. The contractual structures are explained from an agency theory perspective and Carlos concludes that both companies used contracts that were

theoretically sound. The article is a good example of a study using the agency theory framework to analyse and interpret historical evidence.

In the previous two examples, the approach in the exploratory cases is similar. The studies contain a description of an historical case, accompanied by an analytical interpretation. This interpretation can be based on the authors' reasoning or inspired by existing theory. A good starting point for researchers and students to understand and appreciate this approach is the collection of contributions in Bucheli and Wadhvani (2014), which includes a chapter by Yates with an introduction to the historical method.

The selection of cases for an exploratory case study is fairly simple: for any case with access to source materials, a description and interpretation can be provided. The most interesting case studies present new primary sources and the interpretation transcends a mere summary of the findings. Good case studies allow researchers and students a learning experience based on deep and elaborate description, and accompanied by an analytical reasoning that facilitates an understanding of the causal mechanisms in the case study.

The understanding from exploratory cases may be case specific or generalisable over other situations. The value of exploratory cases, in our view, increases when the insights are generalisable. The main weakness of exploratory studies is that—by design—they do not investigate this generalisability. Some authors speculate in the studies about the degree to which a case is representative for a broader set of instances, while others claim uniqueness of their chosen case. In the end, it is up to the reader to judge the relevance of the case beyond the individual instance.

Unique Case Studies

A particular type of exploratory cases are known as unique cases. A good example is Drelichman and Voth (2011), who present a case study of the war-financing strategies by Philip II in the years 1556–1598. Philip II is historical figure of great importance and the article discussed how he could accumulate debts up to 60 per cent of GDP and suspend payments to his lenders four times. Based on archival sources, the authors investigate over 400 lending contracts and provide descriptive statistics and network analyses. The case study is a deep description of the developments over more than 40 years.

Another unique case study is the article by Gelderblom et al. (2013) about the first two decades of the Dutch East India Company founded in 1602. This firm is often perceived to be the first modern corporation, and therefore understanding the motivation for its corporate structure is of historical rele-

vance. The authors provide several reconstructions of financial and strategic developments over the years 1602–1623 and demonstrate that this new corporate form, to which so much historical importance is attached, was the outcome of piecemeal engineering where the directors took two decades to remedy flaws in the initial structure.

Of course, the distinction between exploratory case studies and unique cases is a matter of perspective. For one reader, a unique case may be interesting for its historical significance, while another reader may derive more general understandings from the case. The study of Drelichman and Voth (2011) on Philip II, for example, can be appreciated for Philip's role in world history but also as an illustration of sovereign lending practices. In our view, unique cases are defined by their value in understanding the course of history, while this is not a requirement for exploratory case studies.

Testing Hypotheses with Case Studies

As described before, exploratory and unique case studies contribute to the academic literature and are relevant pieces of research, both for teaching purposes and for subsequent research. Many students and researchers find case research inaccessible because authors of cases normally do not provide literature reviews with comparable and related cases. Therefore, in order to distil ideas from multiple cases, one has to wade through many individual case studies published in books and journal articles, making the research very time-consuming. Moreover, discussions of case study research often reveal an undertone of cases being not scientific because the analyses do not include hypotheses formulation and testing. This non-scientific appearance is largely caused by the difficulty to generalise from cases, as discussed before. In the remainder of this chapter, we discuss approaches to remedy this lacuna.

De Jong et al. (2015) present several approaches to test hypotheses in case study research. Hypotheses can be based on economic theory (or theory from other disciplines) but also stem from inferences from a single case or multiple cases. The selection of cases and the method for the purpose of testing hypotheses are crucial. Even a single case study can be used to test a hypothesis. For example, a hypothesis can be refuted by case evidence, when the case studied meets all assumptions for the hypothesis but fails to yield a confirmation.

An example of a well-selected case study that tests a specific hypothesis based on a single observation is Freeland's (2000) publication on General Motors' (GM) full acquisition of its supplier Fisher Body in 1926. Freeland revisits this "paradigmatic" case to refute the general applicability of transaction-

costs economics in explaining vertical integration. Several earlier publications argue the takeover was meant to solve a “hold-up” problem, which was attributed to the ineffectiveness of long-term contracts between supplier and customer (Klein et al. 1978; Klein 1991). In contrast, Freeland denies the existence of a hold-up problem at the time of vertical integration. Basing himself on a much more extensive investigation of primary sources, and emphasizing the impact of changing market circumstances dramatically increasing the strategic importance of closed car bodies, Freeland concludes that GM fully acquired Fisher Body mainly for financial, strategic and anticompetitive reasons. Ironically, Freeland also shows that a hold-up of GM by the six Fisher brothers only occurred *after* they had sold out to GM in 1926. This author thus illuminates the vital importance of carefully studying the historical setting to assess the general applicability of economic theories.

Next to testing theory with a single case, a small set of carefully selected cases can be used to test a hypothesis with necessary condition analysis (Mahoney et al. 2009). For example, when a hypothesis states that a strong legal system is a necessary condition for economic development, one needs to select cases with high economic development, because the absence of a strong legal system will refute the hypothesis. In this approach, the hypothesis dictates the case selection and there is no need to study low-development countries (for an in-depth explanation, see Byrne and Ragin 2009).

In addition to hypothesis testing with a single case or with a small number of selected cases, researchers can also use cases as input to testing with standard cross-sectional or time-series regression models. In this approach, the input for the variables in the regression models is case study research. For example, Larsson (1993) discusses the so-called case survey method: he collected 55 case studies on mergers or acquisitions and with the help of 11 people coded the case studies (each case was coded by two persons) into a set of variables, which is analysed with regression models to test hypotheses.

To conclude, case studies are probably the most overlooked and underappreciated research method among economists and business scholars. However, this is unwarranted, given the broad array of application of case research.

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Analytic Narratives

Mark Koyama

Modern economic research usually involves the use of models in combination with empirical testing. Models are simplifications of reality that deliver predictions. These predictions may be explicitly derived from the model or more loosely motivated by economic theory and tested econometrically using data that may be experimental or observational. This positivist method continues to attract criticism, but it remains firmly entrenched in the way economic research is conducted as it has proven itself highly successful in advancing knowledge.

Since the Cliometric Revolution of the 1960s, economic historians have employed this research paradigm to study a host of important historical questions: Could the US have developed as rapidly without tariffs (Irwin 2000)? What was the relationship between the Gold Standard and the Great Depression (Eichengreen and Sachs 1985)? Did unemployment benefits and trade unions cause high unemployment in Britain in the 1920s and 1930s (Benjamin and Kochin 1979)? Such questions are highly amenable to a cliometric approach. High-quality data exist for the late nineteenth and early twentieth century. More importantly, these are questions for which it is possible to construct a counterfactual and hence derive empirical estimates that are plausibly causal.

Not all historical questions, however, can be resolved through this approach. There are interesting questions for which we lack the data required to employ econometrics or for which it is difficult to construct a counterfactual. We know a lot about medieval commerce, but rarely know much about aggregate quantities and prices (at least before medieval states started taxing it). Similarly,

we have an abundance of evidence for sophisticated commercial and financial relationships in the ancient world but are only able to implement the simplest regressions due to a lack of observations (see Temin 2010). Moreover, there are questions for which econometric techniques may be inherently unsuitable—one-off events where no comparison group can be constructed (such as the decision to go to war in World War I).

What Are Analytic Narratives?

One approach to questions is through “analytic narratives”. The terminology analytic, or analytical, narrative was pioneered in the late 1990s to describe papers that used an explicitly rational choice or game-theoretic approach to study topics in economic history and political economy (Greif 2006; Bates et al. 1998, 2000). It can also be used more generally to describe papers that are strongly informed by economic theory or arguments but do not necessarily employ a formal model. Peter Boettke provides a useful taxonomy that distinguishes between “thin” and “thick” descriptions of the world and “clean” or “dirty” empirical work. Traditional economics emphasises thin descriptions—parsimonious models with simple assumptions—and clean empirical work. In contrast, anthropology and sociology use thick descriptions and messy empirical work. Research in the analytic narrative tradition combines parsimonious theory with messy or dirty empirics (Boettke 2000).

This makes the analytic narrative approach a valuable way to approach a complex historical phenomenon. Studying the effects of a minimum wage law or new workplace regulations probably does not require an analytic narrative. But how to explain the incentive system used by the Royal Navy in the eighteenth century (Allen 2002)? The ways in which pirate captains encouraged their crews (Leeson 2007)? Or how private prosecution associations functioned in Industrial Revolution England (Koyama 2012)? These are questions that require the author to devise a theoretical framework that can explain often complex historical and institutional details; they are questions that are well suited to the analytic narrative approach.

An economic model, formalised or verbal, is an important component of an analytic narrative. It is the discipline provided by economic theory that distinguishes this approach from that employed by qualitative social scientists, or, indeed, from historians. The resulting narrative will be more compelling and have greater explanatory power than one that relies on individuals making mistakes or otherwise idiosyncratic decisions. In employing any formal model, a researcher faces a trade-off between generality and specificity.

General models can be applied more widely but may not be best suited for understanding a particular case study or historical institution. In contrast, the types of models employed by scholars in the analytic narrative tradition are often context-specific (Greif 2006).

One frequent criticism is that one can write a model to explain almost any social phenomenon. It is important therefore that the predictions and assumptions of the model are consistent with the historical situation being examined. This is not a matter of *realism* per se. Unrealistic assumptions are frequently necessary and innocuous (“we assume the cost function is continuous and twice differentiable”). But the critical assumptions of a model should not be at odds with the historical evidence. The narrative component is also important in analytic narratives. Narratives are used to explore and explain the relevant institutional and historical detail. The narrative style allows to the economist to access the “messy” particulars of specific historical and institutional contexts.

In particular, analytic narratives are a fruitful way to study institutions. Historical institutions have features that can appear baffling to modern observers. But seen through the lens of economic theory, many of their apparent mysteries can be unveiled. Carefully constructed analytic narratives, informed by an understanding of both historical details and economic theory, can shed light on how they function (Greif 2006).

Applications of the Analytic Narrative Approach

Among the best known set of papers in the analytic narrative tradition are Greif (1989, 1993). What distinguishes the approach taken by Greif is the care in which he selects an appropriate formal model and applies it to explain a historical puzzle. This puzzle arises from the situation that confronted Jewish traders in the Islamic world in the eleventh century. Conducting long distance trade in the absence of a reliable and centralised legal system, these Maghribi traders faced a fundamental commitment problem. If a merchant hired an agent to conduct his business in another part of the Mediterranean, what guarantee did he have that the agent would not cheat him?

This problem can be thought of as a one-sided prisoner’s dilemma game. If the game is only played once, the agent will always cheat the merchant so the merchant never hires an agent and there is no long distance trade. Even repeating this game does not solve the problem. A merchant may punish a cheating agent by not hiring him in the future, but this punishment carries little weight if that agent can simply find work with another merchant in the future.

Bilateral punishment can only sustained trade in a limited set of situations. What is required is a multilateral punishment strategy in which agents who have cheated in the past are not hired by *any* merchants in the future. This multilateral punishment would incentives honest behaviour so long as there is a reasonably effective means of transmitting information about contractual breaches. Greif (1989, 1993) provided historical evidence in favour of such an information sharing coalition.

Greif's work was highly influential. But it also has attracted criticism because the very nature of the approach depends on the interpretation of qualitative historical evidence. Handling qualitative historical information is a different, and often more involving task, than reading a regression table, especially when it involves the Geniza documents that form the basis of Greif's study. For example, Edwards and Ogilvie (2012) argue that the Maghribi traders *did* use the formal legal system and argue that this counts as evidence against Greif's interpretation. Greif responded by pointing that out that isolated instances of Maghribi traders using courts of commercial disputes does not refute his argument, and that the examples Ogilvie and Edwards present do not concern two Maghribi traders in an agency-related dispute (Greif 2012).

Allen (2002) studies the British navy—one of the most successful organisations in history—but one characterised by numerous rules and regulations that have puzzled historians. The British navy faced an agency problem. On the one hand, the British admiralty wished captains to use their expensive capital ships for strategic purposes—to engage the enemy or to blockage hostile ports. On the other hand, the admiralty had no up-to-date information about conditions at sea or the day-to-day movement of fleets, and so they had to place a lot of discretion in the hands of the captains and admirals. Captains, however, had an incentive to avoid risky and onerous duties. Prizes were one way to incentivise captains. The problem was that at the margin they could encourage captains to pursue valuable prizes (a richly laden merchant ship) rather than more strategic objectives (the main enemy fleet).

The British navy partially resolved this dilemma by paying efficiency wages; these wages were a share of the prize based on rank. As these exceeded market wages, they created a surplus supply of captains, many of whom were retained on half-pay for long periods of time. These high wages were combined with punishment for suspected shirking and a strict set of rules for engagement—the fighting instructions. Captains suspected of cowardice were sent to the bottom of the captains list and hence would not be employed again. This encouraged navy captains to follow the admiralty's directions. Allen argues that the constraints of the battle line imposed by the fighting instruments were suboptimal from a tactical perspective, but made sense as a monitoring

device. By fighting in a rigid battle line, British admirals could monitor their captains and hence ensure that none shirked. They were thus less likely to drift away in battle (something that frequently occurred in the fleets of Britain's rivals, the French and Spanish). As a result, because, and not despite, of its tactical inflexibility, the British navy came to rule the waves.

The analytic narrative approach often benefits from being comparative. Allen (2002) compares the rule of the British navy to the very different rules applied in the British army. Army officers were also incentivised through prizes. But unlike naval captains, they purchased their positions through commission, did not appear to earn efficiency wages, and were not subject to the same degree of monitoring. Allen argues this was because the principal-agent problem was much less severe in land warfare than at sea: while targets at sea are movable, targets on land are fixed, and with 'fixed targets, it was clearer whether or not an army carried out its stated mission. Armies that attacked the wrong city would not get paid' (Allen 2002: 227). As a result, army officers could be granted greater tactical flexibility and discretion.

Leeson (2007) compares the autocratic rules used by merchant ships in the early modern period to the more egalitarian rules employed by pirate ships. The owners of merchant ships were absentee owners. To deal with the principal-agent problems that they faced, owners hired captains and paid them a combination of a fixed wage and a bonus based on successful completion of a voyage. The consequence was to incentivise merchant captains to use coercion to maximise effort from the crew. Merchant ships required autocratic captains. In contrast, Leeson notes that pirate ships did not confront the issue of absentee ownership. Pirate crews were the *de facto* owners of the ships they sailed. They required captains but not autocratic captains, and instead pirate ships employed a set of checks and balances to limit the authority of the captain—piratical constitutions (Leeson 2009).

As the comparative approach reduces the degrees of freedom available to the researcher, this helps to make the analysis more credible. Similarly, it is important that the predictions of the theory can in principle be falsified by the historical record. For instance, if evidence arose of merchant ship captains employing constitutions of the kind observed on pirate ships, this would be strong evidence against the framework Leeson uses to study maritime organisational forms.

Private prosecution associations proliferated in eighteenth- and early nineteenth-century England. Koyama (2012, 2014) documents how these associations supplied the public good of deterrence in an era where there was no professional police force. Contemporaries were worried about a crime wave in the late eighteenth century driven by rapid population growth, urban-

isation, and inequality. As the traditional volunteer system struggled, many individuals joined prosecution associations who subsidised the costs of a criminal prosecution and helped paid for local policing. Textbook economics might suggest that these associations should have unravelled due to free-riding. In fact, Koyama (2012) shows how these associations were able to surmount the free-rider problem through a combination of price discrimination and social sanctions. Through the use of rich evidence from both newspapers and local archives, Koyama shows that prosecution associations sometimes subsidised membership for the poor and documents that they were able to engage in patrolling and other forms of preventative policing.

The analytic narrative approach has also been applied in business history. Brownlow (2015) applies this approach to study the failure of the DeLorean Motor Company in Northern Ireland. This was an American car company lured to invest in Northern Ireland by Northern Ireland's Industrial Development Board. Brownlow finds that while the failure of DeLorean is illuminated by theories of rent seeking and the limitations of activist industrial policy, to fully understand this episode it is necessary to understand how John De Lorean was able to exploit the institutional environment for his own benefit. Drawing on archival records, Brownlow shows how De Lorean's rent-seeking entrepreneurship was facilitated by a political environment that believed in government support for private investment and was strongly influenced by the ongoing Troubles helps to explain the stark failure of industrial policy in Northern Ireland in comparison to other parts of the world.¹

Some Lessons

An analytic narrative should deliver more than a just-so-story. The analytic narrative approach is unconvincing when it leaves the impression that the scholar has simply cherry-picked the historical anecdotes that happen to confirm his theory, setting aside evidence that would falsify it. This makes scholarship in this field difficult as it requires authors who can master both the relevant tools in economics and the skills required to conduct serious historical research. It also requires editors and referees who appreciate these skills

¹ There are of course many other examples of analytical narratives that cannot be surveyed here due to space constraints. Examples include Johnson and Koyama's (2014) analysis of the evolution of tax farming in early modern England and France and Hoffman (2012) on the military rise of Europe. In addition to his work on pirates, Leeson has written on the conditions under which trade and cooperation can only in the absence of a formal legal system (Leeson 2014) and on the role of superstitious beliefs in supporting social order and market exchange (Leeson 2017). In medieval economic history, analytical narratives have been used to study craft guilds (Richardson 2005; Richardson and McBride 2009) and the evolution of usury laws (Rubin 2010; Koyama 2010).

and understand that historical research is quite different to the collection of “stylised facts” or selected quotations. As in the papers I have considered in this chapter, the best work in this field does not merely consist of a model followed by some historical evidence but rather reveals deep immersion in the historical evidence and secondary literature. Anchoring an analytic narrative as deeply as possible in the relevant historiography and confronting one’s theory with the different perspectives that exist in the literature force the author to deliver sharper predictions.

Students interested in the analytic narrative approach can begin by reading the papers I have discussed in this chapter for inspiration. They should also read deeply in the historical period that they are studying, looking for puzzles or institutional details that either cannot be explained by historians or are skirted over in the existing literature. If these puzzles are of sufficient interest to call for an explanation based on economic theory, then one has the basis for a potentially interesting analytic narrative.

The use of the analytic narrative does not rule out using other forms of evidence. Indeed, as economic historians have developed exciting new data sets, it has been increasingly common for economic historians to shift towards econometric analysis. Nevertheless, analytic narratives offer the economic historian an important alternative set of tools that can both generate fascinating case studies and shed light on otherwise intractable historical puzzles.

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44

Measurement and Metrics

Matthias Blum

Economic history is an empirical subject, and observing—and differentiating between—cause and effect is key to investigating past events and developments. While multiple strategies exist to make such observations, constructing relevant indicators and using them in quantitative analyses is increasingly popular among economic historians. The data work of any empirical project includes the following questions: What is the true effect and which indicator reflects this effect accurately? What is the ideal unit of measurement and, if more than one indicator is feasible, which one should I use? Is it available in satisfactory quality, and are the resources it takes to acquire justified by its added value to the project?

These are crucial questions to answer since in the event of excessive mismeasurement, any identification strategy may be doomed to fail. The benefits of compiling historical data, however, are manifold besides. Hand-collecting a unique and novel dataset is an excellent way to get to know a historical context and to start to notice hidden regularities; manually entering information into a spreadsheet and observing how new observations alter patterns in the data can be a meditative experience that often results in interesting research ideas. When collecting a dataset, *you* get to choose what is compiled and which variables are relevant; this option is unavailable when using other people's datasets provided for download by international institutions or fellow researchers. The “identification strategy” of a historical researcher may hinge on their profound understanding of the data, the variables derived from them, and the context in which they were originally

recorded. Compiling new datasets also satisfies a key requirement of cutting-edge research: novelty.

An idealised beginner in empirical research often tends to one of the following two extremes. First, he (or she) fantasises about the ideal indicator that reflects the precise effect of interest. This indicator is certainly not subject to mismeasurement, typos or any other biases or missing data. Needless to mention that in this universe, time and money are available infinitely, and long research travels to remote archives—where all of human history has been perfectly and copiously preserved—and availability of research assistants are not limiting factors. The other extreme is a quite pragmatic one, where ideals are rough guidelines rather than dogmas. The typical sympathiser of this strategy starts his (or her) research project by acquiring an overview of the available indicators, considers time, costs and usefulness (not necessarily in this order) and chooses the indicator that offers the greatest net utility. Although both strategies are appealing in their own ways, reality forces most researchers to make compromises.

True Effect and Its Proxy

Despite a natural disadvantage in terms of data availability compared to other fields in economics, economic historians have developed numerous indicators to proxy the effect of interest. This variety is a blessing for the social sciences, but they come with the requirement of understanding the various facets each of these indicators reflect. Take the example of “economic development”: a considerable part of research in economic history pertains to tracing human wellbeing through time. The most widespread approach is to use estimates of gross domestic product (GDP). GDP is a monetary approach that aims at measuring the value of all final goods and services produced (alternatively: consumed) in a given period that are included in official statistics. Once inflation is taken into account, real GDP does a fairly good job at keeping its promise. It is an important unit of measurement as it reflects economic power, it is a basis for economic policy and expressed as a per capita figure it serves as a proxy of the average productivity of a population (see Bolt and van Zanden 2014 for set of historical national accounts in global perspective). Elsewhere, researchers use net capital formation to estimate the productive basis of an economy over time rather than the flow of output. Net capital formation can be used as a baseline to add the value of other forms of investment, such as human capital and technology, and deductions such as the depreciation of

assets (see Hanley et al. 2014 for an example). Both GDP and investment-based indicators are similar in nature and are likely to be correlated; yet, they reflect slightly different facets of development, and these differences may affect the result of an empirical analysis.

GDP and related monetary income indicators, however, by definition exclude other forms of income that are important to comprehensively picture economic development: unrecorded income, such as subsistence farming and activities in the shadow economy, income inequality, environmental damage, health effects on the population and the resilience of the economy in the event of crises. Similarly, changes in the quality of products and services is difficult to incorporate in a monetary figure, and the willingness to trade-off monetary income for leisure time is not reflected in official income statistics although it undoubtedly affects well-being.

Several indicators have been proposed to address these shortcomings. For example, average height is used if the desired effect is “net nutrition” of a cohort around its time of birth, that is, gross intake of the quality and quantity of food less “expenses” for maintenance and metabolic processes and combatting unfavourable environmental conditions such as bad housing, heavy work load and diseases. Similarly, life expectancy and measures of morbidity allow assessing development from a health perspective (see Engerman 1997 for an overview). These output-oriented indicators reflect total income, regardless of the source of that income; and these indicators are found to be sensitive to inequality. Alternatively, as a result of the multidimensional character of development, composite indices, such as the Human Development Index (HDI), and its historical counterpart HIHD, are used in the attempt to provide a more comprehensive view (see Prados de la Escosura 2015 for an introduction). Each of the aforementioned indicators are important in their own right as they represent different facets of development and well-being. It is the researcher’s challenge to choose the most appropriate indicator, reflecting the effect of interest.

Are all problems solved once the ideal indicator has been identified and all typos and gaps in the data are removed? Estimating and interpreting historical data can be a difficult exercise since estimates are often based on assumptions, and different approaches may lead to different estimates (see, e.g. Clark’s 2009 critique of Maddison’s 2007 GDP estimates). Also, it is important to understand the historical context in which a measurement took place. Is there reason to believe anyone involved with the estimation had a reason to be imprecise? Was there an incentive to produce an underestimation or overestimation?

Exploiting Mismeasurement and Bias

One researcher's challenge can be another's key to solve a puzzle, as Xu et al.'s (2016) study on the long-term health effects of China's 1959–1961 famine illustrates. This group of researchers needed to address measurement error in death rates during the Great Leap Forward famine; these statistics were published by China's State Statistical Bureau, and hence might be subject to data distortion. The empirical strategy to address this potential measurement error, ironically, is based on a second source of measurement bias: the systematic exaggerations of grain yields by county officials. Local cadres in pursuit of career advancement demonstrated their loyalty to the central state by falsely claiming unprecedentedly high grain yields to confirm the effectiveness of the officially advocated agricultural measures. This misreporting, in turn, led to an inefficient reallocation of resources, leading to subsequent famine after 1959. The variable measuring overestimation of harvest quantities is exogenous to other factors and has a positive impact on excess mortality during the famine in the first stage of their regression. (see Angrist and Krueger 2001 for a discussion of the instrumental variable approach). Another example of exploiting mismeasurement is the phenomenon of "age heaping", used to capture societal human capital (see Blum et al. 2017 for a discussion of this methodology).

An increasingly popular technique in quantitative economic history systematically exploits large scale misreporting in population data to derive estimates of basic numeracy. When asked about their age, individuals who do not know it are unable to calculate it, or live in a society that deems such information not to be very useful, tend to round their age to the nearest number ending in zero or five. If individuals round their ages in large numbers, such age distributions inevitably show heaps, hence the phenomenon has been named "age-heaping". Various measures have been developed to estimate the accuracy of age statements to receive an impression of a society's numeric skills. While demographers have used similar techniques for a long time to assess the quality of census data, economic historians have started to use this method on a larger scale (see A'Hearn et al. 2009 for an introduction).

Avoiding Common Pitfalls

A recurring strategy of economic historians to obtain information about socioeconomic conditions in the past relates to the use of specific individual datasets that ideally allow conclusions to be drawn on the population as a whole. Since full census returns rarely survive history, information on the full population is rarely available for historical settings. Economic historians

instead rely on convenience samples, such as prisoners, students, conscripts, migrants or passport holders. Unless a non-representative sample is the centre of interest, such a choice brings about a very specific challenge, that is, mismeasurement due to selection bias. Data that are based on specific subgroups of the population rather than the full population or a representative sample are likely to be subject to selection, and the degree of selection and its implications for the empirical analysis have to be assessed.

For example, volunteers for military service are often self-selected, and the degree of self-selection depends on factors such as their social status, labour market opportunities and other types of opportunity costs and the willingness to take risks. Bodenhorn et al. (2017) use this example to discuss the development of stature in the United States. They argue that the decline in stature of volunteer military recruits in the United States observed by Komlos (1998) during the nineteenth century—a period of rapid income growth—could be the result of changing sample selection bias caused by unobservable characteristics. These authors argue that the observed decline in stature could as well be caused by booming labour market conditions, making jobs in the military less attractive for well-trained, productive and tall young adults. If a disproportionately large numbers of tall, adults choose to get jobs outside the military, the *observed* stature of those willing to enlist is naturally reduced. It is possible to assess the degree of bias, as shown by Bodenhorn et al. (2017), but it is impossible to remove the bias entirely without obtaining a perfectly representative sample or the full population (see Blum et al. 2017 for a discussion of the selectivity of and biases in census, prison and workhouse data).¹

Other, more obvious, issues relate to mismeasurement of variables. Also, when picking the “right” variable, the choice of variable and how well this variable proxies the true, unobservable effect has to be taken into account, and a great deal of time and effort must be invested to minimise mismeasurement. The challenge of minimising mismeasurement tends to be especially large in economic history, since for the most part of human history, official records were not kept by centralised statistical offices—most statistical agencies adapted modern procedures only during the twentieth century.

If challenges related to data quality and data availability cannot be ruled out, one must be aware of the consequences. Economic historians rely on a similar toolkit of econometric techniques as other empirical economists would do. We must, therefore, ask the same questions of our data. Does the variable accurately measure the desired effect, and can you rule out noteworthy mea-

¹ Needless to mention, there is also a need to assess the magnitude of bias. If the magnitude of a bias is small or negligible, then Bodenhorn et al.’s (2017) arguments only add an academic discussion to an empirical study.

surement error? Hausman (2001) discusses mismeasurement of variables in light of its use in regression frameworks and states that in OLS regression analysis, mismeasurement may downward bias the estimated magnitude towards zero, or reduces precision and t-statistics. In other words, mismeasured variables may distort the analysis considerably, in extreme cases making the result unreliable and useless.

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45

Econometric Identification

Matthias Blum and Arcangelo Dimico

An increasing number of studies in economic history apply quantitative empirical methods to data sets which have the explicit goal of distinguishing cause from effect. The econometrics lingo describing this strategy is “identification”, with the aim of identifying the true, unbiased and precisely measured relationship between two variables. While an ordinary least squares (OLS) estimate may allow the researcher to assess correlation, or even to *suggest* causality, more sophisticated empirical methods enable us to ascertain with far more confidence whether a correlation is indeed due to a causal connection. And these methods also permit us to make a more precise measurement of an effect’s magnitude. The main challenges to conducting a high-quality empirical study of this nature in historical settings include measurement errors due to sometimes long and tedious data transcription processes and omitted variables.

Leading peer-reviewed outlets in economics are keen to see a solid identification strategy before an empirical study is deemed publishable. This is because providing evidence on effects with precision can be valuable for academic advancement but also for policy analysis. Policymakers aim to know the effectiveness of a policy measure, or wish to compare alternative strategies. A policymaker needs to rule out alternative causal channels when designing policies. Economic history provides a solution as we are able to measure the impact of past policy interventions, including those that would not pass ethics committees today!

While empirical techniques successfully used in economic history are essentially the same as in economics and other social sciences, the entirety of the econometrics toolkit is not suitable for researching historical settings. This chapter provides a brief overview of the most appropriate identification techniques for use in an economic history research project. We illustrate our text with examples and further readings.

Identifying Correlation

Gareth Williams (2013) in his book on the story of polio provides one of the most famous examples of why an inappropriate analysis can have disruptive effects, and why identification is so important in science. It is the beginning of the twentieth century, and the US is experiencing one of the worst polio epidemics in history. Although the disease had already been noted in the eighteenth century, it was still virtually unknown, with no clue of the potential causes. Fear reached extreme levels when in the summer of 1916 the epidemic swept the nation. Benjamin P. Sandler, a nutritionist from Asheville, NC, blamed ice creams, arguing—on hindsight quite ludicrously—that a diet high in refined sugar was responsible for the polio infections. The fact that outbreaks of polio were frequent in the summer, when the consumption of ice cream peaked, convinced people that ice cream was truly responsible for polio. As a result, consumption of ice cream plummeted, ‘with one company’ from Asheville ‘selling one million gallons of ice-cream less than expected’ (Williams 2013: 94).

The example above shows that a simple correlation between two variables (polio and ice cream consumption) does not generally imply causation because the two variables (dependent and independent) may be the result of a third factor, resulting in omitted variable bias. Under certain circumstances, the OLS estimator can be used to deal with such a problem. A common approach from this point of view is to control on observables. The idea is that if the relation between X and Y is affected by a third variable, Z , then by controlling for such a variable Z , we should be able to identify the causal effect. This is, of course, a very ambitious approach, since we may never actually be sure to have identified all putative causes—there might be multiple Z s with some of them being unknown to us. As a result, we may never be able to unambiguously identify the causal effect.

That said, there are potential tests that allow researchers to check the impact of putative factors which may be imputed to omitted variables. One such test is developed by Oster (2017), who notes that the robustness of estimates to potential omitted variables can be to observe the stability of the coefficient when new control variables are entered in the model. Based on this intuition, she develops a simple test which permits the evaluation of the possible degree of omitted variable bias under the assumption that the selection on the observed controls is proportional to the selection on the unobserved controls.¹ The result

¹This test can be run in Stata by downloading the command “psacalc”. Oster (2017) also considers it important to look at movements in the R^2 , given that some controls may not explain a lot of the variance in the dependent variable, and therefore are unlikely to have an impact on the coefficient of interest.

of this test is a ratio of selection on “unobservables” to selection on observables, which is needed to attribute the entire effect to selection bias.

A common approach in financial and macroeconomic history to assess correlation are Granger causality and cointegration tests, which can be applied to data with a time series component. Granger causality tests aim to assess whether one time series is useful in forecasting another time series. Granger causality tests allow the researcher to exploit timing and sequencing; for example, if factor X occurs chronologically before factor Y, one can motivate a straightforward test if a factor X “G-causes” factor Y. More generally, cointegration tests allow to assess if two time series are related. See Morck and Yeung (2011) for a discussion of how these tests are used in historical settings.

Identifying Causation

Besides omitted variables, data quality may be a concern when using historical data. For example, data used in economic history are often generated using manual labour to transcribe and digitise lists and manuscripts, which may be handwritten. This may naturally lead to typographical errors and coding mistakes. While such isolated cases can be addressed using reasonable data cleaning procedures, more systematic mismeasurement may result in measurement error. Severe and systematic cases of measurement error and omitted variable bias may, in turn, result in attenuation bias (i.e., bias the regression slope towards zero), making the identification of an effect more difficult or impossible.

The instrumental variable (IV) approach is a potential strategy that helps to address these sources of endogeneity (i.e., measurement error, omitted variables bias and reversal causality). An IV strategy requires an IV, Z, which only affects the potentially “endogenous” variable, X, but has no direct effect on the outcome variable, Y, other through X. In other words, we need a new variable which is correlated with X, but uncorrelated with other potential factors which may affect the equilibrium level of Y (i.e., the error term of the regression) to make this instrument “exogenous”. Because this instrument must not affect factors influencing the equilibrium level of Y, researchers normally look at potential instruments that are virtually independent of the economic theory. Angrist (1990), for example, is interested in whether military service affects labour market outcomes. To address the likely endogenous relationship between the probability to join the army and labour market

outcomes, he exploits the exogenous variation in the probability to serve during the Vietnam War; he bases his instrument on the random US draft lottery. Similarly, Miguel and Roland (2011) use the north-south distance from a district to the 17th parallel, the provisional military demarcation line between North and South Vietnam as an IV for bombing intensity in Vietnam to test the effect of warfare on economic growth. Other instruments which have been widely used include rainfall (Miguel et al. 2004), distance from the coast (Nunn and Wantchekon 2011) and wind speed and direction (Feyrer and Sacerdote 2009).

It is important to note that there is a difference between economists and economic historians in how they develop an IV strategy. The conventional approach taken by many non-economic historians is to use Z to instrument an endogenous variable; here, Z is a means to an end, as it helps to identify an effect. In contrast, IV approaches in economic history may include Z into the narrative. See Dimico et al. (2017) for an example, where the number of frost days, Z , explains citrus fruit production, X ; X in turn provides a potentially unbiased effect of citrus fruit production on the presence of the mafia, Y .

Difference-in-differences and discontinuity regression designs are other potential identification strategies which can address omitted variable bias. These methods are quite common in economic history because historical exogenous events, such as policy interventions or “accidents”, may provide enough spatial and/or temporal variation. The rationale behind both of these approaches is to have two groups, a treated group and a control group. The treated group is exposed to a treatment (e.g., a policy intervention) and a control group that remains untreated. The control group in this case is used to understand what would have occurred if individuals were not exposed to the treatment in the first place. Both strategies rely on the fact that there is no selection into the treatment or control group (i.e., the treatment is independent from the error) and, as a result, any change in outcomes can be solely attributed to the treatment.

As for the regression discontinuity design, this assumption is generally ensured by the fact that the treatment is determined by a discontinuity (a threshold determined by an “*ad hoc*” numerical threshold or an actual exogenous border) and that individuals within a certain interval close enough to the threshold should have similar characteristics independent of whether they have been treated (those on one side of the threshold) or untreated (those on the other side of the threshold).

For the difference-in-differences approach, the independence of the treatment from the error is much trickier and involves a battery of tests to show

that there are no factors correlated with the treatment.² Dell (2010) and Miller (2008) provides two examples in which historical changes/factors are exploited using regression discontinuity and difference-in-differences approaches. Dell (2010) uses regression discontinuity to examine the long-run impacts of the *mita*, a forced mining labour system in effect in Peru and Bolivia between 1573 and 1812, on consumption and childhood stunting. A peculiar feature of the *mita* which allows her to use a regression discontinuity design is related to the fact that the ‘contribution to *mita* conscripts changed discretely at the boundary of the subjected region: on one side, all communities sent the same percentage of their population, while on the other side, all communities were exempt’ (Dell 2010: 1863). As a result, she can exploit such a discrete change at the border in the contribution to *mita* to identify the persistence of the effect of such a forced mining labour system. Meanwhile, Miller (2008) uses a difference-in-differences approach to estimate the effect of state-level women’s suffrage laws after 1900 on local public health spending and child mortality. He exploits the fact that suffrage to women was extended in different US states at different times between 1869 and 1920. As a result, he can exploit the variation across states and time and estimate a simple difference-in-differences model.

Placebo regression is an innovative method to identify cause from effect that uses the idea of counterfactual or hypothetical settings that differ from aforementioned approaches in one key respect: they exploit the *lack* of treatment. Jedwab and Moradi (2016), for example, assess the importance of colonial railways for the development of poor countries. After a set of tests where a positive correlation between distance to railway and agricultural production is identified, the authors use “placebo regressions” to show exactly the opposite: without railroads no such effect can be observed. In nineteenth-century Ghana, authorities considered six railroad lines, but not all projects were implemented. Jedwab and Moradi (2016) make use of the fact that all potential railroads are similar with respect to economic potential, feasibility, cost-benefit ratios and so on. Also, most of those placebo railroad lines follow historical trade routes and have become roads later, so there was reason to believe that all routes would have been economically sensible to build. Judged by observables, the proposed lines were very similar to the actual lines built but differed only in one key respect: some of them were realised, others were not. Placebo controls mimic the tested treatment—such as railroad suitability—in all ways except the treatment, that is, the presence of a railroad.

²The most important test in a difference-in-differences approach is to evaluate whether the treated and the untreated group follows a common trend.

Pitfalls and Caveats

Before any researcher gets overly excited about an allegedly effective identification strategy, she must consider a few words of warning. When entering the ring of advanced econometrics, for example, when submitting a paper or giving a seminar, knowledgeable audiences will test any IV strategy for internal *and* external validity (just like in any quantitative analysis), in addition to the plausibility of the instrument.³ Obviously, the identification strategy must be plausible and meet the usual criteria when applied to the sample population. This is the basic requirement any applied econometrician aims to meet and this is the very reason attenuation bias, sample selection bias, simultaneous causality bias, misspecification and measurement error ought to be on the checklist.

As outlined above, IV techniques can be powerful tools to identify cause from effect, especially in historical settings. However, there are limits to testing the validity of an instrument since many potential problems are related to correlation with an unobserved error term. Neither statistical software nor mathematics in general are able to assist with this challenge. The only way forward is through a deep knowledge of the historical and economic setting and by making a convincing discussion of the suitability of the instrument within this setting. In the end, the scholar must rely on her own judgement.

A related problem is the repeated use of the same instrument in different settings.⁴ Take Acemoglu et al.'s (2001) influential study on the impact of settler mortality on the number of European settlers who, in turn, increase the propensity to establish growth-enhancing institutions. While the identification strategy was considered sound and well-crafted at the time of publication (it was published in one of the top journals in economics after all), a number of follow-up studies criticised the violation of a series of IV requirements.⁵ Glaeser et al. (2004), for instance, argue that settlers did not exclusively introduce growth-enhancing institutions but also growth-enhancing human capital. Obviously, if Glaeser et al. (2004) are correct, the exclusion restriction had been violated in the original study. But what if a pioneering study had argued

³ External validity is often considered more difficult to comply with since the empirical test must be applicable to the general population. Obviously, large and representative samples should be given preference. Ideally, any statistical test is applied to the full population. That said, in econometric day-to-day operations, often a trade-off between internal and external validity is observed. The harder we try to isolate an effect from confounds, the more unlikely it is that the results can be generalised since the outside world is complex, and there are many effects interacting with each other.

⁴ See Morck and Yeung (2011) for a broader discussion of identification in historical settings.

⁵ Also see Albouy (2012), who argues that the lack of accuracy of historical mortality rates and other data uncertainties caused a weak instruments problem.

that settler mortality in former colonies influenced development through human capital accumulation instead? The author of such a fictitious study might have “got away” with such an argument while violating the exclusion restriction himself (by ignoring institutions).

So who is right and in whose result should we believe? Those who publish their (possibly incorrect) study first? Or the study that provides the “best” identification strategy combined with the most convincing narrative, regardless of the time of publication? Or perhaps none of these authors are correct, since they discredit each other’s identification strategies? Experience suggests that early studies benefit from a first-mover advantage, since no benchmark study is available yet. Second movers benefit from this basic research but ought to engage with the existing literature and relate their study to that of the “pioneers”.

The ambitious economic historian, however, should not solely consider the feasibility of an empirical study and the quality of an identification strategy but also its potential benefit. Imagine someone planned a research project exploiting the Christmas Truce of 1914 as a natural experiment. This truce was a spontaneous, local and unauthorised ceasefire during the First World War, involving French, British and German troops. The historical setting allows for an application of a difference-in-difference setup, as affected and unaffected sectors on the Western front qualify as panel variables. There is also an intertemporal dimension as periods before, during and after the ceasefire can be unambiguously identified. Such a research project allows us to identify a causal link between military engagement in trench war and the mortality of soldiers. In such a setting, one would identify an dependent variable (e.g., the number of casualties by day and sector) and independent variables including the exogenous effect (ceasefire) and a series of control variables—such as the quality of defence facilities, the number, type and firepower of the available weapons on each side and quality and equipment of military personnel.

While an archetypal econometrician may be amazed by the sheer technical possibilities such a hypothetical setting would provide to precisely identify cause from effect, plain common sense suggests that a ceasefire dramatically reduces mortality among front-line soldiers. Similarly, an archetypal historian may simply refer to the historical record and materials available in each country’s national and military archives, concluding that the effect of trench war on mortality is devastating and should be avoided at all costs since more modern and effective means of warfare and conflict resolution have been developed since the First World War.

What are the take-home lessons? The econometrics toolkit provides numerous options. These tools must be used wisely, should be combined with a

convincing narrative and need to result in scientific value added. A starting point for students wishing to make some first-hand experiences without investing fortunes in research time and data acquisition could be a replication of a study that potentially does not meet high modern standards. These are not difficult to find—just browse older issues of the leading journals in our field and look for empirical studies using basic econometric techniques. Ideally, you will look for an influential study that has been uncontested; confirming or revising the results of such a study is a worthwhile exercise that may bear a valid contribution to science. Some journals are willing to publish replication studies, such as the *Journal of Applied Econometrics*, if the contested study was published in a top journal in economics.⁶

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⁶See Colvin and McCracken (2017) for a recent example of a successful replication study, and the Replication Wiki, hosted by the University of Göttingen, for advice on how to start a replication project (<http://replication.uni-goettingen.de>).

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46

Historical National Accounting

Herman J. de Jong and Nuno Palma

The reconstruction of national income and production levels (per capita) over the long run is a prerequisite to testing competing hypotheses about why economies grow over the long run—or fail to do so. Economic historians have been actively building such estimates over the last few decades with considerable success. These reconstructions have already born fruit, for instance, by showing that economic growth in Europe started earlier and was more gradual than previously thought and that Europe’s divergence with respect to other parts of the world dates from the early modern period (1500–1800).

Angus Maddison was one of the first scholars to compare income and production levels across countries and over time. In particular for the period before 1950, his was the only database that provided systematic and broad cross-country information on comparative income and production levels (Maddison 1995, 2001, 2007). Since Maddison’s death in 2010, the development of the Maddison Project Database (MPD) has moved to a new generation of scholars (Bolt et al. 2018).

In this chapter, we review recent developments in historical national accounting, and provide a short introduction to their methodologies.¹ We can make a distinction between reconstructions for three periods of history. For the period after 1950, we can use official estimates of gross domestic product (GDP) made by the statistical agencies of individual countries. Between 1850 and 1950 we

We are grateful to Chris Colvin and Diane Coyle for discussions.

¹ For information about the underlying sources, see Palma (2019).

rely on reconstructions based on historical statistical data that are reasonably trustworthy for most developed countries (see, e.g. Feinstein (1972) for the UK). Such data include production series, price data, wages, and employment. For less-developed countries we still have to rely on indirect measures based on, for example, import or export statistics of major products. For the period before 1850, more indirect methods and stronger assumptions have to be applied to arrive at plausible data, as we discuss in the second part of this chapter.

Statistical Age Reconstructions

GDP can be defined as the sum of the values of all marketed goods and services within an economy. This measure of economic performance was developed during the 1930s (Kuznets 1934). Because these values are expressed in prices of a certain currency and for a specific year, we need to make adjustments if we want to make comparisons of GDP between countries and across time. Central to the reconstruction of historical national accounts is therefore the measurement and comparison of proper price levels in order to get a price index for national inflation. For comparisons between countries, values of production or income need to be expressed in a common converter. We can use nominal exchange rates, but these may only be representative for traded products. Prices of non-traded products are generally lower in low-income countries.² As a result, differences in income levels would be substantial if the comparison were to be based on exchange rate-converted expenditure. Therefore the objective should be to estimate real GDP per capita based on a comparison of prices of traded and non-traded goods and services, so-called purchasing power parities (PPPs) (Deaton and Heston 2010).

A price converter between two countries can be calculated by taking the average of two estimates, where we combine the price of a good in country A with the volume of output in country B and vice versa (Bolt et al. 2018). A similar methodology can be applied for within-country estimates of price developments where we can compare prices in year one with the volume of output in year two and vice versa. The conceptual difficulty with this procedure is that the basket of known products for which there is information on quantities and prices in a specific country or year needs to be representative of the production structure or consumption pattern of the total economy. But products and their quality change across countries and over time. Moreover, production and consumption patterns differ between countries. This potentially may lead to measurement error.

²This is known as the Balassa-Samuelson effect, or the “Penn” effect.

The concept of real GDP per capita refers to a series being based on a common set of prices across countries. In Maddison's work, such data were compiled by starting from a modern-day income comparison (1990) and then using growth rates of real GDP per capita from reconstructed historical national accounts to make comparisons for earlier years. An attractive feature of these data is that the change in real GDP per capita over time matches the growth rate from the national accounts estimations. This internal consistency can come at the expense of distorted real GDP per capita comparisons for earlier years—standalone benchmark comparisons or independent estimates of relative income for earlier periods can diverge substantially from the backward-extrapolated time series (see Ward and Devereux 2018).

While time series of GDP per capita growth from the national accounts (or reconstructions) may be considered reliable in modern times for many countries, periods like the world wars, or periods of economic instability or large price volatility, may see less reliable statistics. This was illustrated by Prados de la Escosura (2000), who argued that PPPs based on extrapolations from a recent year (say 1990) led to implausible results for the years before 1950. His solution was to rely on the regularity of the price-income relationship to estimate what relative prices would have been if we had been able to observe them historically.

Another approach to this issue is to rely on historical benchmarks, that is, independent real GDP per capita benchmarks from historical studies. This methodology has also been developed in the Penn World Tables (PWT), where long-term income series are tied to relative income levels, thereby taking into account relative price changes between the different benchmark years (Feenstra et al. 2015).

The number of available historical benchmarks between and across countries has increased significantly in recent times. Various methods have been employed, making use of the output/value-added approach, the income approach, and the expenditure approach. Usually, these studies compare the leading economy (USA/UK) with one or more other economies (e.g. Germany, France, or Japan; see Broadberry 1998; Fukao et al. 2007). Applying the results of all known benchmark studies makes it possible to re-anchor historical time series following the PWT methodology, so that the comparisons no longer depend solely on one present-day benchmark, as is the case with the original approach by Maddison.

A new combination of the multiple historical benchmarks with the long time series of per capita income from the new MPD changes the pattern compared to the original series produced by Maddison. Sometimes this is the result from the switch to a new set of relative prices, but it could also result from new estimates or updates of national accounts statistics. Figure 46.1 shows GDP

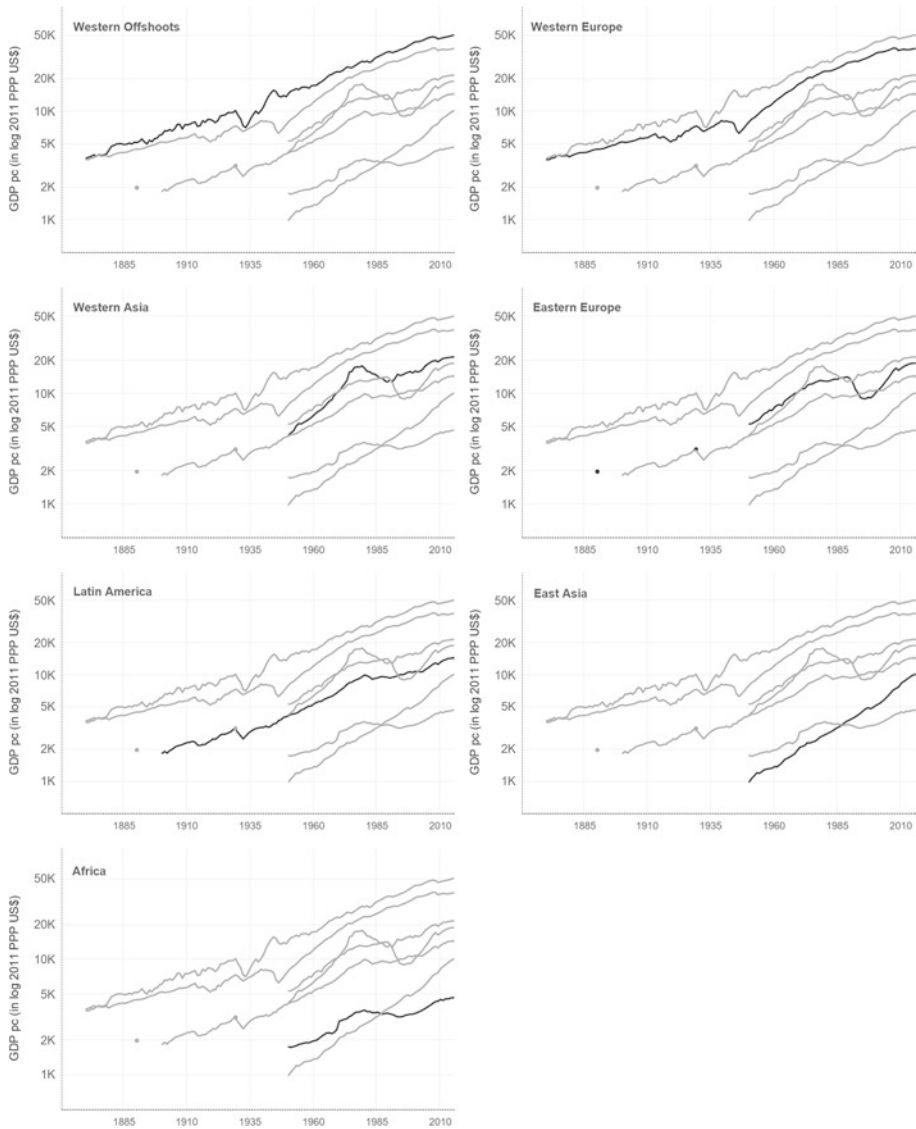


Fig. 46.1 GDP per capita in constant 2011 US\$ prices (log scale of base 2). Source: Bolt et al. (2018)

per capita by world region.³ It illustrates a Great Divergence period, with very low East Asian income levels until the 1950s. It also shows volatile patterns of relative improvement and relative decline, as in Western Asia in the 1980s, Eastern Europe in the 1990s, and Africa in both the 1980s and 1990s.

³In dividing the world into regions, we follow the usual labelling convention, though there is no good reason why countries such as the USA or Australia are classified as Western offshoots, but Argentina or Brazil are in a different category.

Reconstructions for the Pre-statistical Age

Recent work on the pre-statistical period is producing more time series of per capita GDP. In the literature, two methods have been used to quantify historical GDPs for this period. The first is an output-side approach, which has been used, for instance, for Britain (Broadberry et al. 2015) and the Netherlands (van Zanden and van Leeuwen 2012). These countries have a sufficient research tradition in quantitative history such that it is possible to calculate the different yearly components of output at current prices, which are then aggregated and transformed in real values by using a weighted index of prices. Proxies are used to calculate the value of services, for which less information typically survives.

For countries with a poorer track record of research in quantitative economic history, the data required to produce output-side estimates are not available. The alternative is an indirect, consumption-based method. The countries for which such estimates now exist, going back to the sixteenth century or before, include Italy, France, Spain, Portugal, and Poland.⁴ The method first produces an estimate for the agricultural product,⁵ which has been calculated in a relatively standard way across these studies. Alternative methods have been used to estimate other sectors of each of these economies, finally leading to GDP.

It is hence important to realise that the demand-side estimates are themselves the result of a model and rely on several assumptions. Domestic consumption of agricultural products is assumed to be equal to agricultural production (the external sector can be adjusted for, if necessary). This is then deflated to arrive at constant prices of a given year.

The basic input across all demand-side studies is real wages. Plenty of data exist with which to construct nominal wage series for many occupations, for both skilled and unskilled workers, and for both urban and rural occupations. A prominent source of difficulty is that historical wages are most frequently registered as day wages.⁶ This means that assumptions about work time are needed to calculate annual income or productivity statistics. Real wages are determined by dividing the nominal wage by a Consumer Price Index (CPI) usually calculated as in Table 46.1.⁷

⁴ Malanima (2011), Ridolfi (2016), Álvarez-Nogal and Prados de la Escosura (2013), Palma and Reis (2018), Malinowski and van Zanden (2017).

⁵ Deaton and Muellbauer (1980), Wrigley (1985), Crafts (1984), Allen (2000).

⁶ See, however, Humphries and Weisdorf (2015).

⁷ A more restrictive, “barebones” basket is considered by Allen et al. (2012).

Table 46.1 Robert Allen's respectability CPI basket

	Quantity per person per year	Spending share (%)	Calories per day	Grams of protein
Bread	182 kg	30.4	1,223	50
Beans/peas	52 litre	6.0	160	10
Meat	26 kg	13.9	178	14
Butter	5.2 kg	4.3	104	0
Cheese	5.2 kg	3.6	53	3
Eggs	52 units	1.3	11	1
Beer	182 litres	20.6	212	2
Soap	2.6 kg	1.8	–	–
Linen	5 m	5.3	–	–
Candles	2.6 kg	3.1	–	–
Lamp oil	2.6 litre	4.7	–	–
Fuel	5.0 millions of BTU	5.0	–	–
Total		100	1,941	80

Source: Allen (2001: 421)

This basket is based on the conditions in mid-eighteenth-century Strasbourg⁸. But relative prices changed over time, so by using a fixed basket, consumer demand is implicitly assumed to be price and income inelastic⁹, an assumption which is only defensible on pragmatic terms, due to the data limitations that this kind of studies face.¹⁰ But across time and space, relative prices changed due to different agricultural conditions related to land quality, technology, organisation, available crops, and weather. Allen (2001: 421) made some substitutions in the form of olive oil and wine consumed in the southern Europe instead of butter and beer.¹¹

Once an index for real wages is obtained, a short-cut method is applied to arrive at an estimate of agricultural production (see, e.g. Palma and Reis 2018 for details). The next step aims to arrive from this measure of agricultural product to an index for GDP. Two alternative methodologies have been used. The first uses an inter-sectorial productivity gap calculated at T, a year in which this gap is known. Year T should take place before modern economic growth with significant structural transformation and changes in relative prices took place.

⁸ Strasbourg was chosen by Allen (2000) largely for reasons of convenience. Allen (2017) improves on this by allowing food baskets to vary with respect to local and time-specific consumption patterns.

⁹ There is no response as suggested by Engel's law. With more income, people are here assumed to buy more baskets under the same proportions, and excluding luxuries.

¹⁰ Also, no housing costs are considered (Allen 2001: 422).

¹¹ As is the case for modern economies, it is hard to control for quality changes and the appearance of new goods, many of which appeared for the first time in Europe in the early modern period, including maize, potatoes, and tomatoes.

This is then assumed to be a constant which can be extrapolated back in time and combined with the relative share of labour and land income at any point in time in order to calculate per capita GDP for previous periods. A second methodology consists of a regression which uses as a main input the size of the urban sector (Malanima 2011; Álvarez-Nogal and Prados de la Escosura 2013). With these real volume indices at hand, real GDP per capita levels for any given year can be obtained by going backwards from the first available solid benchmark at constant prices (e.g. 1850 or even 1820).¹²

Concluding Thoughts

Having good data on long-term development and economic performance at the level of national or regional economies is a prerequisite for theoretical and empirical growth studies and for convergence analysis. Nonetheless, the concept of GDP has limitations. One problem relates to the appearance of new products, and the quality adjustments to existing products, which may not be fully reflected in the estimated price levels.¹³ Another limitation is that non-market services—especially government services—are difficult to measure.¹⁴ Finally, GDP relates to an economy's productive, income-generating capacity for a given year. There may be a relationship with the standard of living in a country or with the well-being of its population, but it is not the same concept, because GDP excludes the non-monetary dimensions of well-being. It also does not consider matters related to the distribution of income. Hence, GDP per capita is a measure that can diverge from more specific measures of living standards of consumers and workers, or more comprehensive measures of welfare, that account for differences in health, leisure, and inequality. However, an important benefit of GDP per capita is that it can be used not only as an indicator of living standards but also as the basis for productivity comparisons, which have the potential to shed light on the sources of income differences across countries.

¹² An important alternative method to arrive from PPPs to the Geary-Khamis 1990 “international” dollars is the short-cut method proposed by Prados de la Escosura (2000).

¹³ For recent papers which tackle some of these problems, see Aghion et al. (2017), Abidrahman et al. (2017) and Coyle (2018).

¹⁴ This problem exists not only with historical, but also modern national accounts and is perhaps worse for modern economies, given the much larger size of their public sector. Strong assumptions to estimate value are typically made using cost data.

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47

Productivity, Innovation and Social Savings

Gerben Bakker

A key issue in economic history is how to quantify the impact of new technology.¹ Some innovations, like railways in the past and computers today, are clearly visible everywhere around us, and the informal impression emerges that they must be very important. Yet, economic historians would like an objective way to assess the impact of the innovation, and this is how they can sometimes “hack” previously qualitative debates. This chapter will first discuss growth accounting, the discipline’s key workhorse, and then the attempts made to quantify the welfare improvements brought about by new goods.²

Growth Accounting

From the 1930s to the 1950s, economists were under the spell of a big unsolved mystery in economic history. Studying the recently developed national accounts, they noted that a large part of US output growth since the nineteenth century could not be explained by a proportionate increase in weighted inputs (Copeland 1937; Copeland and Martin 1938; Stigler 1947). They considered this “residual” due to the more efficient use of inputs and were surprised by its enormous size. Originally, Tinbergen (1942) estimated it at 27 per cent of output growth for 1870–1914; later Schmookler (1952)

¹ I thank Chris Colvin, Matthias Blum and Pieter Woltjer for comments and suggestions. The usual disclaimer applies.

² The latter can be seen as the objective function to which growth accounting provides the supply-side constraints (Hulten 2001).

estimated it at 37 per cent for 1869–1938 (manufacturing only), Kendrick (1956) at 60 per cent for 1899–1953, Abramovitz (1956) at 48 per cent for 1869–1953 and, finally, Solow (1957) at 52 per cent for 1909–1949. Similar estimates were made for agriculture between 1900 and 1950 by Barton and Cooper (1948), Johnson (1950) and Ruttan (1956).³ Some started using the term total factor productivity (TFP) to describe the residual (e.g. Kendrick 1956).⁴

Solow (1957) advanced the insight into the mystery that these studies spanning two decades had found again and again. Rather than starting from the data, he developed an elegant growth model that provided a consistent and formalised conceptual framework to interpret the results (Hulten 2001). The key insight is that the degree that output is growing faster than weighted inputs can be characterised by a Cobb-Douglas production function with constant returns to scale:

$$Y = AK^\alpha L^{1-\alpha} \quad (47.1)$$

where Y is output, K is capital, L is labour, α is the income share of capital and $1 - \alpha$ the income share of labour. A is the “scaling factor”, and when comparing outputs and inputs over time, an increase in A will reflect an increase in efficiency, in TFP. In percentage growth terms, the equation becomes:

$$\Delta\%Y = \alpha\Delta\%K + (1 - \alpha)\Delta\%L + \Delta\%A \quad (47.2)$$

where $\Delta\%$ denotes the percentage change per annum. Dividing both sides of equation 47.1 by L and taking logs, equation 47.2 can be rewritten as:

$$\Delta\% \frac{Y}{L} = \alpha \Delta\% \left(\frac{K}{L} \right) + \Delta\%A \quad (47.3)$$

In other words, the growth in output per hour can be decomposed into the growth due to the increasing amount of capital per hour of labour, and to an increase in TFP. Later refinements, which included accounting for rising labour and capital quality, reduced the residual.

³Comparative residuals reported from Griliches (1996) and Kendrick (1956). Stigler (1947) reports a residual of 89 per cent for 1904–1937 for the output per person-hour in selected manufacturing industries, which includes the effect of the increasing capital-labour ratio.

⁴For a historiography of growth accounting, see Griliches (1996), Hulten (2001) and Crafts (2009).

The efficiency growth over time that became characterised as TFP growth was a key discovery. Some see it as primarily the result of technological change, but it included other factors and became known as ‘a measure of our ignorance’ (Abramovitz 1956). Many factors can cause the shift in the production function, including better competition policy, better alertness to arbitrage, process, product, supply, market and organisational innovations, market integration enabling a deeper division of labour or a fuller comparative advantage, omitted variables, measurement errors and many more factors (Hulten 2001). The TFP growth should not be equated with things such as R&D outlays. It reflects a far broader set of factors. Bakker et al. (2018), for example, find that R&D input growth was not the dominant factor in US interwar TFP growth. Also, R&D outlays involve labour and capital that produce outputs (inventions) that in principle could be priced and sold, so at least part of R&D could show up as conventional input and output increase rather than TFP.

When we look at growth accounting across countries using expression 47.3 at times that were close to the onset of industrialisation (Table 47.1), it is immediately clear that labour productivity growth ranged widely. The residual’s contribution varied from 92, 81 and 80 per cent in Portugal, the United States and Britain, to just 20 per cent in the USSR and Spain. Both the high absolute value of TFP growth in the USA, and its larger share in labour productivity growth, led to the discovery of the residual by economists.

Before the nineteenth century, detailed input and output data are often not available, and the price dual of equation 47.2 is often used, which allows the calculation of TFP growth:

$$\Delta\%p = \alpha\Delta\%r + (1 - \alpha)\Delta\%w - \Delta\%A \quad (47.4)$$

where r is the rental cost of capital and w is the wage rate. Intuitively, this equation states that if the sum of weighted increases in input prices exceeds the rise in output prices, then the difference must be due to using those inputs more efficiently, that is to TFP growth. The only sources that are needed are wages, cost of capital and prices. Hoffman (1996) used this method to estimate agricultural TFP growth in France during the eighteenth century, and Antràs and Voth (2003) applied this method to the industrial revolution, corroborating work by Crafts and Harley (Harley 1999).

Bakker (2012) provides a walk-through, step-by-step example of how one can apply growth accounting to an individual industry, motion pictures in this case; and Bakker et al. (2018) show how one can apply growth accounting to a complete set of industries, 38 in this case, to analyse aggregate growth and the additional insights this yields.

Table 47.1 Approximate productivity growth decomposition for selected countries and periods, 1801–2004

		Annual growth rate of			TFP
		Y/L	K/L	TFP	
		(% p.a.)	(% point p.a.)	(% point p.a.)	
Britain	1801–1831	0.50	0.10	0.40	80
Germany	1871–1891	1.10	0.39	0.71	65
Austria	1870–1890	0.90	0.64	0.26	29
Hungary	1870–1910	1.65	1.18	0.47	28
Netherlands	1870–1890	0.94	0.61	0.33	35
Spain	1884–1920	1.00	0.80	0.20	20
Sweden	1890–1913	2.77	0.94	1.83	66
United States	1899–1929	2.04	0.38	1.66	81
Japan	1913–1950	1.72	0.62	1.10	64
Portugal	1910–1934	1.17	0.09	1.08	92
Italy	1920–1938	0.88	0.38	0.50	57
USSR	1928–1940	2.50	2.00	0.50	20
Korea	1960–1990	5.06	2.84	2.22	44
Singapore	1960–1990	4.97	3.34	1.63	33
Taiwan	1960–1990	6.07	3.17	2.90	48
China	1978–2004	7.30	3.20	4.10	56
India	1978–2004	3.30	1.30	2.00	61
Minimum	1801–1934	0.50	0.09	0.20	20
Mean	1913–2004	2.58	1.29	1.29	52
Maximum	1960–2004	7.30	3.34	4.10	92
Range	1801–2004	6.80	3.25	3.90	72
Coefficient of variation		0.78	0.87	0.81	0.41

Sources: United States: Bakker et al. (2018); all other countries: adapted from Crafts (2009), tables 3 and 5. For detailed sources on individual countries, see Crafts (2009).

Notes: Y/L = output per unit of labour; K/L = capital per unit of labour; p.a. = per annum. TFP refers to “crude TFP” and includes increases in labour quality.

Social Savings

A different way to measure the impact of new technology is through the social savings approach pioneered by Fogel (1962), who examined the impact of railways on American economic growth. He calculated the counterfactual price if transport needs would have had to be met by the next best alternative, waterways, and from that the social savings, which is the price difference times the final-year quantity consumed. Fogel assumed zero price elasticity, multiplying actual demand by the counterfactual price, to estimate an upper bound, since he did not include gains in the transport-using sectors. Fogel found that, without railways, US GDP in the 1890s would have been only a few per cent smaller. This surprised many who, reading the qualitative histories of railways,

had expected a large impact. It underlines how important measurement is in economic history, because it can “hack” debates through quantification, debates that otherwise could go on forever.

Social savings assume that the labour and capital used in railways could be invested at the going rate of return in other industries. What mattered was the higher efficiency of railways. By setting final-year counterfactual prices, social savings already take account of changes in factor costs since their emergence, meaning the price difference, will reflect accumulated TFP growth:

$$\frac{S}{pq} = \frac{\Delta p \cdot q}{pq} = \frac{\Delta p}{p} = \frac{\Delta A}{A} \quad (47.5)$$

where S is the social savings as percentage of final-year consumption. The equivalence between the two approaches is precisely how Foreman-Peck (1991) extended Hawke’s (1970) social savings estimate for British railways to 1865–1890 (see also Crafts 2010).

Fogel assumed inelastic demand to compensate for gains in the transport-using sectors. But including actual price elasticity (ε), social savings as percentage of expenditure become:

$$\frac{S}{pq} = \frac{\Delta p}{p} + \varepsilon \frac{\Delta p}{p} + \varepsilon \left(\frac{\Delta p}{p} \right)^2 \quad (47.6)$$

This gives a lower bound to elasticity-adjusted social savings by assuming a linear (uncompensated) demand curve.⁵ The first term shows the Fogellian social savings; the second term can be rewritten as $\Delta q/q$ and subtracts the effect of actual demand being higher than counterfactual demand (Δq is negative) and the third term subtracts the joint effect of quantity and price (ε is also negative). In the Fogellian case, the last two terms collapse to zero. The higher the price difference or the lower elasticity, the higher the social savings. If we have price elasticity and actual and counterfactual prices, we can estimate social savings, even if we do not know aggregate quantity.

The relevance of price elasticity is supported by Bakker’s (2008) social savings estimate of cinema for 1900–1938, which ranged from 2.3 per cent in the United States, and 1.4 per cent in France, to 0.3 per cent in Britain. Two-thirds of the US-British difference could be explained by lower British live

⁵ For simplicity, expression (47.6) assumes a linear downward sloping demand curve and a horizontal supply curve. With a compensated demand curve and an upward sloping supply curve, social savings would likely have been even larger. Derivation available from the author.

entertainment prices and one-third by differing tastes for filmed versus live entertainment. This suggests that tastes are important when estimating consumer goods' social savings, and therefore, it might be useful to take price elasticity and consumer surplus into account.

Hausman (2003) shows how a lower bound to the consumer surplus as percentage of expenditure can be expressed as minus half of the inverse of the price elasticity of demand ($C/pq = -1/(2\varepsilon)$). This assumes a linear demand curve and thus provides a lower bound. For the case of mobile telephones, Hausman (1997), using a more refined method, estimated that regulatory obstacles that delayed their introduction in the USA for ten years had accumulated social costs of about \$100 billion in 1997.

Using Hausman's expression in (47.6) shows the relation between elasticity-dependent social savings and the consumer surplus:

$$\frac{S}{pq} = \frac{\Delta p}{p} - \frac{1}{2C^*} \left[\frac{\Delta p}{p} + \left(\frac{\Delta p}{p} \right)^2 \right] \quad (47.7)$$

where C^* is C/pq . This shows that the higher the consumer surplus, the higher the social savings. On the relationship between social savings, price elasticity and consumer surplus, see Leunig (2010).

The advantage of social savings compared to TFP growth is that it makes the user benefits immediately clear through the price decline net of factor cost changes and through exclusively focusing on the domestic economy. It gives a handy intuitive number in share of national income. For export-led industries, TFP growth and social savings will differ substantially because of declining terms of trade. In Britain in 1841, for example, the welfare gain from cotton textiles was about 11 per cent of income, while not correcting for the terms of trade would yield a gain more than double that (Harley 1999; Crafts 2010).

Social savings or TFP growth rates or levels can be compared by both industry and internationally. For tradable goods industries, Revealed Comparative Advantage (RCA) is a different technique. It uses an industry's share in a country's exports over the worldwide industry's share in global exports, and industries can so be ranked to be compared within or across countries. Crafts (1989) provides an illustrative economic history application, estimating the RCA of 16 sectors of several major countries between 1899 and 1950. Another way to compare productivity is through frontier analysis.

Welfare Benefits

The TFP growth is often seen as providing a supply-side constraint to welfare improvements and social savings as better capturing the user benefits of new technologies. It is also possible, of course, to attempt to estimate welfare benefits directly. Hedonic pricing studies generally treat goods as bundles of characteristics and try to estimate the role of each characteristic in the price. Griliches (1961), discovered far slower growth in US car prices between 1937 and 1960 than official price indices, while Raff and Trajtenberg (1997) determined that two-fifth of the fall in US real car price between 1906 and 1940 was due to quality increases. Requena-Silvente and Walker (2006) provided a detailed hedonic analysis of British car prices between 1971 and 1998. Nordhaus (1997) examined the price of light since Babylonian times and measured the price per lumen-hour, rather than per candle, oil lamp or light bulb. He found an enormous price decrease that was severely understated in official statistics. He conducted a similar study for computing (2007), focusing on the cost per calculation. Nordhaus suggests official measures severely overstate inflation, which would mean that real output was higher and therefore also TFP growth.

Yet consumers still need to buy a minimum unit size of a light bulb and thus pay more than the price decrease per lumen-hour might suggest. Likewise, the price decrease of computers in calculations per second is much sharper than that of the minimum buyable size. Looking back, a good always looks more essential to society than at the time it was introduced. Without large hedonic price falls, economies could simply stick to substitutes. As Angus Maddison once pointed out, without electric light we would use daylight more effectively when designing buildings, the working day and the working year. Without the car, the early twentieth century would simply have had denser city layouts and more public transport, as Bresnahan and Gordon (1997) note, much like the late nineteenth-century economy without railroads would simply have adapted.

Another way to quantify welfare benefits of new goods is to look at the spillovers of each good on the entire consumption bundle. Broda and Weinstein (2006), for example, find that the welfare benefits of the three-fold increase in the variety of US imports between the 1970s and the 2000s added 2 per cent to per capita income. This again suggests the inflation rate might be overstated. Hersh and Voth (2009) estimated the welfare benefits of the new goods coffee, tea and sugar since 1492 and found that by 1800 English consumers would have been willing to forego 10 or more per cent of their income to maintain access to sugar and tea alone. They suggest that the estimated welfare gain of each of the goods may have been

several times larger than that of the personal computer, a top welfare-increasing product today.⁶ Economic historians can treat many everyday products as new goods because for most a historical time exists in which they once were.

Another reason why production-side data are not always a very good indicator of welfare improvements is that many goods and services require a substantial time input from the consumer. Goolsbee and Klenow (2006), for example, find that measured as expenditure, internet use seems to be a negligibly small consumer good, but when they estimate the value of the time people spent using it (derived from wages), internet suddenly becomes very important. They suggest time costs may be a better welfare indicator than price. Likewise, Leunig (2006) included passengers' opportunity costs while estimating the social savings of Victorian British railways and found them higher than previously estimated (Hawke 1970).

The value of time seems also essential in assessing the welfare benefits of healthcare costs. The valuation of outcomes affects how efficient ever-increasing healthcare expenditures really are. Nordhaus (2005), for example, finds that over the entire twentieth century, the value of increasing life expectancy was about as large as the value of measured growth in nonhealth goods and services. And Cutler and Richardson (1997) find that between 1970 and 1990, the increase in quality-adjusted health capital per capita was about five times the increase in medical spending per capita. Frech and Miller (1999) suggest that doubling drug expenditure at age 40 increases life expectancy by 2 per cent (about a third of the effect of doubling GDP), implying again that the output achieved with healthcare inputs may not be properly measured. Depending on how outcomes are evaluated, the health sector could have a high or low productivity increase, and the doom about increasing costs might be misplaced (Crafts 2003).⁷

Intertemporal comparisons of the welfare benefits of new goods stumble upon the classic tormentor of economic history: the index number problem. We cannot simply examine the costs of goods: proportions of goods in the household basket change, the quality of the goods changes, old goods disappear and new goods arrive. Therefore, costs-of-living indices are used that proxy what bundle of goods is needed in the latter year to derive the same

⁶New goods can, of course, also have welfare costs, sometimes unmeasured. Prime examples are negative externalities such as the health effects of tobacco, traffic congestion and pollution.

⁷A concept that includes broader welfare increases is the Human Development Index (HDI), which averages indicators for education, income and life expectancy per capita. On the HDI in economic history, and related indices, such as the Dasgupta and Weale index, see Crafts (1997).

utility as in the initial year. Comparing consumption bundles intertemporally is difficult, as at present both the old and the new goods are available, but in the past only the old good was. This leads to a corner solution of the budget constraint, and thus to a lower indifference curve than if we would be somewhere in between the corners (Crafts 2003).

Also, the ratio of price to opportunity costs may change over time, so a decrease in market price might not precisely equate to increasing welfare benefits. In Britain between the 1930s and the 2000s, for example, the CPI-deflated price of movie tickets increased 1.1 per cent per annum, on average, while real wages increased 1.8 per cent. The full costs (time and ticket) increased 1.7 per cent annually, 80 per cent of the costs growth being driven by time becoming more valuable (Bakker 2014: 436–437).

Finally, over time, if we see the price of a good going up and its demand increasing substantially, this often reflects substitution. This can be for various reasons, such as an unaccounted-for quality increase or changing consumers' opportunity costs and concomitant moves to or from time-intensive goods.

In conclusion, this chapter has discussed the extent to which economic historians can quantify the effects of innovation. On the production side, we learned about growth accounting, which measures a much wider set of effects than merely technological innovation, and social savings. We also reviewed several ways to quantify the welfare benefits of new goods and in the end learned, to follow Becker (1993), that the 24-hour limit to our days makes it impossible that prolonged TFP growth will carry us into a utopia, because as goods and services get ever cheaper, our opportunity costs get ever higher.

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Frontier Analysis

Pieter Woltjer

In both economics as well as business studies, a great deal of research is devoted to the study of efficiency. Firms aim to maximise profits or minimise costs while, in a macroeconomic setting, policymakers aim to increase economic production by utilising labour and capital more efficiently. The problem is essentially the same: how can output be increased without increasing the inputs used or, reversely, how can inputs be decreased without also decreasing output (Koopmans 1951: 60). Frontier analysis studies these issues by identifying the most efficient firms, industries, or countries and comparing the performance of the remainder of the sample to this “frontier”. This provides insights into how resources are allocated differently, how technology is utilised, and what impact these choices have on final production. This differs from standard productivity analysis, which uses parametric (econometric) techniques to intersect the data with a production function rather than surrounding it with a (production possibility) frontier (Färe et al. 1994: 3). In this chapter, I show that frontier analysis can be used as a valuable supplement or even replacement for traditional growth accounting.

For students in economic history, frontier analysis is a particularly useful tool for three reasons. Firstly, compared to more traditional growth accounting or productivity analysis it provides a much more detailed picture of the sources of growth (Coelli et al. 2005). As illustrated in the decomposition below, frontier analysis not only captures the effects of technological change, but also measures efficiency—which in turn can be decomposed into technical efficiency and allocative efficiency. This breakdown is a welcome addition to, for instance, the measure of total factor productivity (TFP), which in historical studies is

often quite large and difficult to attribute to specific causes. Secondly, frontier analysis tends to require less stringent assumptions. It can incorporate variable returns to scale, deal with market imperfections, allow for multiple inputs *and* outputs and, most helpfully, the functional form of the production function or the output elasticities need not be known in advance. Thirdly, frontier analysis has so far been underutilised in economic history, a vacuum which begs to be filled. Students looking for a suitable thesis topic can draw inspiration from the myriad of economic history studies that tackle engrossing research questions, but rely on outdated methods. The data required for frontier analysis are generally no different from standard productivity studies and thus widely available, be it total economy data on production, labour, and capital or a panel of firm-level inputs and outputs.

For educators, frontier analysis is an appealing subject to teach because of its interdisciplinary origins. As it has been applied extensively in both contemporary economics as well as in operations research and management studies, it will appeal to both business and economics students. Most students will also be familiar with the basic maths underlying frontier analysis; both microeconomics and operations research cover optimisation techniques required to estimate distance functions. In addition, the parametric econometric techniques taught in macroeconomics will make it easier to get started with, for instance, Stochastic Frontier Analysis (SFA).

Frontier analysis is not without weaknesses, however, and it has garnered its share of detractors over time (Färe et al. 1994: 13).¹ Still, most of these issues can be addressed and, as frontier analysis methods have advanced, papers in economics and business studies applying them have soared (see, e.g. Emrouznejad and Yang 2017). Surprisingly, it has not yet caught on in the economic history literature, despite its obvious historical applications. This chapter helps students of economic history contribute by providing a brief introduction to the basic methodology, reviewing some relevant examples of frontier analyses in economic history, and listing resources that will help students to get started with their productivity analyses.

Introduction to Frontier Analysis

The study of efficiency using frontier analysis is ubiquitous, covering ‘virtually every country and [...] every conceivable market or nonmarket production activity’ (Färe et al. 1994: 2). The development of frontier analysis in the

¹ Reluctance to adopting linear programming techniques, which underlie frontier analysis, mostly concerns the fact that no account is taken of noise or measurement error in the data and that it may be sensitive to outliers.

(macro) economics literature and the micro-oriented business and operations research literature has been quite distinct, however. To illustrate frontier analysis, this chapter provides a brief introduction of the main non-parametric technique, Data Envelopment Analysis (DEA). This technique is more frequently used in business studies and tends to be the most accessible and the easiest to visualise. Its parametric counterpart, SFA, is not discussed here, but the “getting started” section below provides plenty of reference material on this subject.

The purpose of a DEA, or any type of frontier analysis for that matter, is to estimate a global production frontier which represents the various “best-practice” production techniques observed for the entire feasible range of input combinations. By tightly enveloping data points with linear segments using mathematical programming methods, the structure of the frontier can be revealed. The global production frontier lends itself more readily to the decomposition of productivity growth as, in contrast to traditional growth-accounting exercises, it distinguishes between both the effects of (global) technological change, and relative efficiency change (Färe et al. 1994: 12–13). Efficiency turns out to be a crucial factor in explaining the differences in firms’ or countries’ growth trajectories, as well as in the cross-country divergence of income levels.

Figure 48.1 depicts a basic example of a DEA involving three producers (A, B, and C) which use two inputs to produce a single output. Assuming constant returns to scale, the global production frontier can be presented in

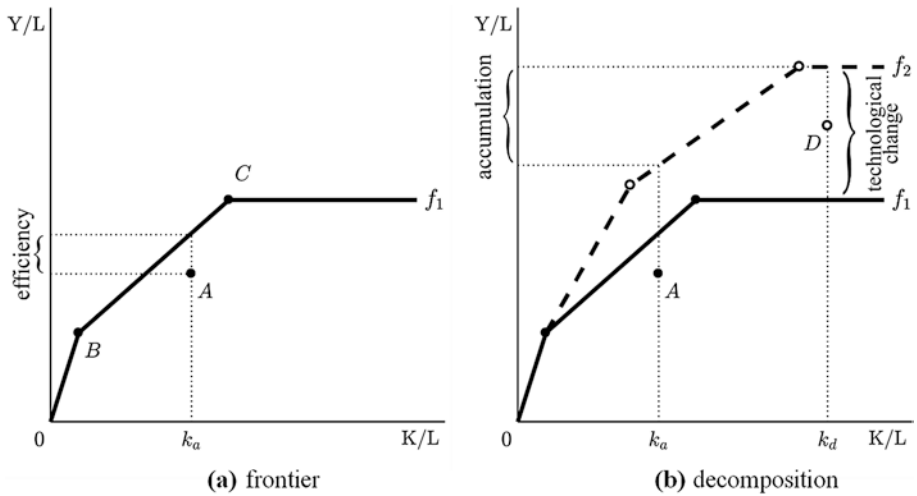


Fig. 48.1 Illustration of data envelopment and growth decomposition: (a) frontier; (b) decomposition

$\langle k, y \rangle$ space, where y is labour productivity (Y/L) and k is capital intensity (K/L). The frontier (f_i) for the observations, depicted by the solid line in panel (a), is formed as linear combinations of observed “best-practice” activities (Salter 1966). As noted in the introduction, an observation is best practice if increasing any output or decreasing any input is possible only by decreasing some other output or increasing some other input (Koopmans 1951). In this example, only B and C are classified as best-practice techniques. As shown by Färe et al. (1994: 68–69), the identification of these fully efficient observations can be reduced to a basic linear programming problem in the form of a distance function.

More intuitively, producers B and C are both fully efficient. Producer C is more productive (higher Y/L) than B, however, because he applies more capital-intensive production methods (higher K/L). The frontier itself is a subset of all feasible techniques that attain the highest labour productivity for the capital intensity levels they correspond to (Timmer and Los 2005). Panel (a) also shows that the last remaining observation (A) is located below the frontier. Observation A’s vertical distance to the frontier indicates the potential for labour productivity increase through more efficiently using the production factors at hand. This distance can thus be interpreted as a measure of technical efficiency.

The frontier approach can be used in a decomposition of TFP, a process described by Kumar and Russell (2002: 528–529) as ‘growth accounting with a twist’. The example in panel (a) is expanded to include a second period. Panel (b) now includes six observations and shows two frontiers and two inefficient observations (A and D)—which represent the same producer at time 1 and 2, respectively. Labour productivity change, between A and D, can be decomposed into a change in *efficiency*, *capital accumulation*, and *technological change*. The change in *efficiency* is captured by the difference in D’s vertical distance to frontier 2 compared to A’s distance to frontier 1. *Capital accumulation* captures the potential change in labour productivity resulting from a shift in the capital-labour ratio. This component represents the average productivity gains or losses as a result of the movement from k_a to k_d along the frontier. Finally, *technological change* measures the increase in labour productivity as a result of a shift in the frontier. In panel (b), this is captured by the vertical distance between frontier 1 and 2 at D’s capital intensity level.²

² For more details on the growth decomposition, see Kumar and Russell (2002) and Timmer et al. (2016).

Examples in Macroeconomic History

As with frontier analysis, central to the traditional productivity analysis in economics is how to describe the relationship between inputs and outputs in the aggregate production function. In economic history, this relationship is still often captured using a standard Cobb-Douglas production function. TFP is generally interpreted as a measure of technology, summarising how intensively and efficiently inputs are used in production. The typical finding for developed countries is that TFP explains the bulk of growth in output over time. Most famously, Solow (1957) showed that over 80 per cent of US labour productivity growth between 1909 and 1949 came from TFP. Still, even recent studies that employ more encompassing measures of inputs set this share as high as 60 per cent (Bakker et al. 2018). In cross-country studies, we see that TFP is generally much higher in high-income countries (Feenstra et al. 2015).

This leaves open a lot of questions. How does technology change over time? Why are there such striking differences in technology between countries? How should we interpret and measure technology—is it factor neutral or does it augment one production factor in particular, that is, is it biased towards labour or capital (Bernard and Jones 1996: 1043)? The theme of technological change is central to the economic history literature, ranging from the Great Divergence debate, the analysis of General Purpose Technologies, to rising income inequality. Frontier analysis is able to address most, if not all of these questions.

Using the basic techniques of data envelopment, Allen (2012) shows that technological change was decidedly biased towards higher levels of capital intensity between 1820 and 1990. Most technological progress was achieved by rich countries that employed a great deal of physical capital in their production process, shifting the production frontier only locally. Remarkably, developing countries appeared to be no more productive in 1990 than countries with a similar capital to labour ratio in 1820 (Allen 2012: 9–11). In contrast, traditional productivity analysis generally assumes technological change grows uniformly at all levels of capital intensity. As noted by Timmer and Los (2005: 49–50), the assumption that advances in say (capital-intensive) high-speed maglev trains would improve the performance of (capital-extensive) rikshaws by an equal factor is clearly wrong, undermining the credibility of growth models based on a classical Cobb-Douglas production function.

Another example of frontier analysis in economic history is the study by Timmer et al. (2016), who use a DEA growth decomposition to show that the

gap between German and US labour productivity in manufacturing in 1936 was primarily the result of the inefficient assimilation of modern production techniques in Germany. This finding contradicts traditional explanations that blame the labour productivity gap on the use of different technology and factor endowments. In other words, it was not a lack of capital in German production, but the inefficient use of that capital that prevented Germany from catching up to the US prior to the Second World War.

Examples in Microeconomic History

Scholars in operations research, management science, and microeconomics also commonly apply frontier analysis. The goals are very similar: identifying the most effective organisations, providing a single summary measure of relative efficiency, handling multiple or constrained inputs and outputs (thus addressing resource scarcity, environmental regulation), or evaluating qualitative factors like customer satisfaction (Färe et al. 1994: 5–6). Crucially, for historians engaged in business or microeconomic research, frontier analysis is particularly well suited to provide insights into the factors that contribute to relative efficiency.

As an example, Lampe and Sharp (2015) evaluate the performance of Danish dairy farmers during the nineteenth century using SFA. They show that efficiency of production was closely linked to education. In addition, they show that Danish farmers exploited returns to scale, which allowed them to incorporate new technologies in their production process and increase the use of commercial feedstuffs, which both helped to improve dairy production significantly.

Going back even farther in time, McDonald (2010) assessed productive efficiency of eleventh-century English estates using DEA. He shows that efficiency was strongly correlated with the arable/livestock mix and the administrative division of which the estate was part. The estate's tenant-in-chief did not affect efficiency, however, showing that feudal landlords were not conducive to the spread of new technology.

Getting Started

There is a wealth of excellent textbooks on frontier analysis. For undergraduate students or graduate students less familiar with microeconomics and linear programming techniques a good place to start is *An Introduction to Efficiency*

and *Productivity Analysis* by Coelli et al. (2005). More advanced textbooks include *Production Frontiers* by Färe et al. (1994), *Stochastic Frontier Analysis* by Kumbhakar and Lovell (2000), and the more applied *Data Envelopment Analysis* by Zhu (2016).

Contrary to most econometric methods, frontier analysis is generally not a built-in function in many of the commonly applied software packages (SAS, Stata, R, MatLab, Eviews). Fortunately, there is a host of user-built software tools and packages available on the internet that can perform even the most demanding types of frontier analysis for most of these languages. For those just starting with DEA/SFA or less familiar with computing software, excellent stand-alone programmes include the Centre for Efficiency and Productivity Analysis *DEAP* (Data Envelopment Analysis (Computer) Program) and *FRONTIER*. Neither require a software licence and both have outstanding supporting documentation.

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Geospatial Information Systems

Noel D. Johnson

At the 1969 meeting of the International Geographical Union, Swiss geographer Waldo Tobler proposed what has come to be known as the First Law of Geography: ‘everything is related to everything else, but near things are more related than distant things’ (Tobler 1970). While seemingly banal, Tobler’s insight, when combined with modern geospatial information systems (GIS) software and the massive increases in desktop computing power that occurred in the 1990s, has led to a revolution in empirical social science. Its impact within economics has been influential in many of the more fundamentally spatial subfields, such as urban and trade. However, arguably no field has been more affected by the GIS Revolution than economic history. This influence has manifested itself in two ways: by opening up new sources of data, and by allowing for new avenues of analysis.

GIS and New Sources of Data

The combination of GIS software with powerful desktop computing has allowed economic historians to access and create both more and, in many cases, better data. These new sources of data have tended to be of two different types. First, GIS allows modern data sets to be effectively matched with historical units of analysis. One situation in which such matching can be appropriate is when the spatial variable of interest is relatively time-invariant and thus can be legitimately used to explain events in the past. For example, Nunn and Qian (2011) employ a modern measure of soil’s suitability to grow

potatoes from the Food and Agriculture Organization (FAO) in order to evaluate the historical hypothesis—due originally to Crosby (2003) and others—that the introduction of the potato to the Old World after opening of the Columbian Exchange led to historical population growth. Nunn and Qian effectively leverage the highly disaggregated nature of the FAO data—it covers roughly the entire world at a resolution of 0.5 degrees by 0.5 degrees (about 56 by 56 kilometres at the equator)—in order to make their argument. In addition to studies using modern soil suitability measures (e.g. Michalopoulos et al. 2016), other examples of modern GIS data that have been effectively matched to historical sources include measures of topography (Nunn and Puga 2012) and climate (Anderson et al. 2017; Fenske and Kala 2015; Grosfeld et al. 2017).

Another situation in which modern GIS measures have been effectively exploited is in studying the very long-run effect of some historical variable on contemporary outcomes. For example, Michalopoulos and Papaioannou (2013) are interested in the question—‘what was the effect of pre-colonial state capacity in Africa on current economic development?’ A major obstacle to studying this question is that pre-colonial political boundaries in Africa differed a great deal from their present configuration. To solve this problem, Michalopoulos and Papaioannou exploit modern data on night light intensity captured from space using satellite observations. Since these night light data are both highly spatially disaggregated and correlated with economic activity, they can be used to test the authors’ hypotheses. Other prominent examples of so-called “persistence papers” that use modern GIS data include Alesina et al. (2013), who look at how a region’s soil suitability for either hoe or plough agriculture has an impact on present-day gender preferences, and Galor and Ozak (2016), who look at a region’s soil suitability for certain crops and measures of present-day time preferences.

A second major innovation in data creation brought about by GIS has been to facilitate the use of existing historical datasets in a manner that allows for empirical analysis. For example, data on plague recurrence in Europe from Biraben (1975) have been matched to cities by Dittmar and Meisenzahl (2017), and historical maps of rail networks have been used to test theories of urban agglomeration and trade (e.g., Donaldson and Hornbeck 2016; Donaldson 2018; Jedwab et al. 2017b). One effect of the ability to map historical data using GIS has been a general shift towards disaggregation. Whereas the natural unit of analysis used to be nations or states, in the last decade there has been a wave of papers focusing on cities (e.g., Bosker et al. 2013; Jha 2013; Dinicco and Onorato 2016). One notable early example of this approach is Dittmar (2011), who matches a database of the locations

of early printing presses with historical city growth to investigate the effect of print technology on economic development.

To illustrate how GIS technology can be leveraged to analyse existing historical databases, consider recent work on the causes of witchcraft trials in Europe. For decades, historians have been compiling lists on where and when individuals were tried as witches in early-modern Europe. Despite the fact that many historians proposed that witch trial frequency was associated with political fragmentation, there was not much that could be done in the way of consistent empirical work with these data until the advent of GIS. Consider Fig. 49.1 which is adapted from Johnson and Koyama (2019) and maps out witch trial locations between 1300 and 1850 compiled by Leeson and Russ

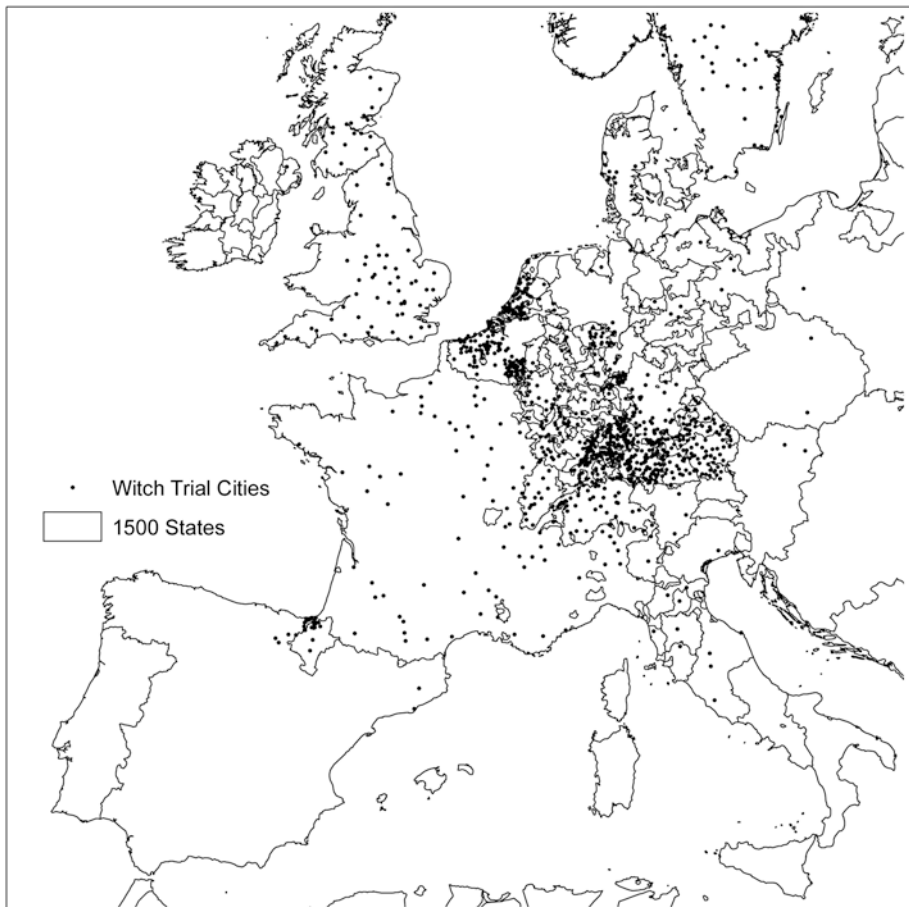


Fig. 49.1 Locations of witch trials and executions, 1300–1850 and political borders in 1500

(2017) as well as political boundaries in Europe in 1500. These trials were collected from numerous secondary sources, but only when they have been geocoded and mapped, do obvious patterns begin to emerge. It is clear, for example, that the majority of the trials occurred in regions that were politically fractionalised (i.e., states that were small). It should also be noted that an additional benefit of mapping the historical data is that shortcomings or important caveats in any hypotheses are also easier to spot. For example, in Fig. 49.1, there are hardly any trials in the Iberian Peninsula, a strong clue that the dynamic of persecution of heretical belief was different there than in other parts of Europe.

GIS and New Avenues of Analysis

The GIS Revolution has not just been about the creation of new datasets. It has also introduced new options for the analysis of data. Many of these methods involve exploiting Tobler's First Law. For example, if things that are close to each other spatially are more related, then it is often possible to extrapolate the unobserved characteristics of a nearby object based on the observed characteristics of its neighbours. For example, Jedwab et al. (2017a) observe Black Death mortality rates for a subset of cities in Europe between 1347 and 1352 but wish to predict the impact of the disease on the likelihood that Jews are persecuted in a non-overlapping set of cities. They thus use spatial interpolation techniques to predict the mortality rate for all cities based on their nearest neighbours.

Another way in which Tobler's First Law has been used is in the analysis of spatial networks. GIS technology has facilitated these approaches in that it allows for the calculation of the least-cost travel paths between locations. As an illustration, consider Fig. 49.2. Adapted from Johnson and Koyama (2019), it illustrates the network of four different travel technologies in Europe during the early-modern period: seas, rivers, roads, and "none" or portage. Using historical sources, it is possible to attach costs of travelling along these different routes. Then, one can apply an algorithm which determines what the least-cost path is between two locations on the map as well as the cumulative cost of taking this path. In Fig. 49.2, the least-cost path between London and Rome is illustrated as an example. "Optimal path" measures such as these have played a prominent role in recent research. For example, Dell (2015) studies how Mexican drug trading networks reconfigured themselves after enforcement cut off certain routes.



Fig. 49.2 Travel technologies and the least-cost path between London and Rome. Darker colours correspond to higher cost technologies

Another highly fruitful area of research has been to investigate the effect of historical changes in infrastructure on market access. Market access is a measure of a location's potential for trade that combines information on surrounding market sizes (usually populations) with the least-cost travel path of reaching those markets. A major benefit of doing analysis in terms of market access is that trade theorists have shown that under a set of assumptions it is a sufficient statistic to capture all of the direct and indirect impact of changing costs on trade. Recent work focusing on market access includes Donaldson (2018), who looks at the effect of railroad construction in nineteenth-century India, and Donaldson and Hornbeck (2016), who look at railroad construction in the US.

Tobler's Law has also been exploited in the creation of instrumental variables. An example of this in economic history is Dittmar (2011), who uses the

distance to Mainz—the origin city of Johannes Gutenberg’s printing press—as an instrument for printing press presence in other European cities (see also Becker and Woessmann 2009). Of course, a major concern with using geographic distances as instrumental variables is that Tobler’s Law might hold for more than a single characteristic, thus invalidating the exclusion restriction. One way researchers have attempted to minimise this possibility is by testing placebo locations (e.g. does Wittenberg also explain printing press presence?). Another, more recent innovation, is to exploit both “push” in addition to “pull” forces in the instrument. An example of this is found in Johnson and Koyama (2017). They are interested in predicting whether a city’s population growth is affected by the presence of a Jewish community between 1400 and 1850. As an instrument for Jewish presence in a city, they exploit the fact that Jewish communities tended to locate near each other for historical reasons. In order to satisfy the exclusion restriction, they construct their instrument for city j as a variable measuring whether any city further than 100 kilometres away recently expelled its Jewish community (a “push”) with a measure of how close that distant city is to j (the “pull”).

One final way that GIS has played a major role in analysis is by exploiting failures in Tobler’s First Law. Regression Discontinuity (RD) approaches have played a major role in what has become known as the “credibility revolution” in economics. Only relatively recently have spatial RD’s been exploited. This is true especially in historical contexts due to the fact that political boundaries have often arisen for random reasons—for example, the front line of a battle may have been cemented by a peace treaty negotiated far away, colonial cartographers liked to draw in straight lines, or perhaps a relatively innocuous landmark was used to anchor a border. An early example of spatial RD is Dell’s (2010) study of the effect of the Peruvian mining Mita on present-day consumption and stunting.

Getting Started with GIS

Given the benefits of using GIS, it should be in the toolkit of all economic historians. Unfortunately, the barriers to entry into the GIS world can be significant. Among these barriers is the fact that the most common programme used for GIS is the Arc Suite developed and licensed for a substantial fee by the ESRI corporation. While most research institutions maintain licences for on-sight use of the programme, many faculty and graduate students will still have trouble gaining access. Alternatives include the open-source programme QGIS, as well as the statistical package R—though the learning curve for the latter is steeper. If one has access to the Arc Suite, then a practical and free guide to the basics of using the programme is Dell (2009).

By facilitating the creation of new data, allowing for the exploitation of existing data, and generating new empirical approaches, GIS has opened new approaches in economic history. Many questions asked by economic historians, even if not primarily spatial in nature, can benefit from taking spatial issues into account. As more maps are digitised and databases geocoded, the library of resources at the command of the researcher will certainly make this claim even more true.

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Network Analysis

Gabriel Geisler Mesevage

Economic historians have long been fascinated by the web of linkages formed by the interaction and communication of individuals and firms, and this preoccupation has provoked continued interest in networks. From family ties to trade to technological diffusion, network structures are ubiquitous in economic interactions and thus have garnered corresponding attention from economic historians. ‘The networks of trade encircled the world’, declared Braudel in 1982 while describing the ties of kinship and correspondence that underpinned pre-nineteenth-century international trade (1982: 149 and 153–154). However, despite long-standing interest in network structures, the formal analysis of those structures—using the methods of social network analysis as they have been developed in the social sciences, in computer science, and in physics—is of relatively recent vintage within economic history.¹

Network analysis in economics has recently received several textbook treatments (Jackson 2010; Goyal 2012). Much of the early interest in networks in economics has been theoretical, using network structures as arenas within which games unfold, or else using the resulting network as the product of strategic decisions by agents. However, the growing availability of network data has stimulated recent work in economics on the econometrics of networks (for a review, see Boucher and Fortin 2016). The growth in methods of

¹ The interdisciplinary development of network analysis is surveyed in Newman (2010). As with other areas of the social sciences, formal analysis of networks has benefited from the increased power of personal computing, which has greatly reduced the computational burden of visualising and analysing social networks.

econometric analysis of network data presents an opportunity for applied researchers to explore the importance of networks to their areas of expertise.

In this chapter, I develop the notation sufficient to understand a discussion of networks and review two analytical approaches to network structures. I then survey some of the areas in which economic historians have deployed social network analysis. Finally, I touch on the problem of measurement error in network analysis—as this problem may prove particularly thorny with incomplete historical data.

A Sparse Set of Notation

A network consists of vertices and edges (interchangeably referred to as nodes and links). I refer to the set of nodes as V with individual elements v_i and the set of edges as E with individual elements e_{ij} . If e_{ij} is in E , that implies that vertex v_i is connected to vertex v_j .

Edges can be directed ($e_{ij} \neq e_{ji}$), or there may be edges of multiple types. Vertices—are typically individuals, firms, or polities, and may have “vertex-level” attributes, such as age, industry, or government type. The entirety of the network structure can then be compactly represented by a matrix, and this matrix yields the corresponding network representation (see Fig. 50.1).

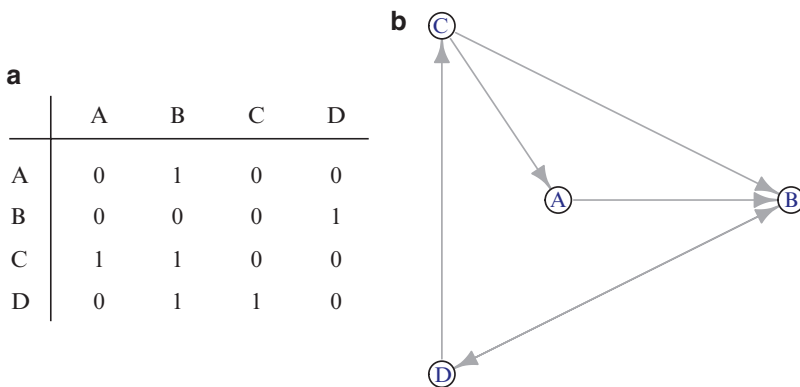


Fig. 50.1 (a) Network adjacency matrix; (b) network graph

From this basic structure, interest in the use of network analysis has tended to come in two flavours: an interest in the network itself—often its evolution over time or an interest in an outcome observed at the vertex level, which may be affected by the placement of those vertices within the network. Studies in

which the unit of observation is the network tend to be descriptive, such as Haggerty and Haggerty's work on business networks in Liverpool (2011, 2015). These descriptive studies usually focus on network-level properties, such as density,² and the analysis of network properties can be fruitfully applied to studying the evolution of a network over time (Erikson and Bearman 2006) or the comparison of networks between locations as in Musacchio and Read's (2007) comparison of corporate interlinkages through elite business networks in Brazil and Mexico.

Studies that focus on the *effects* of networks are rarer, as strategic network formation impedes the clear estimation of network effects. A typical "network-effect" type study might take the form of a regression in which the units of observation are nodes, and the effect of interest is a function of the network. For instance, we might seek to relate the failure (Y_i) of some bank i to its own characteristics and the failure of its counterparties in the interbank market—where connections between banks are described by a network G .³ Thus we might estimate a regression of the form:

$$Y = \alpha + \rho \bar{g}(Y, G) + X\beta + \epsilon,$$

where $\bar{g}(Y, G)$ is a function that computes the average failure rate among all the neighbours of each node in G . This type of regression model—sometimes referred to as a "peer-effects model" as interest centres on the influence of one's peer group on one's outcome—is sensible in theory, but its application demands caution for at least two reasons.

First, there are several different ways in which network effects could be present, and in certain cases, the researcher will be unable to solve for a unique value of these effects or to distinguish them—what Manski denoted the "reflection problem" (1993). Recent theoretical work in econometrics has greatly improved our ability to identify unique solutions (Bramoullé et al. 2009; De Giorgi et al. 2010), and the methods necessary to compute them under certain assumptions (Kelejian and Prucha 1998, 2010), but ensuring that the model can be estimated remains a necessary starting point for the applied researcher.

Second, and more insistently, all network effects—whether estimated or simply argued for—suffer from the difficulty of distinguishing between "homophily" and "contagion" when working with observational data (Shalizi

²Density is the number of observed connections over the number of possible connections.

³There are many studies of financial contagion networks (Mitchener and Richardson 2016; Calomiris and Carlson 2017); for a review see Summer (2013).

and Thomas 2011). We are typically interested in contagion, meaning that when something happens to a node (a bank defaults) we want to know whether that event will spread to that node's neighbours (cascading defaults). But it is often just as likely that nodes tend to be neighbours when they share underlying characteristics (homophily). For instance, suppose that banks sort on quality in the interbank market, with good banks lending to good banks and with bad banks lending to bad banks. If bad banks are more likely to default, we will observe that any given bank's probability of default is correlated with the average default probability of its neighbours—but this may simply reflect the greater propensity for bad banks to be neighbours. Clearly, this is analogous to an “omitted variables” problem in a linear regression, and therefore “knowing more” about each node can help to mitigate the problem. The gold standard for inference would be the random assignment of neighbours—an impractical standard for an economic historian. However, as in other applied research areas, detailed institutional knowledge can generate ideas about whether and when these models can be fruitfully deployed.

It is worth remarking that whatever the difficulties of estimating network effects, a failure to account for network effects in a regression analysis can be just as problematic. If interest focuses on some characteristic of the nodes, and the researcher deploys, for instance, a linear regression, the fact that the outcomes of some observed units will be correlated with that of their neighbours will result in a violation of the assumption that observations are independent. Viewed in this way, a social network is a dependency structure in the applied economist's data set, and even if it is not of primary interest, it must be dealt with for statistical inference to proceed.

Applications in Economic History

The complexity of estimating network models—as well as the relatively recent development of easy computer methods to do so—has meant that most work on networks in economic history is either descriptive or comparative across networks. Business historians have proven some of the most enthusiastic consumers of networks,⁴ stemming no doubt from the obvious appeal of understanding the associates and backers of entrepreneurs (Buchnea 2014; Haggerty and Haggerty 2011, 2015; Musacchio and Read 2007). The suitability of network methods to business historians are strengthened by the obvious manner in which interlocking directorates

⁴ See, for instance, the collection of studies in Gestrich and Beerbühl (2011).

create networks between companies (e.g. Lluch 2014). In a similar vein, links between banks and firms—a long-standing preoccupation of economic historians (e.g. Fohlin 1998; Colvin et al. 2015)—also create natural network structures, and there is much interesting work to be done re-examining hypotheses from business and financial history with a more explicit attention to the underlying network structure.

A weakness of the business history approach is that it is frequently methodologically constrained to depart from the point of view of one or several nodes. For instance, exchanges of letters between businesses typically derive from the archives of some business, and (as we will see in the following section) this “incompleteness” can lead to largely erroneous conclusions if the researcher is careless. Hancock (2005) provides a critical view from within the business history literature.

Many studies of networks depart from the view that position within a network can be a source of power or influence. This point of view has been strongly influenced by historical sociologists, with the classic paper in this tradition written by Padgett and Ansell (1993). In this study, the authors theorise success within Renaissance politics as a function of structural position within the networks that tied together leading Florentine families (see also McLean and Padgett 1997). Political networks remain an area of active research by economic historians (Esteves and Geisler Mesevage 2017), sociologists (Hillmann 2008), and political scientists (Lazer 2011; Ward et al. 2011).

An interest in networks as structure has extended to the international financial system. In a 2005 study, Flandreau and Jobst analysed the network formed by quotations of international currencies (2005). They examined the structure of the resulting inter-currency network to evaluate the idea of a “core and periphery” in the international financial system, finding that the best-fitting models imply currencies clustering into three groups representing centre, intermediary, and periphery.

Another naturally occurring network structure in international economic relations arises from trade treaties that form connections between countries. Lampe (2011) studied the determinants of the network formed by the Cobden-Chevalier treaties of the 1860s and 1870s. Models of strategic network formation have garnered attention recently in economics, and strategic networks such as trade agreements represent a natural area of application (for strategic network formation see the reviews in Jackson 2016).

Networks have also been fruitfully deployed to understand historical diffusion processes. For instance, in a review essay on the economic history of the Protestant Reformation, Becker, Pfaff, and Rubin highlight both the central contributions to reformation history that have been achieved with network methods (for instance, Nexon 2009) and the value of this approach for future work as ‘social networks should influence the diffusion of ideas and institutions through mechanisms such as information flows, the exercise of influence, and the capacity of groups to coordinate’ (2016: 22).

Challenges and Opportunities

It is well known that measurement error can be particularly pernicious in a network context. Thus, missing nodes or edges can result in severe bias in the estimation of quantities such as centrality (Wang et al. 2012). This problem can be ameliorated but not necessarily resolved with sampled network data—collecting information on some but not all nodes can still generate biases depending on what one is measuring.⁵ Historical data are often limited in their availability, and the inevitable elisions of the archive can result in a biased or incomplete network that could potentially confound inference.

A related problem frequently arises in the researchers’ choices as to how to measure and encode the relations between nodes. In some applications what constitutes a “connection” between individuals can be reassuringly discrete and simple to measure—such as, for instance, shared membership in a political party. In other settings, however, a connection can be more a matter of degree than discrete. For instance, we might imagine the network as being formed by the imports and exports of countries. In this setting, almost all countries might be “connected”, although in certain instances, the size of the trade flow might be negligible. This can be encoded as a weighted network (with real values populating the cells of the adjacency matrix), but the density of connections can present special problems for the estimation of network effects (Mizruchi and Neuman 2008). In the spatial econometrics literature, this problem is occasionally treated by discretising the adjacency matrix—picking a threshold below which connections are coded zero and above one, but this decision requires justification by the author.⁶

⁵ See the comprehensive survey on this topic by Chandrasekhar and Lewis (2016).

⁶ The review of spatial econometric models in Vega and Elhorst (2015) provides a useful discussion of this issue.

I have already addressed the importance of a clear understanding of the process of network formation in order to speak meaningfully of the effect of a network. As a result, the advantages of a historical approach are as apparent here as they are in any quantitative context in which the researcher seeks to identify an effect: concrete historical knowledge of the underlying process can allow researchers to credibly identify instruments or natural experiments that permit identification.

Often, however, this stricture may be unsatisfactory, for example, when we would like to explain the history of some event and no clever quasi-experiment presents itself. The importance of networks, however, cannot be confined to the clever identification of a network effect; network analysis should be central to applied work in economic history because *networks* are ubiquitous. And when the individuals, firms, or countries that constitute the subjects of analysis are linked via a network structure, a failure to model that network explicitly will always result in biased and unreliable conclusions. For this reason, if no other, future work in economic history needs to engage more explicitly with network analysis.

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Chapter Abstracts

1. Introduction, or Why We Started This Project

Matthias Blum and Christopher L. Colvin

This introductory chapter, written by the editors of this book, provides a personal account of the origins of this unique teaching and learning resource. Part of its motivation lies in the paradox that economic historians are perceived to be an endangered species at the exact moment there is newfound demand for their services. The other is the active economic history scene that is populated by researchers with diverse academic backgrounds, exemplified by the contributors to this book.

JEL Classification: A11, A21, A22, N01.

2. Economics Versus History

Christopher L. Colvin and Homer Wagenaar

Economic history is an interdisciplinary field that fuses economics with history, two disciplines that often misunderstand one another. This chapter bridges these two disciplines by discussing archetypical approaches and research strategies in each. The authors contrast the differences between deductive, inductive and abductive reasoning in scholarly enquiry. They conclude with a call for consideration to (historical) context when conducting research in economics and economic history.

JEL Classification: A11, A12, A22, A23, B41, N01.

3. Economics, Economic History and Historical Data

Vincent J. Geloso

There has been no progress in the modest interest in economic history in the top five economics journals since the 1970s. This is despite an apparent newfound enthusiasm for the field among economists. This chapter explains this contradiction by highlighting the differences between research in economic history and economics that exploits historical data. In so doing, the author defines what, in his view, constitutes good economic history research.

JEL Classification: A11, A12, N01.

4. Economic Theory and Economic History

Robert P. Gilles

This chapter argues that the dismal science is in a dismal state. The problem is caused by the existence of very difficult methodologies, complicating the relationship between economic theorising and empirics. As part of a possible remedy, the author discusses how economic theorising could relate more productively to (economic) history. While analytic narratives can be used to explain the particular economic aspects of a historical episode, history can also provide insights about human economic endeavours that help formulate better general theories.

JEL Classification: B41, D85, L14, N01.

5. Economic History and the Policymaker

Tim Leunig

Politicians, and the officials who serve them, live in the present. But making good policy requires an understanding of the past. History is important when making economic policy, because economics has many regularities that can be understood, and which yield useful predictions. This chapter, written by a senior policy advisor to the UK government, argues that historical knowledge is of particular importance to prepare for large and uncommon events, and to assess whether or not “this time is different”.

JEL Classification: A22, F13, N40, N70.

6. Economic History, the History of Economic Thought and Economic Policy

Graham Brownlow

This chapter discusses the interaction between contemporary economic policy, the history of economic thought and economic history. Reflecting the works of Cairncross, Eichengreen, and Offer and Söderberg, the author argues that students suffer from a reduction in their economic literacy as a result of a curriculum that marginalises the past. This failure has wider social costs and may result in inferior policymaking. The chapter concludes by advocating a shift in economics towards a more policy-driven direction.

JEL Classification: B21, B41, N01.

7. Teaching Economics with Economic History

Matthias Flückiger

Economic historians speak the same language as economists. They share a common disciplinary core, and they employ similar research approaches to address related questions. The contribution of this chapter is to highlight how the similarities between economic history and other fields in economics can be used to enrich existing teaching materials and pedagogical concepts. Its author presents teaching ideas that use the material collected in this book.

JEL Classification: A22, A23, N00.

8. Money and Central Banking

John D. Turner

This chapter asks why money and central banks exist. Its author makes a plea for a historical approach to monetary economics. Students need to understand the evolution of money and banking so they understand why our modern monetary systems are designed the way they are. The chapter concludes that money and central banking is not a purely technocratic issue; from their inception, sovereigns have been involved. Only through economic history can scholars understand the political economy of our mediums of exchange.

JEL Classification: E42, E58, N10.

9. Globalisation and Trade

Alan de Bromhead

Trade policy has returned as an important issue in domestic politics around the world. This chapter provides a historical perspective on globalisation and trade, including views on its political economy, and its impact on well-being and economic development. It focuses on how history can help economists to address a number of important contemporary questions in international trade policy.

JEL Classification: F13, F60, N70.

10. Immigration and Labour Markets

Sebastian T. Braun

The increased importance of migration in many developed countries has sparked heated policy debates on its economic effects on immigrant-receiving countries. These debates, along with the increased demographic importance of migrants, have also fuelled academic interest. This chapter summarizes the core topics and debates in immigration economics, and then discusses how economic history informs them.

JEL Classification: F22, N70, O15.

11. Financial Institutions and Markets

Meeghan Rogers

The financial services industry plays a major role in the economy by making it possible for households, companies and governments to access capital. Examining the characteristics and behaviours of financial institutions and markets in the past aids in understanding contemporary market changes, informs corporate policies and leads to better regulations. Long-run and historical analysis can also provide insights into a wide range of modern financial puzzles.

JEL Classification: G12, G21, G32, N20.

12. Financial Crises and Bubbles

William Quinn

Since financial bubbles and crises are rare events, modern empirical evidence on these phenomena is very limited. Economic history has therefore been fundamental to the development of this field. Histories of financial crises traditionally address questions about individual incidents. Enquiry into the Great Depression has proved especially popular. This chapter explains how the traditional approach is now complemented by studies applying econometric techniques to long-run data.

JEL Classification: G01, G18, G21, G41, N20.

13. Sovereign Debt and State Financing

Larry D. Neal

The management of the national debt does not correspond to the rules for management of the personal debt of individuals, households, or business enterprises. This chapter argues that economic history is of vital importance in elucidating this difference. Economic historians explore past episodes of unusually large expansions of sovereign debt to uncover the economic consequences when taxes are raised or not, and when the money supply is increased or not. This chapter uncovers the economics of sovereign debt by surveying academic debates on a selection of such historical events, including the Mississippi and South Sea bubbles and the sovereign default of Spain.

JEL Classification: H50, H63, N40.

14. Health and Development

Vellore Arthi

Issues related to human and health capital formation are of particular importance because interventions on this front can have a direct impact on living standards, well-being, and economic development. Historical evidence can be especially valuable to understanding how human and health capital is formed, and how it contributes to development. Issues discussed in this chapter include water, sanitation and hygiene, maternal and child health, and climate and the environment.

JEL Classification: I15, J24, O15, N50.

15. Education and Human Capital

Sascha O. Becker

In modern economies, more educated people typically earn more, live healthier lives, are less likely to be divorced, are more future-oriented, less likely to have children while teenagers and less likely to be ever arrested. This chapter discusses some of the drivers of education, its relationship to culture and virtues as well as its impact on demography and economic development. Economic history is presented by its author as a means of answering the question of causality.

JEL Classification: I21, I25, I26, J24, N30.

16. Famine and Disease

Guido Alfani and Cormac Ó Gráda

The infrequency of severe mortality crises and the low prevalence of famine and disease are characteristics of modern industrial and post-industrial societies. Understanding the processes leading to their decline, and the associated improvements in living standards and life expectancy, is a precondition for knowing what is needed to prevent their re-emergence. This chapter provides a historical perspective on famine and disease to help overcome the lack of knowledge on these rare events.

JEL Classification: I14, I15, I18, N30.

17. Women and Children

Jane Humphries

Feminist historians have long complained that women and children have been written out of mainstream history. Similar concerns on the absence of women are now being expressed by economists. This chapter introduces some of the recent economic history literature that addresses this gap. Key aspects discussed include workplaces and wages of children and women, and their roles in the context of economic development.

JEL Classification: J13, J16, N30.

18. Slavery and Discrimination

Richard H. Steckel

This chapter argues that modern-day discrimination against African Americans had roots in the racism that surrounded slavery in the United States. Deprivation suffered by slaves permanently stunted their physical and cognitive development. However, the generation of African Americans born after slavery had much improved cognitive abilities. The author argues that discrimination subsequent to abolition was a reaction to the arrival and maturation of this new generation, which challenged whites in the economic and social realms.

JEL Classification: J15, J71, N30.

19. Crime and Violence

Rowena Gray

Crime is a relatively recent concept, a construction of an era that focuses more on the legality of a person's actions than their morality. This chapter illustrates how economic history can help gain a new perspective on crime, and how crime and violence can be studied by looking at historical policy experiments and their consequences.

JEL Classification: J15, K42, N40.

20. Business Ownership and Organisation

Michael Aldous

Understanding the relationship between the way business is owned and organised is crucial due to its effect on the performance of firms and economies. Optimising the form of ownership and organisation can increase the scale and scope of operations, while improving productivity, efficiency and the capacity for innovation. This chapter discusses the importance of the corporation, or the joint-stock company, in historical perspective.

JEL Classification: D82, G34, L20, L21, L24, N50, N70, N80.

21. Competition and Collusion

Alexander Donges

The preservation of competitive markets is a major cornerstone of modern economic policy. Cartels and anti-competitive practices are restricted, and competition authorities control mergers to avoid the excessive accumulation of market power. This chapter demonstrates that historical evidence is important to understand why cartels arise and how they work. It argues that the study of historical cartels from periods before the introduction of effective competition laws is necessary to understand how competition affects innovation and growth in the long run.

JEL Classification: L40, L41, M21, N10, N40.

22. Human Resources and Incentive Contracts

Andrew Seltzer

Personnel economics is the study of contracts between workers and firms. The principal (the employer) hires an agent (the worker) to perform a series of tasks. However, the principal and agent share neither the same underlying objectives nor information sets. This chapter illustrates how research in economic history can help to address challenges relating the employment relationship, including hiring, training, job assignments, and compensation.

JEL Classification: M51, M52, M55, N30.

23. Global Divergence and Economic Change

Jared Rubin

An explosion in wealth occurred in Western countries in the second half of the second millennium. Meanwhile, the economies of the rest of the world remained relatively stagnant. Understanding the causes of this divergence is one of the great tasks of economic history. This chapter discusses how warfare, institutions, and culture play pivotal roles in recent theories of global divergence.

JEL Classification: O10, O40, O57, N10, N40.

24. Industrial Revolution and British Exceptionalism

Christopher L. Colvin and Alexandra M. de Pleijt

The British Industrial Revolution is probably the most important event of the last 10,000 years. This chapter reviews some of the more recent literature on this topic. The focus is primarily on the timing and location of the Industrial Revolution. Contributing factors discussed include institutions, culture and human capital, factor prices, consumer goods and household work, female agency, finance, trade and geography.

JEL Classification: O10, O40, O57, N10.

25. Innovation and Technical Change

Gerben Bakker

Developed countries would need an economy three to four times their present size to provide the same outputs, but with 1820 technology. This astounding technological advancement demands an explanation. This chapter contrasts three different approaches to the study of innovation across the long run: conceptual, direct and indirect. For each, its author provides important examples from the economics and economic history literatures.

JEL Classification: O14, O31, O34, N70.

26. Culture and Religion

Christopher L. Colvin

Culture is the result of, and is itself expressed through, religion, language, institutions, and history. Culture is persistent but does change slowly over time. Religion is either one manifestation of culture or itself shapes that culture. This chapter discusses recent contributions to the economics of culture and religion, a literature which was instigated by economic historians and has long been dominated by their work. Its author especially focuses on the role of culture and religion in engendering industrial progress and institutional change.

JEL Classification: O43, N10, N30, Z12.

27. Agriculture and Rural Development

Paul R. Sharp

It is only within the last decades that the rural population of the world was overtaken by that of the cities. Thus, any economist working on a topic in economic history needs to take agriculture seriously. This chapter discusses the significance of agriculture since its invention during the Neolithic Revolution, and highlights the role of agriculture in the course of economic development up to the present day.

JEL Classification: Q10, Q12, Q13, Q15, N50.

28. Environment and Natural Resources

Eoin McLaughlin

This chapter provides a long-run perspective on environmental and resource economics. The issues discussed include resource depletion, pollution, climate change and the intergenerational relationship between sustainability and future well-being. Important lessons are drawn by comparing historical settings and contemporary development economics that together demonstrate fundamental environmental challenges.

JEL Classification: Q50, Q51, Q56, N50.

29. Economic Prehistory

Eva Rosenstock

Prehistory encompasses the period from the origins of human culture to the advent of the written record. This chapter discusses why prehistory should be important for economists and economic historians. Its author places an emphasis on hunter-gatherer economies, farming economies, craft specialisation and trade. The chapter concludes with a discussion of how, practically, economics can help scholars to better understand this period.

JEL Classification: N00, N10, N50.

30. The World Wars

Jari Eloranta

The world wars were the most destructive conflicts in human history, both in terms of the economic cost and the human misery they inflicted. This chapter discusses the origins, impact, and outcomes of these conflicts and why they did not occur before or after the twentieth century. Issues discussed broadly relate to defence economics and include economic warfare, mobilisation, demobilisation, resources, and military spending.

JEL Classification: N41, N42, N43, N44, N45.

31. Western Europe

Matthias Blum

A great deal of economic history has been dedicated to questions concerning Western Europe. This chapter recommends a selection of works in this genre. The second part then sets out a pathway towards writing novel contributions to the economic history of this world region. The author advocates that scholars invert the typical approach and hold the rest of the world up as a mirror through which to explain European exceptionalism.

JEL Classification: N01, N13, N14, N23, N24, N33, N34.

32. Central and Eastern Europe

Peter Foldvari

This chapter provides an overview of the economic history of Central and Eastern Europe. The focus is initially placed on the region's first round of modernisation, between 1850 and 1914. Subsequently, the chapter discusses the introduction of socialism and central planning after World War II and its implications for the region's economic growth and development. A key feature explored is that this region is in many respects similar to Western Europe but remains significantly poorer.

JEL Classification: N10, N13, N14, N23, N24.

33. Sub-Saharan Africa

Alexander Moradi

In 2000, sub-Saharan Africa was the poorest region in the world in terms of GDP per capita—and the home of the majority of the world's poor. Issues discussed in this chapter include the roles of geography, institutions and historical legacies, and how these factors resulted in low productivity and low incomes. It includes a devastating criticism of the historical persistence literature. The author concludes by advising new scholars on conducting research on this region.

JEL Classification: N17, N27, N37, N47, N57, N77.

34. South Asia

Tirthankar Roy

This chapter outlines two competing ways of studying the economic history of Pakistan, India, and Bangladesh. One of these starts from the premise that this is a geographically diverse landmass, home to an unequal and hierarchical society, ethnically very mixed, and an area that was never politically integrated until the late nineteenth century. The other says that no matter the diversity, scholars can still find an average to represent the whole region. The discussion concludes with advice on how not to study this world region.

JEL Classification: N01, N15, N35, N95.

35. East Asia

Stephen L. Morgan

During the second half of the twentieth century, world economic activities shifted from the trans-Atlantic world to the economies of East Asia. Demographic factors and human capital were major sources of this growth, which in turn was facilitated by the social capital embedded in the networks of businesses that underpin these economies. These two dimensions of East Asia's past are the focus of this chapter.

JEL Classification: N15, N25, N35, N45, N85.

36. Australasia

Les Oxley

Both Australia and New Zealand share similar historical characteristics, having European, typically British, settler origins. This chapter discusses the two original economic drivers of this world region: agriculture and mining. Differences between Australia and New Zealand discussed here include the enfranchisement of indigenous populations, land use and ownership structures, sources and nature of immigration and the management of natural resources.

JEL Classification: N17, N47, N57, N77.

37. North America

Price V. Fishback

North America is a large continent endowed with large amounts of fertile soil and natural resources. The economic histories of the USA, Canada, and Mexico offer contrasting examples that can illustrate multiple themes in world economic history. In particular, this chapter highlights differences in the institutional structures of these economies and argues that they are key to understanding the different development paths of these three nations.

JEL Classification: N10, N11, N12, N16, N21, N22, N26.

38. Latin America

Leonardo Weller

Like the USA and Canada, Latin American countries were colonies of Western European empires. But despite similar European backgrounds and factor endowments, Latin America remains poorer than its northern neighbours. This chapter discusses the historical, geographic, and climatic conditions that are at the centre of the predominant theories relevant to understanding the divergence between North and Latin America.

JEL Classification: N16, N26, N36, N46, N56.

39. Impact and Communication

Judy Z. Stephenson

Impact is the difference a piece of research makes to the theory and practice of an academic discipline. The potential for impact in economic history is broad because it deals with big questions that are at the forefront of social science and political economy. This chapter sets out how readers can plan their pathways to impact through effective communication.

JEL Classification: A11, A22, A23.

40. Publishing Economic History

William J. Collins

Most scientific texts are written for the purpose of publication, and most authors prefer publication in prestigious peer-reviewed outlets. Written by the editor of a leading journal in economic history, this chapter discusses how a compelling question, clear analysis and description, original and persuasive insights, and efforts to relate the findings to existing and future research form the key elements of successful studies that are published well.

JEL Classification: D85, L14, N01.

41. Archival Evidence

Graham Brownlow

Archival research is required to acquire original source materials, both quantitative and qualitative. This chapter illustrates the value of cultivating a historical temperament in economic research and argues that nurturing archival interpretation is key to developing such a temperament. Examples demonstrate how newly acquired archival materials formed the basis of recently published studies in economic history.

JEL Classification: A10, B41, N01.

42. Case Studies

Abe de Jong and Hugo van Driel

Case study methods are used to research individual units, such as an entrepreneur, a firm, an industry or a country. This chapter introduces three types of case studies: exploratory case studies, unique cases and cases that allow for explicit testing of a theory or hypothesis. The authors illustrate how each of these approaches can be used in economic history research using a series of examples.

JEL Classification: B41, N01, N80.

43. Analytic Narratives

Mark Koyama

Analytic narratives are used to understand historical phenomena where the data required to employ econometrics is lacking. This methodological approach allows for the analysis of a historical setting using economic theory or arguments combined with historical evidence that can be diverse in nature. Analytic narratives offer interesting alternatives to general formal models which may not be suited to understanding a particular case study or historical institution. A series of examples illustrate potential areas of application in economic history.

JEL Classification: A12, B41, B52, N01.

44. Measurement and Metrics

Matthias Blum

Measuring variables that accurately reflect the desired effect is part of designing successful statistical and econometric studies. This chapter discusses challenges related to data compilation and how inadequate proxies, selection bias and measurement error can undermine empirical projects. Two examples illustrate how mismeasurement has been successfully exploited in recent studies in economic history.

JEL Classification: C43, C52, C81, C82, E01, N01.

45. Econometric Identification

Matthias Blum and Arcangelo Dimico

Econometric identification is essential to distinguish cause, effect and correlation in econometric studies. This chapter discusses some of the most common econometric techniques used in economic history today, including a series of examples, areas of application, advantages and caveats. Techniques discussed include ordinary least squares regression, time series analysis, regression discontinuity designs, placebo regressions and instrumental variable approaches.

JEL Classification: C01, C10, C31, C32, C33, C36, N01.

46. Historical National Accounting

Herman J. de Jong and Nuno Palma

Historical national accounting helps to reconstruct productivity and income indicators for past periods. This chapter presents recent results of such a reconstruction for seven world regions. Strategies for both the pre-statistical age and more modern periods, in which national accounting data are more abundant, help to anticipate opportunities and challenges when constructing and interpreting historical income and productivity measures.

JEL Classification: C82, E01, N01, N10.

47. Productivity, Innovation and Social Savings

Gerben Bakker

Since the late 1930s, economists have noted an increase in economic efficiency, leading to higher welfare levels and social savings. Key to understand this development is the quantification of the impact of innovations and new technologies. This chapter discusses growth accounting and allied methodologies and how they may help to quantify innovation-led increases in efficiency and welfare benefits.

JEL Classification: E01, N70, O47.

48. Frontier Analysis

Pieter Woltjer

A great deal of research in economics and business is devoted to the study of the efficiency of individuals, organisations or entire economies. This chapter introduces a family of innovative techniques that help in the analysis of efficiencies by comparing inputs and outputs to estimate an efficiency frontier. A series of examples from both macroeconomic history and microeconomic history illustrate common areas of application.

JEL Classification: C10, C14, D20, C61, N01.

49. Geospatial Information Systems

Noel D. Johnson

Geospatial Information Systems (GIS) allow datasets to be matched with (historical) spatial units of analysis. The visualisation, and subsequent analysis, of geographical patterns that emerge from the use of this methodology constitutes a powerful tool in the toolkit of a researcher. This chapter illustrates some of the main areas of application, such as the visualisation of events and the mapping of distances and travel routes in historical settings. The chapter concludes with tips on how to make first steps towards using GIS methods in economic history research.

JEL Classification: C31, N01, R12.

50. Network Analysis

Gabriel Geisler Mesevage

Network analysis allows for the identification and analysis of linkages formed by the interaction, communication, and trade of individuals, families, and firms, as well as the spread of information and technologies. This chapter develops the notation sufficient to understand a discussion of networks. Its author reviews two analytical approaches to network structures and surveys some of the areas in which economic historians have deployed social network analysis.

JEL Classification: D85, L14, N01.

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