Chapter 7
Complexity and the Future of Economics

7.1 The Evolution of Economics

The neoclassical era in economics has ended. Based on the views presented in this book, I think an argument can be made that it has been replaced by the *the complexity era*[1]. This new era has not arrived through a revolution. Instead, it has evolved out of the many strains of neoclassical work, along with work done by less orthodox mainstream and heterodox economists. It is the wave of the future.

Imagine for a moment that one were looking at the economics profession in England in 1890. One would say that Alfred Marshall, with his blend of historical and analytical economics, was the economics of the future; Walras and Edgeworth, both of whom adopted a more mathematical approach, would be considered minor players. Now fast forward to the 1930s—Marshall is seen as a minor player, while the mathematical approach of Walras and Edgeworth has become the foundation for Samuelson's cutting-edge economics (although Marshall has continued to be cited somewhat since). Now imagine economics in 2050. Much of what is currently done in economics will not be cited or even considered important. Some parts of economics, which today are considered minor, will be seen as the forerunners of what economics will become.

The point of this comparison is to make clear that to judge the relevance of economic contributions one must be forward-looking. One must have a vision of what economics will be in the future, and judge research accordingly. Current journal publication and citation metrics don't do that; they have a status-quo bias because they are backward looking, and thus encourage researchers to continue research methods and approaches of the past, rather than developing approaches of

[1]Regarding the "end of neoclassical economics," see Colander (2000a), with Veblen (1898) coining the term "neoclassical" pejoratively at the same time he argued for economics to adopt an evolutionary approach. For identifying its successor as being the "complexity era" see Holt et al. (2011).

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the future. They are useful, obviously, because they show current activity, but they are only part of the picture. Articles dotting i's and crossing t's, even ones that are cited relatively often in the short term, are far less important than articles that strike out in new directions. These are the ones that will change the direction of economics and be remembered in future history of economic thought texts.

Any literature assessment has to be based on a judgment about the future direction of economics. If one does not, one is, by default, accepting the judgment that the current approach in the profession will continue. But for the future of economics—there will be more acceptance that the economy is complex, and the profession, over time, will adopt certain kinds of technical mathematical, analytical and statistical tools to deal with that complexity. Models based on a priori assumptions will decrease, and be replaced by empirically driven models and assumptions. Behavioral economics will expand; experiments will become part of economist's tool kit, as will complex technical tools such as cluster analysis, ultra metrics, and dimensional analysis. This increasing complexity will be accompanied by a division of labor—theorists and statisticians will be become more and more specialized, but they will be complemented by economists who have a broad overview of where economics is going, and are trained in applying economics. Economics will stop trying to answer grand questions such as whether the market is preferred to command and control, or if the market is efficient, and answer smaller questions such as what market structure will achieve the ends that policy makers are trying to achieve.

Arguably the term "complexity" has been overused and over hyped, so this vision is not of a grand complexity theory that pulls everything together. It is a vision that sees the economy as so complicated that simple analytical models of the aggregate economy—models that can be specified in a set of analytically solvable equations—are not likely to be helpful in understanding many of the issues that economists want to address. Thus, the Walrasian neoclassical vision of a set of solvable equations capturing the full interrelationships of the economy that can be used for planning and analysis is not going to work. Instead, analysis should be based on experimental and empirical data. From there *we build up*, using whatever analytic tools we have available. This is different from the old vision where economists mostly did the opposite—starting at the top with grand mathematical theories of a Bourbakist axiomatic sort, and then working down.

The complexity vision not only connects the various research threads that will be the future of economics; it is also provides the best way to look at the economics profession itself—the economics profession as an evolving complex system that has competing forces operating at all times. It is a profession that can only be understood as a system in constant change and flux.

7.2 More on the Nature of Complexity

Adopting a complexity vision does not require choosing among the many specific definitions of complexity. However, a useful general definition of a complex system comes from Herbert Simon (1962, p. 267):

> Roughly by a complex system I mean one made up of a large number of parts that interact in a non-simple way. In such systems, the whole is more than the sum of the parts, not in an ultimate metaphysical sense, but in the important pragmatic sense that, given the properties of the parts and the laws of their interaction, it is not a trivial matter to infer the properties of the whole. In the face of complexity, an in-principle reductionist may be at the same time a pragmatic holist.

Simon then goes on to emphasize how this definition leads to a focus on the hierarchical structure of systems and emphasizes that he is drawing on older literatures, particularly general systems theory (von Bertalanffy 1974), which he sees as including the work of economist Kenneth Boulding (1978) with cybernetics (Wiener 1948), and information theory (Shannon and Weaver 1949). Of these, cybernetics can be seen as a foundational form of dynamic complexity, while information theory can be seen as a foundational form computational complexity.

The emphasis on the problem of the whole and the parts raises two central issues in economics and for more recent approaches to complexity. One is the problem of the relationship between micro and macro in economics, which calls to mind the old problem of Keynes's "fallacy of composition". Walrasian approaches to macroeconomics have attempted to avoid this problem through the use of representative agent models. Others have proposed dealing with this problem through the invocation of an intermediate zone between the micro and the macro, the "meso," which is seen as crucial to evolutionary dynamics of a complex economy (Ng 1980; Dopfer et al. 2004). Further development of this approach has been due to Potts (2000), Metcalfe and Foster (2004), Dopfer (2005), Shiozawa (2004), Shiozawa et al. (2019), and Rosser Jr. (2021), with Hodgson (2006) arguing that Darwinian evolution is the most fundamental of all complex systems, drawing deeply on Veblen (1898) who first clearly argued for economics to adopt an evolutionary approach.

Simon's general definition also has the virtue of being close to the original meaning of the word "complex" as found in the *Oxford English D*ictionary (OED 1971, p. 492) where it is first defined as "a whole, comprehending in its compass a number of parts," from the Latin "complectere," meaning "to encompass, embrace, comprehend, comprise." Among its partial synonyms is "complicated," although, as Israel (2005) points out, this comes from a different Latin root, "complicare," meaning "to fold together" or "interwoven". Israel takes the strong position that this latter is a merely epistemological concept while the former is fundamentally ontological, complaining that such figures as von Neumann (1966) mistook them as identical, although this is arguably an overly strong position.

A final virtue of this general definition is that it encompasses one of the current cutting edge areas of economics—the behavioral and experimental approaches, which are not identical. Some who follow these approaches do not consider the

complexity view to be all that relevant to what they do (Ken Binmore and Matthew Rabin for example, even as these two disagree strongly with each other on certain matters (Colander et al. 2004a)). However, at the foundation of behavioral economics is the concept of *bounded rationality*, introduced originally by Herbert Simon. It is not just Simon, but many since who have seen complexity as implying that rationality must be bounded (Sargent 1993; Arthur et al. 1997a; Rosser Jr. and Rosser 2015), and thus is lying at the foundation of behavioral economics, with Sent (1997) discussing the relation between the views of Sargent and Simon.

Looking forward a crucial part of dynamic complexity economics is the heterogeneous interacting agents approach. This approach emphasizes dispersed and interacting heterogeneous agents (Arthur et al. 1997a; Tesfatsion, 2006; Hommes 2021). For many economists this is what they mean when they refer to "complexity models." However, as discussed earlier in this book, dynamic complexity competes with computational complexity as the most important approach to complexity economics.

Advocates of the computational complexity approach (Albin and Foley 1998; Velupillai 2000, 2005a, b, 2009; Markose 2005) argue that its greater precision makes it a superior vehicle for scientific research in economics. It must be admitted that there is some truth to this. Nevertheless, the vast majority of research in economics that identifies itself with complexity tends to be more of the dynamic variety described above. Furthermore, this definition is certainly less useful when we consider the question of the economics profession itself as a complex evolving system. Here we consider that the first two definitions provide a more useful construct for analysis than this admittedly challenging and substantial view of complexity, which we expect has the potential for important future research in the area of economic complexity. Not only is the economics profession a set of hierarchies, but it also evolves through a set of local interactions among dispersed networks of influence.

7.3 What is Cutting Edge Complexity Work?

The definitions of complexity are important because they provide a way to integrate the different strains of modern economics into a single unifying theme—the theme of complexity. The acceptance by the economics profession that the economy is complex signals a new openness to ideas from other disciplines and making it a more transdisciplinary field. Some current work falling into this broad tent complexity approach includes the following:

- Evolutionary game theory is redefining how institutions are integrated into the analysis.
- Ecological economics is redefining how nature and the economy are viewed as interrelating in a transdisciplinary formulation.
- Behavioral economics is redefining how rationality is treated.

- Econometric work dealing with the limitations of classical statistics is redefining how economists think of empirical proof.
- Complexity theory is offering a way of redefining how we conceive of general equilibrium and economic dynamics more broadly.
- Agent based computational economic (ACE) analysis is providing an alternative to analytic modeling.
- Experimental economics is changing the way economists think about empirical work, with this being the principal method by which behavioral economics is studied.

These changes are ongoing and have, in varying degrees, entered the mainstream. As that has happened, there have been a broader set of changes in how mainstream economics sees itself. Modern economics is more willing to accept that the formal part of economics has limited applicability. It is also far more willing to question the special status of economics over the other fields of inquiry and to integrate the methods of other disciplines into their methods, with Loasby (1989) and Colander (1995) arguing this is more consistent with a Marshallian rather than a Walrasian approach.

Each of these different strains has certain characteristics that are quite different from what is presented in economic textbooks. In most textbooks today one gets the impression that economics has not changed much during the last 50 years. Essentially, one learns a paradigm that develops a simple analytic deductive model, sometimes called the Max U model. The microeconomics taught in these texts is some variation on the Max U model presented with little contextual flavor that characterized Marshall's use of it. The Max U model presented in the standard text focuses almost entirely on efficiency and optimization, assuming agents are rational, selfish, and are operating in an environment that arrives at a unique equilibrium.

The MaxU model has been explored to death and, from a cutting-edge view, is no longer of much interest. (That doesn't mean it doesn't still have considerable relevance. There are still many practical applications that warrant research; however from a cutting edge standpoint, we've pulled about all we can from it.) That is why a major part of the new cutting-edge work moves beyond these assumptions. While it does not deny the usefulness or insight provided by that model, it does not see a model based only on these assumptions as sufficient, and is therefore pushing the envelope on each of those assumptions. Some examples of how cutting-edge work is questioning these neoclassical assumptions would be the following:

- Cutting-edge economics researchers are expanding the meaning of rationality to include a much broader range of agent actions that reflect actual actions; in the new approach, individuals are purposeful (incentives still matter) but are not necessarily formally rational. The new research considers the behavioral foundations of actions, using experiments to determine what people actually do, rather than simply basing their arguments on what people rationally should do, with Payne et al. (1993) integrating psychology into this. The work in game theory by such economists as Peyton Young (1998) is pushing rationality to its limits to demonstrate the importance of the expectations and information environment in

people's decisions. The cutting edge work that is being done here is going beyond the traditional definition of rationality, with extended versions of Herbert Simon's bounded rationality increasingly being accepted.

- Cutting-edge researchers are moving away from a narrow view of selfishness. While textbook economics generally assumes that agents who care only about themselves, the new work is trying to come to grips with the more realistic sense of individuals who, while they are self-interested, are also social beings, concerned about others and deriving happiness from interacting with others.
- Cutting-edge researchers are moving away from the assumption of a unique equilibrium, and are dealing with complex systems that have multiple equilibria, path dependence, and no clear-cut answer. A complex economy does not have a single equilibrium; it has many basins of attraction. The question researchers ask is which basin is sustainable. In this work equilibrium is not a state of the economy; the economy is continually in flux.

Combined, these changes can be summarized as a movement from an economics of *rationality, selfishness, and equilibrium* to an economics of *purposeful behavior, enlightened self-interest, and sustainability.* Cutting-edge work helps to move that transformation along.

7.4 Changes in Research Methods

Another aspect of cutting-edge work that is consistent with the complexity era involves changes in research methods that can serve as a catalyst for many changes in the profession. For example, advances in computing technology have led to new approaches such as agent-based modeling. This allows economists to analyze complicated systems, with more complicated interactions between the agents, out of which higher-order structures may emerge or self-organize. Also, instead of assuming optimal behavior, economists are using lab, field and natural experiments to determine what people actually do. As economists have started to use these new techniques they are taking notice of institutions, since the incentives embodied in those institutions are often central in understanding people's behavior.

This change is being accompanied by a change in the deductive nature of economic reasoning. The new work is based more on empirical inductive reasoning, and far less on pure deductive reasoning. As this is happening, the math being used in economic analysis is becoming less the Bourbakian math of "theorem-proof," and more applied mathematics, which is designed to come up with answers about policy issues, and not just talk about general issues (Weintraub 2002). Set theory and calculus, which come to definite results, are being replaced by game theory, which seldom comes to a definite conclusion independent of the precise structure of the game. For example, current work on auctions combines insights from game theory with experimental results, which are then used in practice (Banks et al. 2003).

Similarly, information economics is used in designing efficient algorithms for search engines.

7.5 Cutting Edge Complexity Work and Modern Macroeconomics

Interestingly, these cutting edge changes in micro theory toward inductive analysis and a complexity approach have not occurred in macroeconomics. In fact, the evolution of macroeconomic thinking in the United States has gone the other way. By that, we mean that there has been a movement away from a rough and ready macro theory that characterized the macroeconomics of the 1960s toward a theoretically analytic macro theory based on abstract, representative agent models that rely heavily on the assumptions of equilibrium. This macro work goes under the name New Classical, Real Business Cycle, and the Dynamic Stochastic General Equilibrium (DSGE) theory, and has become the mainstream in the U.S.

In part, this development is understandable. The macro theory prevalent in the 1960s claimed a much stronger theoretical foundation than was warranted, and many of the conclusions it came to were supported by neither empirical evidence nor theory. However, while the new theoretical models have done a good job in eliminating the old theory, it is less clear as to what the new theoretical work has added to our understanding of the macroeconomy. At best, the results of the new macro models can be roughly calibrated with the empirical evidence, but often these new models do no better than any other model, and the only claim they have to being preferred is aesthetic—they have micro foundations. However, it is a strange micro foundation—a micro foundation based on assumptions of no heterogeneous agent interaction, when, for many people, it is precisely the heterogeneous agent interaction that leads to central characteristics of the macro economy. This is the essential insight of Keynes' fallacy of composition.

Of course we have seen efforts to introduce heterogeneous agents into the DSGE context, with this leading to the appearance of Heterogeneous Agent New Keynesian (HANK) models. However, often as in Krusell and Smith Jr. (1998) these models do not involve direct interactions between agents. Rather one gets an interval of an infinite number of agents varying on a particular parameter, with, in effect, that interval acting like the representative agent of other DSGE models. This does not lead to a complexity approach to macro modeling. Such an approach will have macro outcomes emerging from a set of behaviorally based interacting heterogenous agents, with a good example being Delli Gatti et al. (2008).

The interesting cutting-edge work in macro is not in the theoretical developments organized around representative agent micro foundations, but the work that views macroeconomy as a complex system. In this work, one sees the macroeconomy as being endogenously organized. The issue is not why there are fluctuations in the macro economy, but why is there so little instability where complex interactions

could generate chaos, although chaotic dynamics do stay within bounds consistent with the "corridor of stability" idea of Leijonhufvud (1973, 2009), which resembles the "resilience-stability tradeoff" studied by Holling (1973) in ecology. The belief that one could develop a micro foundation for macroeconomics without considering the feedback of the macro system on the individual is beyond belief. While it may still make sense to push analytic macro theory as far as one can, to see whether it will provide any insights, in the short term, such analytic extensions of pure theoretical models based on assumptions that are far from reality offer little hope for policy guidance. In the absence of a pure theoretical foundation, macro policy is best based more on statistical models that pull as much information as possible from the data. Empirical macro precedes theoretical macro.

7.6 Complexity Economics and the Debate over Heterodox Economics

The basic argument of this chapter that complexity economics is not only a crucial part of the cutting edge of economic research but in fact substantially underpins the broader future of economics was made in a strong form initially by Colander et al. (2004a) and Colander et al. (2004b), with the first of those a book mostly of interviews with "cutting edge economists," all but one of whom were located in the United States,[2] with this not planned but simply came about out of convenience given we are all based in the U.S. This would be followed up by a similar book largely of interviews focusing on European economists and economics (Rosser Jr. et al. 2010),[3] with one planned for Asia that never happened, although arguably in Japan there is a tradition that has led to such an independent and locally developed such approach (Morris-Suzuki 1989; Ikeo 2014; Shiozawa 2004; Shiozawa et al. 2019; Rosser Jr. 2021).

The second item is an article largely derived from the opening chapter of the book that laid out the framework we had going into the interviews, in which the theme of complexity was a recurring theme. This paper, published in the *Review of Political Economy*, would attract the most attention (and citations) of all these works and set off a considerable debate to be discussed below, with several of our later works focusing heavily on this debate (Colander et al. 2007–08, 2010; Rosser Jr. et al. 2013).

[2]Those interviewed in the (Colander et al. 2004a) US-based book were Deirdre McCloskey, Ken Binmore, Herb Gintis, Bob Frank, Mat Rabin, William ("Buz") Brock, Duncan Foley, Richard Norgaard, and Rob Axtell wirh Peyton Yong, with ex post overviews by Ken Arrow and Paul Samuelson.

[3]Those interviewed in the (Rosser Jr et al. 2010) Europe-based book were Alan Kirman, Ernst Fehr, Cars Hommes, Mauro Gallegati with Laura Gardini, Geoff Hodgson, Joan Martinez-Allier, and Robert Boyer, with ex post overviews by János Kornai and Reinhard Selten.

An issue going back decades actually as one can surmise if one has read this book all the way through to here is that for much of this time ideas associated with complexity economics were not always easily accepted by mainstream economists. The papers often appeared in oddball journals, with some exceptions, or in arguably oddball books, even though in a number of cases these papers and books would later become heavily cited and widely respected and influential. This led us to think seriously about the nature of how economics evolves and how new ideas or approaches develop and enter into economics, moving from some fringe and ridicule to eventually ending up in textbooks, with one of us, David Colander, having long worn the hat of both an economic educator (Colander 2000b) and a historian of economic thought (Colander 2000c), as well as tying these concerns to ideas of complexity economics and even applying them to economics itself as a field (Colander et al. 2009; Colander 2015; Holt and Rosser Jr. 2018).

A centerpiece of this process and debate involves the role of *heterodox economics* and its relationship to non-heterodox economics, with to what extent do new ideas emerge from heterodox economists and how is it that when "successful" they move more into the mainstream. This issue was very live in our first interview book (Colander et al. 2004a) in which indeed those we interviewed themselves differed on how they viewed themselves regarding their status in the profession, with some viewing themselves as clearly heterodox (Duncan Foley) while others viewed themselves as more in the mainstream (Ken Binmore). This pushed us to think harder about what was going on here.

What we came up with was to bifurcate the question to a degree, and to argue that there is an intellectual aspect to it and a sociological aspect to it, with there being three categories under consideration: *orthodoxy, heterodoxy, and mainstream* (although confronting this one of our interviewees, Herb Gintis, joked not totally unseriously that he likes to think of himself as a "homodox economist"). We decided that orthodoxy is an intellectual category, mainstream is a sociological category, but heterodoxy is both, which is where much of the trouble arises. Orthodox economics in its pure form is the old "neoclassical economics" that Colander argued (Colander 2000a) has died, that economics described by the trinity of rationality, greed, and equilibrium. Its purest manifestation was at the University of Chicago for decades, although at a more fundamental level its hardest line exponents were long based in the "sacred zip code" in Cambridge, Massachusetts at Harvard and especially at MIT, with Paul Samuelson as perhaps the supreme godfather, whom we interviewed along with Ken Arrow for the end of our first book after letting them see our other interviews. As it is, even at these bastions this old orthodoxy no longer reins, and all sorts of formerly unacceptable approaches, especially behavioral economics, now infest the hallways and offices.

Mainstream is a sociological category. It is really people, those in charge of the economics profession, those at the top schools, running the top journals, controlling funding for research, and so on. We noted that even by soon after 2000 or so there were quite a few such people in these positions, including Nobel Prize winners, whose ideas were not strictly orthodox, with people like George Akerlof and Vernon Smith sticking out as examples, although Smith has not generally been at top

schools. This would also include a few players from earlier who have been heavily cited in this book as important in developing complexity economics, such as Herbert Simon. All of these won Nobel Prizes and are or were highly respected, but also have long felt somewhat at odds with the hard core of "the Establishment," even as they looked to more serious outsiders as part of that orthodox "Establishment." They are or were "mainstream," but not "orthodox." This was our key claim, and the one that brought much criticism down upon our heads.

This key claim had another part to it, the claim that in contrast to the other two main categories, heterodoxy is both an intellectual and sociological category. Thus heterodox economists are both intellectually opposed to and critical of the old orthodox economics, and they are also not in the top schools and find it hard to publish in top journals, feeling discriminated against and even oppressed. In some cases this has led to them failing to get tenure at various institution due to their troubles publishing sufficiently in sufficiently prestigious journals and otherwise suffering professionally.

Understandably this has led to resentment and anger by many, with some of this arguably justified. For many of these self-identified heterodox economists, the enemy is "the orthodox mainstream," and they abreact to this identifying some of the mainstream economists as "non-orthodox." To these harder core heterodox economists, these erstwhile non-orthodox mainstream are if not outright sellouts, then people who have played a game to make themselves acceptable to those in charge buy not challenging vigorously enough orthodoxy (Lavoie 2012; Lee 2012). That they may be making their ideas accepted to some degree by the mainstream and even old orthodox simply shows that they are assimilating to the mainstream and orthodox, not that they are succeeding in getting the mainstream to accept their ideas and even arguably redefine the nature of orthodoxy. As it is, even among those critical of our formulation there are differences. Thus Marc Lavoie (2012) recognizes a group he calls "dissenters" who are in effect our group of non-orthodox mainstreamers, whereas the harder line Fred Lee (2012) basically dismissed this whole category, arguing that taking them seriously or trying to be like them was simply giving in to domination by orthodoxy and giving up on heterodoxy.

Needless to say, among the heterodox have arisen over time many different schools of thought. This is not the place to get into any detailed discussion of all of these, although throughout this book at times ideas of one or another of them have been called upon or invoked, including Marxist, Austrian, Post Keynesian, evolutionary, institutionalist, behavioral, ecological, and more, especially when their approaches seemed open to or in congruence with elements of complexity economics. Indeed, the origins of many ideas in complexity economics clearly came out of one or another of these schools at particular points in time, and arguably the strongest proponents of some of those ideas remain still firmly identified with one or another of these schools.

Of course a great irony is that each of these schools of thought themselves have developed their own internal orthodoxies and leading individuals and journals and locations that claim authority to define the school and who is in it or not in it, with the result that heresies arise within even these schools leading to the development of

sub-schools that can become so numerous and differentiated one from another by such obscure debates that outsiders find it difficult if not impossible to figure out what is going on or who is what. The wars among the Marxists were among the most famous, and involved at times literally wars and people literally killing each other, as Stalin's assassination of Trotsky most dramatically demonstrated. Austrians are split between Misesians and Hayekians. The divisions among Post Keynesians are especially numerous, with Paul Davidson long holding a dominating position in the U.S. as founding editor of the *Journal of Post Keynesian Economics* while European based rival groups such as the neo-Ricardian Sraffians argued vigorously against his views and those of others. The various schools of the heterodox came to have their own sub-heterodox. In some of these battles some sub-schools are friendlier to complexity ideas than others, with Hayekians more so among Austrians and so-called Kaldorians among Post Keynesians also more so, just to give two examples.

These debates and differences of view have even been present among the three coauthors I have cited here on this matter, myself, David Colander, and Ric Holt. Dave has long taken the harder line of in effect criticizing the heterodox for not trying harder to get along with the mainstreamers, not trying to use "more honey" rather than "more vinegar," which has tended to bring more criticism down on his head from some heterodox, as he has often been very public and articulate about these views to an almost "in your face" way with some heterodox, much to the annoyance of the latter. I have been probably the one more at the other end, more sympathetic to the complaints by many heterodox regarding their being rejected and oppressed and discriminated against, with Ric being the one who often was diplomatically making peace between Dave and me when we worked together. It may be that I personally felt more heterodox, being at a not particularly prestigious state university and for a long time feeling isolated and ignored.

But Dave argued that for all those attitudes I became a mainstreamer, especially after the 1991 publication of my first book, *From Catastrophe to Chaos: A General Theory of Economic Discontinuities,* which became a success after it came out, going into three printings and receiving favorable reviews and lots of citations, even though it had been rejected by 13 publishers before Kluwer took it up at the behest of Zac Rolnik there. My position especially changed when I became editor in 2001 of the *Journal of Economic Behavior and Organization*, which has long been viewed as being "heterodox but respectable," a fine line to walk. Founded ,by Dick Day, it indeed was an early outlet of many complexity ideas, including chaos theory as well as game theory, behavioral economics, and new institutionalist economics. While in the 1980s much of this work was unpublishable in the top journals, that has changed, with leaders of these fields winning Nobel Prizes and such material now published in top journals and even getting into graduate textbooks. This even included to some extent ideas I expressed in that 1991 book, which is now viewed as a reference volume by many. Dave put it to me that I had become mainstream, whether I liked it or not, because "the top people respect what you do," and also because many of the ideas that I have worked on that were viewed as heterodox have become, well,

respectable. Indeed, arguably this is a part of how economics more broadly has entered the complexity era.

I close this section by noting an old joke I heard from Dave Colander that he first heard from Abba Lerner. "But look," the Rabbi's wife remonstrated, "when one party to the dispute presented their case you said 'you are right' and then when the other party presented their case you again said 'you are quite right.' Surely they both cannot be right." To which the Rabbi answered, "My dear, you are quite right!"

7.7 Complexity Economics and Public Policy

If indeed the future of economics is to be heavily influenced by ideas from complexity economics, then for many the proof of the pudding boils down to how useful is it for informing public policy discussions and formulations. This is a matter of ongoing dispute and controversy. Much of this has involved especially the use of heterogeneous agent modeling of the sort discussed earlier in this book that was especially strongly associated with the Santa Fe Institute, where arguably the focus has more recently been upon behavioral economics and game theory than upon that particular sort of modeling. Of course, as Rosser Jr. and Rosser (2015) argue and has been argued above in this book, there are strong links between complexity economics and behavioral economics, with the central role of Herbert Simon in the early development of both a strong sign of this.

It must also be recognized that large parts of each do not particularly belong to the other. But indeed, if the old orthodoxy was highlighted by a trinity of rationality, greed, and equilibrium, both standard behavioral economics and complexity economics challenge all three of those, so it is not surprising that there is considerable overlap, and it is not surprising indeed again, that the journal I edited from 2001 to 2010, the *Journal of Economic Behavior and Organization* (and the one I now edit, the *Review of Behavioral Economics*) have both been major outlets for both approaches, including their overlap.

One area where there is frustration on the part of many complexity-oriented economists has been felt has involved macroeconomics, discussed above. There has been a major push to adopt interacting heterogeneous agent modeling at such crucial policymaking entities as central banks, but aside from study going on at some of them, these have not won the day or been substantially adopted. It is widely reported that at the US Federal Reserve three different kinds of models are used to advise policymakers: DSGE models, structural models that are essentially complicated derivations from the ISLM approach, and atheoretical models based on vector autoregressive methods. While full-blown interacting heterogeneous agent models have not joined this triumvirate reportedly, each of these has absorbed elements of complexity economics. As noted above, DSGE models have changed to include multiple agents as well as some nonlinearities and even essentially ad hoc behavioral fixes. There may be less of this going on with the older structural models, but the VAR-derived models have long incorporated nonlinear methods of various sorts,

with there being a long interaction between complexity and nonlinear econometrics and time series approaches. There has also been an incorporation into all three kinds of models of financial factors, with these parts of the models also often involving various complexity elements. Indeed, at some banks, there is much modeling of networks of financial relationships (Haldane 2013), clearly a complexity approach, if one only touched upon in this book.

More broadly, while Brock and Colander (2000) made an initial stab at a more general approach, Colander and Kupers (2014) try to go beyond conventional formulations and provide a provocative stance, even as it almost certainly has its limits. It effectively relies upon emphasis on emergence of structure and order out of "bottom up" rather than "top down" approaches, emphasizing spontaneity and creativity to seek new and innovative solutions to entrenched problems. They came together while participating in a conference about climate policies. There was a split between those who advocated largely market-oriented policies and those who advocated largely government regulation-oriented policies. They were unhappy with this simplistic dichotomy and sought for a complexity-oriented alternative, which led them to their emphasis on bottom up policies that might well involve both markets and governments.

Their approach is summarized in the following (Colander and Kupers 2014, p. 21):

> "In the complexity policy frame, one starts with a recognition that there is no ultimate compass for policy other than a highly educated common sense. Scientific models provide, at best, half-truths. In our view, the education of that common sense very much includes a basic appreciation of complexity, as well as of humanities, mathematics, and others. Policy compasses are created and evolve, they are fallible products of a particular time and place, and must be treated as such. The nature of the relation between market and government, as well as top-down versus bottom-up solutions, as well as the property that policy itself is part of the complex system, is posited pretty clearly in the following . . . the duality of market versus government is a product of the standard economic policy frame itself. Within a complexity frame, both the more actrive top-down "government" solution and the less active bottom-up are seen as having evolved from the bottom up. Within this frame, the policy solution is an element of the system, not outside it."

Invoking "metapolicy," they avoid advocating specific policies. However, they provide some examples of what they like. An example is the "shared space" system of traffic control in the town of Drachten, the Netherlands, developed by Hans Monderman. When one drives into Drachtem one finds no stop signs or street lights or even sidewalks. Yet traffic flows smoothly and with few accidents. It helps that Drachten is not a large city where such a system simply may not work. This may look like a semi-anarchist "no government market fundamentalism," but they argue that is not the case. This is because this system depends on an existing institutional framework: a preexisting system of myriad rules and regulations, drivers' licenses, car safety standards, a broader legal framework, and more. Thus it is not a spontaneous anarcho-capitalism, but a carefully framed and bounded system that allows for the emergence of order. As they also note, "In the complexity frame, a well-functioning market is a consequence of previous and successful government metapolicy" (Colander and Kupers 2014, p. 25).

Another related issue they get into is one that Rosser Jr. (2001a, 2020e) has also addressed, namely the relationship between the views of Keynes and Hayek and how each of them relate to complexity, with Hayek (1967) having specifically discussed complexity and taking it seriously in his later years, while Keynes never specifically addressed it. For Colander and Kupers they see some overlap of the views of the two, even as on many issues they clearly differed sharply, with indeed Keynes looking more like the top-down government-intervention advocate against Hayek the advocate of bottom-up market-based spontaneous order. Pretty clearly Hayek fits their approach with this approach, so the question becomes where does Keynes fit in with this?

One response they make is that the most famous piece of top-down advocacy by Keynes involved the Great Depression, which he viewed as a "one-off" special case. Otherwise he generally favored bottom-up approaches. They point out the friendly letter Keynes (1944) wrote to Hayek (1944) when he published his *The Road to Serfdom* in which he expressed his "moral and philosophical sympathy" for Hayek's arguments. Even so, the letter itself recognized their differences, with Keynes arguing that ". . .we almost certainly want more [planning]. But the planning should take place in a community in which as many people as possible, both leaders and followers, share your moral position" (Colander and Kupers 2014, p. 40). They claim this shows Keynes supporting bottom-up solutions, but that would "minimize government intervention into the market, but still achieve socially desirable ends" (ibid.). However, pretty obviously others might find them stretching a bit on this point.

As it is on this matter of Keynes and Hayek and their connection with complexity, I see their overlap coming from a different direction. This would be the old bugaboo of fundamental uncertainty, which has been discussed above in this book. Keynes (1921) first made this argument that such uncertainty involves the non-existence of a probability distribution in his *Treatise on Probability*, but brought it back later in his *General Theory* (1936) and some other works. Many have seen this as implying a complexity view of the economy (Davis 1994, 2017).

Hayek did not address this specifically using probability theory, but in his discussion of complexity (Hayek 1967) it is there fitting in with his dismissal of a tendency to a long-run equilibrium and his preference for a constantly evolving economy marked by spontaneous emergence of order. A broader argument of Austrians more generally related to uncertainty is how this opens the door for the important role of entrepreneurs who operate crucially within such a profoundly uncertain environment. When pushed Keynes might be more inclined to fall back on government to rein in and limit the uncertainty, while Hayek might be more inclined to trust the spontaneous order arising from unfettered markets, but they share an understanding of the deep nature of the dynamic processes of the economy that it is complex.

7.8 The Paradox of Economics as a Complex Adaptive System

The question of whether or not the future of economics is to be fundamentally complexity economics or not has a curiously paradoxical aspect. A theme among many complexity economists is that the economics profession is itself a complex adaptive system. It is characterized by the sorts of nonlinearities and positive feedbacks that Brian Arthur (1994) emphasized as the central elements of complex systems. Ironically these characteristics present contradictory forces, one for instability and one for stability.

Positive feedback effects are most famously known as undermining equilibrium. They imply a non-convexity that removes one of the standard assumptions made when one uses a fixed point theorem to prove existence of an equilibrium. In a market if there are increasing returns then if one firm gets larger than others, its long-run average costs may fall below those of others allowing it to undercut its competitors so that they may come to be unable to earn a non-negative profit, which in turn in the end can lead to a natural monopoly as the competitors end up driven out of business eventually, assuming that there is no limit to those economies of scale.

But this outcome brings us to the paradoxical aspect: if indeed there are these unlimited economies of scale, one can end up in a situation where indeed there is an entrenched monopoly that cannot be ousted by newly entering competitors unless there is a fundamental change in technology or some other element of the system that allows for the potentially new entrant to be able to break down this system. But the system can become deeply entrenched and hard to profoundly change. Thus a complex adaptive system might well end up becoming an essentially stagnant and conservative one, stuck in its ways, with all changes simply reinforcing its stasis as positive feedback effects simply drive it deeper and deeper into the condition it has achieved.

So it is that David Colander sees the economics profession having tendencies to simply reinforce itself in an existing state despite being battered by outside forces of change. Some of this pessimism has come from seeing developments in macroeconomics since the financial crisis and Great Recession, when the DSGE model continued to hold sway in a dominant position among practicing policymaking macroeconomists at central banks and in academia, although one that has been tweeked to some degree by ad hoc changes of the sorts mentioned above. Thus he argues (Colander 2015, p. 230): "There are now some discussions in the texts of macro-prudential policy, zero lower bounds, structural stagnation (although much of that discussion goes under the name, secular stagnation), quantitative easing, and even some mention of Minsky moments. But in the underlying macro model of a stable economic system composite aggregate rationality remains."

Furthermore, drawing on Piketty's work (Piketty 2014), trends to greater and greater income and wealth inequality seem to be deeply entrenched and hard to overcome or halt, much less reverse. Obviously this is not a simple or straightforward story, and competing trends can coexist at different levels. Thus at the global

level we see a trend to increasing aggregate equality due to rising incomes in the two largest nations, China and India, even as we have seen increasing inequality inside most nations, thus undermining the optimism of Simon Kuznets (1955) regarding the implications for income inequality of long-run economic development. Nevertheless this is not inevitable, quite aside from the possibility of major revolutionary political economic upheaval as we saw in the early part of the twentieth century. So some of the most unequal nations, notably some in Latin America, have seen some movement towards greater income equality, if not dramatic (Rosser Jr and Rosser, 2019, Chaps. 18–19). The inequality trend is not inevitable or impossible to overcome.

However, getting back to the economics profession itself, especially in the United States, which dominates the world's economic profession increasingly (Rosser Jr. et al. 2010), this tendency to dynamic self-reinforcement and entrenchment in a path-dependent sort of way may be manifesting itself. Colander particularly sees this operating through the educational system, with the system's conservatism enhanced by what he calls "the 15 percent rule," the idea that leading textbooks cannot change by more than 15 percent at a time due to the unwillingness of established faculty in a field to change their class notes too frequently.

But in the case of the economics profession in particular in response to the financial crisis and the Great Recession we saw an ironically peculiar process in effect. Despite widespread calls for fundamental change coming from many quarters, the crisis generated incentives for the profession not to change, with these incentives reinforcing self-satisfaction and inertia. It operated in the following way according to him: "The larger the crisis, the more students want to hear what economics has to say, more sign up for economics, and more revenue flows into economics, reinforcing the institutional structure. This leads the profession to respond: 'Why change what we are doing? We are doing quite well, thank you" (Colander 2015, p. 234).

Thus we have this paradox that the complex adaptive nature of the economics profession with its increasing returns dynamics ends up enhancing its tendency to stasis and not changing in a fundamental way. The move into a full complexity era may continue, but it is extremely hard to overturn the apple cart and dramatically change the way things are done, to move to a fundamentally new and different kind of economics so. But then, it is the nature of dynamically complex systems to generate surprises with new forms emerging unexpectedly when one least expects them to do, even as we have seen in the grandest and most important of all complex systems, the evolutionary process, which certainly operates in the economics profession as it does in the larger socio-economic system and the even larger ecologic-economic system in which we all live.