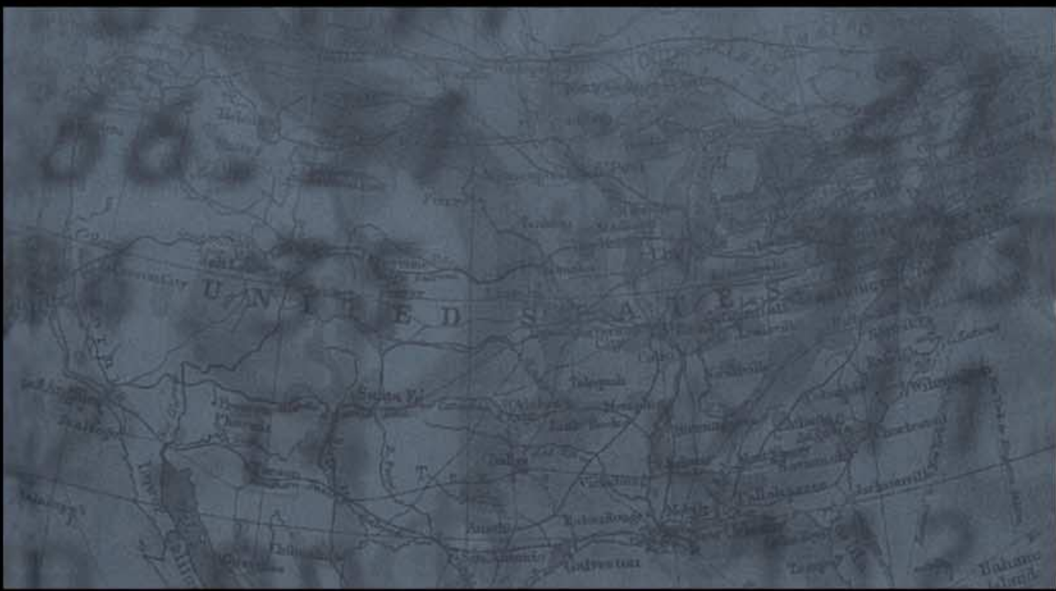
Complexity and the History of Economic Thought

Perspectives on the History of Economic Thought

Edited by David Colander



Complexity and the History of Economic Thought

Recently a new approach to science has developed. It is called the complexity approach. A number of researchers, such as Brian Arthur and Buz Brock, have used this approach to consider issues in economics. This volume considers the complexity approach to economics from a history of thought and methodological perspective. It finds that the ideas underlying complexity have been around for a long time, and that this new work in complexity has many precursors in the history of economic thought.

This book consists of twelve studies on the issue of complexity and the history of economic thought. The studies relate complexity to the ideas of specific economists such as Adam Smith, Karl Marx, Alfred Marshall and Ragnar Frisch as well as to specific schools of economics such as the Austrian and Institutionalist schools.

The result of looking at the history of economic thought from a complexity perspective not only gives us additional insight into the complexity vision, it also gives insight into the history of economic thought. When that history is viewed from a complexity perspective, the rankings of past economists change. Smith and Hayek move up in the rankings while Ricardo moves down.

COMPLEXITY AND THE HISTORY OF ECONOMIC THOUGHT

Perspectives on the history of economic thought

Selected papers from the History of Economics Society Conference, 1998

Edited by David Colander

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CONTENTS

LIST OF FIGURES

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PREFACE

The idea for this book originated as I was setting up the History of Economics Society Meetings for 1998. The new science of complexity, such as that done at the Santa Fe Institute, was much in the news at that time, and a number of us had been discussing the issues that complexity raises for the teaching of economics. In thinking about the complexity vision, it was obvious that the complexity approach to economics had its roots in early economists' work. We felt that it would be useful in understanding both complexity and economics to consider those roots more formally. So, in my initial call for papers for that conference, I stated that the theme of the conference would be complexity in the history of economic thought. The papers that came in from that call for papers and which were recruited by me because of the person's expertise in a specific area make up this volume. These papers will give the reader a good sense both of what the complexity vision is and where its roots in economics are.

Any book involves an enormous amount of work, and this is no different from any other. Thus, I have many people to thank. First, I would like to thank the authors of the papers who did the research that makes this contribution valuable. I would especially like to thank Nathan Rosenberg who I invited to be the keynote speaker at the conference because of my admiration for his work. He took time out of his active research program to relate his work on Charles Babbage to the new work that is being done on complexity. I would also like to thank each of the other authors, who went through a number of drafts and developed papers that fit together to make a book, rather than simply a collection of papers.

After the papers were written, they had to be turned into an acceptable manuscript. My assistant, Melissa Dasakis, handled the initial production work of the volume; she did a great job in preparing the manuscript to send in to the publisher. Aleksander Wolski assisted her. Once the book was at the publishers, Alan Jarvis saw to it that it was handled efficiently; our editor Robert Langham oversaw the general process and our desk editor Ruth

Jeavons saw to it that all the work that must be done to the manuscript went smoothly. I would like to thank the copy editor, Audrey Bamber, and Helen Reiff who helped proof the manuscript and made a number of stylistic improvements.

Finally, I would like to thank Middlebury College and the Christian A. Johnson Foundation, which provided me with the environment in which such esoteric issues can be discussed.

David Colander

INTRODUCTION

David Colander

The history of thought is a set of patterns that we superimpose on the development of ideas and theories from our current perspective. History is much like a magic eye picture: Change one's perspective and one changes what seems important in history. Thus, our account of the history of economics tells us about ourselves and our current views, as well as about what previously transpired.

Since all patterns do not fit equally well, the pattern view of history does not mean that anything goes; some histories fit the pattern of events better than others. But, inevitably, fitting the pieces of an historical pattern together into a composite whole requires forceful and fast writing on the part of the author and some gullibility on the part of the reader. What this means is that there will often be three or four lenses that can reasonably be used to look at the history of economics to convey quite different patterns.

This volume provides an example of this pattern view of history; it considers the history of economic thought from a different perspective than do most contemporary histories of economic thought. Specifically it highlights the contribution of various economists and schools of thought as seen from a complexity view of economics.

The "simplicity of structure" and the "complexity" simplifications of economics

All economists know the economy is complex—very complex. That is one of the reasons why society needs economists—to try to make that complexity somewhat simpler and more understandable. In that endeavor conventional economics has seen itself as a conventional science; it takes complexity and simplifies it by finding a formal structural analytic model—an equation, or set of equations—that fits the data. The model is then tested by comparing the predictions of the model with the empirical data, using formal statistical techniques. These models are generally linear and static, since they are the only ones with unique, deterministic solutions. In these tests classical statistical tests are generally used.

To believe that such a model exists before it has been discovered and tested requires a leap of faith; science, at the edge, is necessarily a combination of art and religion. It requires dedicated researchers who are operating on faith until sufficient empirical verification can be achieved so that we will tentatively accept that our understanding embodied in the model has gone beyond faith and become what we call fact. In many areas, that faith has been borne out; conventional science has numerous successes. In other areas it has been less successful.

Formal models do not develop from a void. The beginnings of science in all areas involve developing informal insights into how some aspect of reality operates. Scientists then begin work on simplifying those informal insights into formal models. All science works on such simplification: The questions in such a simplification process are how to simplify, and whether the simplifications lose important elements of the informal knowledge; they are not about whether to attempt to simplify.

Since formal analytic models compress the data more efficiently than informal models, formal models are preferable as long as they do not lose the crucial insights of the informal models which led to them and which allowed a broader set of explanatory variables and more of a sense of the importance of faith. In most areas where individuals think science has been successful, there has been a general belief that the formal models do not lose the key insights of the informal model. Where that belief is not generally shared, there is more debate about how successful science has been.

Complexity: extending the boundaries of science

The conventional view of science has been that if one decides that formal structural models lose more insights than they gain, one has decided that science is not relevant to that area. For example, we have no science of love; we rely on our informal understanding. Recently, however, a group of scientists, whom I call complexity scientists, have questioned whether that conventional structural simplification process is the best way of understanding highly complex phenomena. They have suggested that such phenomena might best be approached from an alternative perspective. In doing so they have added a new way in which science might approach complex phenomena.

A number of points should be emphasized about this complexity approach to science. First, those following it are committed to science; they accept structural simplicity whenever it can be achieved. But they argue that there are areas—highly complex phenomena—where structural simplicity cannot be achieved, but where science may nonetheless have something to say.

Second, complexity scientists accept that the purpose of science is simplification. Where they differ from standard scientists is not in the desire for simplification but in the proposed simplification process. They suggest that for complex systems a simplification process centered around iterative processes, not structure, may be best. This new approach has come to be called the complexity approach to science. Let me give an example. Say you are trying to understand how ice forms. In conventional science one looks for laws that govern the formation of ice and, in principle, finds a structural relationship explaining why a molecule changes from a liquid to a solid state. One then expands the analysis from the molecule to the whole. In complexity science, one looks for simple iterative processes that, under certain circumstances, lead to a large-scale transformation of water from a liquid state to a solid state.

A leap of faith that complexity scientists make is that they assume that all complex phenomena are subject to similar forces—that, as complexity increases, transformations take place in which large numbers of interactions all work simultaneously to form a pattern that would otherwise be unpredictable. What should be chaos is actually an ordered pattern. Thus, they study the general development of these patterns, and then apply the results of that general study to specific cases.

The study of complexity is highly mathematical and statistical. Where it differs from what I call conventional science is in the nature of the mathematics and statistics it uses. Instead of trying to find a formal analytical model, with a formal solution for these complex phenomena, complexity theory looks for patterns that develop when non-linear processes are repeated for long periods of time. The mathematics used is non-linear dynamics and the models generally are open models with no unique deterministic solution. Many solutions are possible; which one is arrived at depends upon initial conditions and the path the model follows. What complexity scientists have shown is that, given certain dynamic relationships, certain patterns can develop, and that sudden changes in these patterns can occur.

The laws of complexity science are statistical probabilistic laws. They refer to large groups of actors and are not reducible to laws of individual actors. Complexity allows that aspects of reality can emerge from chance and the law of large numbers. Even though the individual components of a complex system are chaotic and indeterministic, and the movement of individuals within the body may appear random, the sum of the parts—the whole—can be deterministic. This structural determinism undermines the development of a deductive microfoundation of the aggregate. It means that composites of elements of smaller components are indecomposible and must be treated as a separate entity and not as the sum of their parts. As complex systems evolve, new patterns can emerge, and these patterns can take on an existence and life of their own.

The complexity approach to science is highly controversial. Some (see especially Horgan 1995) argue that it is ill-defined and far too grandiose; it is attempting to be a science of everything. These critics object to its attempt to extend the boundaries of science. Others argue that complexity science offers little new, that it is non-empirical and is simply speculative. My view of these criticisms can be understood by classifying all phenomena into three types:

1 those areas for which most people believe that standard science provides insight;
2 those areas where there is debate about how much insight standard science provides; and
3 those areas where standard science does not tread.

In category 1, where structural simplification has been shown to be useful, such as particle physics or chemistry, for me there is no debate; these are the realms of standard science. In category 2, within which I classify economics and other social sciences, there is much less scientific agreement about the gains we have achieved from structural simplification; in these fields complexity is challenging the standard approaches to these fields, and the debate is about which simplification process is preferable. Since standard science and scientific methods are being used, complexity science is simply an alternative approach. The critics of complexity science have had little to say about its use in these areas, because, to say anything useful, one would have to compare the success of the complexity approach with the standard approach.

The broad criticism of the complexity approach, such as Horgan's, deals primarily with the third category—the category in which complexity theory is used to analyze areas that conventional science does not touch. Some of the extensive claims that have been made for complexity science—that it will add insight into these areas—have been overdone and are too far-reaching; the critics are probably right for these areas. Complexity science is a long way from unlocking the essence of the development of life. But such criticisms are not relevant to the application of complexity to economics since they do not, in any way, undermine the possibility that complexity science can be useful in analyzing the second category, where economics is found.

I have no definitive view on whether what is currently known as the complexity approach is going to succeed or not. I am drawn to it nonetheless because of my belief that, in economics, we have been unsuccessful in following a standard structural simplification approach. To achieve our current structural models we are assuming away too many important aspects of our complex economy. Whether the new work in complexity will eventually improve our knowledge of physical phenomena and the economy remains to be seen, but the vision that underlies the complexity approach seems strong. Regardless of whether it is successful or not, complexity provides a general framework within which to think about complex systems that is quite different than the conventional approach and is worth considering.

Complexity science and economics

The history of economics has been a history of methodological fights—the famous *methodenstreit* is one well-known example. One of the major reasons for these fights is that it is not clear where economics fits in as a science; usually on one side of the methodological fight have been those who are arguing that the simplifications being used do not do justice to the field, and on the other side is a group who argue that simplification is absolutely necessary, even if bought at the cost of assumptions that do not fit reality. This fight has often been portrayed as a mathematical approach versus a non-mathematical historical/institutional approach, and that distinction has been a dividing line between mainstream and heterodox approaches. Complexity changes that; it is highly mathematical and, as I stated above, accepts the need for simplification. But it argues that the mathematics needed to simplify economics often involves non-linear dynamic models that have no deterministic solution.

In earlier time periods, such a statement would be equivalent to saying that there is no feasible mathematical approach to dealing with economics, but recently that has changed. Developments in computers have made it possible to deal with models that are far more complicated than those that previously could be dealt with. One can simulate and, through rote computer power, gain insight into models with no analytic solution. Thus computers and simulations are the foundation of the complexity approach.

The use of computers and simulations to gain insight into problems involves an enormous technological change in the way economists do economics. Complexity advocates take the position that such a change is a natural shift to new technology. For them, those who are not moving toward solving models with computers are demonstrating Luddite tendencies in an attempt to protect their rents. Thus, complexity advocates find themselves on both sides of the fence in the standard methodological debates; they are agreeing with critics that standard science is lacking, but they favor moving to a new, even more complicated mathematics and statistics than standard economics uses, and are arguing that ultimately it is mathematics that will provide the formal insights into the institutions and history that are needed for economics to be a science.

Let me give an example of how complexity economics differs from standard economics. Say one is trying to understand the nature of production. Standard economics would develop a simple analytically-solvable function—say the CES production function with “nice” analytic properties—and then use that to study a variety of cases. In the complexity approach, one would try hundreds of variations of non-linear models, many with no deterministic solution, and rely on the computer to show which model best fits the data. One would, of course, study the general properties of these non-linear models, but whether the models have analytic solutions would not be a relevant choice criterion as it is now; the choice criterion would be "fit with the data." Elegance and solvability of models are de-emphasized.

There are, of course, limits to what we can discover through simulations, and there is much methodological work to be done before the complexity approach becomes one that can be generally used. But the complexity vision is that this is the important work and that, as computing costs continue to fall, analytic solutions in mathematics will be less and less important. Similarly in econometrics; Monte Carlo and bootstrap methods will replace analytic methods of testing in many cases.

Because the complexity approach involves even more focus on mathematics and statistics than does standard economics, the complexity method may seem even less compatible with many of the heterodox approaches and with the study of the history of thought than is mainstream economics. That, however, is only partly true. Low-cost computing lowers the value of theories. The complexity approach demotes theory to a lower level and replaces it with conjectures and patterns that temporarily fit. Determining whether these patterns are meaningful requires a knowledge of economic history and of the history of economics. Whereas in standard economics the latest theory is thought to include the best of the past, in complexity economics patterns can fluctuate and a variety of theories will be constantly tested. In complexity economics one is not searching out the truth; one is simply searching for a statistical fit that can be temporarily useful in our understanding of the economy. If these fits become good, then we can develop a law but, because of the way dynamic equations work, the laws can change, and they can change suddenly. One period may be quite different than the period before and, instead, be more closely related to an earlier period.

An overview of the chapters

The volume is divided into four parts. The first part, "Introduction to Complexity and the History of Thought," expands on this general introduction. The second, "Specific Economists and Complexity," considers a selection of economists who would be highlighted in a complexity perspective of a history of economics. The third part, "Broader Views of Complexity," relates economics to some ideas that expand upon complexity, and the fourth part, "Alternative Perspectives on Complexity," provides some heterodox perspectives on complexity.

The two chapters in Part I are introductory in nature. In the first paper, "What is Complexity?" James Wible gives a brief introduction to complexity, surveying the work of Nicolis, Prigogine, Hayek, and the Santa Fe Institute. He traces the origins of complexity in science and economics in order to illuminate the various approaches and contributions to complexity that we face today. He points out that they all believed that complexity involves self-organization and that the analysis of complex systems requires a different analysis than that of non-complex systems. In complexity one looks for patterns, not for specific events, and these patterns develop spontaneously over time.

In the second chapter, "A Thumbnail Sketch of the History of Thought from a Complexity Perspective," I develop the pattern idea of the history of thought presented in the introduction. I sketch an outline of the history of thought from a complexity perspective, suggesting that some economists would go up in ranking while others would go down. Economists who would move up include Smith, Hayek, Marshall, and Marx, and those who would move down include Ricardo and Walras. Finally, I discuss two economists, Charles Babbage and John von Neumann, who would move from footnotes to the main body of history of thought texts.

Specific Economists and Complexity

Part II is a consideration of specific writers. Nathan Rosenberg begins the section with his paper "Charles Babbage in a Complex World."[1] The chapter considers the economic work of Charles Babbage, best known for his work on computers. Rosenberg argues that despite enormous contributions to economics, Charles Babbage has received almost no attention either in economics literature or the teaching of the history of economic thought. Rosenberg argues that Smith's work was pre-Industrial Revolution and that Babbage reconstituted Smith's ideas, bringing them into the Industrial Revolution age. In doing so Babbage incorporated a sophisticated treatment of increasing returns, and did it much more within a complexity framework than the standard framework.

Babbage dealt seriously with institutions, and his writing on "The Causes and Consequences of Large Factories" powerfully influenced the treatment of this topic by Mill and Marx. Rosenberg also points out that Babbage's discussion of the determinants of invention is far richer than Smith's, and that Babbage included pursuing technological change as an endogenous activity. Rosenberg concludes with the statement that Babbage clearly deserves to be removed from the dustbin of endnotes and casual allusions. He writes: "Babbage was not only the grandfather of the computer; he was also the grandfather of the study of complexity in industrial economies" (p. 56).

In the second chapter of this part, "Did Marx Know the Way to Santa Fe? Reflections on Evolution, Functionalism, and Class Consciousness," Peter Matthews takes a slightly different approach. He looks at a specific idea in Marx, the idea of class, and relates that idea to some Santa Fe themes, exploring whether one can derive a positive value for class consciousness from those Santa Fe themes. He does so, showing that the Santa Fe complexity approach is compatible with the evolution of class-centered norms and meta-norms, and that these norms can overcome free-rider problems. He concludes the analysis by arguing that there is a possible nexus between the two schools that deserves closer attention. He does not argue that Marx was a precursor of complexity, but he does argue that complexity can be used to introduce some Marxian themes into economics.

In the third chapter, "Complexity in Peirce's Economics and Philosophy: an Exploration of his Critique of Simon Newcomb," James Wible argues that the nineteenth-century American philosopher and physicist, Charles Peirce, made significant contributions to economics that were overlooked because they were in the spirit of the Santa Fe complexity vision, rather than in the standard vision. Specifically, Wible looks at the economic work of Charles Peirce, and finds within it many of the themes that later come up within complexity.

Wible shows how Peirce was interested in first principles and in many ways was beyond the writers of his time, which explains why much of his work is not well known. He considers how Peirce became fascinated with evolution, and how evolution followed from the combination of biology and economics. Peirce challenged the standard thinking about economics. He specifically questioned what is meant by rationality based on utilitarianism and argued that evolution leads to higher levels of motivation that involve trust and love. Wible specifically considers the relationship between Peirce and Simon Newcomb, an astronomer and economic methodologist, showing how Newcomb did his best to limit the acceptance of Peirce's work.

Chapter 6, "The Premature Death of Path Dependence" by David Levy, is a broad-based paper that contrasts the Classical approach of Smith and Mill based on heuristics with the Neoclassical approach based on geometry and algebra. Levy suggests the history of path dependence in economics is itself a path-dependent story. Specifically he argues that within Smith's work there were significant path-dependent aspects, but that in the mathematization of economics along static geometric lines, these elements of Smith's work could not remain and had to be downplayed. As it was, path dependence was lost as often as it was found. He points out that the possibility of path dependence was suggested by William Thornton in 1868. He recounts Thornton's attack on Classical wage theory and on the notion of supply and demand on the basis of the different prices that different methods of auctions arrive at. Such differences in the equilibrium price were inconsistent with the supply and demand theory of price. Levy points out that a Classical economist, John Stuart Mill, accepted this criticism. Mill took the position that path dependence does not negate the law of supply; rather it makes a valuable addition to it.

Neoclassical economics did not accept this inclusive position on path dependence. The standard approach to supply and demand was defended by Fleming Jenkin and George Stigler. Jenkin concluded that Thornton's results assumed "an unusual state of mind," and that those results depend on a "bizarre" demand curve. Levy suggests that modem experimental literature has shown that Thornton was correct, and that the importance of the method of auctions one uses is a replicatable fact. Neoclassical economics was wrong in rejecting the idea; and Classical economics had it correct.

The final chapter in this part, "Complexity, Chaos, or Randomness: Ragnar Frisch and the Enigma of the Lost Manuscript" by Francisco Louçã, considers some of Ragnar Frisch's ponderings about chaos and complexity, which were initially presented in a speech at the Institut Henri Poincaré on April 5, 1933. Unfortunately, the text of the speech has been lost, but Louçã reconstructs its likely contents from other writings.

Frisch, who won the first Nobel Prize for his work in econometrics, was concerned about what information we could truly draw from statistical inference when we moved away from linear specifications. He discussed the problem in his Nobel Prize lecture, and argued that often the assumptions about the sampling itself are the important assumptions, not the assumptions about the universe. Frisch felt that "the analysis of the effects of alternative assumptions is very important for applications to economics." This idea has been taken up by modern complexity theorists in their discussions of patterns that we place on the data. Louçã's chapter brings out an important point. Even those economists most associated with developing standard economics often had an uneasy feeling that some of their assumptions did not do justice to the complexity of the economy.

Broader Views of Complexity

Part III is composed of three chapters that, in different ways, relate the complexity approach to economics to broader biological and sociological approaches. In the first chapter, "Will Complexity Turn Economics into Sociology?" Alex Viskovatoff contrasts the complexity research program of the Santa Fe Institute with the sociological research program of Niklas Luhmann. He begins by reviewing what he considers the central elements of the Santa Fe approach. Then he discusses Luhmann's theory of social systems. To show that this theory can be useful to economists, he draws on it to sketch out an explanation for productivity growth differentials. He concludes that "it does not appear that economics will be turning into sociology anytime soon." His reasoning is that the complexity school is still working in the tradition of orthodox political economics; it still employs a type of deductive theorizing, and is model driven, albeit using significant amounts of induction. Luhmann's work takes the stronger position that complex phenomena cannot be analyzed one by one, and that "theory must have the ability to disassemble the various causal mechanisms that are present while still being able to show how the parts make up the whole" (p. 150).

In "Marshall and the Role of Common Sense in Complex Systems" (Chapter 9), Flavio Comim takes a somewhat different look at complexity. He begins his chapter with a survey of ideas about complexity, outlining the main features of the Santa Fe approach. He then describes Marshall's views on complexity. He suggests that Marshall's appeal to common sense was a way to apply economic reasoning (on complex systems) in practice. He argues that Marshall's views on complexity, rather than just setting an historical precedent for the complexity approach, consist of an original illustration of the role of common sense and judgment in the use of economic theory, and that if the Santa Fe approach is to have policy relevance, it must address some of the issues raised by Marshall concerning the conceptualization of complex systems. If economics does not do this, the complexity approach might be restricted to "a mere substitution of a more useful for a less useful metaphor without discussion of the principles that regulate the use of the metaphor" (p. 162).

Chapter 10 by Mark Picton, "Competition, Rationality, and Complexity in Economics and Biology," relates the complexity view of economics with biological approaches to competition. Picton argues that the Darwinian concept of the survival of the fittest has been misunderstood in economics in the form of the "rational man" assumption—the assumption that economic man is guided by self-interest and selfishness.

The chapter explores in depth the influence of classical political economy on Darwin, the treatment of altruism by biology and economics, and the challenge to the *Homo economicus* assumption from both economics and evolutionary psychology. It discusses the ambiguous use of the term "self-interest," and how evolutionary biology resolves the altruism/self-interest dichotomy by seeing pure selfishness at the gene level but not necessarily at the individual level since it is replications of the gene, not of the individual, that matter. The gene uses organisms or groups of organisms to survive, whichever strategy is the most efficient. His conclusions are that "the importance of informal institutions has been neglected and that standard policy prescriptions based on primacy of explicit and market price-like incentives or sanctions as means to influence individual behavior may be seriously misleading" (p. 206).

Alternative Perspectives on Complexity

Complexity theory is often seen as methodologically consistent with heterodox positions, and the final two chapters deal with similarities and differences between the complexity approach and heterodox approaches. In the first chapter in Part IV, "Complexity and Economic Method: an Institutionalist Perspective," Robert Prasch explores the inductive and deductive approaches to knowledge within economics. He argues that while important distinctions remain, Post Keynesians, and especially Institutionalists, should welcome developments that are coming out of the Santa Fe Institute. He briefly reviews the history of path dependencies and increasing returns, showing that they have a long history in economics; complexity builds on that, helping to highlight the role that norms and institutions play in constructing sound economic relationships that feature progress and property.

The last chapter, "Complexity Theory: an Austrian Perspective" by Michael Montgomery, is not so positive about the complexity movement. While he identifies much that he likes—the focus on path dependency, adaptive evolution, positive feedback, and importance of institutional structure—he is concerned about what he sees as the activist policy conclusions that some economists draw from the complexity approach. Specifically, he argues that path dependency does not undermine his support for *laissez faire*. He distinguishes agent self-influenced path dependence, which he sees as central in economic processes, from technologically determined path dependence, which he sees the activist complexity economists using as their model.

Conclusion

The chapters outlined above just skim the surface of the topic of complexity and the history of thought. While they are only a first step, they do provide useful insights into complexity and its historical forerunners. The story of complexity in economics is a story of vision running ahead of tools. Thus, the complexity vision is most associated with heterodox economists, but that vision was often shared by some cutting-edge standard economic theorists, who nonetheless worked in the standard approach because that was what their tools could handle. Advances in computer technology are in the process of changing that, and will likely change the nature of economics. If that happens, the way the history of economics is told will change, and this volume suggests the way in which it will likely change.

Notes

1 This was the keynote address to the conference upon which the volume is based.

References

Horgan, J. (1995) "From Complexity to Perplexity: Can Science Achieve a Unified Theory of Complex Systems?," *Scientific American,* June: 104–109.