Complexity and the History of Economic Thought

Perspectives on the History of Economic Thought

Edited by David Colander

Also available as a printed book
see title verso for ISBN details

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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

London and New York

First published 2000
by Routledge
11 New Fetter Lane, London EC4P 4EE

Simultaneously published in the USA and Canada
by Routledge
29 West 35th Street, New York, NY 10001

Routledge is an imprint of the Taylor & Francis Group

This edition published in the Taylor & Francis e-Library, 2003.

British Library Cataloguing in Publication Data
A catalogue record for this book is available from the British Library

Library of Congress Cataloging in Publication Data
History of Economics Society. Conference (1998: Montréal, Québec)
Complexity and the history of economic thought:
selected papers from the History of Economics Society Conference/
edited by David Colander.
p. cm.—(Perspectives on the history of economic thought)
Includes bibliographical references and index.
1. Economics—History—Congresses. 2. Complexity
(Philosophy)—History—Congresses. I. Title. II. Series.

HB75 .H5467 1998
330'.09–dc21
99–054695

ISBN 0-203-43600-8 Master e-book ISBN

ISBN 0-203-74424-1 (Adobe eReader Format)
ISBN 0-415-13356-4 (hbk)

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LIST OF CONTRIBUTORS

David Colander is the Christian A.Johnson Distinguished Professor of Economics at Middlebury College, Middlebury, Vermont. He has authored, co-authored, or edited 30 books and over 80 articles on a wide range of topics. He received his PhD from Columbia University and has taught at Columbia University, Vassar College, the University of Miami as well as Middlebury. He is listed in *Who's Who?, Who's Who in Education?,* etc. He has been President of the Eastern Economic Association and the History of Economic Thought Society and is currently on the editorial boards of the *Journal of the History of Economic Thought, Journal of Economic Education, Eastern Economic Journal,* and *Journal of Economic Perspectives.*

Flavio Comim is a Research Associate at Von Hügel Institute, St Edmund's College, University of Cambridge. He received his PhD from the University of Cambridge in 1999, with the thesis *Common Sense Economics: Essays on the Role of Common Sense in the History of Economic Thought.* His areas of research comprise history of economic thought, economic methodology and development economics (with emphasis on poverty issues). He has papers forthcoming in *HOPE, Review of Political Economy* and *Structural Change and Economic Dynamics.*

David M.Levy is Associate Professor of Economics at George Mason University, Fairfax, Virginia. He is currently a member of the American Statistical Association's Committee on Professional Ethics. His book *How the Dismal Science Got its Name: Classical Economics and the Ur-text of Racial Politics* will be finished this year. Levy is currently an Erdos-4, Buchanan-2, Tullock-2.

Francisco Louçã is Professor of Economics at Lisbon Technical University. He was educated at Lisbon and the University of Sussex. His research focuses on methodology and history of complexity in economics (for example, "Intriguing Pendula," *Cambridge Journal of Economics,* and "Nikolai

Kondratiev and the Early Consensus and Dissensus in History and Statistics," *History of Political Economy*). In 1997 he published *Turbulence in Economics*. His book with Christopher Freeman, *As Time Goes By* is to be published in 2000.

Peter Hans Matthews is a professor of economics at Middlebury College. He received his PhD from Yale in 1995. His interests include quantitative methods, both mathematical and statistical, and the history of economic thought. He received a BA (Hons) from McGill University, an MA from Queen's University at Kingston. His most recent work includes "An Econometric Model of the Circuit of Capital," forthcoming in *Metroeconomica,* "What Else Do Bosses Do? A Neo-Hobbesian Model of the Promotion/Discipline Nexus," *Review of Radical Political Economics* (October 1998), and "The Evolution of Class Consciousness: A Game Theoretic Perspective," forthcoming in Eiman Zein (ed.), *Thinking Economics at the End of the Twentieth Century.*

Michael R.Montgomery is Associate Professor of Economics at the University of Maine, Orono, and Visiting Associate Professor of Business Economics at Indiana University, Bloomington. His research centers on the Austrian theme of capital's role in economic theory, and the empirical implications of capital to mainstream macroeconomics. He received his PhD from the University of Florida and has taught at Auburn University, University of Houston, as well as the University of Maine and Indiana University. His articles have appeared in *Public Choice, Journal of Macroeconomics,* and *Economics Letters,* and in other journals and scholarly volumes.

Mark Picton is a research student and Assistant Lecturer at Monash University, Victoria, Australia. His research interests include public finance, public choice, methodology and the economics of altruism. Most of these interests arise from a desire to understand why he gave blood on his eighteenth birthday.

Robert E.Prasch is currently Visiting Assistant Professor at Vassar College; his previous appointments were at the University of Maine and San Francisco State University. He received his doctorate from University of California at Berkeley in 1992. He writes in the areas of history of economic thought, institutional economics, and public policy. He has published over fifty articles, book chapters, and reviews in academic journals such as *History of Political Economy, Review of Social Economy, Journal of Economic Issues,* and *Journal of the History of Economic Thought.*

Nathan Rosenberg is the Farileigh S.Dickinson, Jr Professor of Public Policy at Stanford University. He received his undergraduate education from Rutgers University and his PhD from the University of Wisconsin. His research interests are the economics of technological change, the economic role of science, and economic history and development. His books include *How the West Grew Rich* (with L.E.Birdzell, Jr) (1986), *Technology and the Pursuit of Economic Growth* (with David Mowery) (1989), *Exploring the Black Box* (1994) and *The Emergence of Economic Ideas* (1994).

Alex Viskovatoff is a Visiting Fellow at the Center for Philosophy of Science at the University of Pittsburgh. He received his PhD from Harvard University, has taught at Harvard College, Central European University, and Kuban State University in Russia, and has been a research fellow at Erasmus University, Rotterdam. Aside from computational economics, his research interests include social theory and economic methodology. He is currently working on a book exploring the implications for economics of the Kantian philosophy of science.

James Wible is the Carter Professor of Economics at the University of New Hampshire. He has authored a book, *The Economics of Science* and twenty articles on macroeconomics, the history of economic thought and economic methodology. He received his PhD from Pennsylvania State University and has taught at Penn State, Maine, Northeastern, and New Hampshire Universities. During the spring semester of 1998, he was a visiting scholar in economics at Duke University. In 1993–94, he was a Faculty Scholar at the University of New Hampshire. While in graduate school for economics, he spent a year as a fellow in Penn State's Interdisciplinary Graduate Program in the Humanities. In that program, he studied philosophy of science, American pragmatism, and cognitive psychology.

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.

Part I

INTRODUCTION TO COMPLEXITY AND THE HISTORY OF ECONOMIC THOUGHT

1

WHAT IS COMPLEXITY?

James Wible[1]

Economic phenomena are complex social phenomena. In today's intellectual milieu, this is hardly a novel statement. Dynamic and complex systems theories are now part of the intellectual landscape of the contemporary natural and social sciences. In economics, the mathematics of complex, dynamic systems modeling is now being developed for application to the problems of economic growth, increasing returns, and general equilibrium. Rather than exploring contemporary research in complexity in economics and some of the natural sciences in great detail, in this chapter the focus of attention will be on general aspects of a theory of complexity.[2] An account of complexity is provided which is broad enough to serve as a background for an exploration of various approaches and contributions to complexity theory in both the natural and social sciences.[3] In the paragraphs which follow, the contributions of three mostly compatible conceptions of complexity will be explored. The ideas of natural scientists Gregoire Nicolis and Ilya Prigogine will be presented first, followed by an overview of F.A.Hayek's theory of complexity, and the ideas of those at the Sante Fe Institute will be presented last.

Prigogine and Nicolis on complexity

In the last decade or so, several lines of scientific inquiry have coalesced to form an area of mathematical-scientific investigation now known as complex systems theory. One of its narrower sub-cases, chaos theory, also has gained widespread attention. As economists know, complexity theory is essentially a new stage of systems theory. Systems theory existed before the appearance of complexity theory.[4] However, the idea of complexity has been around for a long time. Thus, in effect there may be older and newer versions of complexity theory. The newer complexity theory can be characterized best by exploring one of the major overviews by two prominent contributors to the field. Gregoire Nicolis and Ilya Prigogine (1989) have published an account of complexity in their book, *Exploring Complexity*. Their work, while it mostly focuses on the natural sciences, also provides some applications to society. In the natural sciences, their survey includes physics, chemistry, biology, materials science, climatic change, cosmology, information theory, and insect societies. They also consider information theory and self-organization in human systems.

Since the end of the nineteenth century, Nicolis and Prigogine maintain that complexity theory has dramatically revised our vision of science and the universe. Essentially, they claim that complexity theory is in the process of revolutionizing science, not suddenly, but over many decades. The boldness of their claim invites extensive quotation. In the prologue to their work, they portray the last century of science in the following way:

> The two great revolutions in physics at the beginning of the century were quantum mechanics and relativity. Both started as corrections to classical mechanics…Today, both of these areas of physics have taken an unexpected "temporal" turn: Quantum mechanics now deals in its most interesting part with the description of unstable particles and their mutual transformations. Relativity, which started as a geometrical theory, today is mainly associated with the thermal history of the universe…
>
> The history of science during the three centuries that followed the Newtonian synthesis is a dramatic story indeed. There were moments when the program of classical science seemed near completion: a fundamental level, which would be the carrier of deterministic and reversible laws, seemed in sight. However, at each such moment something invariably did not work out as anticipated…Today, wherever we look, we find evolution, diversification, and instabilities…
>
> At the beginning of this century, continuing the tradition of the classical research program, physicists were almost unanimous in agreeing that the fundamental laws of the universe were deterministic and reversible. Processes that did not fit this scheme were taken to be exceptions, merely artifacts due to complexity, which itself had to be accounted for by invoking our ignorance, or lack of control of the variables involved. Now, at the end of this century, more and more scientists have come to think, as we do, that many fundamental processes shaping nature are irreversible and stochastic; that the deterministic and reversible laws describing the elementary interactions may not be telling the whole story. This leads to a new vision of matter, one no longer passive, as described in the mechanical world view, but associated with spontaneous activity. This change is so deep that we believe that we can truly speak of a new dialogue of man with nature.
>
> (Nicolis and Prigogine 1989:2–3)

Although Nicolis and Prigogine profess to offer no simple or precise definition of complexity or complex systems, they begin with the following idea. They begin with a suggestion that complexity implies:

> a *pluralistic view* of the physical world, a view that can accommodate a world in which different kinds of phenomena coexist side by side as the conditions to which a given system is submitted are varied.
>
> (Ibid.: 5–6)

They continue in the next paragraph elaborating a view of the physical world as composed of many systems and sub-systems rather than one grand unified order or system of order. The obvious contrast would be to compare the pluralism and multiplicity of systems of complexity theory with the reductionism of Newtonian mechanics. Metaphorically, the Newtonian theory has been portrayed as implying that the world is one vast clockwork mechanism. And if this clock runs perfectly and if its laws of motion are known with extreme precision, then the future of the world can be predicted with unfailing accuracy indefinitely into the future.

Another key aspect of complexity which is not as clearly highlighted as the side-by-side pluralism of systems in Nicolis and Prigogine is the idea of levels of systems. They portray multiple systems of complexity in the physical, biological, and social worlds and only inadvertently imply that systems may be subject to a more general principle of organization into what one might call levels of ontological complexity. The very vocabulary of the specialized sciences such as physics, biology, chemistry, and the social sciences suggests a notion of levels of partially autonomous systems of phenomena. Furthermore, within each level there may be many partially autonomous systems which function side by side. For example, the various sub-systems and organs which make up the human body are sufficiently autonomous to be identified as separate entities. Yet they all function side by side in organismic unity within that very complex entity known as the human body. The human body exists within complex biological and social systems which must provide enough resources to sustain life itself; and for life beyond survival, there must be a whole realm of a great variety of social systems, processes, and institutions for human civilization to have evolved into its present configuration.

Other than systems existing side by side and on many different levels, another important feature for complex systems is their ability to self-organize. Self-organization involves the creation of new patterns of activity by the entities in the system. A system such as a thermal system may go from a homogeneous state of uniform temperature to one of intermeshing circles of convection. The pattern of convection can be quite complicated relative to that of a gas without any temperature variation. Two different patterns of rotation are possible and the pattern of convection which occurs is quite unpredictable. Besides self-organization, according to Nicolis and Prigogine (1989:26), complex systems also are characterized by bi-stability and hysteresis, by oscillation, and by spatial patterns. Thermal convection exhibits all of these features.

Self-organizing complex systems are by their very nature dynamic systems. A dynamic system is one which endures and evolves over time. If there is change in the system, important aspects of such change can be described by dynamic equations which capture the laws of motion of the system evolving through time. There are conservative and dissipative dynamic systems. Conservative systems have something significant which remains constant or invariant throughout the period of change. Dissipative systems typically have no conservation rules or principles. Conservative systems are reversible in time while dissipative systems are irreversible. The best-known conservative system is Newtonian mechanics, which conserves total energy, total translational momentum, and total angular momentum. One of the simplest forms of dissipative processes is friction. Friction is a form of resistance which eventually affects the functioning of a Newtonian mechanical system. Other than friction, heat conduction and diffusion are dissipative and so are social processes and many natural and biological processes.

Complex dynamic systems require new conceptions of order besides equilibrium. In a mechanical system, equilibrium is attained when all of the forces in the system sum to zero. There is either no motion or the motion occurs in a path that is reversible in time. The motion of the planets around the sun is an example of time-reversible motion. In a non-mechanical system, an order or pattern is defined as a steady state, a macroscopic property of an entire system which is not a property of the constituent entities of the system. The thermal steady state does not imply that the individual molecules of a gas are motionless or that their movement is patterned through time. Motion of an individual particle may be quite random. A thermal steady state refers to the uniform mixing of the particles in a gas mixture. The very use of microscopic and macroscopic terminology such as equilibrium and steady state implies a distinction between at least two levels of functioning for thermal systems.[5]

It is also possible for complex systems to be in states far from equilibrium. Such states may persist indefinitely and are called states of non-equilibrium. Non-equilibrium states are those in which self-organizing new patterns may emerge. Complex interlocking circles of thermal convection are an example of self-organizing patterns in non-equilibrium. Nicolis and Prigogine suggest that the universe, since the moment of creation, is a self-organizing structure in a sustained non-equilibrium pattern of evolution. Other non-equilibrium processes of self-organization are all biological organisms, life itself, climate and the weather, most social processes, and the origin and continued evolution of the universe on a cosmic scale. Non-equilibrium is pervasive and seems to be the more general case than mechanical equilibrium, which appears in quite limited and very rigid contexts.

One aspect of complex systems which has garnered a great deal of attention is chaos.[6] Chaotic phenomena mostly occur in dissipative systems and can be described in part as the appearance of turbulence in the system. Weather and climate are two good instances of thermal turbulence. The movement of some particles may show chaos. Chaotic trajectories would be the time paths of entities in phase space when a system is confronted with a sizable perturbation. Chaotic, turbulent behavior is irregular and non-periodic. Often it is both patterned and unpredictable. One very simple model of chaos, the Rössler model, gives rise to unpatterned rotational chaos. This model describes the movement of a particle in three dimensions around an unstable focus using three dynamic equations. Two of the equations are linear and the third is non-linear.[7] The variable rotational path of the equations of evolution of the system means that the behavior of the system is non-periodic. Furthermore, the type of chaos depends on the initial conditions of the system. If the system is started on one side of the vertical plane through the origin, screw chaos appears. If started on the other side, the system exhibits spiral chaos.

Hayek's theory of complex phenomena

Before complexity theory of Nicolis and Prigogine, there was another relatively brief, explicit account of a world with complexity. In the mid-1960s, Hayek (1967a, 1967b, and 1967c) wrote several essays on complexity. Perhaps the most important idea in Hayek's "Theory of Complex Phenomena" is the conception of different levels of existence. Hayek portrays the world and many of the entities and processes in that world as complexities of many levels:

> It has occasionally been questioned whether the phenomena of life, of mind, and of society are really more complex than those of the physical world. This seems largely due to a confusion between the degree of complexity of a peculiar *kind* of phenomenon and the degree of complexity to which, by combination of elements, any kind of phenomenon can be built up. Of course, in this manner physical phenomena may achieve any degree of complexity.
>
> (Hayek 1967a:25–26)

In a long footnote to this passage, Hayek relates the idea of complexity to von Neumann's work. In that note, Hayek suggests one way of imagining the number of entities in any level of order. He describes the levels in exponential terms:

> It may be useful to give here a few illustrations of the orders of magnitude with which biology and neurology have to deal. While the total number of electrons in the Universe has been estimated at 10^{79} and the number of electrons and protons at 10^{100}, there are in chromosomes with 1,000 locations [genes] with 10 allelomorphs 10^{1000} possible combinations; and the number of possible proteins is estimated at 10^{2700}.
>
> (Ibid.: 25, n. 8)

For complex phenomena, Hayek maintains that it is difficult to predict individual events. Instead he argues that many events must be observed. Often the events being observed form a pattern. It is the pattern which is potentially predictable rather than individual events. For many patterns, prediction may give way to a weaker criterion. At most what may be achieved is an explanation of the principle or the central factors which give a systemic pattern its characteristic appearance. Hayek is also critical of statistics. Essentially, he maintains that statistics focuses on the wrong level of analysis. Statistics focuses on classes of phenomena at a lower level of explanation than the one on which the pattern appears. Consequently, knowledge of the structure generating a pattern of complex phenomena could be lost through a preoccupation with a-theoretical statistics.

As an example of complex patterns and his theory of complexity, Hayek (1967a:31) chooses Darwinian evolution. With the theory of evolution, an understanding of the growth and functioning of various classes of organisms only can be understood in patterns above the level of the individual creature. The theory deals with "pattern building forces" and not with conditions which determine the condition of specific individual entities. Because the appearance of new creatures cannot be predicted, evolution does not meet the narrow, orthodox criterion of prediction if it is taken as a hallmark of the scientific method. There are possible courses of events which the theory forbids, so that aspects of the theory of evolution are conceivably falsifiable in Hayek's view.

For Hayek, pattern prediction is what is mostly possible at higher levels such as those of mind and society. Social structures are patterns of regularities which appear among the individuals in society. Again, it is difficult to translate knowledge of the patterns of social structures into predictions of specific events as more positivistic conceptions of science might require. For Hayek, economics and linguistics are two social sciences which have succeeded in building up coherent theories of the patterns of social phenomena in their domain of explanation. Other disciplines and the natural sciences are not mentioned because Hayek was writing about complexity before the recent developments in the natural sciences that were surveyed by Nicolis and Prigogine (1989) in *Exploring Complexity*. For

Hayek in economics, one of the best examples of a theory of complex social patterns is Walras' theory of general equilibrium. The fact that detailed knowledge of specific demands, supplies, and their elasticities is impossible to obtain, so that the Walrasian theory can never be a predictor of detailed economic events at the level of the individual, does not mean that the theory is without value. For Hayek, knowledge of the interrelated patterns of general equilibrium may be knowable and testable, and also quite valuable. However, Hayek (1967a:35–36) is mostly critical of general equilibrium as an understanding of the interrelatedness in actual economic activity.

Another significant aspect of Hayek's approach to complex phenomena can be found in his other writings. Like Nicolis and Prigogine, Hayek emphasizes the self-organizing nature of complex phenomena. He calls this dimension of life processes, spontaneous ordering:

> A *cosmos* or spontaneous order has thus no purpose…A cosmos will result from regularities of the behaviour of the elements which it comprises. It is in this sense endogenous, intrinsic or, as the cyberneticians say, a "self-regulating" or "self-organising" system.
>
> (Hayek 1967b:74)

Self-regulating spontaneous order is a conception which is quite old in economics. It pre-dates Darwin's work in biology and emerges from the writings of ethical thinkers preceding Adam Smith. Hayek comments on the origins of these ideas:

> It was indeed what I have elsewhere called the twin ideas of evolution and spontaneous order, the great contributions of Bernard Mandeville and David Hume, of Adam Ferguson and Adam Smith, which have opened the way for an understanding, both in biological and social theory, of that interaction between the regularity of the conduct of the elements and the regularity of the resulting structure.
>
> (Hayek 1967c:77)

The implications of ideas such as complexity, evolution, and spontaneous ordering are drawn by Hayek for the social sciences in the following way:

> The existence of the structures with which the theory of complex phenomena is concerned can be made intelligible only by what the physicists would call a cosmology, that is, a theory of their evolution. The problem of how galaxies or solar systems are formed and what is their resulting structure is much more like the problems which the social sciences have to face than the problems of mechanics; and for the understanding of the methodological problems of the social

> sciences a study of the procedures of geology or biology is therefore much more instructive than that of physics.
>
> (Hayek 1967c:76)

That contribution for which Hayek may be best known is his claim that the market or price system is a spontaneous order. Systems of exchange, trade, production, and distribution become interconnected in a way that no one individual mind has ever planned. Adam Smith's metaphor of the invisible hand, which captures this idea, has been a touchstone of economic doctrine for more than two centuries. However, for the past century or more, economic theory has been unduly focused on a mechanistic, general equilibrium understanding of the interrelatedness of markets and prices in a decentralized economy. Mechanistic systems are complex systems which adhere to conservation principles and they do not capture the general features of life and social processes. Economic processes of life and society are dissipative complex systems with irreversible evolutionary processes. Like a-theoretical statistics, mechanistic microeconomics is focused on the wrong level of complexity. In contrast to mechanistically interpreted Neoclassical economics, Hayek has kept a conception of the market system as an evolutionary, self-organizing dissipative order in view, perhaps as much as any other economist of the last century. This can be most clearly seen in the following ways. First, the idea of levels in complex systems can be seen in Hayek's insistence that prices are indicators of information. The more fundamental level of information is not prices *per se,* but the knowledge that individual transactors construct in the context of their own circumstances. Hayek calls this knowledge of particular places and times. Second, the side-by-side nature of complex systems is found in Hayek's emphasis on the coordination function of markets. The marvel of the price system is that it conveys indicators of scarcity, constraints, and possibilities from many different specialized localized situations to other parties without such knowledge. Third, like almost every other economist, Hayek recognizes the non-conservative or dissipative nature of economic activity since the economic process is characterized as a positive sum process, as being of mutual benefit, and as offering the prospect of gains from trade.

The scope of Hayek's approach to complex phenomena was quite broad. Other than economics, biology, and cosmology, Hayek made two additional significant applications of his theory of complex phenomena. In *The Sensory Order,* Hayek (1952) developed the view that the central nervous system and the senses have spontaneously evolved as self-organizing systems towards higher levels of complexity.[8] The spontaneous order of the senses and the central nervous system is mostly biological and psychological. Also, at an entirely different level, one can see the outlines of a view of science as an altogether different spontaneous order from that of the central nervous system or the commercial economy. In Hayek's methodological writings, the spontaneous order of science is an individual and social process.[9] Unique rule-governed processes and institutions have emerged to encourage and govern the search for new scientific knowledge. As any scientist would expect, the orders or complex systems of science and those of the senses are interconnected. Scientific evidence originally arose out of sense experience. One must understand the limitations on sense experience and the constructed nature of perception in order to have a viable understanding of the role of evidence in science.[10]

Santa Fe and complexity

An initial exploration of the theme of complexity in relation to economics would be incomplete without some mention of the research on complexity and economics being done at the Sante Fe Institute. The story of Sante Fe can be found in Mitchell Waldrop's (1992) book, *Complexity.* Sante Fe was constituted as an interdisciplinary institute to conduct research on some of the most fundamental problems of science in the mid-1980s. One of the original philanthropists contributing money to the institute was John Reed of Citicorp who hoped to find a fundamentally new approach to understanding the world economy. In 1987, an economics workshop lasting ten days was convened at the institute. Ten economists and ten physicists were invited as the major participants of the workshop.[11] The economists were chosen by Kenneth Arrow and the physicists by Philip Anderson. Anderson and Arrow had both won Nobel prizes in their respective fields. Anderson's prize was for contributions in condensed matter physics and Arrow was rewarded for his contributions to economic theory. Among the economists invited to attend the workshop, the one who later became most closely associated with the institute was Brian Arthur who eventually became its director. Another well-known participant was Thomas Sargent.[12]

The conception of complexity which emerged at Sante Fe has many similarities with those of Nicolis and Prigogine. The Sante Fe idea similarly embraces aspects of complexity such as levels and hierarchies, self-organization, conservative and dissipative dynamic systems, notions of order other than equilibrium, chaotic phenomena, the pluralism of physical systems, and that cosmic disequilibrium phenomenon, the "big bang." There are also some striking similarities to aspects of Hayek's theory of complex phenomena. Like Hayek and under the influence of Brian Arthur, the economy is viewed as an emergent, self-organizing evolutionary process. Agents classify knowledge and arrange it hierarchically and make decisions based on a cognitive hierarchical representation of what they "know" and expect about their highly localized world. The economy is most likely in sustained disequilibrium.

In addition to the ideas expressed above, there are additional aspects of complexity which are brought to the debate by the Sante Fe workshop and by Brian Arthur. Some of the most important of Arthur's contributions were the ideas of increasing returns and positive feedback. Arthur (1989) thought that economic processes were life processes which often exhibited increasing returns and positive feedback. He had many examples: the QWERTY keyboard in typing and computing and the dominance of VHS technology in the development of video recorders have become two of the best known.

A second aspect of novelty in the Sante Fe approach is an emphasis on computer simulation. One of the unifying aspects of the Sante Fe approach to complexity is an attempt to model the dynamic properties of as many complex adaptive systems as possible. Physical, chemical, biological, and economic systems were to be modeled with state-of-the-art computational techniques. The modeling focused on systems with simple rules, with minimal decentralized information on the part of individual entities in the system, and on systems with an ability for non-chaotic patterns of evolution and self-reproduction. At times, the Sante Fe group seemed to view these simulations as a new form of experimentation which they called "computer experiments."[13] There seems to be an attitude that their models can realistically capture the most important rules governing complex adaptive systems in the real world.

In some respects, the Sante Fe approach is consistent with Hayek's emphasis on pattern prediction. Hayek maintained that detailed prediction of future events is difficult given the complexity of economic phenomena. The goal of a social science like economics should be pattern prediction rather than the forecasting of isolated, time-specific events. For Hayek, the goal of prediction involves a mistake of levels. Social phenomena are not necessarily predictable at the level of the individual. The Sante Fe emphasis on computer simulation gives new meaning to the notion of pattern prediction. Apparently very complex dynamic processes can be generated on computers which mimic in many important ways the most important features of real life economic processes. Because there is a pluralism of levels in the Sante Fe view of complexity, computer simulation of aggregate economic events does not necessarily make narrow predictions about outcomes for specific individuals. Apparently, the Sante Fe approach avoids some of the stronger reductionisms of Neoclassical economics which would reduce all economic phenomena to the level of an optimizing individual. Whether computer simulations, no matter how sophisticated, can fully encompass life processes and the higher mental processes of culture, science, and economics without reducing humankind to intelligent machines involves a philosophical debate about artificial intelligence which is beyond the scope of this essay.[14]

Those contributing to the Sante Fe workshops in economics have come to believe that a new approach to economics is being created. Brian Arthur's views have been summarized as follows:

> Here was this elusive "Sante Fe approach": Instead of emphasizing decreasing returns, static equilibrium, and perfect rationality, as in the neoclassical view, the Sante Fe team would emphasize increasing returns, bounded rationality, and the dynamics of evolution and learning. Instead of basing their theory on assumptions that were mathematically convenient, they would try to make models that were psychologically realistic. Instead of viewing the economy as some kind of Newtonian machine, they would see it as something organic, adaptive, surprising, and alive….
>
> Indeed says Arthur, it was only during those conversations in the fall of 1988 that he really began to appreciate how profoundly this Sante Fe approach would change economics. "A lot of people, including myself, had naively assumed that what we'd get from the physicists and the machine-learning people like Holland would be new algorithms, new problem-solving techniques, new technical frameworks. But what we got was quite different—what we got was very often a new attitude, a new approach, a whole new world view.
>
> (Waldrop 1992:252, 255)

Conclusions and consequences

In this brief survey, it can be seen that Gregoire Nicolis, Ilya Prigogine, and Brian Arthur have made the claim that complexity theory is creating a new vision of science. Most fundamentally, complexity is the idea that there is a pluralism of levels, systems, and dynamic patterns of phenomena in the natural, biological, and social worlds. Complexity is emerging from research on complicated processes of evolutionary change in many different disciplines, particularly in the natural sciences. Instead of the dramatic suddenness of a Kuhnian scientific revolution, complexity as a new vision of science appears to be emerging just like its subject matter, as a spontaneous subject of inquiry in many different domains of scientific investigation. By way of contrast, another contributor has claimed that complex phenomena have been studied for over two centuries. F.A.Hayek has written several articles which constitute a theory of complex phenomena. He has maintained that important aspects of complexity can be found in the ideas of Classical political economists. Hayek has interpreted the Smithian "invisible hand" metaphor as an evolutionary concept which had significant influence on Darwin's theory of evolution. For Hayek, aspects of complexity constitute some of the earliest and most fundamental ideas in the discipline of economics.

Regardless of whether complexity is viewed as a new or an old concept in economics, it could be quite useful in constructing a moderate critical appraisal of mainstream economics. Too often critics want entirely to reject

Neoclassical economics in favor of some non-Neoclassical alternative. Complexity might prove useful in a more refined rather than a blunt all-or-none critique of Neoclassical theory. Complexity theory might provide a conceptual context in which the limitations of mainstream Neoclassical economics could be meaningfully portrayed. The general outlines of such a portrayal would take advantage of notions of levels and of conservative and dissipative systems. In terms of complexity theory, most Neoclassical models present a view of the economy and its constituent components as conservative, mechanistic social processes or systems. Such systems are real, they do exist, but they are far from a complete picture of economic phenomena. Instead, economic processes appear to be self-organizing, dynamic evolutionary systems in sustained patterns of non-equilibrium. Furthermore, in complexity theory, conservative and dissipative systems exist side-by-side and at different levels of organization. This suggests a conception of fundamental complementarity between mechanistic systems and life processes in a social science like economics. If this is so, then Neoclassical and evolutionary approaches to economics might be complementary to each other in some fundamental way. Perhaps Neoclassical models focus more on the rigid and deterministic aspects of existing economic processes, while the non-Neoclassical alternatives focus on those evolutionary processes which take economic activity in new directions to unfathomed levels of social, economic, and cultural achievement. Taking this idea one step further, it would appear that the pluralism of the discipline of economics is itself a complex phenomenon. Many of the various theories and schools of economics could be specializing in different kinds and levels of complex phenomena in the economy. The multiplicity of schools of inquiry in economics could be a consequence of the actual complexity of economic phenomena in the economy itself. Thus complexity in conventional economic processes might necessitate disciplinary complexity and pluralism in the economics profession.

Appendix

The three equations of the Rössler model as presented in Nicolis and Prigogine (1989, pp. 124ff.) are:

$$\frac{dx}{dt} = -y - z$$

$$\frac{dy}{dt} = x + y$$

$$\frac{dz}{dt} = bx - cz + xz$$

Notes

1 The initial version of this chapter was written while on sabbatical at Duke University during the spring of 1998. I would like to thank the Department of Economics and my three hosts, Craufurd Goodwin, Neil DeMarchi, and Roy Weintraub, for their graciousness in having me as a guest in their seminars for the first few months of that year. Also I would like to thank Paul Wendt for commenting on an earlier version of this chapter.

2 Various aspects and conceptions of complexity are enumerated and explored by science writer John Horgan (1995) in a widely read article in the *Scientific American*. This article forms the basis of a chapter in Horgan's (1997) book, *The End of Science*. In the article, Horgan reports that Seth Lloyd has enumerated 31 definitions of complexity. In a note to chapter 8 of the book (Horgan 1997:303, n11), Lloyd's list has expanded to 45 definitions. Rather than being 45 definitions of one idea, the various definitions seem to portray different aspects of complexity. Many of these different "definitions" are encompassed in the literature which follows in this survey. An excellent survey of complexity theory and research in the natural sciences is a recent work, *Exploring Complexity: An Introduction,* by Gregoire Nicolis and Ilya Prigogine (1989). There is also a statement on complexity in economics which can be found in F.A.Hayek's (1967a) "Theory of Complex Phenomena" and several other of his works. More recently, notions of complexity in relation to economics have been explored by Brian Arthur (1988, 1995), Kenneth Arrow (1988), Mitchell Waldrop (1992), and others at the Sante Fe Institute.

3 The views of complexity included here are juxtaposed as though they seamlessly provide one overarching meta-theory of complexity. Whether it is appropriate to place them side by side in this manner is beyond the scope of this essay. Obviously the writings of other economists, philosophers, and scientists could have been included in a much more comprehensive presentation of notions of complexity in science and economics. It would be useful to mention others, such as Nicholas Georgescu-Roegen, Kenneth Boulding, David Bohm, and Karl Popper, who seem to have conceptions of complexity similar to those mentioned here. The relevant works are Georgescu-Roegen's *The Entropy Law and the Economic Process* (1971), Boulding's (1968a) "General Systems Theory," Bohm's *Causality and Chance in Modern Physics* (1957), and Popper's (1972b) essay, "Of Clouds, and Clocks." Complexity is also an idea which needs to be used in interpreting Keynes' *General Theory,* although that subject has not been raised in a general way. Wible (1995) considers some consequences of interpreting Keynes' *General Theory* in the context of a theory of complexity like that found in Popper (1972a). Issues about complex dynamic systems more recently have been raised by Philip Mirowski, Roy Weintraub, and Wade Hands. Mirowski's (1989) *More Heat than Light* was in part inspired by Georgescu-Roegen's *Entropy*. Weintraub's (1991) *Stabilizing Dynamics* was partially influenced by Mirowski (1989). Hands (1994) is a reply to both Weintraub and Mirowski. All of these possible extensions are simply not permitted by the scope of this chapter.

4 Boulding (1968b) and Leijonhufvud (1968:387ff.) discuss general and dynamic systems theories respectively.

5 One issue is whether the distinction between micro- and macroeconomics is one of two different autonomous levels of complexity. If so, then one would have to reevaluate the aggregation problem. It would seem that it might be difficult if not impossible to explain macroeconomic patterns based on theories and evidence from another level of complexity. The search for microfoundations of macro could be fundamentally misdirected.

6 A good survey of chaos and research in that area is James Gleick's (1987) work, *Chaos*.
7 The three equations of the Rössler model as presented in Nicolis and Prigogine (1989:124ff) can be found in the Appendix to this chapter.
8 Hayek's conception of complexity and its relation to his economic theories is discussed by Caldwell (1997).
9 Hayek's writings on science are mostly found in three volumes of essays (Hayek 1948, 1967d, and 1978). Butos (1987) provides an overview of Hayek's view of science as a spontaneous order.
10 Mirowski (1989:354–356) criticizes Hayek for not making enough use of his knowledge of the limitations of the natural sciences in his criticism of Neoclassical economics as being scientistic.
11 The papers given at the conference can be found in Anderson *et al.* (1988). An excellent appraisal of Sante Fe's approach was presented a few years ago by Mirowski (1995) at Notre Dame. Mirowski argues that Sante Fe has had an influence on economics analogous to that of the Cowles commission in the 1940s and 1950s. A more recent volume on economics and complexity from the Sante Fe Institute is Arthur *et al.* (1997). In 1992, the *Journal of Applied Econometrics* devoted a special issue to complexity and non-linear dynamics in economics. Pesaran and Potter (1992) introduce and edit the volume. Another interesting article in that volume is Day's (1992) piece on complexity, economic history, theory, and data.
12 The impact of the Sante Fe workshop on Sargent's research has been explored in great detail by Sent (1998). For a time, Sargent focused on learning and adaptive expectations instead of rational expectations.
13 A discussion of computer experiments can be found in Waldrop (1992:250, 268).
14 The Sante Fe approach to complexity has been unsympathetically criticized by science writer John Horgan (1995, 1997).

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2

A THUMBNAIL SKETCH OF THE HISTORY OF THOUGHT FROM A COMPLEXITY PERSPECTIVE

David Colander[1]

Complexity changes everything; well, maybe not everything, but it does change quite a bit in economics. Most of the chapters in this volume look at individual authors or schools, demonstrating their contribution to a complexity vision of economics. This chapter takes a much broader view; it sketches out, in broad outlines, some of the changes that I believe occur in the history of economic thought when one's underlying vision of economics changes from a "structural simplicity" vision to a "complexity" vision.[2] In doing so it briefly considers whether major economists' ratings would rise or fall as our perspective changes. It also briefly discusses two economists who generally do not show up prominently in current history of thought texts, but who would show up prominently in one written from a complexity vision.

Conventional histories of economic thought

Conventional histories of science are stories of how thinking progressed from informal stories and verbal metaphors that capture aspects of reality to more formal structural models. For example, a standard history of physics might go as follows: Kepler discovered informally that planets follow elliptical orbits around the sun, but he did not formalize his understanding. Newton compressed Kepler's understanding of the astronomical data into an inverse square law. Newton's compression was supplemented and extended by Einstein's theory and models which reduced the relationship of energy and mass to $E=mc^2$. Each of these laws and models is an example of structural simplicity. Each is formal and reducible into a simple equation or set of equations.

Just as conventional economics follows standard science, so do conventional histories of economic thought tell the story of how economists have formalized Adam Smith's informal understanding of how competition guides people to serve the common good. Most conventional histories agree that economics is not as far along as is physics, but they see the search for a more formal simple structural model as the right one and present the history of economics accordingly. Thus, they see the formalization of supply and demand, production functions, general equilibrium theory, national income accounting, and the concepts of capital and labor as structural simplifications that have marked progress in economics.

A two-paragraph conventional history of economic thought written for the layperson would go as follows: It all began with Adam Smith's insight of the invisible hand. Classical economics (Ricardo and Mill are usually highlighted) developed that insight, attempting to derive an acceptable theory of value based on all costs or on labor alone. It failed in that attempt, but in the process developed the seeds of many ideas that were later expanded by Neoclassical economists. Neoclassical economics differs from Classical economics in that it stopped looking for value only on the cost side, and developed a supply/demand theory of value in which both supply and demand considerations together determined value (Marshall and Walras are usually highlighted). Although this supply/demand approach was developed by a variety of economists, Walras' contribution is seen as providing the beginnings of the most general theory—that of general equilibrium.

In the 1930s Keynes developed an alternative macro theory, but by the 1990s that had fallen by the wayside. Modern economics (Hicks and Samuelson are usually highlighted) has developed general equilibrium theory and in 1959 that development reached its apex in the Arrow-Debreu theory of general equilibrium. Since then, general equilibrium theory has been further simplified and expanded upon. It has incorporated many variations, but it remains the centerpiece of modern economists' understanding of the world. Along the way to this understanding, many side roads have been followed, (Malthus, Marx, Robinson, Hayek, Veblen, and Keynes are often mentioned briefly) but these are primarily detours; the centerpiece of economic theory is general equilibrium theory.

How one tells the above story depends on the methodological approach one takes to economics. Relativist historians of economic thought, who consider economics as reflecting the problems of the time, see little development of a grand theory; they emphasize the side roads, arguing that there is no overall thread of development in thinking. Absolutist historians, sometimes called Whig historians, see economics as science; they tell the story emphasizing the main lines of development and give much less shrift to the side roads. But, within conventional economics, the trunk is still there. It is a story of the search for simplicity in structure as found in general equilibrium theory.

A complexity perspective on the history of economic thought

The complexity perspective provides a different pattern to be placed on the history of economic thought. It specifically does not accept that the gains from simplicity of structure have been worth the costs in terms of what we have lost in our informal knowledge. It sees an economics that has found a simple structure, but only at the cost of assumptions that make the theory difficult, if not impossible, to relate to empirical reality. Important elements of economic processes—path dependency, increasing returns, multiple equilibria, and technology are downplayed, and the importance of institutional structure is almost lost. The complexity approach to economics is trying to remedy that. It sees the economy as a complex system that follows the same laws as all complex systems. Complexity theory developed spontaneously, in a search for the laws of complex dynamics. The hope is that insight into large complex systems will come from the study of iterative processes involving non-linear dynamics.

The advantage of seeing economics as the result of such iterative processes is that it allows a more direct inclusion of increasing returns and institutions than does the standard approach. Complexity theory differs from conventional theory in that it holds that the complexity of the economy precludes the far-sighted rationality assumed of individuals in conventional theory. If the economy is truly complex, then individuals cannot rationally deal with every part of it, making any model based on full global rationality inconsistent with the complex structure of the model. People will develop institutions to deal with the world, and these institutions will change their behavior. Thus the data reduction program in economics cannot be held together by a general equilibrium system that assumes far-sighted rationality.

Where the complexity approach challenges conventional economics most is in its explanation of macro phenomena. In a highly complex system, the connection between micro and macro that economics has been searching for cannot be discovered. With this view the complexity approach calls into question the entire line of general equilibrium research that is the centerpiece of standard economics.

The complexity interpretation of the history of economic theory is different from the standard heterodox interpretation of the history of thought for a number of reasons. First, the complexity interpretation fits better in the absolutist approach than in the relativist approach that most heterodox economics takes. By that I mean that the complexity approach sees economics as having a grand theory, and sees the purpose of economic science as to find that theory. It differs from the standard histories in that it sees multiple potential patterns of simplification, but it is, in many ways, an absolutist history with a twist. It sees conventional economics as having followed a reasonable, but ultimately wrong, path in its search for simplicity of structure as the central trunk of the story. Instead, it suggests that there is another trunk, which finds simplification in dynamics, not static structure.

Second, the complexity approach also takes a much more sympathetic view of the earlier development of standard economics than do most heterodox approaches to the history of economics. The reason is that, until recently, the high-powered computers and mathematics to explore the alternative complexity simplification path did not exist, so the complexity approach was not viable. What makes it viable now is the development of computer technology that has so increased our ability to get numerical solutions to complicated non-linear equations and to see the implications of iterative processes involving non-linear equations that it changes the way it is reasonable to look for patterns in data. The point is often made with the "12-cent Ferrari" example. Had the same degree of increases in technology occurred in the automobile industry as has occurred in the computer industry, automobiles would be essentially free and a Ferrari would cost 12 cents. That would change our transportation pattern; shouldn't it also change our research pattern?

Before advances in computers made dealing with more complex systems possible, it was only reasonable to explore the alternative structural simplicity path. Thus, many economists may have had a complexity perspective, but they did not follow up on it in their formal work because they did not have the tools to do so. Numerous examples can be given.[3] Consider Joseph Schumpeter's discussion of multiple equilibria. He writes:

> Multiple equilibria are not necessarily useless, but from the standpoint of any exact science the existence of a uniquely determined equilibrium is, of course, of the utmost importance, even if proof has to be purchased at the price of very restrictive assumptions; without any possibility of proving the existence of (a) uniquely determined equilibrium—or at all events, of a small number of possible equilibria—at however high a level of abstraction, a field of phenomena is really a chaos that is not under analytical control.
>
> (Schumpeter 1954)

John Hicks had a similar understanding of the problem. He writes that serious consideration of increasing returns could lead to the "wreckage of the greater part of general equilibrium theory" (1946:84). My point is that many earlier economists understood the implications of complexity, of which increasing returns and multiple equilibria are important elements, but they also recognized that they did not have the tools to deal with it formally, and therefore did not work on them. The role of Kenneth Arrow, one of the economists who made a major contribution to general equilibrium theory but who also was central in establishing the Santa Fe Institute that has explored complexity within economics, is another example.

Any history of thought told from a complexity perspective must also recognize that while conventional economics used general equilibrium theory as its core organizing feature, much of the development of applied economics was not dependent on general equilibrium theory. Other avenues developed, including much work that considered increasing returns, and much of the complexity work will build on that work. It is not the standard practice of economics but the formal general equilibrium centerpiece of economics that complexity challenges.

Conventional wisdom and our intellectual predecessors

Now that I have given a general discussion of how switching to a complexity perspective changes the way we view economics, let me consider how previous writers fare in the changed perspective. I consider the issue in three separate sections. First, I consider general schools of economics; next I consider some economists who would move up or down in my rankings. Finally, I briefly discuss two individuals who, from our current standpoint, are relegated to footnotes, but who would enter into the panoply from a complexity perspective.

I should make it clear that the discussion is a "reporter's treatment" of the field, not a specialist's treatment. That approach was the only way to get the breadth of coverage that I wanted within the space limitations. I leave it to the other authors in this volume to provide in-depth consideration of economists and schools.

Assessment of schools of thought

In the standard history of economics text, the mainstream, or Neoclassical, school is given primary focus, but students are introduced to various other schools including the German historical school, the Austrian school, American Institutionalists, and the radical-Marxian school. The general views of each of these schools is presented, and contrasted with the mainstream development from Classical to Neoclassical to modern.

In a history of thought told from a complexity perspective, the story about these heterodox schools would be much the same as is currently told, but told a bit more sympathetically. Many of the heterodox objections to standard economics would be presented as precursors of the modern complexity school's objections: The German historical school opposed the equilibrium method, believing instead that history mattered and that different periods could be unique; the Institutionalists opposed Neoclassical theory as too simple; Austrians saw the market mechanism as an example of spontaneous order, again providing a precursor to the complexity focus on emergent order; radicals saw that there was more than a single way of looking at the economy.

The above argument deals with the presentation. But the assessment of these heterodox schools of thought would not significantly change. Although one can find many areas where the schools were precursors of complexity work, complexity economics did not develop from these schools. Complexity economics developed out of standard economics and is best seen as a natural evolution—the economics field responding to the changing technology. The complexity approach shares with standard economics a focus on formal mathematics and maintaining a formal scientific approach. Most modern institutionalists argue against the use of formal mathematics; radicals replaced a Neoclassical ideological bias with an alternative bias, claiming that bias was inherent in science; Austrians relied on deduction to the almost total exclusion of induction; and the German historical school was anti-formalist in almost all the forms.[4] Thus, in my view, none of these schools would have led to complexity economics. They would have likely led standard economics elsewhere. Thus, from a complexity perspective, these heterodox schools are seen in roughly the same light that they are seen now by sympathetic interpreters—as schools that had some of the important insights, but which were before their time.

Assessment of individual economists

While the assessment of schools may not change much, the assessment of individual economists will. Table 2.1 provides my fast and dirty overview of selected economists' change in rankings when they are considered within a complexity framework. The discussion gives a brief justification for that assessment.

Table 2.1

Economist	*Change in ranking*
Smith	up
Malthus	neutral
Ricardo	down
Mill	up
Marx	up
Walras	down
Marshall	up
Hayek	up
Keynes	neutral

Adam Smith

Smith goes up in the rankings because of his recognition of the problems of complexity, his emphasis on increasing returns, and his vent for surplus argument, all of which can be seen as leading to ideas that are consistent with the complexity perspective. In the conventional texts Smith is presented as “the father of economics”—the person who codified economic thinking—but he is also presented as a fuzzy thinker whose informal reasoning allowed many inconsistencies. He had a variety of theories of value, a confused presentation of the price level, and unclear policy prescriptions. Later Classical economists had to clarify these. From the complexity perspective, Smith is still inconsistent, but there is good reason for that inconsistency since the mathematical techniques had not yet developed to handle the issues that he recognized were important. His emphasis on vent for surplus, increasing returns, and the need for a broader setting for economics fits well with the complexity vision, as does his proclivity for unclear policy prescriptions—the complexity approach to economics does not lead to definitive policy prescriptions.

Thomas Malthus

Malthus stays neutral in the ranking, but it is a neutrality that results from his rising in one dimension and falling in another. He rises because of his work on population; Malthus saw that the interesting issue in understanding economics was in iterative processes. His ideas on population were central in the development of Darwin’s ideas, which has a close tiein with the complexity perspective. He falls in the rankings because the dynamic process that he emphasized was one that focused on diminishing returns. He did not allow for the development of ongoing endogenous growth. He further falls in the ranking because his macro argument that general gluts were possible fails to use any of the insights of complexity, and thus helped reinforce the view that if one approaches the macro economy logically there can be no unemployment.

David Ricardo

Ricardo is seen in many ways as the hero in the structural simplicity vision; he took Smith’s ambiguous ideas and formalized them into economic laws. The standard history of thought emphasizes how his theory of comparative advantage is one of the greatest developments in economics. It is also generally acknowledged that he developed the labor theory of value as far as it could be developed (the 93 percent labor theory) and clarified the argument for trade. With his theory of rent, he was a precursor of the Neoclassical marginal productivity theory.

In part Ricardo falls because, with his current high ranking, there is nowhere to go but down. While his formalization helped make Smith's ideas specific, that same formalization led economists to the focus on structural simplicity and took away the emphasis found in Smith on increasing returns. Ricardo's search for value within a unique equilibrium system made the theory simpler than it otherwise would have been and directed economic researchers away from taking seriously the complexity of the economy.

John Stuart Mill

Mill is generally presented as the mature Classical economist who had little to add to formal Classical thought. He made a few contributions: He pulled Classical economics towards a humanist mode; he led Classical economics away from the wages fund theory; and he drew a distinction between the social laws of distribution and the technical laws of production. With these insights he moved away from the labor theory of value, but he did not abandon it. In the standard view, he receives high ranking, but not at Ricardo's level. From the complexity perspective, Mill's ranking increases. His focus on social laws of distribution was an important development in recognizing the complexity of the economy. He recognized that social and economic phenomena couldn't be divided. He saw the importance of path dependency in a way that Ricardo did not. (See David Levy, Chapter 6 in this volume.) His nervous breakdown can be seen as a natural reaction when an individual tries to fit the complex world into too simple a structure.

Karl Marx

Marx is generally presented as a Classical economist who did not share the mainstream ideological views. He turned the labor theory of value into a theory of exploitation and, in doing so, showed some deeper problems with it. He focused on increasing returns, the tendency of economies to grow, and how distribution was tied into that growth process.

Marx's ranking improves from a complexity perspective not only because of his formal analysis, but also because of his informal analysis of class, and his emphasis that individuals' rationality is shaped by the social situation around them. His failure to see social relations evolving to maintain stability in the economy still limits his being considered a precursor of complexity economics.

Léon Walras

The standard view of Walras is that he led economics into general equilibrium theory as well as that he developed marginalism. He receives high marks for both. In the complexity perspective, since general equilibrium and marginalism fall in the ranking, it is not surprising that Walras also falls. Walras directed economics into a particular mathematical structure that did not fit.

Alfred Marshall

In the standard history Marshall receives a positive, but subdued, treatment. He played a key role in introducing supply and demand concepts, but he simultaneously failed to formalize theory. Thus he is often accused of following the "zigzag windings of the flowery path of literature," and his work is characterized by Paul Samuelson as having "paralyzed the best brains in the Anglo-Saxon branch of our profession for three decades."[5] In his *Principles of Economics* Marshall struggled with how to include both the Classical focus on dynamics and growth—which called for explicit treatment of increasing returns—and the Neoclassical focus on diminishing marginal returns in the production of goods.

As Marshall typically did when faced with conflicts, he used what might be called "the Marshallian Straddle" to circumvent them. He discussed each but never integrated them. Still, his stature rises somewhat relative to Walras because the reason he refused to formalize was precisely that his perception of the economy was that it was complex and that the tools were not up to formalizing the insights; he recognized the limitations that Walras did not. He rejected any simple specification of general equilibrium and in Note 21 in *Principles* argued that the true foundation of aggregate economics was to be found in thermodynamics, an idea that clearly places him in the complexity perspective.

Friedrich von Hayek

Hayek usually receives only a short mention in the standard history of thought, in part because he lived until the 1980s and his work was continually evolving and, in part, because he did not fit the standard mold. Starting in the 1940s with his publication of *Abuse of Reason* (1940) Hayek's work was evolving outside of the Austrian tradition of Ludwig von Mises. Because it did so, it better fits a complexity tradition. In his vision, Hayek came very close to the modern definition of complexity; however, he did not conduct any deeper empirical or analytic research into the field of complexity.

Hayek understood the nature of complex phenomena and correctly associated them with the economy. His emphasis on spontaneous organization has complexity written all over it, as does his focus on prices as storing systemic information that individuals did not have and could not compute. In his Nobel address in 1974 he developed the arguments in his earlier work and argues that the reductionist approach of economics, which is mimicking that of the pure sciences, is inapplicable to economics since economics studies phenomena of "essential complexity, that is with structures whose characteristic properties can be exhibited only by models made up of relatively large numbers of variables."[6] In the same address Hayek also recognizes that "organized complexity" is dependent on the "manner in which the individual elements are connected with each other" at least as much as the individual properties of the agents in the system. Thus Hayek, especially in his later writings, will significantly move up in the rankings.

John Maynard Keynes

Keynes stays neutral in the rankings in large part because he currently gets such high ratings. His questioning of econometrics, his emphasis on the non-ergodicity of the economy, and his work on probability all can be seen as positive from a complexity perspective. But in policy he never shied away from the simple static models that were developed to capture his views, and never made clear that his analysis rested on a complexity perspective, even though he had opportunities to do so. Thus, the foundations of his *General Theory* were ambiguous; despite many possibilities in which he could have laid its foundations in complexity, he did not do so. His primary interest in policy took him away from theoretical issues, and his willingness to accept standard theory, if it arrived at the policy result he wanted, leave him unchanged in my rankings.

Two economists who move from footnotes to the main text

In this penultimate section I consider two economists, Charles Babbage and John von Neumann, who seldom show up prominently in standard histories of economic thought, but who have a significant place in a history told from a complexity perspective. Since Babbage's contribution is nicely discussed in the paper by Rosenberg (Chapter 3 in this volume) I will only discuss him briefly. Babbage applied complexity reasoning to the economy as he expanded Smith's ideas to the industrial age. He emphasized non-linear costs and thought of the economy within a complexity perspective. Had economics focused on his work rather than Ricardo's its history likely would have been quite different.

Unlike Babbage, who is hardly ever mentioned at all in histories of economic thought, John von Neumann frequently gets a short section in history of economics texts for his work on game theory. That work, however, is not what earns the main focus from a complexity perspective. Instead, it is his work on self-replicating systems. That work, which is a precursor to work in artificial intelligence and is related to the development of the computer, is central in the development of the complexity perspective, and places von Neumann as a pivotal figure in any history of economic thought told from a complexity perspective.

The importance of John von Neumann to economics should come as no surprise. He is already considered by many to be one of the most brilliant minds of the twentieth century. He contributed significantly to numerous branches of science, including mathematics, quantum mechanics, and computer science. He specified the minimum requirements needed for self-replicating systems in his theory of automata. That specification is an important cornerstone in complexity theory, and is particularly useful in trying to understand and simulate artificial life, which is central for understanding and simulating iterative processes.

An automaton is a general-purpose universal machine which can execute the instructions of any algorithm. There are natural automata, such as the nervous system, and artificial ones, such as computers. Von Neumann argued that, similarly to natural living systems, artificial systems should also be able to reproduce themselves. He imagined a machine floating on the surface of a lake where a lot of machine parts float as well. Following instructions, the machine—a universal constructor—assembles another machine. It can reproduce itself if it has a blueprint of itself within itself. Reproducing automata are description copiers—they include a blueprint of each machine they construct within it. The "genetic" information of the machine is thus both interpreted as an algorithm to be followed and as genetic material copied uninterpreted, as a piece of the machine being copied.[7] That work is far from standard economics but it lies at the heart of complexity theory.

Conclusion

This quick and dirty overview of the history of thought is presented from a complexity perspective. In many ways the most interesting part of the story is its relation to heterodox economists, many of whom had something close to a complexity vision. But the complexity approach did not develop from heterodox economics. The line of descent is through standard economics. The complexity approach developed from the standard approach to economics for two reasons. The first is the limitations and problems with the standard approach; it simply did not provide a meaningful model of the aggregate economy. But that failure was not enough to cause the mainstream of economics to abandon the approach, and those who did became known as heterodox economists. What was necessary for the mainstream of economics to consider the complexity vision was the second reason: a change in research technology, primarily the computer, which allowed economists to study dynamic systems that previously were beyond analytic study.

The methodology of complexity economics is quite consistent with the methodology of standard economics and, as such, does not fit well with many heterodox traditions. Still, many heterodox economists have had a complexity vision, and thus complexity economics can be seen as a melding of heterodox vision with standard scientific methods, which is only now coming of age.

Notes

1 I would like to thank Robert Prasch for helpful comments on this paper.
2 Having spent much of my time during the last twenty years writing textbooks, I have learned the importance of simplifications, and have also learned the enormous injustice such simplifications do to the various writers. The ideas of the writers discussed in this chapter, and the history of economics it conveys, are much more complex than this chapter suggests. I provide this introduction to, and quick overview of, how taking a complexity perspective would change our telling of the history of economic thought to make the ideas in this volume more accessible to non-specialists.
3 See Louçã on Frisch (Chapter 7 in this volume) as one example.
4 The schools did not necessarily start in those ways, and it can be argued convincingly that early heterodox economists were simply waiting for the right mathematics to come along. But there seems to be a path dependency argument. Arguing against current mathematical approaches attracts followers who oppose math, and thus these heterodox schools did not attract the individuals who developed complexity theory.
5 Edgeworth, (1925:II. 282) and Samuelson (1967:109).
6 Hayek (1989:4).
7 It is interesting to note that only later was it discovered that DNA within all natural living organisms has the same dual function, and that natural systems reproduce according to von Neumann's abstract method.

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Part II

SPECIFIC ECONOMISTS AND COMPLEXITY

3

CHARLES BABBAGE IN A COMPLEX WORLD

Nathan Rosenberg[1]

I would like to begin on a cautionary note. Alfred North Whitehead once remarked that everything of importance has been said before, but by someone who did not discover it. This is an especially wise observation in the realm of intellectual history.

Once we have become familiar with a Leontief input-output matrix, it is easy to "see" elements of it in Quesnay's *Tableau Economique* of two hundred years earlier. But one should not read into Quesnay's *Tableau* insights that could only have been derived from a later and much more sophisticated formulation of an idea. Ideas and theories become valuable when they are enriched by more detailed articulation and analytical structure—not to mention empirical support. After that has been accomplished, it becomes disarmingly easy to read meaning and insights into the writings of earlier writers that those writers cannot possibly have possessed.

I begin with this cautionary note because I am not entirely certain that my own discussion of Babbage on the issue of complexity will be free of the error that I have just cautioned you against. So you should be fore-warned—and forearmed.

Economics as a discipline emerged in the course of the eighteenth century in order to deal with an exceedingly complex phenomenon: the complexity of an economic system consisting of a very large number of maximizing agents, in which order was established through the operation of impersonal market forces. The house of Adam Smith was spacious; it had many rooms, indeed it even had several floors. The genius of Adam Smith was to demonstrate how the rooms in this house were all interconnected by the operations of the market place. But ironically, as it happened, the organization of economic life was on the verge of becoming far *more* complex at precisely the time when Smith's great masterpiece fell from the press. New forces were at work in the last quarter of the eighteenth century that were soon to require explicit treatment for which a reader of the *Wealth of Nations* was not prepared. An additional irony of history lies in the fact that these forces were being set in motion by Adam Smith's fellow Glaswegian, James Watt.

The most important new source of complexity in economic analysis after Adam Smith wrote was the need to incorporate the phenomenon of increasing returns into the analytical framework. Smith captured one key element in his portentous announcement "That the Division of Labor is Limited by the Extent of the Market" (title of chapter 3 of Book I of the *Wealth of Nations*). Smith went on to observe that economic growth had to be understood as a process involving increasingly complex patterns of specialization. Inherent in this growing division of labor were learning experiences that took the form of improvements in human skill and dexterity, on the one hand, and the invention of new technologies on the other. Thus, a major source of increasing returns was the development of new technologies whose effects were so pervasive that they are routinely referred to, by later generations, as the Industrial Revolution.

Smith did not, himself, pursue the increasing returns implications of the division of labor. He turned his attention to the functioning of competitive markets, an analysis that could be pursued rigorously only under the assumption of constant returns to scale. And I would like to add that such an assumption was not nearly so far-fetched in the 1770s as it was to become over the course of the nineteenth and twentieth centuries.

James Watt's ingenious invention of a separate condenser served to set into motion forces that were to add wholly new dimensions of complexity to the organization of economic life—dimensions that were to require a number of new rooms, or at least very significant alterations to old rooms, in the house of Adam Smith. I would like to argue that it was Charles Babbage who first attempted such a reconstruction, but it was premature and aborted, terms that apply just as well to Babbage's other great enterprise, for which he is far better known, the computer. I would also like to suggest that, just as the second half of the twentieth century witnessed the completion of Babbage's aborted exercise in the construction of a computer, it is now witnessing the beginning, but so far only just the beginning, of the attempt to incorporate a sophisticated treatment of increasing returns into the corpus of economic analysis. I am sure that we all share the hope that this achievement will not take as long as the construction of a working computer did after it had been first conceptualized by Babbage in the early nineteenth century.

Babbage has lived a furtive, almost fugitive existence in the literature of economics. Joseph Schumpeter, in his magisterial *History of Economic Analysis,* refers to Babbage's book, *On the Economy of Machinery and Manufactures,* as "a remarkable performance of a remarkable man" (Schumpeter 1954). Nevertheless, although Schumpeter's well-known book is more than 1200 dense pages long, the treatment of Babbage is confined to a single footnote in a discussion of the writings of John Stuart Mill, who was indeed greatly influenced by Babbage. Mark Blaug, in his excellent book, *Economic Theory in Retrospect,* uses the same adjective as Schumpeter: "remarkable" (Blaug 1978:198). But he too cites Babbage's book only to point out its influence on John Stuart Mill's *Principles of Political Economy.* Mill quoted Babbage at very great length, as also did Marx.

Babbage surely deserves better treatment than this. His book contains important contributions to economics that have received unduly short shrift. His book, at the time of its publication in 1832, provided a considerable improvement upon a topic as seminal as Adam Smith's treatment of the division of labor. But, more important for our present purposes, the book also offered the first reasonably systematic analysis of the economies associated with returns to scale which, in my view, has been a centerpiece of the increasing complexity of advanced industrial economies.[2] Such an economist surely deserves to be rescued from the dusty obscurity of footnotes and parenthetic allusions.

Babbage and the division of labor

Babbage was attempting to explain the operation of an economy that had become far more complex in the course of just several decades. This necessarily involved dealing with the institution that was central to the Industrial Revolution: the factory, a manufacturing establishment that had to be operated on vastly different principles from Adam Smith's workshop. Smith's workshop included tools, but not machines, and it was the chief purpose of Babbage's book to explain in precisely what respects production that was organized around machines had to be different from production involving only the use of tools and skilled workmen. Indeed, it was Babbage who first offered a clear distinction between a tool and a machine:

> A *tool* is usually more *simple* than a machine; it is generally used with the hand, whilst a machine is frequently moved by animal or steam power. The simpler *machines* are often merely one or more *tools* placed in a frame, and acted on by a moving power, [p. 12]…When each process has been reduced to the use of some simple tool, the union of all these tools, actuated by one moving power, constitutes a machine [p. 174].
>
> (Babbage 1835)

Marx borrowed these conceptual distinctions directly from Babbage, and used them as the starting point of his own analysis, which brought him, of course, to very different conclusions.[3]

In this context, Babbage's ambitious purpose in writing *On the Economy of Machinery and Manufactures* was to examine "the mechanical principles which regulate the application of machinery to arts and manufactures" (ibid.). Like Adam Smith, he begins with the division of labor, but then he goes on to spell out what he perceives as its far-reaching implications throughout the realms of various technologies and then eventually, as we will see, even to the progress of science. Babbage recognized that, well into the nineteenth century when he wrote, the division of labor was necessarily leading to the establishment of large factories. Indeed, Babbage provided the first extended discussion in the literature of economics of an issue of immense future significance: the economies associated with large-scale production. The chapter devoted to this topic, Chapter XXII, "On the Causes and Consequences of Large Factories," in turn powerfully influenced the treatment of this topic by two of the most influential, perhaps *the* two most influential, economists of the nineteenth century, John Stuart Mill and Karl Marx.

Babbage had shown earlier, in Chapter XIX, "On the Division of Labour," that a critical advantage of that division was that it enabled the employer to purchase only the precise amount of each higher skill category, and no more, that was required by the different processes under his roof. Ideally, although the ideal was hardly ever fully achieved, no worker was ever paid at a rate that was higher than that appropriate to his assigned activity. But in Chapter XXII he specified an important implication of such an arrangement. In order to produce at minimum cost, it will be necessary to expand the factory by some multiple whose size will depend upon the specific labor requirements imposed by the division of labor. It follows from the principle of the division of labor that:

> *When the number of processes into which it is most advantageous to divide it, and the number of individuals to be employed by it are ascertained, then all factories which do not employ a direct multiple of this latter number, will produce the article at a greater cost.*
>
> (Ibid.: 212, original emphasis)

Thus, Babbage's well-known "improvement" upon Smith's analysis of the division of labor was also one that recognized an important efficiency improvement that was uniquely available to firms of larger size.

But the benefits of larger size do not follow only from the division of labor. There are other advantages. The most common denominator involves certain fixed costs that would fail to be fully utilized in smaller establishments. These include the availability of higher-wage workmen who are skilled in the repair or adjustment of machinery (although Babbage does not make it clear why such a worker needs to be in constant attendance so long as the machines are above some minimal threshold of reliability). A small factory with few machines could not fully utilize such a highly skilled worker.

Similarly, there are other sizable fixed cost inputs that would inevitably be underutilized at low levels of output. Babbage mentions the introduction of lighting for night work and an accounting department (to which he attached great importance). Interestingly, he also cites the possibilities for effectively utilizing waste materials, the possibilities for which are greater in a larger plant, and this is sometimes facilitated by "the union of the trades in one factory, which otherwise might have been separated" (ibid.: 217). Thus, agents who are employed by large factories frequently provide services that cost little more than those provided to smaller establishments, even though the benefits of the service to the large factory are far more valuable.

The general point is that the machinery of industrial societies involves the willingness to undertake high fixed costs that can be justified only by a very large volume of output and sales. This takes us into a world far distant from Smith's pin factory, whose efficiency was determined by the acquisition of human skills and the utilization of simple tools. Babbage's analysis of this point is especially evident in Chapter XI, "Of Copying," by far the longest chapter in the book. Babbage brings together in this chapter a wide array of industrial processes involving specific applications of printing, casting, molding, engraving, stamping, punching, and so on. What unites the material in this chapter is that each process is made possible by first committing highly skilled labor to the design and shaping of some specialized instrument that will subsequently become the basis for many thousands of copies, that is, a machine. This situation, involving the common denominator of a large fixed cost that lays the basis for cheap per unit costs, is typical of the mass production technologies that were just beginning to emerge in Babbage's time.

Furthermore, Babbage points to large minimum size as a critical factor in the willingness to undertake the risks associated with technological change. He quotes approvingly a Report of the Committee of the House of Commons on the Wool Trade (1806) which asserted that:

> it is obvious, that the little master manufacturers cannot afford, like the man who possesses considerable capital, to try the experiments which are requisite, and incur the risks, and even losses, which almost always occur, in inventing and perfecting new articles of manufacture, or in carrying to a state of greater perfection articles already established.
>
> (Babbage 1835:223)

Babbage and technology

I suggest that Babbage's observations on the state of the British factory system pertain to a world that was vastly different from the house that Adam

Smith built. Babbage's characterization of British factories circa 1830 illuminated a technological system of far greater organizational and decision making complexity. Furthermore, Babbage offers useful, although obviously not definitive, treatments of a number of related issues. His Chapter XVI, "On the Influence of Durability on Price," which considers the case of durability with respect to consumer goods, does not go much beyond Adam Smith's treatment. However, his Chapter XXIX, "On the Duration of Machinery," exhibits insights concerning the economics of technological change that are powerful and were certainly highly original in Babbage's time.

This chapter deals with what we would today call "technological obsolescence," especially as the problem applies to capital goods with long useful lives, "such as wind-mills, water-mills, and steam-engines" (Babbage 1835:283). Babbage here introduces a table (ibid.: 284) of the average annual duty performed by steam engines in Cornwall over the period 1813–33, as well as the "average duty of the best engines." These engines, which were employed in Cornwall's extensive mining operations, provide impressive evidence of improvements in the construction and management of such engines. One wishes for more information concerning their operation; nevertheless, on the face of it, they show a strong upward trend in performance. For the 21-year period as a whole the average duty of the *best* engines more than tripled, from 26,400,000 hours in 1813 to 83,306,092 in 1833. Over the same period the average duty of all the engines rose from 19,456,000 hours to 46,000,000.

In such an environment, technological obsolescence is a dominating commercial consideration, and the length of the physical life of a capital good becomes of secondary importance. Babbage here offers a powerful insight that, it seems fair to say, is still not fully absorbed even today:

> Machinery for producing any commodity in great demand, seldom actually wears out; new improvements, by which the same operations can be executed either more quickly or better, generally superseding it long before that period arrives: indeed, to make such an improved machine profitable, it is usually reckoned that in five years it ought to have paid itself, and in ten to be superseded by a better.
>
> (Babbage 1835:285)

The effect of such obsolescence was a rapid downward revaluation of the market price for older machinery, which indeed is soon rendered commercially worthless. Babbage cites technological improvements in frames for making patent-net "not long ago." As a result, a machine that had cost £1200 and was still "in good repair" a few years later sold for a mere £60. But even more extreme evidence of the impact of rapid ongoing technological improvements in that trade was the decision to abandon the construction of unfinished machines "because new improvements had superseded their utility."[4]

Babbage ends this chapter, "On the Duration of Machinery," by pointing out that the effect of competition with respect to durable goods has been to render them *less* durable. He makes the observation that, when manufactured articles are transported a considerable distance, it is not uncommon for broken articles to be deemed unworthy of the cost of repair if the price of labor is higher than in its original place of manufacture. It is cheaper to purchase a new article (ibid.: 292). This appears to have been a practice of recent vintage when Babbage wrote.

Babbage and invention

Babbage had a great deal to say about one of the most complex of all economic activities: the invention of new technologies. It is a fair criticism of the state of economic theory today to point out that the discipline still encounters great difficulties in modeling the process, although some important progress has indeed been made over the past decade.[5] But the deeper point is that it is extremely difficult to identify general principles in analyzing the inventive process, and theorists have preferred to remain at a highly abstract level of analysis where they do not have to address some of the intractable aspects and the contingent nature of inventive activity.

It will be recalled that Adam Smith's third advantage of the division of labor was that it gave rise to inventions. But Smith's treatment of the determinants of inventive activity was extremely sparse and contained little explicit economic reasoning; the textual treatment of the subject in Chapter 1 of the *Wealth of Nations* occupied not much more than a single page. In Smith's view, in the early stages of industrial development most inventions were the work of the users, that is, workmen whose attention was increasingly fixed upon a single object or activity. Eventually, however, when the division of labor gave rise to more specialized makers of machinery, the ingenuity of these machine makers comes to play an increasingly important role; and finally, a more prominent role is exercised by those to whom Smith refers as "Philosophers or men of speculation, whose trade it is not to do any thing, but to observe everything; and who, upon that account, are often capable of combining together the powers of the most distant and dissimilar objects."[6]

Babbage's discussion of the determinants of invention is far richer than that of Smith, which is hardly surprising in view of the fact that he was writing some sixty eventful years later and examining a British industrial establishment that was far more complex than the largely pre-industrial society in which Smith had lived. Perhaps most important for Babbage, an extensive division of labor is itself an essential prerequisite to technological change. This is so for two related reasons. First of all, technological improvements are not generally dependent upon a few rarely gifted individuals, although the more "beautiful combinations" are indeed the work of the occasional genius (Babbage 1835:260). Rather, and second, inventive activity needs to be seen as a consequence as well as a cause of the division of labor. This is so because "The arts of contriving, of drawing, and of executing, do not usually reside in their greatest perfection in one individual; and in this, as in other arts, *the division of labor* must be applied" (Babbage 1835:266, his emphasis). Thus, the division of labor has the important consequence that it largely liberates mankind from a dependence upon the "occasional genius" as the source of technological improvement and generates such improvement, via the division of labor, almost as a matter of routine, involving men of only ordinary abilities. Clearly this is a point of great importance (and also reminiscent of Schumpeter's "bureaucratization" of the entrepreneurial function).

It is also important to note that Babbage shows an acute awareness of the economic forces that drive inventive capability in specific directions and that influence the timing of inventive effort. In fact, his observations deserve to be regarded as possibly the earliest treatment of the economic determinants of inventive activity. Technological change is not, for him, some totally exogenous phenomenon. On the contrary, he clearly saw the direction of technological improvements as responding to the relative prices of factor inputs, and the commitment of resources to the improvements of machinery as directly connected to the state of demand for the final product that the machines produce. In urging the importance of careful cost accounting, Babbage points out that one of its main advantages "is the indication which it would furnish of the course in which improvement should be directed" (ibid.: 203–204), that is, a firm would invest in those technological improvement activities that offered the highest payoff in terms of cost reduction, but only if it had a close understanding of those costs. On the demand side, he observes that "The inducement to contrive machines for any process of manufacture increases with the demand for the article" (ibid.: 213). And he further observes that "overmanufacturing" (that is, a business recession) is likely to lead to efforts to reduce costs through machinery improvement or the reorganization of the factory (ibid.: 233). Babbage is obviously pursuing technological change as an endogenous activity.

Finally, in discussing the numerous difficulties that confront an innovator, Babbage identifies what we today might refer to as "learning curves." Assuming that all the initial "teething troubles" have been overcome, subsequent units of the new product are likely to be produced at far lower cost than the first. Babbage's words deserve to be quoted in full:

> It has been estimated roughly, that the first individual of any newly-invented machine, will cost about five times as much as the

> construction of the second, an estimate which is, perhaps, sufficiently near the truth. If the second machine is to be precisely like the first, the same drawings, and the same patterns will answer for it; but if, as usually happens, some improvements have been suggested by the experience of the first, these must be more or less altered. When, however, two or three machines have been completed, and many more are wanted, they can usually be produced at much less than one-fifth of the expense of the original invention.
>
> (Ibid.: 266)

I would like to stake the claim for Babbage's book that it represented one of the first attempts to establish explicit causal links between economic forces and inventive activity, and to identify the numerous opportunities for increasing returns in the exploitation of industrial technologies. But I also want to go farther than this.

So far I have examined the role of the division of labor in shaping the activities carried out within the factory, including the invention of new machinery. But Babbage carried the division of labor one step farther, and here I must make use of the adjective that Schumpeter invoked in his footnote on Babbage and say that he carried the division of labor one *remarkable* step farther. The ultimate role of the sciences, in Babbage's view, is to examine the complexity of the physical universe, and progress in this realm too must be made via the division of labor. In his view, "the arrangements which ought to regulate the interior economy of a manufactory, are founded on principles of deeper root than may have been supposed, and are capable of being usefully employed in preparing the road to some of the sublimest investigations of the human mind" (ibid.: 191).

Babbage's allusion here to "principles of deeper root" may reasonably be interpreted as an acknowledgment that the central issue is one of information processing. And, of course, this interpretation is powerfully supported by the fact that Babbage was the acknowledged inventor of the technology that eventually made high-speed information processing possible. Moreover, in a very direct biographical sense, Babbage's book was a byproduct of his lifelong pursuit of a technology that would eventually make information processing a simple everyday reality.[7]

As a country progresses in its arts and manufactures, Babbage argues, continued progress comes to depend increasingly upon a growing intimacy between science and industry. In the final chapter of the book, "On the Future Prospects of Manufactures as Connected with Science" (Chapter XXXV), Babbage asserts that *science itself* is becoming subject to the same law of the division of labor that is the central theme of his book. Science needs to be cultivated as a full-time, specialized activity by those with the "natural capacity and acquired habits." Such specialization is unavoidable because "the discovery of the great principles of nature demands a mind almost exclusively devoted to such investigations; and these, in the present state of science, frequently require costly apparatus, and exact an expense of time quite incompatible with professional avocations" (ibid.: 379–380). The reference to "costly apparatus" is especially apposite, coming from the pen of the grandfather of the computer: One of the most costly of all research instruments today is, of course, a large Cray computer.

Babbage closes a long apotheosis to science by pointing out that the progress of science itself will be increasingly governed by progress in the ability to calculate: "It is the science of *calculation,*—which becomes continually more necessary at each step of our progress, and which must ultimately govern the whole of the applications of science to the arts of life" (ibid.: 387–388, original emphasis). In short, it is Babbage's view that mankind's future prospects will be dominated by the fact that "machinery has been taught arithmetic." Babbage was in this respect, of course, remarkably prescient, but the possibility of teaching arithmetic to machinery would have to await the age of electronics. Ironically, the first, tentative step toward that age occurred while Babbage was writing his book, and by someone who, in the close scientific community of London at that time, was a friend of his: Michael Faraday. Faraday (who was born in 1791, the same year as Babbage) first demonstrated the principle of electromagnetic induction in 1831.

Babbage was not only the grandfather of the computer; he was also the grandfather of the study of complexity in industrial economies.

Notes

1 I thank David Colander for some incisive comments and thoughtful suggestions in the preparation of this chapter. I have drawn freely on an earlier paper, "Charles Babbage: Pioneer Economist," in Nathan Rosenberg, *Exploring the Black Box,* Cambridge: Cambridge University Press, 1994.

2 In fact, it has been the very complexity, or indeterminacy, that increasing returns introduces into the analysis of the behavior of markets that has accounted for the popularity of the simplifying assumption of constant returns.

3 Karl Marx, *Capital,* New York: International Press, 1996, Modern Library Edition, p. 376, and Karl Marx, *The Poverty of Philosophy,* especially Chapter 2, "The Metaphysics of Political Economy." The pervasiveness of Babbage's influence on Marx is revealed by a textual comparison of Babbage's treatment of the causes and consequences of the division of labor with that of Marx in the two central chapters of volume I of *Capital,* chapter 14 on "Division of Labour and Manufacture," and Chapter 15 on "Machinery and Modern Industry." For further discussion, see Nathan Rosenberg, "Charles Babbage: Pioneer Economist," in Rosenberg, *Exploring the Black Box,* Cambridge: Cambridge University Press, 1994, ch. 2.

4 I wrote an article some years ago on the role of technological expectations (Nathan Rosenberg, "On Technological Expectations," *Economic Journal,* 1976). The article discussed the complexity of the decision-making process when technological change is not only rapid, but is expected to continue to be rapid in

the future. I suspect that Babbage would not have been surprised at any of my conclusions.

5 I am thinking here primarily of the line of research that has been initiated by the work of Paul Romer, especially "Endogenous Technological Change," *Journal of Political Economy,* 1990.

6 Adam Smith, *Wealth of Nations,* Modern Library Edition, New York: 10. For a more extensive treatment of Smith's views on this subject, see Nathan Rosenberg, "Adam Smith and the Division of Labor: Two Views or One?", *Economica,* May 1965.

7 Babbage opened the Preface to his book with the following observations: "The present volume may be considered as one of the consequences that have resulted from the Calculating-Engine, the construction of which I have been so long superintending. Having been induced, during the last ten years, to visit a considerable number of workshops and factories, both in England and on the Continent, for the purpose of endeavouring to make myself acquainted with the various resources of mechanical art, I was insensibly led to apply to them those principles of generalization to which my other pursuits had naturally given rise. The increased number of curious processes and interesting facts which thus came under my attention, as well as of the reflections which they suggested, induced me to believe that the publication of some of them might be of use to persons who propose to bestow their attention on those inquiries which I have only incidentally considered" (Babbage 1835).

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4

DID MARX KNOW THE WAY TO SANTE FE?

Reflections on evolution, functionalism, and class consciousness

Peter Hans Matthews[1]

Instead of an introduction, I shall offer a confession. The title of the paper—the question, that is—was chosen before I knew its contents: a confluence of research interests in the classical/Marxian and Santa Fe perspectives, a provocative call for conference papers and the rehabilitation of Burt Bacharach's music prompted an impulsive choice. Worse, perhaps, I never intended to chart some exact, and no doubt serpentine, course from Victorian London *to fin de siècle* Santa Fe. The revelation, for example, that some of us hear echoes of Marx's (1894) "anarchy of production" in Bak *et al.*'s (1993) complex characterization of output and inventories does not mean that either (a) scrupulous readers will discern a protodefinition of "self organized criticality" in the third volume of *Capital,* or (b) the contributions of Bak *et al.* should be counted part of the modern classical tradition.[2] Instead, I envisioned this as the prelude to a tentative, even suspicious, conversation between otherwise indifferent schools. In particular, the chapter considers if, and then how, recent developments in "complex economics" could illuminate, even recontextualize, elements of the Marxian vision. In this sense, at least, the question is less "Did Marx know the way to Santa Fe?" than "Can Santa Fe find its way to Marx?".

I should confess, too, that a second, more rhetorical, purpose also animates this chapter. It seems to me that "spontaneous order" has now replaced "path dependence" as the dominant metaphor in mainstream discussions of the "complexification" of economics, and while the substitution is, in principle, a reasonable one, the term's Austrian roots (Hayek 1960) have caused some to infer (much) more than is warranted.[3] In his review of "population games" for the second Santa Fe volume, Blume (1997) reminds readers that "there is no argument that the 'spontaneous order' of the Austrians is necessarily beneficent…[t]he invisible hand could equally well be Shakespeare's bloody and invisible hand of night as Adam Smith's hand of Pangloss." In different terms, the notion of an "emergent structure" (Arthur 1994) is (for the moment) a flexible one: the norms and institutions consistent with "positive local feedback" need *not* be those which Austrians and/or classical liberals favor.

In the spirit of Blume (1997), much of this chapter is devoted to an "appreciative" model of the evolution of certain "class norms," one with obvious affinities to Santa Fe-based models of social norms and conventions. Its purpose is to recast the Marxian notions of "class consciousness" and "collective choice" in different and perhaps provocative terms and, in the process, to reconsider the criticism, familiar to most historians of economics, that Marx's economics were too often functionalist.

Class consciousness and functionalism

Rosner (1998) discerns the presence of *both* "collective" and "individual" actors in Marx's economics, and identifies the former as a German historicist influence. Their coexistence is sometimes an uncomfortable one, however, more so because the connections between Marx's definitive discussions of class—Elster's (1985) choice of *The Eighteenth Brumaire* (1852) as canonical is common—and the "laws of [economic] motion" articulated in *Capital* are difficult to establish. If some believe that no reconciliation is needed, others (Elster 1982, 1985) insist that none is possible, and have criticized the derivation of the laws as teleological.

Consider the most common example of Marx's ostensible functionalism, the rationale for the historical factor bias in new methods of production, often dismissed as "explanation" in terms of consequence, not cause. The proposition that British capitalists introduced capital-intensive methods in the face of a secular rise in the relative price of labor power *because of the benefits to capitalists as a class* is indeed problematic.[4] The problem, however, need *not* be the potential conflict between individual and class, but rather an absence of context. The context is one in which capitalists are endowed with "positive class consciousness," defined as the "ability to overcome the free rider problem in realizing class interests" (Elster 1985), and here understood to mean those norms, "metanorms" (Axelrod 1986) and institutions that, from time to time, overcome both the free rider and "commitment" (Schelling 1960, 1978) problems of collective choice. If capitalists' innovative activities are "norm sensitive," in other words, and these norms are consistent with (a limited form of) rational individualism, it will not be functionalism that needs to be overcome, but other obstacles: What capitalist norms would have had such an effect? How were these norms sustained? How did these norms evolve? And so on.

The classical distinction between "labor power" and "labor," and the attendant "effort extraction problem," provides another example. The observation that capitalists devote substantial resources—in the form of "unproductive" supervision, otherwise inefficient methods of production, and so on—to the separation of labor (value) from labor power is difficult to rationalize in terms of its collective benefits without reference to context, and the formalization of this context is the focus of the next sections.

Effort extraction without class consciousness

The standard inspection game (Fudenberg and Tirole 1993) provides a crude, but tractable, characterization of the "contested exchange" (Bowles and Gintis 1993) between capitalist/supervisor and worker, and so offers a convenient point of departure:

$$
\begin{array}{cc}
 & \begin{array}{cc} CS & LS \end{array} \\
\begin{array}{c} IE \\ NE \end{array} & \begin{pmatrix} w-e,\ v-w-h & w-e, v-w \\ 0,-h & w,-w \end{pmatrix}
\end{array}
$$

Here, the capitalist chooses between "close supervision" *CS* and "loose supervision" *LS,* while the worker must counter, at the same time, with either "normal effort" *NE* or "intense effort" *IE.* The relevant parameters are v, the value of the incremental output that intense effort *IE* produces; w, the premium the worker receives (from the capitalist) if normal effort *NE* is not detected, either because s/he expends intense effort *IE* and/or the capitalist chooses loose supervision; h, the cost of close supervision to the capitalist; and e, the cost of intense effort to the worker; where all are measured relative to some benchmark. The restriction $v>w>e>h$, which ensures that neither side has a dominant choice, captures (some of) the conflict in the transaction.

Under these conditions, the inspection game (IG hereafter) will have a unique Nash equilibrium in which both sides randomize: the capitalist chooses close supervision *CS* with likelihood $p = e/w$ while the worker chooses normal effort with likelihood $p_N = h/w$. The capitalist's expected profits p_K are therefore:

$$
\pi_k = \frac{e}{w}\left(\frac{w-h}{w}\right)(v-w-h) + \left(\frac{w-e}{w}\right)\left(\frac{w-h}{w}\right)(v-w)
$$

$$
+ \frac{e}{w}\frac{h}{w}(-h) + \frac{h}{w}\left(\frac{w-e}{w}\right)(-w) = v - w - (hv/w) \qquad (4.1)
$$

in equilibrium.

If, on the other hand, the existence of some "irrational" and identifiable *animus* motivated the capitalist to "choose" close supervision without fail—a manifestation, perhaps, of Frank's "passion within reason' (1988)—the "rational" worker would be forced to increase her/his effort, and profits would rise to *v-w-h*. In other words, the capitalist who was unable to convince her/himself that the individual worker would ever choose intense effort would fare better than her/his more "rational" peers.

Because the "rational capitalist" understands that s/he would "defect" to loose supervision *LS* if, for whatever reason, s/he believed effort would be intense, there is some incentive to "precommit," *à la* Ulysses, to close supervision *CS*. Under "normal" circumstances, however, no *credible* precommitment mechanism is available. The proposition that "emotional" or "norm-based" behavior can leave individuals better off than "deliberative" behavior to the extent that it resolves the commitment problem is often attributed to Schelling (1960). Critics see the introduction of norms and metanorms as a *deus ex machina,* however, more so, at least, than Frank's (1988) "strategic passions" or Young's (1993) "conventions": adherence is, in some measure, a matter of choice, and maintenance, in the form of sanctions, is seldom costless.

It becomes useful, in this context, to consider a four-player/two-stage modification of IG, denoted IG': in the first, two pairs of capitalists and worker each contest IG, while in the second, each of the capitalists is free to "punish" the other, at the cost of c_1 to her/himself and c_2 to the other, where $c_2 > h$. Suppose, for the moment, that each capitalist follows the rule "choose *CS* in the first round, and punish *LS* in the second"—consistent with the enforced norm "workers cannot be trusted, and close supervision is in the interest of all capitalists"—and that each worker chooses intense effort. It is not difficult to show that this is a Nash equilibrium for IG': neither capitalist has an incentive to "deviate" because s/he would "save" the costs of supervision *h* but then incur the costs of punishment c_2, and neither worker has an incentive to reduce her/his effort level because both capitalists have chosen *CS*. Each capitalist receives *v-w-h* in profits—neither is required to punish the other—and the commitment problem seems, at first blush, to have been resolved.

The resolution is suspect, however, because the "Nash test" fails to differentiate between "credible" and "incredible" threats: if each capitalist believes the other to be "rational," then neither expects to be punished, no matter what her/his previous behavior, since the potential "punisher" will then inflict further costs on her/himself. In other words, these behaviors are Nash over IG' as a whole, but not over(all) of its proper parts—that is, not "subgame perfect" or SP. In this case, the SP equilibrium is one in which, as before, capitalists and workers randomize in the first round, after which neither capitalist punishes the other, an outcome that leaves little room for norms/class consciousness.

Game theorists have understood for decades, of course, that repetition—either an infinite or finite, but uncertain, number of times—of IG' would, under some conditions, undermine this "determinism," but the folk theorem (Fudenberg and Tirole 1993) asserts that the "norm-based" outcome would then be (no more then) one element in a (substantial) set of possible outcomes, without a well-established selection mechanism. Furthermore, the "collective consciousness" which Marx and others described was *not* one predicated on the repeated interaction of a small (in this case, two) and fixed number of capitalists. The microfoundations of "corporate culture" (Kreps 1990) must therefore be found elsewhere.

Collective norms and class consciousness: a Santa Fe perspective?

There has been considerable interest over the last decade(s) in "evolution" as a selection mechanism—see, for example, Blume (1997) for a "Santa Fe perspective" or van Damme (1994). Consider a final variation on the inspection game, denoted EIG, in which two pairs of capitalists and workers are "drawn" from separate and substantial (if the complications of "reputation effects" are to be avoided, infinite) populations each period to contest IG'. It follows that EIG, a "one-sided" version of the "norms game" first described in Axelrod (1986) and Witt (1986) and later formalized in Sethi (1996), will perhaps best be understood as a model of "dead end" labor markets, where reputation and/or tenure do not matter much. Furthermore, suppose that capitalists are not "optimizers" in the usual sense, but are instead boundedly rational—that is, follow rules of thumb that are responsive to the pressures of natural or economic selection. In particular, suppose that there are three kinds, or "strains," of capitalist—"*CS* capitalists who punish *LS* capitalists," "*CS* capitalists who never punish," and "*LS* capitalists who never punish"—and just one kind of worker, "best responders" who observe the current proportions of capitalists and then optimize. Capitalists who do not do well relative to their peers either learn from this experience and/or produce fewer successors. (A better treatment would of course start with a "complete" set of population strains for both capitalists and workers—that is, one for each possible "rule of thumb" and, in some cases, "best responders" (Sethi 1996, and Banarjee and Weibull 1995); but the evolutionary dynamics are then (much) more complicated, despite the fact that dominated strains can be eliminated *a priori*.)

Given the assumed behavior of workers, EIG can be reduced to pairwise and (since the number of capitalists is infinite) anonymous interactions between members of a single population, about which much more is understood.[5] The profit matrix is then:

$$\begin{array}{c} \\ CSP \\ CSN \\ LS \end{array} \begin{pmatrix} CSP & CSN & LS \\ v-w-h & v-w-h & v-w-h-c_1 \\ v-w-h & v-w-h & v-w-h \\ v-w-c_2 & v-w & v-w \end{pmatrix}$$

when the sum of the proportions of *CS* capitalists who punish, p_1, and *CS* capitalists who don't, p_2, is more than or equal to *e/w,* and:

$$\begin{array}{c} \\ CSP \\ CSN \\ LS \end{array} \begin{pmatrix} CSP & CSN & LS \\ -h & -h & -h-c_1 \\ -h & -h & -h \\ -w-c_2 & -w & -w \end{pmatrix}$$

otherwise, where the entries represent the profits of the "row capitalist."[6]

Two distinct, but related, solution concepts are standard in the literature on population games. The first, the "evolutionary stable state," or ESS, is static. To illustrate for the simple(r) linear[7] case, if $A=[a_{ij}]$ is the profit matrix and $p=[p_1, p_2,\ldots, p_N]^T$ is the column vector of capitalist proportions, p^* is defined to be an ESS if:

$$p^{*T} Ap^* > p^T Ap^* \text{ or } p^{*T} Ap^* = p^T Ap^* \text{ and } p^{*T} Ap > p^T Ap \qquad (4.2)$$

for all p?p*. The rationale for (2) can be expressed in terms of the interaction(s) between a "reference population" of capitalists with composition p* and a "mutant population" with composition p?p*. The first of the conditions requires that the mean profits of reference population capitalists be more than those of *all* mutant populations when both interact with (other) members of the reference population. The second condition allows these profits to be equal, but then requires that the mean profits of reference population capitalists exceed those of all possible mutant populations when both interact with the *latter.* In more evocative terms, the reference population must either be invulnerable to "invasions" or must itself be able to invade all other populations. It follows that while all ESS are Nash, not all Nash are ESS—in other words, ESS is a "Nash refinement."

The interactions in EIG are more complicated than this, however, and two refinements of the ESS criterion are needed. First, because profits are state dependent—that is, the profits of capitalists of sort *i* in interactions with capitalists of sort *j* are a function (in this case, a simple one) of their proportions—a "localized" definition is required. Second, and no less important, the definition is limited to population *points,* but class consciousness could be achieved if (some subset of) $p_1+p_2=1$ was stable. Both are accounted for in the definition(s) of a "local (neutral) evolutionary stable state" (L(N)ESS): if A(q) is a state dependent profit matrix, p* is a L(N)ESS when:

$$p^{*\mathrm{T}}\,\mathrm{A}(p^*)p^* > p^{\mathrm{T}}\,\mathrm{A}(p^*)p^* \tag{4.3}$$

or:

$$p^{*\mathrm{T}}\,\mathrm{A}(p^*)p^* = p^{\mathrm{T}}\,\mathrm{A}(p^*)p^* \quad \text{and} \qquad p^{*\mathrm{T}}\,\mathrm{A}(p)p \geq p^{\mathrm{T}}\,\mathrm{A}(p)p \tag{4.4}$$

for all p "sufficiently close" to p^*.

The second, and more natural, solution concept identifies the rest points/sets of "evolutionary dynamics" as possible equilibria. There is some debate (Sigmund and Young 1995) about the precise specification of selective pressures in economic environments, but the "replicator dynamics" (Taylor and Jonker 1978) for EIG would assume the form:[8]

$$\dot{p}_i = p_i(\pi_i - \pi_{\mathrm{K}}) \tag{4.5}$$

where $\dot{p}_i$ is the rate of increase (or decrease) in the proportion of type i capitalists, π_i are the mean profits of type i capitalists, and π_{K} are the mean profits of all capitalists. In other words, the rate at which the fraction of *CS* capitalists who punish will rise or fall, for example, is proportional to their relative fitness.

The solution concepts are related, of course. It has been understood for some time, for example, that an ESS will be "locally asymptotically stable" under the replicator dynamics, but that the converse is not quite true (Zeeman 1980). Since then, it has been shown (Hofbauer and Sigmund 1988) that a LESS must also be locally asymptotically stable, and that a LNESS will be (just) stable. It follows, therefore, that the search for LESS and LNESS in EIG can be limited to the stable points of its replicator dynamics. Furthermore, with three strains, attention can be restricted to (in this case) the first two, a convenient simplification.

To this end, the expected profits π_1 of *CS* capitalists who punish are:

$$\pi_1 = \begin{cases} p_1(v-w-h) + p_2(v-w-h) + (1-(p_1+p_2))(v-w-h-c_1) \\ p_1(-h) + p_2(-h) + (1-(p_1+p_2))(-h-c_1) \end{cases}$$

$$\rightarrow \begin{matrix} \text{if } p_1 + p_2 \geq e/w \\ \text{otherwise} \end{matrix} \tag{4.6}$$

or, after simplification,

$$\pi_1 = \begin{cases} v - w - h - (1-(p_1+p_2))c_1 & \text{if } p_1 + p_2 \geq e/w \\ -h - (1-(p_1+p_2))c_1 & \text{otherwise} \end{cases} \tag{4.7}$$

Likewise, the expected profits of *CS* capitalists who do not punish and *LS* capitalists are:

$$\pi_2 = \begin{cases} v - w - h & \text{if } p_1 + p_2 \geq e/w \\ -h & \text{otherwise} \end{cases} \tag{4.8}$$

and

$$\pi_3 = \begin{cases} v - w - p_1 c_2 & \text{if } p_1 + p_2 \geq e/w \\ -w - p_1 c_2 & \text{otherwise} \end{cases} \tag{4.9}$$

Profits per capitalist $\pi_K = p_1\pi_1 + p_2\pi_2 + (1-(p_1+p_2))\,\pi_3$ are therefore:

$$\pi_k = \begin{cases} v - w - (p_1+p_2)h - p_1(1-(p_1+p_2))(c_1+c_2) & \text{if } p_1 + p_2 \geq e/w \\ -(1(p_1+p_2)w - (p_1+p_2)h\, p_1(1-(p_1+p_2))(c_1+c_2) & \text{otherwise} \end{cases} \tag{4.10}$$

After some simplification, the replicator dynamics are then:

$$\dot{p}_1 = \begin{cases} -p_1(1-(p_1+p_2))(h + c_1 - p_1(c_1+c_2)) & \text{if } p_1 + p_2 \geq e/w \\ p_1(1-(p_1+p_2))(w - h - c_1 + p_1(c_1+c_2)) & \text{otherwise} \end{cases} \tag{4.11}$$

$$\dot{p}_2 = \begin{cases} -p_1(1-(p_1+p_2))(p_1(c_1+c_2) - h) & \text{if } p_1 + p_2 \geq e/w \\ p_2(1-(p_1+p_2))(w - h + p_1(c_1+c_2)) & \text{otherwise} \end{cases} \tag{4.12}$$

where, in each case, the last terms comprise a measure of relative fitness $\pi_i - \pi_K$.

There are four rest points/sets consistent with the previous parameter restrictions: (i) $p_1+p_2=1$, in which all capitalists are close supervisors of one kind or another; (ii) $p_1=p_2=0$, in which no capitalist is a close supervisor; (iii) $p_1=(c_1+h)/(c_1+c_2)$ *and* $p_2=0$, a combination of *CS* capitalists who punish and *LS* capitalists; and, less obvious perhaps, (iv) (some sub-set of) $p_1+p_2=e/w$, in which the proportions of *CS* capitalists (combined) and *LS* capitalists are those associated with the simpler IG. The first of these could be understood in terms of a certain "collective consciousness"—capitalists realize their common interest—while the second, in contrast, reflects its absence. The fourth, on the other hand, is consistent with the notion that the second round of EIG is, in effect, superfluous. It is important, therefore, to determine not just which of these are stable, but to demarcate the "basins of attraction"—that is, to determine the final (stable) state to which each combination evolves.

A formal characterization of (11) and (12) is not difficult, but involved—in particular, the behavior of p_1 and p_2 near both $p_1+p_2=1$ and $p_1+p_2=e/w$ calls for more elaborate methods[9]—and therefore outside the scope of the present chapter. The essential properties of EIG can be inferred, however, on the basis of a simulation exercise. Suppose that v=40, w= 30, e=15, h=10, c_1=5 and c_2=15, in which case the replicator dynamics become:

$$\dot{p}_1 = \begin{cases} -p_1(1-(p_1+p_2))(15-20p_1) & \text{if } p_1+p_2 \geq 1/2 \\ p_1(1-(p_1+p_2))(15+20p_1) & \text{otherwise} \end{cases} \tag{4.13}$$

$$\dot{p}_2 = \begin{cases} p_1(1-(p_1+p_2))(20p_1-10) & \text{if } p_1+p_2 \geq 1/2 \\ p_2(1-(p_1+p_2))(20+20p_1) & \text{otherwise} \end{cases} \tag{4.14}$$

The "direction field" or "arrows of motion" will then have the pattern(s) depicted in Figure 4.1.

Consistent with intuition, the combination p_1=0, p_2=0 is *not* stable and therefore cannot be a LESS for EIG: when $p_1+p_2<1/2$, all workers will choose normal effort *NE,* and "natural selection" will favor the close supervision of workers. Indeed, as the direction field reveals, the proportions of *CS* capitalists who punish—"norm enforcers"—and *CS* capitalists who do not—"free riders"—will *both* increase whenever p_1+p_2 is less than 1/2. If p_1=0.017, p_2=0.007 and therefore p_3=0.976, for example, profits per capitalist π_K are equal to -29.85, but norm enforcers, free riders and *LS* capitalists will extract profits of -14.87, -10 and -30.255 in their respective encounters with workers. Under (4.13) and (4.14), therefore, *LS* capitalists (or their successors) will become *CS* capitalists of *both* kinds, since both *CS*-based "rules" lead to better than mean profits. In some measure, the evolution of the *relative* proportions of the two kinds of *CS* capitalists will reflect initial "imbalances": the self-inflicted costs of sanctions mean that the percentage rate of increase in the proportion of norm enforcers must be less than that of free riders , but an initial preponderance of "norm enforcers" can sometimes persist. If, for example, a perturbation of (0, 0) produces the previous combination (0.0017,

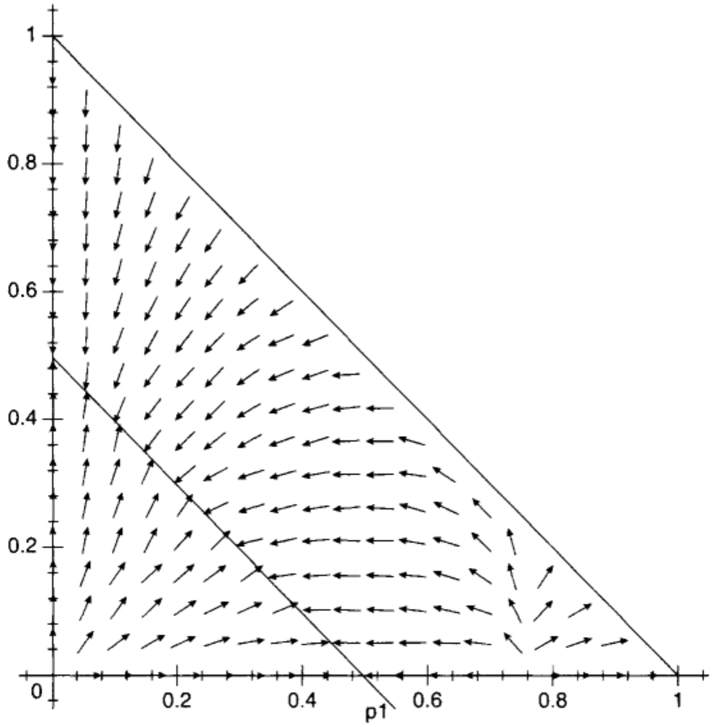

Figure 4.1 Direction field for EIG: case 1

0.007), evolution will drive EIG to the point where 50 percent of all capitalists are close supervisors, and 60 percent of these (or 30 percent of all capitalists) are norm enforcers, as illustrated in Figure 4.1. In effect, the pool of potential "switchers" vanishes before the norm enforcers are overwhelmed.

The surprise, perhaps, is that the whole of $p_1+p_2=1/2$—that is, the interior and both endpoints (0, 0.5) and (0.5, 0)—seems to be an attractor. To be precise, this does not mean that if the combination (*x,* 0.5—*x*) is perturbed, the same proportions will be re-established, but rather that p_1 and p_2 will tend toward some combination on the line. It seems reasonable, of course, that (0, 0.5), the Nash equilibrium of IG, should be stable, but the existence of a stable set of combinations in which neither norm enforcers nor *LS* capitalists are driven to zero calls for further comment. Consider the combination (0.3, 0.2), for example, identified above as the final state for the initial condition (1.7, 0.7): substitution in $\dot{p} = -1.35$ and $\dot{p} = -0.40$ tells us that and which in turn implies (once more, consistent with intuition) that selective pressures will favor *LS* capitalists over both sorts of *CS* capitalists, with norm enforcers least favored. If the total number of *CS* capitalists fell, however, workers would switch, *en masse,* to normal effort *NE,* and these pressures would then be reversed! In other words, because the limits from below $\lim_{(p_1,p_2)\to(0.3,\,0.2)^-} \dot{p}_1 = 3.15$ and $\lim_{(p_1,p_2)\to(0.3,\,0.2)^-} \dot{p}_2 = 3.90$ are both positive, the assumed behavior of workers "freezes" these proportions. (The situation is more complicated, of course, if "drift" is introduced into the model.)

It also follows, therefore, that if (small) mutations disturbed the simple Nash outcome (0, 50)—in which there are no norm enforcers—the proportion of *LS* capitalists would return to 50 percent, but *CS* capitalists who punish would not be driven to extinction. In this case, at least, the norm's survival is associated with a *decrease* in capitalists' profits.

For the present purposes, however, it is the stable sub-set of $p_1+p_2=1$ that could be labeled "achievement of positive class consciousness" in Figure 4.1 that demands the most attention.[10] It will be convenient to divide the set of population mixtures S such that $1/2 \leq p_1 + p_2 \leq 1$ into three distinct "zones." In the first, S_1, the proportion p_1 of norm enforcers is less than 50 percent, and the behavior of EIG resembles that of IG: all workers choose intense effort *IE,* and *CS* capitalists of both sorts become *LS* capitalists. (The difference, of course, is that the proportion of norm enforcers need not tend to zero.) If the proportion of norm enforcers lies between 50 and 75 percent, however, free riders and loose supervisors will both be favored over norm enforcers. To understand the curious behavior of population strains within S_2, consider the selective pressures operative at (0.6, 0.3), for example. Despite the fact that *LS* capitalists are often, but not too often, paired with *CS* capitalists who punish, each receives profits of 2, while norm enforcers, once more the least favored, receive profits of -0.5. Because the profits (per encounter) of each free rider are 0 and total profits per capitalist are -0.10, however, norm enforcers or their successors become both free riders and loose supervisors. As the direction field hints, most of the combinations in S_2 will tend, over time, to drift into the first zone S_1 and from there, to $p_1+p_2=1/2$, as illustrated in Figure 4.1. It is also possible, however, that populations "close" to the achievement of class consciousness will be "pushed" to $p_1+p_2=1$, if the pressures (downward) on norm enforcers are mild.

If the proportion of norm enforcers exceeds 75 percent, however, the differential in favor of loose supervision evaporates, and close supervision, of both kinds, will be favored: in the third zone S_3; in other words, evolution will drive (p_1, p_2) toward line $p_1+p_2=1$. To see this, consider the combination (0.85, 0.05), in which, as in the previous example, 90 percent of all capitalists are *CS* capitalists of one sort or another. The profits of *LS* capitalists, now almost certain to be punished in each encounter, are -2.75, while the profits of norm enforcers and free riders are once more -0.5 and 0, both of which exceed the mean of -0.70. In this case, the poor performance of *LS* capitalists "pulls" the mean fitness of capitalists down so far that otherwise "irrational" norm enforcement becomes viable in an evolutionary sense. It is important to note, however, that the relevant sub-set of $p_1+p_2=1$—from (0.67, 0.34) to (1.0, 0)—will be stable in the sense that $p_1+p_2=1/2$ was stable: that is, "small perturbations" return to the set, not particular points.

Since dynamic stability is necessary but not sufficient for evolutionary stability, it remains to show that the "achievement of positive class consciousness" is in fact a LNES. Note first that there is a neighborhood of $p=(p_1, 1\text{-}p_1, 0)$ in which the profit matrix is:

$$A = \begin{pmatrix} 0 & 0 & -5 \\ 0 & 0 & 0 \\ -5 & 10 & 10 \end{pmatrix}$$

It follows that $q^{\mathrm{T}}\mathrm{A}p = q_3(10 - 15p_1)$ will be less than if $p^{\mathrm{T}}\mathrm{A}p = 0$ (and only if) $p_1 > 2/3$ when q_3 is non-zero. If q_3=0, then $p^{\mathrm{T}}\mathrm{A}p = 0 = q^{\mathrm{T}}\mathrm{A}p$ in which case the second criterion in (4.4) will be (trivially) satisfied since $p^{\mathrm{T}}\mathrm{A}q = 0 \geq 0 = q^{\mathrm{T}}\mathrm{A}q$.

Thus, for populations in the sub-set's basin of attraction, evolution will favor the establishment of a class norm that allows the collective benefits of close supervision to be realized, despite its costs. The relevance of this outcome is obvious: the methodological principle of (bounded, at least) rational individualism is not *a priori* inconsistent with behavior(s) that *seem* to be the result of collective, rather than individual, choice. In more provocative terms, Marx's selective use of "capital" as a collective actor need not be functionalist in the presence of effective capitalist norms: in this particular model, it is individual capitalists who "choose," but it will be the "collective rationality" of close supervision that seems to motivate these choices.

To illustrate, consider the response of capitalists and workers to an increase in the premium w, from 30 to (say) 35. The "norm free" IG predicts that the proportions of *CS* capitalists and *NE* workers will both fall, for sensible reasons. Despite the fact that the profits of close supervision *CS* will fall *less* than the profits of loose supervision *LS* when the composition of workers (or, in IG, the probabilities of *NE* and *IE*) is held fixed, the benefits of intense effort *IE* will rise faster than those of normal effort *NE,* and it is the second of these which dominates. Given the parameter values assumed here, profits will, as expected, fall, but this is not inevitable: (1) implies that profits will in fact rise if $w < \sqrt{hv}$. It should be noted, however, that even if profits rise and, as must be the case, the profits of class conscious capitalists in EIG fall, the former will still be smaller than the latter.

The new replicator dynamics for EIG, however, are:

$$\dot{p}_1 = \begin{cases} -p_1\,(1-(p_1+p_2))(15-20p_1) & \text{if } p_1 + p_2 \geq 3/7 \\ p_1\,(1-(p_1+p_2))(20+20p_1) & \text{otherwise} \end{cases} \tag{4.15}$$

$$\dot{p}_2 = \begin{cases} p_1\,(1-(p_1+p_2))(20p_1-10) & \text{if } p_1 + p_2 \geq 3/7 \\ p_2\,(1-(p_1+p_2))(25+20p_1) & \text{otherwise} \end{cases} \tag{4.16}$$

with the direction field depicted in Figure 4.2. The salient feature of (4.15) and (4.16) is that the evolution of proportions is *identical* to that described in (4.13) and (4.14) for $p_1 + p_2 \geq 1/2$. The reason, of course, is that the increase in w causes each of the entries in the first (but not the second) profit matrix to be reduced the same amount, and so has no effect on the "relative fitness" of capitalist rules of thumb when all workers choose intense effort *IE*. This means, however, that the subset of $p_1+p_2=1$ that represents the "achievement of class consciousness" will also be the same, and that it will be dynamically and evolutionary stable.

It also follows, therefore, that if capitalists are class conscious in this sense, their *individual* reactions to the increase in w will be consistent with a

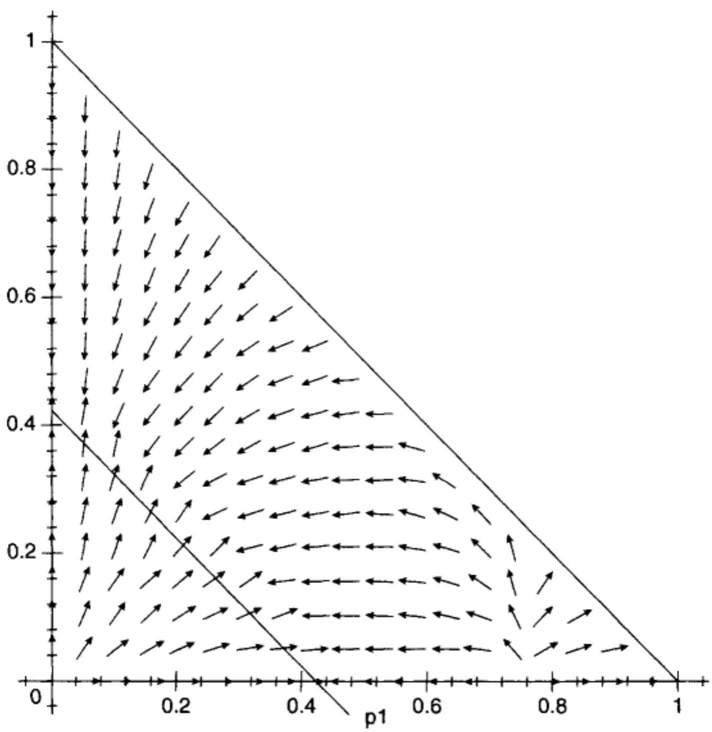

Figure 4.2 Direction field for EIG: case 2

sense of *collective* rationality. In other words, despite the fact that "rational" capitalists would choose close supervision *CS* less often, (small) invasions of *LS* capitalists will not thrive and this "resistance" will lead to increased profits for all.

If the norm/metanorm that sustains such behavior exists, however, it will be part of the "culture," the broader environment, in which capitalists operate. Economists have often overlooked such context, however, and have therefore tended to misunderstand the work of those who have not.

Conclusion

The criticism of Marx's functionalist tendencies is often couched in terms of the possible inconsistencies between individual and class/collective rationality. To the extent that free rider and commitment problems undermine realization of the latter, however, the evolution of class-centered norms and metanorms, a particular form of positive class consciousness, will sometimes overcome these. In this sense, there is at least one road from Santa Fe to Victorian London.

Notes

1 This is a substantial revision of the History of Economics Society conference paper I presented in Montreal. It has benefited from the criticisms of its principal discussant, Francisco Louçã, and from conversations with Antonio Callari, Carolyn Craven, Perry Mehrling, Michael Perelman and Peter Rosner. The illustrations in the fourth section were constructed with *Maple V*.

2 Bak himself would no doubt concur. In *How Nature Works* (1996), he observes that "the most robust state for an economy could be the decentralized self organized critical state of [capitalism], with fluctuations of all sizes and durations." This said, it should be noted that (a) the proposition is "could be," not "is," a reminder that this is *speculation,* not inference, and (b) with the substitution of "supercritical" for "critica," some modern classicals/Marxists would have little reason to demur.

3 For a review of the renaissance of "spontaneous order" in economics, see Sugden (1989).

4 Elster (1985) also reminds us, however, that the difficulties with the standard neo-classical (Hicks 1963) "explanation" were not well understood until Salter (1960).

5 As Weibull (1996) notes, for example, even the definition of "evolutionarily stable states" in multi-population models remains a matter of some debate. For some recent advances, see Cressman (1995).

6 It is assumed that if the sum of *CS* capitalists, $p_1 + p_2$, is equal to e/w, all workers will choose intense effort *IE*.

7 Because profits in EIG are "state dependent," it is *not* linear.

8 The use of replicator dynamics is more common in the natural than the social sciences, and was first rationalized in terms of inheritance. Cultural transmission would produce similar behavior, however, and there have been some efforts (Gale *et al.* 1995) to rationalize the specification in terms of learning.

9 The methods are similar to those which Sethi and Somanathan (1996) use to describe the evolution of common property resource (CPR) norms.

10 The third of the rest points/sets, (0.75, 0), is itself unstable, but is a "corner" of the sub-set's basin of attraction.

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5

COMPLEXITY IN PEIRCE'S ECONOMICS AND PHILOSOPHY

An exploration of his critique of Simon Newcomb

James Wible[1]

Peirce was the first post-Newtonian physicist and philosopher.
(Popper 1972:213)

Charles Sanders Peirce (1839–1914), one of the founders of pragmatism, created a philosophy and cosmology of evolutionary complexity in the 1890s. He used this vision of complexity to fashion a critique of nineteenth-century political economy and Simon Newcomb's *Principles of Political Economy* (1886). That critique is found in Peirce's 1893 article, "Evolutionary Love," Peirce argues that a political economy based on utilitarianism and Darwinian competition confines the discipline to lower levels of motivation, action, and struggle more characteristic of animals and plants than human beings. As an alternative vision, Peirce proposes an expansion of conceptions of evolution to include altruistic purpose. Peirce believes that human individuals can recognize the highest purposes and aims of other individuals and can choose to act in support of those high aims and purposes. He called this aspect of evolution "agapastic evolution". The logic of agapastic evolution is what characterizes the higher social processes of science, art, education, culture, history, and institutions.

In "Evolutionary Love," Peirce criticizes Simon Newcomb for taking an exceptionally narrow view of self-interest in his *Principles*. Newcomb discusses three forms of self-love, none of which rises to the level of agapastic evolution in Peirce's view. Some harsh and repressive attitudes toward the poor and disadvantaged emerge in Newcomb's discussion. He couches poverty and disadvantage in terms of supply and demand and views them as mostly voluntary social situations. It appears that Newcomb acted on the basis of these attitudes in his opposition to Peirce on many occasions during their lives. Newcomb stifled Peirce's career on five significant occasions, which left Peirce in poverty and ostracized from academic and social circles for the last three decades of his life. Based on his remarks, it would seem that Newcomb felt morally justified in opposing the advancement of Peirce's life and career. For someone so brilliant as Peirce, can there be any doubt that Newcomb would have concluded that Peirce's failures were voluntary?

Perhaps more than for other historical figures, Peirce's writings require context. Ideally, several levels of context would be helpful in explicating his conception of complexity and his critique of political economy. The first layer of context would be a coherent presentation of Peirce's ideas about complex phenomena. His manuscripts and papers were never organized or presented as a coherent unity in his lifetime. For the last twenty-five years of his life Peirce was without a job or a steady source of income. During this period he wrote approximately eighty thousand pages of handwritten manuscripts. Some of these have been published in the form of *Collected Papers* (*CP*) and the *Writings of Peirce* (*WP*).[2] It is in these manuscripts that Peirce elaborates an evolutionary philosophy which provides a theory of complexity.

A second layer of context would be a coherent account or meta-theory of complexity which would range over the domains of the current natural and social sciences. Peirce's writings ranged over an extremely wide scope. Thus a conception of complexity with a similar breadth of scope might be useful in understanding important aspects of Peirce's contributions. Since theories of complexity have been explored in an earlier essay in this volume, here only a brief recapitulation of the major aspects will be needed.

A third layer of context would be an exploration of the historical, social, and intellectual context in which Peirce created his critique of political economy. In this account, as much attention as possible will be paid to this third layer of context.

Readers should also know that little scholarship exists on the broader context of Peirce's ideas. An exception is the biography by Joseph Brent (1993). Most Peirce scholars are philosophers who are interested in the content and novelty of his ideas relative to those of other philosophers and scientists. Here Peirce's scientific and philosophical ideas provide the context for his critique of political economy. The broader context for Peirce's conception of science and philosophy would require a much more encompassing effort beyond the relatively restrictive limits of this essay. To the extent that the third layer of context is incomplete, it may appear that this account is somewhat antiquarian or whiggish. One temptation might be to give an overly simplified interpretation which would go something like this: Here is a modern conception of complexity which Peirce anticipated in its major features—look how advanced Peirce was for his time. The story is much richer than a conclusion of this sort. Peirce's critique of Newcomb in the context of an evolutionary cosmology of complexity is worthy of some attention on its own merits.

Some background on Peirce's life and economic writings

Almost a century after his death, Charles Sanders Peirce is widely recognized as being the single greatest philosophical and scientific mind that has ever been produced on the North American continent. He thought about and wrote in many areas of inquiry. In philosophy, Peirce is known for several contributions: as the founder of American pragmatism; for the creation of a system of metaphysics based on categories of mathematical relations; and for his own theory of evolution, agapastic evolution.[3] Similarly, in science, Peirce is known for path-breaking research in several disciplines. In astronomy, he was the first to estimate the shape of the Milky Way galaxy as a disc. In geodesy, he conducted pendulum research aimed at estimating the shape of the earth's curvature and he created a more uniform standard of linear measurement by relating the meter and the yard to the wave length of sodium light. In experimental psychology, he conducted experimental research using the first system of randomized experimental controls on the psychology of sensation. In chemistry, he published a table of elements before Mendeleeve. In mathematics, Peirce made important contributions to set theory. Additionally, he is widely credited with creating a theory of representation, a theory of signs and meaning known as semiotics. At an early age he wrote about the concepts of "I, IT, and THOU."[4]

Charles Sanders Peirce was the son of Benjamin Peirce, America's greatest mathematician in the mid-nineteenth century, an astronomer, and professor of mathematics at Harvard. Charles was personally schooled by his father for much of the first two decades of his life. Peirce attended Cambridge High School, graduated with a bachelor's degree from Harvard in 1859, took a master's degree from Harvard in 1862, and received a bachelor's degree in chemistry *cum laude* in 1863 from Lawrence Scientific School in Cambridge. He was tutored in biology and classification by Louis Agassiz. In his twenties, Peirce did research at the Harvard Observatory and worked as an aide in the Coast Survey. He also gave lectures on the logic of science in 1866 to Harvard graduates. These lectures are now seen as the beginnings of graduate education at Harvard. In 1867 he wrote a long article on the logic of relatives. In his thirties, Peirce took charge of the pendulum experiments for the Coast Survey, traveled in Europe to observe and conduct pendulum research, and published a monograph on astronomy. In his forties during the 1880s, Peirce was appointed to and dismissed from the faculty at Johns Hopkins University. Two of his students were Thorstein Veblen and John Dewey. He was divorced and remarried during this period. This led to his being treated as a social and academic outcast for the remainder of his life. In his fifth decade, he renewed his work on his system of metaphysical categories, he ran into severe financial difficulties with schemes to replace lost income due to his resignation from the Coast Survey, and he wrote a great deal on the history and methodology of science. In his sixties during the first decade of the twentieth century, Peirce applied for a grant from the Carnegie Institution but his request was denied. In 1903, he gave a now famous set of "Lectures on Pragmatism" at Harvard, which William James refused to publish. He died in the spring of 1914.[5]

While much of the background and history of Peirce's life is now widely known, his contributions to economics have remained in obscurity. Much of his work on economics was done in handwritten manuscripts and was not published during his lifetime. The major exceptions are: the "Note on the Theory of the Economy of Research" which was published in 1879 as a government publication; his critique of the legal system, "Dmesis," in 1892; and of course his critique of Simon Newcomb in an 1893 article, "Evolutionary Love." Peirce's interest in economics falls into several broad categories which can be further subdivided as follows:

1 Mathematical political economy:
   (a) Darwinian competition, insurance, and probability;
   (b) competition, monopoly and duopoly;
   (c) consumer theory and the axiom of transitivity;
   (d) a mathematical, utility model of research project selection.

2 Economics of science:
   (a) research project selection;
   (b) economic aspects of hypothesis formation;
   (c) an economic critique of the reconstruction of Aristotle's works and the time horizon of Plato's life.

3 A critique of Simon Newcomb and nineteenth-century political economy.
4 Law, economics, and punishment.

Peirce's interest in mathematical political economy is a reflection of his father's interest in applying mathematics to economics and the intellectual milieu of Cambridge, Massachusetts in the mid-nineteenth century. Benjamin Peirce was one of the towering intellects at Harvard at that time. The most prominent figures of science, philosophy, mathematics, law, and some of the performing arts and literature were visitors in the Peirce home. Benjamin started a club or two to discuss topics of interest to the members. From 1854 to 1857 the Peirces were prominent participants in the Cambridge Astronomical Society which was succeeded by the Mathematics Club. In 1871 and 1872 the Metaphysical Club was created. The Metaphysical Club is thought to be the context for the birth of pragmatism. In late 1871, both

Charles and Benjamin had a keen interest in mathematical political economy. In December Benjamin made a public presentation for which Charles drew the graphs.[6] For the month of December of 1871, there are several letters about political economy. In one letter, Peirce (1871a) writes to Zina Fay, his first wife, about discussing political economy with Simon Newcomb. In a letter to Newcomb on December 17, Peirce (1871b) explores aspects of monopoly and competition and provides the mathematical conditions of the profit-maximizing monopolist. In a third letter, to his father, Charles questions one of the fundamental assumptions behind the duopoly model of Cournot (Peirce 1871c). Charles remarks that Cournot's result does not hold if the duopolists do not assume the same price. Three years later, in response to a letter from Abraham Conger, Peirce (1873) again explores the profit-maximizing conditions for monopoly. Also in 1874 is a short manuscript composed of two fragments. One fragment develops the mathematical theory of monopoly in more detail. The other fragment explores some basic aspects of consumer theory and provides what may be the earliest statement of the axiom of transitivity (Peirce 1874).[7]

Peirce's (1879) most elaborate publication on economics, the "Note on the Theory of the Economy of Research," exhibits his superb mathematical skills and his almost unique ability to apply economics to science.[8] The mathematics of the "Note" are as sophisticated as any piece of mathematical economics in the late nineteenth and early twentieth centuries. Peirce develops a mathematical model of multiple project selection in science and solves for the first-order conditions. Then he reduces the model to the special case of two projects and illustrates the marginal conditions of selection of research projects with a graph. Other than the sophistication of the mathematical model, the "Note" is unique for taking an economic approach to science. At this point in his career, Peirce was conducting pendulum experiments for the Coast Survey. He was aware of the limitations of the equipment and concerned about increasing the precision and exactness of the measurements. Often he used two pendulums or varied the heavy end up or down because he was concerned with the inaccuracy of measurement. The pendulums were costly instruments and the measurements were quite time-consuming, involving continuous observation and record-keeping for twenty four hours a day for days on end. This is the context in which Peirce imagined the economic application which became the "Note." In the "Note" Peirce claims that the ratio of greater precision to cost for swinging one pendulum should equal the ratio of greater precision to cost for swinging the second pendulum.

The "Note" represents the beginning of a life-long interest in the economic aspects of science. Two decades later, Peirce would return to these themes in two other writings. In the "Logic of History" (1901) Peirce creates a scientifically based methodology for interpreting evidence about ancient manuscripts and the lives of Aristotle and Plato.[9] This monographlength manuscript contains about the equivalent of ten published pages on economic aspects of hypothesis formation. In another piece in the 1890s, Peirce (1896b) includes economic aspects of research in a manuscript on the history and methodology of science. Then, in 1902, he includes the "Economics of Research" as one of the essays he would write as a memoir which was to be funded by the Carnegie Institution. The grant application was denied. Peirce (1892a) also wrote "Dmesis," a published article criticizing the view of punishment based on utilitarian philosophical and social doctrine. He questioned the disincentive effects of incarceration, believing that it was not as effective as had been imagined.

Complexity in Peirce's thought

Among the least known of Peirce's writings are those on evolutionary complexity. Peirce wrote about evolutionary complexity in the natural and social world and in the sciences. However, what has preoccupied the attention of most of the commentators, most of them philosophers, has been Peirce's metaphysical categories. Here the focus will be on Peirce's vision of the creation, and evolution of the universe from the moment of the creation through the appearance and development of biological, social, cultural, intellectual, and even scientific life. These are the ideas which provide the context for his critique of Simon Newcomb. In these writings on creative evolution, Peirce uses much of the same vocabulary as late twentieth century complexity theory.[10] He considers the cosmos and the social and natural worlds as being organized into hierarchies and levels of phenomena and entities. He writes about the spontaneity and self-organization as the spontaneous emergence of habits and laws. He clearly suggests that life processes violate the conservative laws of physics with regard to ordinary matter. He recognizes and writes in no uncertain terms that the processes of life, of society, and of history are irreversible in time. The idea of order in phenomena is almost always described without the vocabulary and concepts of equilibrium unless it is mechanical order which is under consideration. Peirce recognized that the birth of the cosmos, what is today called the "big bang," places all of creation and subsequent evolution in the context of a vast non-equilibrium process. Instead of dynamic equilibrium, Peirce uses the term hyperbolic to describe the evolution of the universe: It had a beginning, it grows and develops new hierarchies of order and pattern, and it is moving toward an end, rather than toward some eternal equilibrium or stasis. Peirce also thought that mathematics could be used to describe and investigate somewhat stable patterns of order and habit in natural and social phenomena. He was one of the first to separate the process of mathematical modeling from the collection of data and the drawing of valid statistical inferences. Perhaps his most abstract and least understood contribution was his theory of evolutionary complexity in terms of the complexity of abstract mathematical relations which are possible in the evolutionary process.

The theory of evolution had a profound impact on Peirce's philosophy and conception of economics. He had his own approach to the theory of evolution and its relevance to economics. One of the more prominent statements of his approach can be found in "Evolutionary Love." Peirce's interest in evolution stemmed from his reading of Darwin's *Origin of Species*.[11] When the *Origin* first appeared, Peirce was in the field on assignment for the Coast Survey. It was a half year or more after it appeared that he was able to read the work. At this point, he was in his early twenties in the early 1860s. In 1871 and 1874 we know that Peirce was interested in mathematical political economy. In later years, he focused more explicitly on evolution. In his view, Darwin had taken some of his ideas from the political economists, so that it was legitimate to associate nineteenth century political economy with Darwinian evolution.[12] In 1882, he wrote:

> The scientific specialists—pendulum swingers and the like—are doing a great and useful work; each one very little, but altogether something vast. But the higher places in science in the coming years are for those who succeed in adapting the methods of one science to the investigation of another. That is what the greatest progress of the passing generation has consisted in. Darwin adapted to biology the methods of Malthus and the economists.... Cournot adapted to political economy the calculus of variations.
>
> (Peirce 1882, *WP* 4:380)

Peirce's theory of evolution broadened and deepened to the point that it exhibited many of the aspects of a theory of complexity. In some respects, almost a century after Peirce was doing his best work, we should not expect as complete a vision of a complex universe as that of late twentieth century complexity theory. In other respects, Peirce's views are more developed than those of contemporary scientists. Like late twentieth century physicists, Peirce has an evolutionary cosmology of complexity for understanding the birth and further development of the universe. In "A Guess at the Riddle" in 1890 Peirce wrote:

> Our conceptions of the first stages of the development, before time yet existed, must be as vague and figurative as the expressions of the first chapter of Genesis. Out of the womb of indeterminacy we must say that there would have come something, by the principle of Firstness, which we may call a flash. Then by the principle of habit there would have been a second flash. Though time would not yet have been, this second flash was in some sense after the first, because resulting from

> it. Then there would have come other successions ever more and more closely connected, the habits and the tendency to take them ever strengthening themselves, until the events would have been bound together into something like a continuous flow. We have no reason to think that even now time is quite perfectly continuous and uniform in its flow…
>
> (Peirce 1890, *CP* 1:225)

While Peirce does not explicitly use the terms self-organization or spontaneous order of more recent complexity theory, the key aspects of those ideas are present in his views. The idea of spontaneity is present in the preceding account of the birth of the cosmos. With regard to the newness and novelty of creation after the birth of the cosmos, the ideas of spontaneity and creativity are evident in another passage harking back to the description of creation in Genesis:

> What the world was to Adam on the day he opened his eyes to it, before he had drawn any distinctions, or had become conscious of his own existence—that is first, present, immediate, fresh, new, initiative, original, spontaneous, free, vivid, conscious, and evanescent.
>
> (Peirce 1890, *CP* 1:183)

About fifteen years later in now-published notes on scientific philosophy, Peirce comments on the emergent orders of phenomena and how evolution is incompatible with mechanism. He focuses on the sheer variety of things which is a consequence of both spontaneity and chance operating simultaneously side by side with lawfulness and determinacy:

> Evolution means nothing but *growth* in the widest sense of that word. Reproduction, of course, is merely one of the incidents of growth. And what is growth? Not mere increase. Spencer says it is the passage from the homogenous to the heterogenous—or, if we prefer English to Spencerese—*diversification*… But think what an astonishing idea this of *diversification* is! Is there such thing in nature as increase of variety? Were things simpler, was variety less in the original nebula from which the solar system is supposed to have grown than it is now when the land and sea swarms with animal and vegetable forms with their intricate anatomies and still more wonderful economies? It would seem as if there were an increase in variety, would it not? And yet mechanical law, which the scientific infallibilist tells us is the only agency of nature, mechanical law can never produce diversification…. How can the regularity of the world increase, if it has been absolutely perfect all the time?
>
> (Peirce 1905, *CP* 1:71–72)

Besides the spontaneous, self-organizing variety of entities which grow and evolve in nature, Peirce also recognized the difference between conservative and dissipative systems, although he did not use those terms explicitly. Peirce recognized that evolutionary processes are incompatible with mechanism and the laws of conservation:

> We at once recognize that almost all the phenomena of bodies here on earth which attract our familiar notice are non-conservative, that is, are inexplicable by means of the Law of the Conservation of Energy. For they are actions which cannot be reversed. In the language of physics they are irreversible. Such, for instance, is birth, growth, life. Such is the motion resisted by friction or by the viscosity of fluids, as all terrestrial motion is. Such is the conduction of heat, combustion, capillarity, diffusion of fluids. Such is the thunder bolt, the production of high colors by a prism, the flow of rivers, the formations of bars at their mouths, the wearing of their channels; in short, substantially everything that ordinary experience reveals, except the motions of the stars.
>
> (Peirce 1898, *CP* 6:52)

In another passage, which is quite reminiscent of the Rössler model presented by Nicolis and Prigogine (1989) in their book *Exploring Complexity,* Peirce discusses spiral and hyperbolic orbits which are "non-conservative actions which seem to violate the law of energy" (Peirce 1898, *CP* 7:286 and 291).[13] In one of his published articles, "The Architecture of Theories," Peirce (1891a) extends concepts of evolution to the domain of ideas and institutions. Here he considers the possibility of rather sudden, cataclysmic changes implied from paleontology:

> This mode of evolution, by external forces and the breaking up of habits, seems to be called for by some of the broadest and most important facts of biology and paleontology; while it certainly has been the chief factor in the historical evolution of institutions as in that of ideas; and cannot possibly be refused a very prominent place in the process of the universe in general.
>
> (Peirce 1891a, *CP* 6:17)

Like the more recent complexity theorists, Peirce realizes that most things, entities, and processes in the world are in a state of non-equilibrium. Equilibrium is a term retained most often for the state of reversible order in mechanical systems which obey conservation laws. For the orders and patterns of evolutionary phenomena, Peirce uses non-mechanical adjectives such as regularity and uniformity rather than equilibrium:

> Now there are three characters which mark the universe of our experience in a way of their own. They are Variety, Uniformity, and the passage of Variety into Uniformity. By the Passage of Variety into Uniformity, I mean that variety upon being multiplied almost in every department of experience shows a tendency to form *habits*. These habits produce statistical uniformities. When the number of instances entering into the statistics are small compared with the degree of their variation, the laws will be extremely rough, but when the number runs up into the trillions, that is to say cubes of millions, or much higher, as in the case of molecules, there are no departures for the law that our senses can take cognizance of.
>
> (Peirce 1903b, *CP* 6:75)

In another passage, Peirce recognizes that the whole universe is far from equilibrium. In this process of non-equilibrium, the growth of the world and many things and processes in the world takes place:

> I may mention that my chief avocation in the last ten years has been to develop my cosmology. This theory is that the evolution of the world is *hyperbolic,* that is, proceeds from one state of things in the infinite past, to a different state of things in the infinite future…Between these, we have on *our* side a state of things in which there is some absolute spontaneity counter to all law, and some degree of conformity to law, which is constantly on the increase owing to the growth of *habit.* The tendency to form habits or tendency to generalize, is something which grows by its own action, by the habit of taking habits itself growing. Its first germs arose from pure chance.
>
> (Peirce 1891b, *CP* 8:214)

Much of the preceding summary of Peirce's conception of the world is remarkably similar to late twentieth century complexity theory. Peirce summarizes some of the most important implications of his vision of the nature of the universe as an evolving, pluralistic complex entity:

> Consider the life of an individual animal or plant, or of a mind. Glance at the history of states, of institutions, of language, of ideas. Examine the succession of forms shown by paleontology, the history of the globe as set forth in geology, of what the astronomer is able to make out concerning the changes of stellar systems. Everywhere the main fact is growth and increasing complexity …From these broad and ubiquitous facts we may fairly infer, by the most unexceptionable logic, that there is probably in nature some agency by which the

> complexity and diversity of things can be increased; and that consequently the rule of mechanical necessity meets in some way with interference.
>
> (Peirce 1892b, *CP* 6:40–41)

Besides sharing a view of the world as an evolving, multi-leveled, ontologically pluralistic complexity, Peirce also attempted to portray the fundamental properties of the universe and the processes of evolution in a mathematical way. Besides his contributions to sciences and philosophy, Peirce was also an accomplished mathematician. He had a lifelong interest in mathematical logic. In his late twenties, Peirce wrote a paper on mathematical logic which gained the attention of mathematicians around the world, including the British logician and mathematical economist, Stanley Jevons. Obviously much of Peirce's scientific research in astronomy and geodesy required state of the art computations which made him a leader in applied mathematics in his day. However, it was his interest in theoretical mathematics which he used to represent the mathematical properties of the processes of evolution. In 1870, he wrote a long paper on the "logic of relatives" which dealt with the theoretical foundations of mathematics. Before Peirce (1870) wrote his paper, George Boole had created a new approach to sets. Set theory had focused on classifying objects which had similar characteristics. Set theory was content-oriented and focused on the similarity if not identity of things in a set or collection of objects. In the 1870 paper, Peirce focused on relations between objects or processes rather than on the shared identity of a class of objects. Attention was directed to the relation of one thing being greater than or equal to another. Of particular attention for Peirce was the relation of transitivity among three entities, designated A, B, and C. It is obvious that this research in theoretical mathematics is behind the formulation of the axiom of transitivity in consumer theory which is found in Peirce's manuscript fragment "On Political Economy," penned in 1874:

> The dependence of demand on price arises from this fundamental proposition. The desire of a person for anything has a quantity of one dimension, and a person having a choice will take that alternative which gives him the greatest satisfaction. In other words *if a person prefers A to B and B to C he also prefers A to C. This is the first axiom of Political Economy.*
>
> (Peirce 1874, *WP* 4:176, italics added)

What is so unusual is that Peirce not only conceived of mathematical, scientific, and economic relationships in terms of relational logic, he also thought about the processes of evolution using mathematical relational logic. Perhaps inspired by his father, Peirce characterized the processes of evolution in terms of the complexity of their mathematical relations.[14] In a series of manuscripts whose significance has not been fully realized, Peirce attempted to theorize about creative evolutionary complexity in terms of abstract mathematical properties. It appears that he set out to mathematically categorize the processes of creative evolution. These manuscripts are known as his "Guess at the Riddle" (Peirce 1890).[15] Peirce conceived of evolution in terms of three interrelated categories of mathematical relations which are nested within each other. One category is used to portray that anything is possible in terms of the mathematical, relational properties before anything has ever been created. This is the first category, which is also called firstness. Firstness is conceived as a category of relations before any thing, process, individual, or time had come into creation. All conceivable worlds, processes, and individuals were possible. Peirce saw the moment before time began as the logical equivalent of some undifferentiated cosmic plasma or soup before anything was differentiated from anything else. Since everything was possible before anything was created, there was not anything which was related to anything else. Mathematically, this suggests a relational oneness of any process, object, or entity with itself. Peirce called this imagined hypothetical situation before creation "firstness," and he used the term "monad" to refer to the relational oneness of its mathematical character.

Moving to the second category of creative evolution, once something has come into being, created entities can exhibit relations which can be characterized by their abstract mathematical properties in relation to one another. This category is called secondness. In a manner similar to his conception of how time began as quoted above, Peirce held that something would come into being by sheer chance. Then, once something was created, dyadic mathematical relations are possible. "A" can be related to "not-A." After the very first thing, entity, or process was created another entity can come into being. When a second thing is created and it persists, two entities can be related, or A can be related to B. Such coming into being is the basis of facts. Furthermore, the fact that A and B have come into being has consequences which may forbid other things, entities, or processes, from coming into being. This gives rise to the economic idea of opportunity cost raised to the level of an ontological principle and expressed as a mathematical relation. Another aspect of anything such as A and B coming into being is that they are finite. This gives rise to the idea of constraint which is fundamental to the notions of scarcity in economics. In terms of the mathematical relations which Peirce uses to characterize evolution, facts, constraints, and scarcity are dyadic and called secondness.[16]

Once the second category of evolutionary relationships appear, as more and more things and processes come into being, more complicated patterns of mathematical relationships are possible. As more and more things and processes are created and beings appear with intelligent self-consciousness, relational comparisons can be made. Three-way mathematical relationships are possible, and this category was called thirdness. One example of thirdness which Peirce used over and over was inherently economic, that "A gives B to C."[17] He maintained that this complex relation could not be further reduced to a dyadic or monadic relation. It was one of his principal arguments of the existence and reality of thirdness as an evolutionary category. In Peirce's mathematical, evolutionary cosmology, the first category is nested within the second, and the first and second categories are nested within the third. So the categories form an interrelated and nested hierarchy of abstract mathematical relationships of increasing complexity.

Having established his three mathematical relational categories as the logic of a cosmology of evolutionary complexity from before time began, Peirce set out to persuade others of the fertility of his approach.[18] One aspect of his argument was an attempt to show that every scientific discipline could be conceived in terms of his abstract mathematical, relational categories of firstness, secondness, and thirdness. Another aspect of his argument took up the rhetoric of mathematical proofs. Peirce sought to argue that any evolutionary relationship in nature or society which was more complicated than a triad could be reduced to a triad. In spirit, this argument seems to foreshadow the mathematical formalism of the early twentieth century which sought to find the smallest number of axioms on which a mathematical system could be based. Peirce seems to argue that the smallest number of abstract mathematical and metaphysical categories which can be used to philosophize about the nature of the universe are three categories such as firstness, secondness, and thirdness.

As any contemporary reader might suspect, very few in his own time understood Peirce's relational, mathematical, and metaphysical categories of creative evolutionary complexity. Hardly anyone in Peirce's time understood relational set theory and the work on the foundations of mathematics. Peirce made the triadic categories a significant part of his Harvard lectures of 1903. The lectures came across as being very muddled and murky. William James did not understand Peirce's mathematical categorical, relational metaphysics and refused to have the 1903 lectures published. Consider the burden placed on anyone who would attempt to understand Peirce's categories. An evolutionary cosmology of the universe and everything in it synthesizes research across many sciences and philosophy. It would be full of strange, bizarre, and unfamiliar concepts for even an extremely well-educated contemporary of Peirce such as James. Translating the specific contributions of the particular sciences into their logical, relational equivalents raises the level of abstraction and unfamiliarity even further. It is no wonder that so few have understood Peirce's relational cosmology of evolutionary complexity in his own time.

Complexity in Peirce's theories of evolution and the self

Having explored Peirce's cosmology of evolutionary complexity, his critique of Simon Newcomb and nineteenth-century political economy now can be considered. In the 1890s, when the critique of Newcomb was written, Peirce was preoccupied with developing and applying his evolutionary cosmology in terms of the relational mathematical categories. Contrast the preceding material with the usual characterization of Peirce. For many, if not most readers, he is known as a pragmatist philosopher who was much admired by the logical positivists of the early twentieth century. Typically, his most important philosophical contribution is thought to be the now famous, pragmatic methodological maxim:

> Consider what effects, that might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception.
>
> (Peirce 1878:31)

For those who venture beyond the pragmatic maxim, there are several other methodological papers about science and scientific inquiry. While these methodological papers are worthy of attention, they cannot serve as an adequate basis for understanding Peirce's critique of political economy. We need the more complete picture of his cosmology of evolutionary complexity and its extension to the world of human thought.

During the 1890s Peirce worked a great deal on his mathematical, relational and metaphysical cosmology of evolutionary complexity, his "Guess at the Riddle." This is the context for the critique of Newcomb. "Evolutionary Love" was the last of a series of five articles to appear in *The Monist* in the 1890s. In this article, Peirce suppresses almost all explicit mention of the process of creative evolution in terms of the mathematical categories. However, the high level of abstraction required to make sense of the logic of mathematical relations is apparent in the essay. The very first sentence of "Evolutionary Love" begins by invoking the context of an evolutionary cosmology: "Philosophy, when just escaping from its golden pupaskin, mythology, proclaimed the great evolutionary agency of the universe to be Love" (Peirce 1893, *CP* 6:190).

Instead of using his own story of creation as fashioned in his "Guess at the Riddle" and instead of returning to the Genesis narrative of creation in the Old Testament, Peirce begins by considering the creation narrative in the New Testament. In the New Testament, it is St. John, the writer of the Gospel, the three Epistles, and Revelation, who presents a cosmology of the beginning and end of time and everything in between. Compared to the Genesis narrative, what is striking about the creation prologue of the Gospel of John is its high level of abstraction.[19] The creation prologue is the first eighteen verses of the first chapter. The narrative begins with the assertion that generalized conception, "The Word," existed before anything was created or any process had begun. In Peirce's view and that of the gospel writer, creation is authored from and by the sheer fecundity of some sort of generalized conception to which divine agency is often attributed. From "The Word" being the source of the beginning of the universe, the gospel writer turns to what was created first. Like the Genesis narrative, one of the first, if not the very first, "things" to be created was light. For Peirce, the appearance of light makes the universe fundamentally different. Like the commencing of time, light signals the beginning of a qualitatively different universe. We could debate which occurred first: the beginning of time or light. The appearance of either time or light or if they commenced simultaneously would signal a universe which could be described with a whole new level of mathematical relationships. From the creation theme of light, St. John moves to the theme of love. Love is the theme of much of the Gospel of John and that of the three Epistles of John. In contrast, judgement, destruction, and wrath are the themes of Revelation. Peirce makes "Evolutionary Love" the centerpiece of his critique of Newcomb and nineteenth-century political economy.

In "Evolutionary Love," Peirce's portrays love as the highest principle of creative evolution. Besides the introduction which is couched in terms of "love" and "light" as they are used by St. John, the article "Evolutionary Love" has three substantive parts. First was the critique of Newcomb and nineteenth-century political economy; then Peirce presents three interrelated theories of evolution; and the piece ends with illustrations to support Peirce's view of "Evolutionary Love" from the sciences and the history of science. Conceptually, it is easier to begin with Peirce's view of the theories of evolution as presented in this piece, to consider the illustrations second, and then to consider the critique of political economy last.

There are three theories of evolution which are presented in Peirce's "Evolutionary Love."[20] The first theory considered in some detail is Darwin's. By 1893, when Peirce wrote this essay, it had been over thirty years since he had first read the *Origin of Species*. In Peirce's view, Darwinian evolution is about the struggle for existence. Chance plays a role. Fortuitous variations occur which create a strength or advantage for some varieties so that natural selection occurs. Those species and individuals which acquire an advantage are the ones which dominate this competitive struggle for existence.

The second theory of evolution is mechanism or a principle of mechanical necessity. If change and growth are thought to be predetermined by some force or principle of necessity and if this mechanical necessity is thought to be unalterable, then we have a mechanistic view of evolution. At the time when Peirce was writing, he thought that many naturalists in embryology and genetics and many geologists embraced a principle of mechanical necessity in explaining apparent change in their domains of study. Also, in the domain of human history, Peirce thought that Hegel's theory and philosophy of history was deterministic and mechanistic.

The third theory of evolution is the Lamarckian theory. According to Pierce, there may be "hypertrophies or atrophies" which individuals transfer to their offspring. The variations among individuals arise neither by chance nor by a principle of mechanical necessity. Instead, an individual in a species strains to accomplish a task or, as Peirce remarks: "the straining of endeavor and the overgrowth superinduced by exercise" (Peirce 1893, *CP* 6:201). For Peirce, the action of an individual within a variety or species has an end, thus there is an implicit and inherent psychological aspect to Lamarckian evolution:

> Now, endeavor, since it is directed toward an end, is essentially psychical, even though it be sometimes unconscious; and the growth due to exercise…follows a law of a character quite contrary to that of mechanics.
>
> (Ibid.: 201)

It is in the context of Peirce's interpretation of Lamarckian evolution that human purposiveness appears. Action directed towards an end has purpose. The highest end or purpose in the universe is love, according to Peirce. Peirce's view of evolution, "Evolutionary Love," as he conceives it, is Lamarckian. Since there are several conceptions of love, somehow we need to select the highest form of it. Peirce briefly considers eros as a conception of love. However, he believes that eros dominates the Darwinian level of evolution. He sees a logically superior form of love in the New Testament. Here Peirce turns to St. John and his conception of the role of love and light in the creation and development of the universe. Peirce begins with the golden rule. He believes that St. John presents an evolutionary cosmology based on *agape* as found in the logical principles presented in the life and teachings of Christ in the New Testament:

> The movement of love…is fully summed up in the simple formula we call the Golden Rule. This does not, of course, say, Do everything possible to gratify the egoistic impulse of others, but it says, Sacrifice your own perfection to the perfectionment of your neighbor. Nor must it for a moment be confounded with the Benthamite, or Helvetian, or Beccarian motto, Act for the greatest good of the greatest number.
>
> (Ibid.: 191)

With regard to the golden rule, Peirce generalizes in the following way:

> Everybody can see that the statement of St. John is the formula of an evolutionary philosophy, which teaches that growth comes only from love, from I will not say *self-sacrifice,* but for the ardent impulse to fulfill another's highest impulse. Suppose, for example, that I have an idea that interests me. It is my creation. It is my creature; for as shown in last July's *Monist,* it is a little person. I love it; and I will sink myself in perfecting it. It is not by dealing out cold justice to the circle of my ideas that I can make them grow, but by cherishing and tending them as I would the flowers in my garden. The philosophy we draw from John's gospel is that this is the way mind develops; and so for the cosmos, only so far as it yet is mind, and so has life, is it capable of further evolution. Love, recognizing germs of loveliness in the hateful, gradually warms it into life, and makes it lovely.
>
> (Ibid.: 192)

At this point it might appear that there are three entirely separate theories of evolution with one preferred by Peirce. Of course, Peirce places great stock in his view of agapastic evolution. However, if the ontological pluralism of evolutionary complexity theory is kept in mind, all of the theories of evolution may have a domain of application. The pluralism of complexity theory suggests a pluralism of systems of evolution. As found in "Evolutionary Love," Peirce portrays evolution as occurring at different levels of complexity, so that the three theories of evolution are homing in on different aspects of a more general evolutionary process. At one point Peirce renames the three theories and suggests that they are all simultaneously operative in the universe:

> Three modes of evolution have thus been brought before us: evolution by fortuitous variation, evolution by mechanical necessity, and evolution by creative love. We may term them *tychastic* evolution, or *tychasm, anancastic* evolution, or *anancasm,* and agapastic evolution, *agapasm*… On the other hand the mere propositions that absolute chance, mechanical necessity, and the law of love are severally operative in the cosmos may receive names of *tychasm, anancasm,* and *agapasm.*
>
> All three modes of evolution are composed of the same general elements…tychasm and anancasm are degenerate forms of agapasm.
>
> (Ibid.: 203)

In the realm of ideas, social history, and human institutions, Peirce maintains that all three conceptions of evolution have been operative. In what comes as an argumentative surprise, according to Peirce one example of evolution by chance or Darwinian evolution is the first few centuries of the history of Christianity. He asserts that: "the largest example of tychasm is afforded by the history of Christianity, from about its establishment by Constantine to, say, the time of the Irish monasteries, an era or eon of about 500 years" (ibid.: 207). What accounted for the spread of Christianity in the early centuries was the greed and hard-heartedness of the Romans. These were chance factors external to Christianity and were unsystematically related to the message and conduct of the movement, in Peirce's view. Moving from early Christianity as an illustration of Darwinian evolution, examples of the mechanistic theory can be considered. Mechanistic evolution or anancastic evolution "advances by successive strides with pauses between" (ibid.: 209). An external cause of mechanical evolution could be an abrupt event such as war which changes the course of evolution. An internal cause of mechanistic evolution is a predestined course of historical development whether the end is foreseeable or not. Peirce attributes this idea of mechanistic evolution to Hegel. He remarks:

> The anancasticist…makes development go through certain phases, having its inevitable ebbs and flows, yet tending on the whole to a foreordained perfection…The Hegelian philosophy is such an anacasticism… Yet, after all, living freedom is practically omitted from its method.
>
> (Ibid.: 204)

The third theory is Peirce's view of evolution. The last few pages of "Evolutionary Love" contain examples of agapastic evolution. These are instances of human growth which exemplify the highest principles of human motivation. Peirce cites examples from architecture and simultaneous discovery in science and technology:

> If it could be shown directly that there is such an entity as the "spirit of an age" or of a people, and that mere individual intelligence will not account for all the phenomena, this would be proof at once of agapasticism… I believe that all the greatest achievements of mind have been beyond the powers of unaided individuals; and I find, apart from…the purposive character of many great movements, direct reason for so thinking in the sublimity of the ideas and in their occurring simultaneously and independently to a number of individuals of no extraordinary general powers.
>
> (Ibid.: 212–213)

In architecture, Peirce suggests that Gothic architecture appeared in many places concurrently. In science, the discovery of a planet beyond

Uranus, the principle of the conservation of energy, the kinetic theory of gases, the simultaneous presentation of evolution by Darwin and Wallace, and the authorship of a periodic chart of elements are all given as examples of simultaneous discovery. In technology, the telegraph and the use of ether as an anaesthetic are achievements driven more by higher levels of sentiment than by the desire for material gain, according to Peirce.

Complexity and the critique of Newcomb

Peirce's critique of Newcomb's *Principles* and nineteenth-century political economy in "Evolutionary Love" can now be placed in context. The context is the triadic hierarchy of levels of evolutionary complexity in Peirce's cosmology. Peirce's evolutionary cosmology ranges over natural and social phenomena and much of human history. His critique is that political economy confines human motivation to a lower level than is necessary. Political economy is Darwinian in Peirce's view, which limits its scope to lower-level processes of plants and animals. The vision of economic activity in nineteenth-century political economy is that of a narrow self-interest or eros rather than a higher-level motivation exhibiting an interest in the highest aims of others or *agape*. Again, in "Evolutionary Love," Peirce argues that Darwin's theory of evolution was an application of political economy to the domains of biological phenomena:

> The *Origin of Species* of Darwin merely extends politicoeconomico views of progress to the entire realm of animal and vegetable life. The vast majority of our contemporary naturalists hold the opinion that the true cause of those exquisite and marvelous adaptations of nature for which, when I was a boy, men used to extol the divine wisdom, is that creatures are so crowded together that those of them that happen to have the slightest advantage force those less pushing into situations unfavorable to multiplication or even kill them before they reach the age of reproduction. Among animals, the mere mechanical individualism is vastly reinforced as a power making for good by the animal's ruthless greed.
>
> (Peirce 1893, *CP* 6:196)

Greed as a high-level principle of evolutionary motivation is rejected by Peirce in comments which caustically criticize political economy. Such comments invite extensive quotation so that the tenor of Peirce's remarks in "Evolutionary Love" can be conveyed more accurately:

> The nineteenth century is now fast sinking into the grave, and we all begin to review its doings and to think what character it is destined to

> bear as compared with other centuries in the minds of future historians. It will be called, I guess, the Economical Century; for political economy has more direct relations with all the branches of its activity than has any other science.
>
> (Ibid.: 192)

At this point, Peirce takes up contrasting *agape* and the golden rule with self-interest in political economy:

> Well, political economy has its formula for redemption, too. It is this: Intelligence in the service of greed ensures the justest prices, the fairest contracts, the most enlightened conduct of all the dealings between men, and leads to the *summum bonum,* food in plenty and perfect comfort. Food for whom? Why, for the greedy master of intelligence.
>
> (Ibid.)

Having focused on greed as the motivation at the level of animals in a Darwinian view of evolution, he offers this sharp and cutting criticism:

> What I say, then, is that the great attention paid to economical questions during our century has induced an exaggeration of the beneficial effects of greed and of the unfortunate results of sentiment, until there has resulted a philosophy which comes unwittingly to this, that greed is the great agent in the elevation of the human race and in the evolution of the universe.
>
> (Ibid.: 193)

From a general critique of nineteenth-century political economy, Peirce moves to considering views specifically expressed by Simon Newcomb in his *Principles of Political Economy.* Near the end of the *Principles,* Newcomb presents policy implications of economics. Peirce strongly objects to these policy implications and the conception of human motivation on which they are based.

In the preface to his book, Newcomb tells us that his *Principles* condenses from economics what is most valuable to the student, balancing the usefulness of the presentation with the level of difficulty. His purpose in writing is to present "those principles of economic science which must be mastered by every one who would form an intelligent judgement of the causes which influence the public well-being" (Newcomb 1886:iii). The work is organized into five books. Book I contains five chapters relating economics and scientific method. Books II, III, and IV are about production and exchange, supply and demand, and "societary circulation." Book V contains the applications of economic science. Newcomb maintains that the scientific aspects of economics were placed in the first four books and the practical sides of the subject are in Book V. For Book V, he confesses to having "allowed himself more freedom of discussion and treatment than elsewhere" and he also reveals that "this book would have been entirely omitted, as detracting from the purely scientific character of the work, were it not that the applications of a science are essential to a good mastery of its first principles" (ibid.: iv). The passages which Peirce criticizes so intensely are those of Book V.

Peirce's critique of Newcomb's *Principles* begins with a brief overall appraisal which might not be that different from a contemporary historian of thought. In Peirce's view, Newcomb's *Principles* is just an average work. However, the preoccupation with Darwinian conceptions of motivation as greed and eros appear at the end of the work:

> I open a handbook of political economy—the most typical and middling one I have at hand—and there find some remarks of which I will here make a brief analysis. I omit qualifications, sops thrown to Cerberus, phrases to placate Christian prejudice, trappings which serve to hide from author and reader alike the ugly nakedness of the greed-god. But I have surveyed my position.
>
> (Peirce 1893, *CP* 6:193)

At this point, Peirce quotes the following passage from Newcomb's *Principles* on three forms of love which motivate economic activity:

> Our present stand-point leads us to consider three motives to human action:
> The love of self;
> The love of a limited class having common and feelings with one's self.
> The love of mankind at large. (Newcomb 1886, p. 535)
>
> (Peirce 1893, *CP* 6:193)

With regard to the love of self, Newcomb suggests that self-interest would be destructive if it were extreme and unresponsive at all to the needs and happiness of others. He believes that any individual knows his own interests better than those of others and consequently can promote them better than anyone else. He asserts in Smith-like fashion that self-interest would lead to activities which promote the interests of one's fellow man. To this point in the relevant passage there is little to find disturbing, unless one finds a general statement of the idea of the invisible hand disturbing. However, Newcomb's applications take a darker turn. With regard to love for a limited class of individuals, Newcomb feels that such narrowly focused affection is dangerous. Newcomb seems to suggest that love with such a limited domain of inclusion is a source of social strife. At a later point, he returns to this topic again particularly with regard to labor unions. He quickly moves to the third form of love, where things become quite dismal. With regard to love for humanity, Newcomb believes there are "great difficulties in the way of its most effective operation" (Newcomb 1886:535). He believes that criminals and paupers should be discouraged or prevented from bringing forth more children. He seems to propose draconian possibilities: "No measure of repression would be too severe in the attainment of the latter object" (ibid.: 536). His restrictive policy proposals extend to life after birth. Newcomb believes that children should be raised with a proper education. If not raised properly, children would be prone to becoming criminals. Such children should be placed in correctional institutions, but Newcomb believes their rehabilitation would be doubtful. After presenting a discussion of the three forms of love which motivate social and economic activity, Newcomb returns to the second category, the love of a limited class of interests, for a more extensive discussion. Here the rhetoric has shifted from love to selfishness. Newcomb identifies labor unions as a category of selfishness which could be "much more injurious to society at large" (ibid.: 537). He has presented these views on social issues because these are the things which are necessary if the student of political economy "desires to take an active part in the improvement of society" (ibid.: 538).

If these negative, judgmental, and repressive attitudes on selfishness were not enough, Newcomb in the immediately preceding pages had discussed the role of charity in civilization. Here he takes up the problem of the beggar. Begging and its relief through charity are nothing more than economic processes:

> Let us first take up the familiar case of a beggar. A gentleman is implored for relief by a repulsive piece of humanity, enshrouded in rags and covered with dirt. Moved by pity, he gives him a dime and passes on. What is the economical nature of this transaction? We reply that the transaction is one of supply and demand, belonging to the same class as the supply and demand of personal services. The combined willingness and ability of a number of persons in the community to give dimes to beggars constitutes a demand for beggary…the supply of this service arises according to the same economic laws that the supply of any other service arises.
>
> (Newcomb 1886:526–527)

The next two pages investigate the question of whether the situation of the beggar is voluntary. Newcomb suggests that it is. Then he considers how charitable institutions ought to deliver their services so that the beggar is encouraged to pursue another line of activity. The last paragraph of this section contrasts the circumstances of the beggar with that of the saver.

Newcomb maintains that saving is socially beneficial and that the successful capitalist creates profits which work toward creating greater advantages for the whole community. Whether Newcomb was as repressive and morally critical in most of his administrative roles and attitudes as suggested in the last pages of his *Principles* may not be known. We do know that Newcomb acted on his views of moral responsibility with regard to Peirce and repeatedly foiled Peirce's professional career, altering the path of his career development and essentially keeping him in poverty the last two decades of his life.

Simon Newcomb was a Canadian who attended Harvard and studied mathematics with Benjamin Peirce, Charles' father.[21] Newcomb was about ten years older than Charles. He was often a guest in the Peirce household both before and after Charles' graduation from Harvard, while Benjamin was still alive. Later in life, Newcomb became Professor of Mathematics at Johns Hopkins University. Perhaps because of Charles' eccentric lifestyle and because he was such a prickly personality, Newcomb came to oppose Peirce on many occasions of life-altering significance. There were five incidents where it is now known that Newcomb proved to be an obstacle in Peirce's career. Newcomb played a crucial role in getting Peirce dismissed from Johns Hopkins University in 1884. He withdrew the acceptance of the second part of one of Peirce's mathematical papers when he became the editor of the *American Journal of Mathematics*. In another incident, Newcomb refused to approve the publishing of Peirce's second monograph on the gravity experiments. In the 1890s, Newcomb opposed Peirce's appointment to the Office of Weights and Measures of the Coast Survey. Last, in 1903, about a decade after Peirce had publicly critiqued Newcomb's political economy text in "Evolutionary Love," Newcomb recommended that Peirce be denied a Carnegie grant which Peirce had proposed to organize and synthesize his research into a more coherent whole. In spite of all of this active opposition to Peirce by Newcomb, can there be any doubt that he would have thought that Peirce's subsequent poverty was voluntary?

For someone like Peirce who had been without the income of a regular job for about eight years by the time "Evolutionary Love" was authored, Newcomb's negative and judgmental moralizing was just too much. In the 1890s, Peirce faced destitution, hunger, homelessness, and was a fugitive from the law for non-payment of mortgage debt. In "Evolutionary Love," Peirce summons all of his knowledge of the sciences, evolution, philosophy, mathematics, theology, and economics and weaves them into a critique of his life-long nemesis, Simon Newcomb. In Peirce's view, political economy and Simon Newcomb's *Principles* have based their analysis on too narrow a conception of the creative cosmos. They are the epitome of Darwinian evolution. More specifically, a narrow version of self-interest, what Peirce calls eros in the context of a Darwinian evolution dominates the vision of economic processes. There is a much higher level of human motivation, the ability to act with purpose in the highest interest of another, agapastic evolution. In this part of his article, Peirce also rejects the narrowness of the classical view of savings which Newcomb adopts. Again the critique is that the classical theory of savings depends on lower rather than higher levels of human motivation. This passage that is strikingly reminiscent of Keynes' views of classical theory in the *General Theory:*

> So a miser is a beneficent power in a community, is he? With the same reason precisely, only in a much higher degree, you might pronounce the Wall Street sharp to be a good angel, who takes money from heedless persons not likely to guard it properly, who wrecks feeble enterprises better stopped, who administers wholesome lessons to unwary scientific men…and who by a thousand wiles puts money at the service of intelligent greed, in his own person. Bernard Mandeville, in his *Fable of the Bees,* maintains that private vices of all descriptions are public benefits, and proves it too, quite as cogently as the economist proves his point concerning the miser. He even argues, with no slight force, that but for vice civilization would never have existed. In the same spirit, it has been strongly maintained and is today widely believed that all acts of charity and benevolence, private and public, go seriously to degrade the human race.
>
> (Peirce 1893, *CP* 6:195–196)

Near the end of the critique of Newcomb, Peirce summarizes as follows:

> Here, then, is the issue. The gospel of Christ says that progress comes from every individual merging his individuality in sympathy with his neighbors. On the other side, the conviction of the nineteenth century is that progress takes place by virtue of every individual's striving for himself with all his might and trampling his neighbor under foot whenever he gets a chance to do so. This may accurately be called the Gospel of Greed…My own passionate predilection…[is] an argument of some weight in favor of the agapastic theory of evolution.
>
> (Ibid.: 196–197)

Peirce's conceptions of self-interest and love make even more sense if the context of evolutionary complexity is reconsidered. A context of complexity suggests multiple levels of evolution and motivation operating simultaneously. Peirce is not denying that narrow self-interest and greed are operative in animal or human evolution. Rather, he believes that human beings have the potential to choose higher purposes and through those choices to grow, develop, and pursue logically superior courses of human activities. From a perspective of evolutionary complexity, an individual begins with lower levels of motivation and grows to higher levels. Peirce himself expressed this in an unpublished review of Josiah Royce's *The Religious Aspect of Philosophy.* Royce was a philosopher of religion who was familiar with much of Peirce's writing. It was Royce who later took possession of the Peirce papers and manuscripts for the department of philosophy at Harvard after Peirce's death. With regard to the growth of the self in the context of evolutionary complexity in his review of Royce, Peirce remarks:

> For altruism is but a developed egoism; that some sensitiveness which in its lowest state is selfishness, first transforms itself into esprit de corps or collective selfishness; then, passing from feeling for others collectively to feeling for them individually, it becomes philanthropy, pity, sympathy tossed hither and thither rudderless on the ocean of human misery; finally, steadying itself by the conception of an ideal humanity and a divine providence, it passes into Christian charity, which gathers up all selfishness and all pities and is ready to give each its due measure.
>
> (Peirce 1885, *CP* 8:5)

Conclusions

In his 1893 article, "Evolutionary Love," Charles Sanders Peirce presented his conception of evolutionary complexity and his critique of Simon Newcomb's *Principles of Political Economy* (1886) and nineteenth-century political economy. Peirce's vision of complexity shares many features with late twentieth-century theories of complexity. Because he viewed nature and the whole universe as an evolving complex entity of many constituent sub-entities, Peirce also viewed human beings in the same way. Human beings grow and evolve into more complex entities who have the capacity for higher levels of habits and purpose. Peirce thought that human beings could recognize and choose to pursue higher purposes and aims in their own lives; and with self-sacrifice they could choose to facilitate the highest aims and purposes of others. He identified this level of human growth as agapastic evolution or "Evolutionary Love." The logic of agapastic evolution is what characterizes the higher social processes in economics, science, art, history, and culture. Other, lower levels of human evolution would continue to function in those domains of conduct where higher purposes were not cultivated.

In "Evolutionary Love," Peirce criticized Simon Newcomb for presenting an exceptionally narrow view of self-interest in his *Principles*. In that work, Newcomb discussed three forms of self-interest or self-love. What is apparent is that each of these three forms of self-interest never rises to the higher levels of human aims and purposes as envisioned by Peirce. Instead, Newcomb paints a very dark conception of self-interest and advocates harsh and repressive programs for the poor and disadvantaged. In his discussion of poverty, Newcomb presents their plight as a voluntary social situation which can be analyzed in terms of supply and demand curves. It appears that Newcomb treated Peirce in much the same way. Apparently, Newcomb thought that Peirce had consciously decided to violate the mores of the position of privilege into which he had been born in the mid-nineteenth century in Cambridge, Massachusetts. Newcomb thwarted Peirce's professional life on five separate occasions. As a consequence, Peirce and his second wife confronted extreme poverty and he was ostracized from academic and social circles for the last three decades of his life. Given Newcomb's treatment of Peirce, it would seem that the harsh moral principles of his *Principles of Political Economy* were put into practice when he was called to make decisions related to Peirce. Peirce's biographer, Joseph Brent (1993:239, 293) suggests that Peirce may not have been aware of the animosity that Newcomb had for him until the last decade of his life. A reading of Peirce's "Evolutionary Love" suggests that, in a general way, Peirce knew how mean Newcomb could be. Newcomb's treatment of Peirce may be the epitome of the ruthless Darwinian principles of competition operating between two men who were supposed to be colleagues in American science. Peirce was aware that Newcomb would have viewed his own extreme poverty as a voluntary situation. But Newcomb's opposition suggests that Peirce's plight was anything but voluntary.

Notes

1 An earlier version of this chapter was written while on sabbatical at Duke University during the spring of 1998. I would like to thank the Department of Economics and my three hosts, Craufurd Goodwin, Neil DeMarchi, and Roy Weintraub, for their graciousness in having me as a guest in their seminars for the past few months. My thanks to Paul Wendt who has also read and commented on a previous version of this chapter.

2 *The Collected Papers of Charles Sanders Peirce* is an eight-volume collection of Peirce's papers and writings published by Harvard University under the auspices of the department of philosophy. Six of the volumes were published in the 1930s and two in the 1950s. The *Writings of Charles S. Peirce* were begun in the late 1980s to provide a chronological edition of Peirce's publications and manuscripts by the Peirce project at Indiana University. To date five volumes have appeared. In this chapter, references found in the two collections are abbreviated as *CP* for the *Collected Papers* and *WP* for the *Writings of Peirce*.

3 For an excellent discussion of pragmatism and economics, consult Hoover (1994).

4 The biographical and historical aspects of Peirce's life are drawn from Brent (1993), Fisch (1982, 1984, 1986), and Houser (1986, 1993).

5 See note 4 for the sources of the material in this paragraph.
6 Fisch (1984:xxxv).
7 "On Political Economy" has been explored in detail by Wible (1995b).
8 The "Note" is discussed by Wible (1994, 1998b) and by Rescher (1976).
9 The "Logic of History" is discussed by Wible (1998a).
10 Conceptions of complexity in late twentieth-century science are surveyed by Wible in Chapter 1 in this volume.
11 Fisch (1986:xxxvi).
12 Darwin ([1859] 1968:117) has written that the struggle for existence which is a significant element in his theory of natural selection and transmutation of species "is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms." The biography of Darwin by Desmond and Moore (1991:264–267) portrays the influence of Mathus as much greater than is suggested by references to Malthus in the *Origin of Species*. Ernst Mayer (1982:478–479), a biological historian, maintains that Darwin's theory of evolution was intact before he encountered Mathus' ideas on population. Mayer maintains that the Malthusian connection "is vigorously opposed by the biological historians."
13 The Rössler model is presented in "What is Complexity," by Wible (Chapter 1 in this volume).
14 Peirce's evolutionary cosmology can be found in his "Guess at the Riddle" and the "Lectures on Pragmatism" (Peirce 1890, *CP* 1, 1903a, *CP* 5). Brent (1993:131) suggests this connection. Peirce (1867) also wrote about metaphysical categories relatively early in his career.
15 An excellent discussion of Peirce's "Guess at the Riddle" can be found in Sheriff (1994).
16 Wible (1997) has explored the relationship of Peirce's mathematical categories to economics.
17 Peirce (1894, *CP* 1:176).
18 This attempt to persuade others can be seen in the "Lectures on Pragmatism" by Peirce (1903a, *CP* 5:55ff).
19 Peirce's interest in "the Word" or *"logos"* in Greek would seem to parallel his deep interest in the lives and works of Aristotle and Plato (Peirce 1901).
20 Peirce's agapasm and evolutionary philosophy are explored in great detail in Hausman (1974, 1993).
21 The historical details are drawn from Brent (1993) and Houser (1986, 1993).

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6

THE PREMATURE DEATH OF PATH DEPENDENCE

David M.Levy

Introduction

Consider two statements about perception. The first is from Adam Smith's *Theory of Moral Sentiments:*

> As to the eye of the body, objects appear great or small, not so much according to their real dimensions, as according to the nearness or distance of their situation; so do they likewise to what may be called the natural eye of the mind: and we remedy the defects of both these organs pretty much in the same manner. In my present situation an immense landscape of lawns, and woods, and distant mountains, seems to do no more than cover the little window which I write by, and to be out of all proportion less than the chamber in which I am sitting. I can form a just comparison between those great objects and the little objects around me, in no other way, than by transporting myself, at least in fancy, to a different station, from whence I can survey both at nearly equal distances, and thereby form some judgement of their real proportions. Habit and experience have taught me to do this so easily and so readily, that I am scarce sensible that I do it.
>
> (Smith [1759] 1976:134–135)

The second comes from David Hilbert's *Foundations of Geometry:*

> Geometry, like arithmetic, requires only a few and simple principles for its logical development. These principles are called the axioms of geometry. The establishment of the axioms of geometry and the investigation of their relationships is a problem which has been treated in many excellent works of the mathematical literature since the time of Euclid. This problem is equivalent to the logical analysis of our perception of space.
>
> (Hilbert 1996:2)

It is obvious that both of these claims cannot describe the same reality. Smith's claim that our perception of distance depends upon the *status quo* from which we judge is inconsistent with Hilbert's account in which our perception follows the real numbers.[1] The distance between two real numbers is independent of the *status quo*. The distance between 1 and 2 is the same regardless whether the *status quo* is 47 or -89. Any economic model in which "more is preferred to less" holds is implicitly betting with Hilbert against Smith.[2] It ought not therefore to be surprising that the most emphatic identification in modern economics between choice and the real numbers comes from Hilbert's associate John von Neumann in his collaborative axiomatic treatment of utility.[3]

This note considers an early debate about path dependence in economics. I use this historical context to ask the circumstances in which we might expect a rational account of choice to exhibit path dependence. It seems inevitable that a rational choice account of an individual's decision will depend upon the knowledge and beliefs of that individual. Suppose that it makes a difference to the decision whether the individual believes in a or in ß. Suppose further that it is the case that a and ß refer to the same reality even though we have agreed that the decision makers do not know this. In this context it will make a difference if by happenstance the decision is made by those who hold with a or those whose faith is with ß. On the contrary, such heterogeneity of belief will not matter if there is at least one individual who knows the true relation between a and P and that individual's knowledge informs the decision. It has been customary since the economics profession absorbed the lesson of F.A.Hayek's "Use of Knowledge in Society" (1945) to claim that prices in a decentralized competitive market have such a property.

There are surely other contexts in which such a property of using the extremal information is found. I find it plausible that the discipline of mathematics—outside of the context of a totalitarian society willing to impose belief even at cost—has the property of allowing the decisions to be made by the best informed.[4] If a theorem is blocked by ignorance of the possibility of substituting a and ß then the first person who knows this equality and its importance obtains the theorem.

The argument about path dependence has a largely forgotten historical basis. In 1870 Fleeming Jenkin was attacked on the possibility of one sort of path dependence introduced by William Thornton in 1868. The importance of the particular episode—where Jenkin introduced graphical methods from engineering into the demand and supply analysis of British economics[5]—is well known.[6] Obviously, this is an immensely important contribution in its own right, and as such its influence on Jevons and others has been long studied.[7] Jenkin's contribution of freeing economics, at least for a while, of considerations of path dependence in markets has I think not been fully appreciated, except of course by George Stigler who thought it obvious that

Jenkin was right.[8] And, as far as I know, no one cared to argue the case with Stigler.

The episode we study provides evidence for a larger claim which I believe to be true. That is, general members of the Smith-influenced Classical school adhered to the position that the *status quo* mattered whereas in general the Neoclassical school which supplanted them adhered to the position of the irrelevance of the *status quo*.[9] The difference between the schools is, I believe, explicable in terms of what assumptions we make about how individuals perceive the world about them. This difference can be partly traced to the Neoclassical model which supposes that perception follows from the real numbers.[10]

While we consider the substantive issues between Thornton and Jenkin about path dependence in markets, we might also reflect upon how, in the progress of mathematics, false beliefs were purged in the literature. It is an interesting fact that in the mathematical context in which Jenkin wrote, it was believed that geometry was in some formal sense different than algebra.[11] As a belief about matters of formal proof, this belief seems not to have survived Hilbert's proof of relative consistency of geometry and the algebra of the reals.[12]

The classics and the *status quo*

William Thomas Thornton launched an attack on Classical wage theory by first attacking the notion of demand and supply. He did this by offering something between a counter-example and a thought-experiment. Consider two methods of auction: one we start high and move down; another we start low and move up. Why do we believe that the results from the two methods of auction will always be the same?

> When a herring or mackerel boat has discharged on the beach, at Hastings or Dover, last night's take of fish, the boatmen, in order to dispose of their cargo, commonly resort to a process called "Dutch auction." The fish are divided into lots, each of which is set up at a higher price than the salesman expects to get for it, and he then gradually lowers his terms, until he comes to a price which some bystander is willing to pay rather than not have the lot, and to which he accordingly agrees. Suppose on one occasion the lot to have been a hundredweight, and the price agreed to 20s. [Footnote omitted] If, on the same occasion, instead of the Dutch form of auction, the ordinary English mode had been adopted, the result might have been different. The operation would have then commenced by some bystander making a bid, which others might have successively exceeded, until a sum was arrived at beyond which no one but the actual bidder could afford or was disposed to go. That sum would

> not necessarily be 20*s*: very possibly it might be only 18*s*. The person who was prepared to pay the former price might very possibly be the only person present prepared to pay even so much as the latter price; and if so, he might get by English auction for 18*s* the fish for which at Dutch auction he would have paid 20*s*. In the same market, with the same quantity of fish for sale, and with customers in number and every other respect the same, the same lot of fish might fetch two very different prices.
>
> (Thornton [1869] 1969:47–48)

If the only difference is where we begin the auction, if there is a difference in the resulting price then the *status quo* matters.[13]

I know of two important Classical economists who wrote on Thornton's argument. Here is John Stuart Mill's judgment that Thornton had identified the possibility of multiple equilibria, a nice addition to the standard doctrine:

> This instance, though seemingly a trivial, is really a representative one and a hundred cases could not show, better than this does, what Mr. Thornton has and what he has not made out. He has proved that the law of equalisation of supply and demand is not the whole theory of the particular case. He has not proved that the law is not strictly conformed to in that case. In order to show that the equalisation of supply and demand is not the law of price, what he has really shown is that the law is, in this particular case, consistent with two different prices, and is equally and completely fulfilled by either of them. The demand and supply are equal at twenty shillings, and equal also at eighteen shillings. The conclusion ought to be, not that the law is false, for Mr. Thornton does not deny that in the case in question it is fulfilled; but only, that it is not the entire law of the phenomenon. The phenomenon cannot help obeying it, but there is a some amount of indeterminateness in its operation—a certain limited extent of variation is possible within the bounds of the law; and as there must be a sufficient reason for every variation in an effect, there must be a supplementary law, which determines the effect, between the limits within which the principal law leaves it free. Whoever can teach us this supplementary law, makes a valuable *addition* to the scientific theory of the subject.
>
> (Mill [1869] 1967:637)

John Cairnes is possibly more emphatic:

> More briefly, what is the explanation of market prices? This question, after having been discussed by economists from Turgôt and

> Adam Smith to Mill, was at length supposed to have received its definitive solution in the chapter on "Demand and Supply" in the Principles of Political Economy by the latter authority. That solution, however, has lately been challenged by Mr. Thornton, I must own it seems to me, so far as the negative portion of his criticism is concerned, with success.
>
> (Cairnes 1874:110)

Thus, two Classical economists of high regard considered the possibility of multiple equilibrium with considerable composure.[14] Interestingly enough, F.Y.Edgeworth criticized Cairnes for not taking multiple equilibrium seriously enough in wage theory.[15]

And the *status quo*

Economists have, now and again, been criticized for not extending our theories to our own behavior. In this context it is worth pointing out how fitting it is that Jenkin published his doctrine that the starting point does not matter in a volume mysteriously titled *Recess Studies*. What passes for a unifying theme of *Recess Studies* was that the individual studies were composed by university teachers over their recent recess![16] When we reflect that Thornton's original contribution was in the *Fortnightly Review* and that *On Labour* was extensively discussed in the leading periodicals, Jenkin's act is possibly the extremal act of reflexive closure.[17]

Jenkin begins his article with a methodological attack on recent economic debates. He traces the difficulties of the recent debates to insufficient mathematical machinery:

> Recent discussions on the laws determining the price of commodities seem to show that these laws are neither so well understood nor so clearly expressed in the writings of economists as is sometimes supposed. Men are too much in the habit of speaking of the laws of political economy, without attaching to the word law the same rigid meaning which it bears in the physical sciences. There are, however, some truths concerning the subjects treated by the economist which do deserve the name of laws, and admit of being stated as accurately, and defined in the same manner, as any mathematical law affecting quantities of any description.
>
> The following essay is an attempt to state in this rigorous manner some propositions concerning the market price of commodities, using what is known as the graphic method of curves to illustrate the laws and propositions as they arise.
>
> (Jenkin 1887, 2:76)

After explaining how demand and supply curves can be used to describe choice, Jenkin considers Thornton's example:

> In both forms of auction buyers judge whether at a given price the demand is above or below the supply, partly by the quickness of the bids, and partly by their former experience and general knowledge. In a Dutch auction buyers are as likely as first tentatively to let the seller offer below the market price as to close with him above that price. In an English auction, buyers are as likely at first to run up above the market price as to stop bidding below it. It is only by experience of former markets, and a considerable number of tentative transactions, that the theoretical price is approached. The device by which Mr. Thornton has made it appear that in a Dutch and English auction there might be two market prices, is to assume that the demand at prices in the neighbourhood of the market price is constant at all prices; that the same number, and no more, fish would be bought at 18s. as at 20s. In this case the demand curve becomes horizontal near the market price; and as the supply curve is also horizontal, the market price is indeterminate. This case is not peculiar to any form of bargain, but represents an unusual state of mind.
>
> (Jenkin 1887, 2:84–85)

Jenkin's conclusion—Thornton assumes "an unusual state of mind"—is the basis of Stigler's judgment that Thornton depends upon a "bizarre" demand curve.

And the winner is?

Note that Jenkin has transformed a difficult probabilistic problem—what are the other bidders going to do?—into a demand curve in which probabilistic elements have vanished. Jenkin does not allow for the possibility that bidders form different beliefs in the different institutional setting. If this happens then there will be no reason for us to predict that the bidders will behave the same in two type of auctions. Fortunately, we need not linger overlong on the details of Jenkin's argument: the question of the English and Dutch auction is a staple of the experimental economics literature. The answer is in: The Classics were right. The method of auction one uses matters. This difference can be routinely replicated.[18]

Neoclassical economics thus begins with the claim, *contra* the Classical consensus, that the *status quo* does not matter. What are we to make of the fact that, in the precise context of the debate, the *status quo* does matter? I would propose that we think rather seriously whether the importation of the real numbers from the traditional mathematical disciplines might not be rather more problematical than one would like to believe.

I would conjecture that Jenkin's engineering background made it natural for him to think of geometrical methods as the obvious model to describe the world. After Jenkin, economic processes became embedded in a mathematical universe.[19] After Hilbert's demonstration of the relative consistency of geometry and the algebra of the real numbers, a mathematical universe became the unique mathematical universe.[20]

It is not in my mind a coincidence that in the twentieth-century economic debates, perhaps the most important theoretical discussion of the importance of the *status quo* has revolved around whether the Allais paradox serves as a counter-example to the expected utility axioms proposed by von Neumann and Morgenstern.[21] The mapping of economics to mathematics which Jenkin began was largely completed by von Neumann.[22] The question which remains is whether the mapping is true.

Notes

1 George Berkeley's work is central to the contested ground. Smith's account assumes the truth of the Berkeley's theory of vision (Levy 1995). In common with the contemporary logicians with whom he disagreed about the foundations of mathematics, e.g., Bertrand Russell, Hilbert presumed that Berkeley's attack on the foundations of the calculus had been repelled by (i) the development of analysis based on limits, and (ii) the convention that infinitesimals are not to be used in proofs (Hilbert 1967). This confidence assumes that analysis was complete so that no proof depended upon the existence of infinitesimal elements. After Kurt Gödel demonstrated that analysis, whether axiomatized by Russell-Whitehead, Zermelo-Fraenkel or von Neumann, was not both complete and consistent, the way was open for T.Skolem to demonstrate that if analysis is consistent, it remains consistent when "non-standard" elements (infinitesimals) are added. Rosser (1978:542–560) has the formal overview. Levy (1992a) reconsiders Berkeley's theory of vision in the light of the revaluation of Berkeley's mathematics forced by the development of non-standard analysis. In particular, one cannot get rid of infinitesimals by the rhetorical device of not talking about them.

2 To see the issue consider the one-commodity case. Then one can prove that if "more is preferred to less" preference is transitive. How? The relation > is transitive over the reals and preference follows from >. Various important economic substitution principles depend upon the transitivity of equality where because we know that 2>1, we also know that (1+1)>1 since 2=1+1. Without the infinite precision of the real numbers, this substitution cannot be depended upon, Levy (1985, 1988a).

3 There is a clarity in von Neumann and Morgenstern's axiomatic system which is obscured by the modern convention of using distinct symbols for preference between commodity bundles and the mathematical relation greater than. This obfuscation von Neumann and Morgenstern disdain (for example, 1964:617): "As was pointed out at the beginning of 3.5.1, we are using the symbol > both for the 'natural' relation u > v affecting utilities u, v and for the numerical relation ?>s affecting numbers ?, s… When discussing the Archimedean property, von Neumann and Morgenstern (1964:630) note: "We are making free use of the

concept of the real number, while this is usually avoided in the literature in question."

4 Hostility to "Jewish physics" and "bourgeois genetics" point to the cost which the Nazi and Soviet regimes were willing to bear in order to impose belief. Famously, George Orwell, in *Nineteen Eighty-four* proposed the belief that 2+2=5 as the test for doublethink. I find readers of *Nineteen Eighty-four* not to have understood that the "war" between Orwell's powers is fake. Real wars require real bombs which require real mathematics.

5 Bronfenbrenner (1985:15) describes Alfred Marshall's *Principles* this way: "The new Great Book also combined, with ingenious eclecticism, much of the familiar 'old school' paradigm on its supply side with the 'new school' utility analysis on its demand side. Further enhancing its scientific overlay, it not only expanded mightily the 'engineering' supply-demand diagrammatics of Fleeming Jenkin but added mathematical footnotes and appendices in 'language' resembling physics." Jenkin's "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery" is found in Jenkin (1887, 2:271 ff).

6 Fisher (1898:127): "It is significant of the slow growth of economic science that these graphic pictures of supply and demand, now in almost universal use in text-book and class-room, were ignored or forgotten by Cournot's contemporaries, and were only restored in 1870, which independently obtained by Fleeming Jenkin. With his name, rather than with Cournot's, they are generally associated to-day." Schumpeter (1954:837): "he was the first Englishman to discuss, with nearly the same clearness as had Verri and Cournot, demand functions..." Klein (1995) summarizes and extends the recent discussion.

7 Young (1912:578): "Some correspondence with Fleeming Jenkin relative to the use of the mathematical method in economics and the publication, in 1870, of Jenkin's 'Graphical Representation of the Laws of Supply and Demand' in Grant's *Recess Studies* seem to have led Jevons to hasten this completer presentation of his own theories." Also, Robertson (1951:242–43). Jevons' mss. notations and the letters from Jenkin to Jevons are printed in Jevons (1977, 3:166–178). Mirowski (1989:410) thinks Marshall's debt to Jenkin's analysis was rather more than Marshall let on. Patinkin (1973) has pointed to Jenkin's influence on Frank Knight's "wheel of wealth" diagram to explain why Knight made no claim of originality. Young's interest in Jenkin makes Patinkin's conjecture even more plausible. Klein (1995) puts Jenkin's graphical methods in historical perspective.

8 Stigler (1954:113): "We have seen that serious work on the estimation of empirical demand curves began just before the first World War. Yet, putting aside Cournot's work as uninfluential, demand curves became common in economic literature after 1870. Fleeming Jenkin made good use of them to display Thornton's bizarre criticisms of the 'law of demand and supply'..." Schwartz (1968:92, 274) follows Stigler here.

9 The immediate counter-example to this larger claim—Ricardian equivalence—is spoiled by the fact that David Ricardo himself did not believe it to hold (O'Driscoll 1977). I provide evidence against the strong version of the thesis—replacing "in general" with "always"—in Levy (1995).

10 Let me give examples of the sort of pieces of analysis I'm thinking of in Smith's work where perception is unlike the real numbers. "Moral" rulefollowing behavior in terms of utility-enhancing constraints—which I argue Smith developed from classical roots—depends upon (i) the stipulation that the starting-point for the consumer's optimization problem, the *status quo,* matters,

and (ii) the stipulation that individuals have imperfect perception of the material world (Levy 1988b, 1992b). I would argue that all of Smith's language-linked work is related to imperfect perceptions. And to complete the circle of analysis the grammatical trajectory of a language can be analyzed by rational choice principles (Levy 1997).

11 Klein (1995:100) quotes Karl Pearson to this effect. In light of John Tukey's exploratory data analysis, one might make a distinction between graphical and algebraic methods of exploring data and thus discovering relationships (Tukey 1977). If the perception of a real-world scholar is limited there is no reason to believe that equivalent methods will be equally useful. Again, imperfect perception blocks the possibility of substituting equivalents in belief statements (Levy 1985, 1988a). I thank Judy Klein and Philip Mirowski for impressing upon me the importance of the belief in Jenkin's language community in the difference between algebra and geometry.

12 Hilbert (1996:30): "Every contradiction in the consequences of the line and plane axioms I-IV, V, 1 would therefore have to be detectable in the arithmetic of the field O."

13 I am prepared to believe that the example is attractive for Thornton as it opens a space in a determined order for free will (Thornton 1873:100–101).

14 Hollander (1985, 1:275–279) has an illuminating discussion of the episode, including the details of Mill's correspondence. Hollander (1985, 1:279) calls attention to Mill's statement in the 1871 edition of *Principles* about the discussion of demand and supply "not yet ripe for incorporation in a general treatise" and suggests Mill was rather less composed in private correspondence than public statements. Pearson's investigations of the bimodal distribution supposed to characterize evolution began with reflections on the fish market (Klein 1997:181).

15 In discussing Cairnes' reaction to Thornton, Edgeworth (1881:121): "It would have been suggested that the Wage-Fund or -Offer, though for a given rate of wages it have a determinate, has not necessarily a unique value." Jenkin's argument is not discussed as far as I can see.

16 Mill's response came in the *Fortnightly Review* where chapters of *On Labour* had appeared prior to publication. Thornton (1870) contains an extensive discussion of the various reviews of the first edition. The essay was reprinted in Jenkin (1887), a subset of which forms Jenkin (1931). The reader who knows only the latter has missed the wonderful graphics found in Jenkin (1870, 1887): the lines indicating price and quantity equilibrium pairs are printed in red! The two contemporary notices of *Recess Studies* that I know come (i) in Jevons' complaint that he had not been cited (Jevons 1977, 3:166), and (ii) from Mirowski's report of Marshall's 6 April 1896 letter to Edwin Seligman where Mirowski (1989:410) quotes Marshall: "His paper in *Recess Studies* was a good deal talked about and I heard of that quite early." Mirowski's announced edition of Thornton's work will surely help emphasize the importance of this episode and will help explain why Thornton, who seems to reply to every review, does not respond to Jenkin.

17 Jenkin is not "just" an engineer. His writings on theater are found in Jenkin (1887). Someone who was "just" an engineer would seem unlikely to have his life written by the likes of Robert Louis Stevenson (1887). Perhaps there is a link between Jenkin's knowledge of the theater and his interest in graphical methods?

18 Smith (1982:943–944). Just why there is a difference is a matter of debate. The experimentalists' focus on "double auction" methods is in part an attempt to avoid the difficulties of the English and Dutch auctions.

19 Dore (1995:439–440) discusses von Neumann's contributions in terms of embedding economic problems in terms of a general mathematical structure and

then analyzing the structure. This approach seems von Neumann's signature. Bernays (1968:41) describes one of von Neumann's contributions to set theory: "From von Neumann's axiomatics we adopt first the idea of embodying in the system a part of the formalization of the metamathematics…"

20 Supposing that there is a fixed minimum in perception allows one to prove that perception and therefore preference which follows from perception is fuzzy (Levy 1992a). Fuzzy mathematics is a discovery of the 1960s.

21 A translation of Allais' original paper is found in Allais (1979a). The famous questionnaires and the response to his counter-example are discussed in Allais (1979b).

22 Rashid (1994) and Dore (1995).

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7

COMPLEXITY, CHAOS, OR RANDOMNESS

Ragnar Frisch and the enigma of the lost manuscript

Francisco Louçã

On Tuesday April 5, 1933 at 17.45—and one may presume it was precisely at that time—Ragnar Frisch delivered a paper at the Institut Henri Poincaré in Paris. Its title was "Conclusion: La signification des lois sociales et méchaniques. Invariance et rigidité. Remarques sur une philosophic du chaos." It was the final paper of a series of eight, dealing with mathematical economics and advanced topics of the newly born branch of econometrics.

It was a very important event, at least for three major reasons. First, it was presented to a selected audience at the Poincaré Institute, allowing for discussion among some of the better-equipped mathematicians of the time.[1] Second, it was presented by one of the founders of the newly born econometric movement and of the Econometric Society, and it was a crucial opportunity for the presentation of its purpose. Third, it dealt with chaos, a rather obscure notion encompassing some of the margins of the science: at that time, a contribution on chaos in economics would have been an outstanding innovation. Given the fact that the emergence of this concept in our science is generally traced back only to the writings of von Neumann in the 1940s,[2] and that econometrics was based for a long time in what we might call a linear orthodoxy, this manuscript could highlight a crucial part of the history of the discipline.

The current chapter presents the outline of the argument, as far as one can guess from partial and disseminated evidence. The topic is reassessed from the point of view of the debates surrounding the introduction of the probabilistic approach in economics. Finally, these early concepts of chaos and randomness are discussed.

The missing link

In spite of the fact that Frisch had prepared the text of all the previous papers for publication,[3] apparently he did not do so for the final one. Indeed, the manuscript could not be found either in the archive at Oslo University or in that of Oslo University Library, which are large, systematic and organized archives. Neither could it be found at the Poincaré Institute itself or among the papers of those who took responsibility at that time for its management, such as Fréchet, whose papers at the Académie des Sciences do not include any version of this lecture. Schumpeter's archive (Harvard University) was also checked for any clue about this manuscript. Available evidence points to the hypothesis that it was never put on paper. On the other hand, since there is no trace of any reference to this topic in his copious correspondence with his young colleagues and co-workers in econometrics, one may further guess that this philosophically inclined dissertation was a rare piece in the rich Frischiana collection.

Of course, the study of non-linear systems and, *a fortiori,* of chaos, was far from Frisch's preoccupations. Most of his work in the late 1920s and in the 1930s was devoted to the establishment of econometrics—the new branch of economics, expressed by the Society and with *Econometrica* as its major journal—based on linear specifications, and therefore excluding complexity and chaos. Frisch became one of the dominant characters in the mathematization of the discipline thanks to his disciplined, rigorous, and insightful contributions, which did not include these badly behaved and non-computable systems of equations. As a member of the early generation of econometricians, Frisch developed his life work in the framework of simplicity or of disorganized complexity (more on that later) and did not investigate the organized complexity alternative.

We would thus be completely in the dark about Frisch's 1933 argument in Paris if he had not again taken up the issue much later, in his Memorandum of February 21, 1951, section 25F, relating to a lecture given at Oslo Institute of Economics.[4] Moreover, the content of his 1951 lecture, based on the Paris paper, was again emphasized in no less than his later Nobel speech.[5] This was delivered in 1970: Frisch had broken a leg in 1969 while climbing a mountain, and could not attend the reception and present his acceptance speech at the same time as the other scientists who received their awards that year. Consequently, his speech was not delivered until the next year, on June 17, 1970.

It was a quite solemn occasion: Frisch and Tinbergen were the first economists to be awarded the Nobel Prize, and that represented a very fair recognition of their long work in economics, econometrics, and science. It was the opportunity to present an overview of economic science. But the text of the speech is a surprising piece. It just includes sketches of arguments rather than a coherent orientation, and it concludes rather abruptly after presenting some quite independent investigations and comments, as if Frisch was merely delivering his agenda for future research. Maybe that is just what he did: the intensity of his work and his devotion to new ideas was legendary, and after a whole life at the frontier of his science he felt, perhaps more than ever, compelled to provide new leadership, since he was so deeply disappointed with the current course of econometrics.

That is eventually why he emphasized so strongly the need for a "much broader perspective" and he suggested a challenging discussion on the "ultimate reality," "in the sense of a theory of knowledge." After arguing about the difference between intelligence and wisdom, atomic theory and the experimental nature of the principle of symmetry, Gell-Mann's concepts of the quantum world of elementary particles, matter and antimatter, astronauts and Jevons, Frisch took up the problem of the "Philosophy of Chaos," reminiscent of his concluding Poincaré lecture.

The argument runs briefly as follows. Frisch considered the empirical distribution of two variables, x_1 and x_2, and their linear transformation into another variables, y_1 and y_2:

$$y_1 = b_1 + a_{11} x_1 + a_{12} x_2$$
$$y_2 = b_2 + a_{21} x_1 + a_{22} x_2$$

Of course, if the Jacobian of the transformation is singular, whatever the distribution of x_i, the distribution of the pairs of y_i will be onedimensional: y_i will lie in a straight line. Let's take a very simple example, in line with Frisch's argument. Consider the pairs of x_i in Figure 7.1.

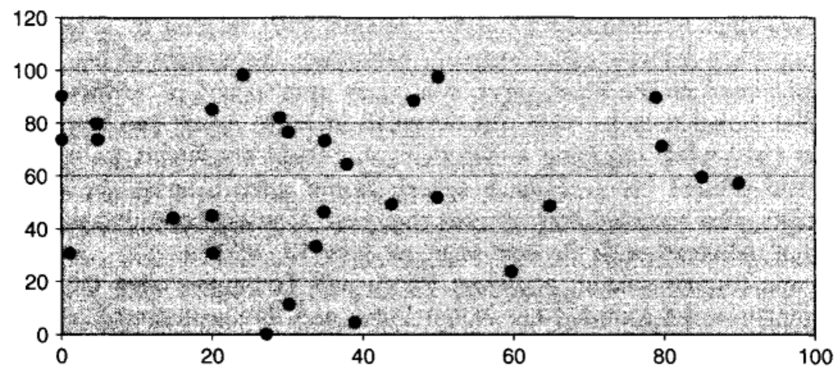

Figure 7.1 Empirical distribution of the X_i

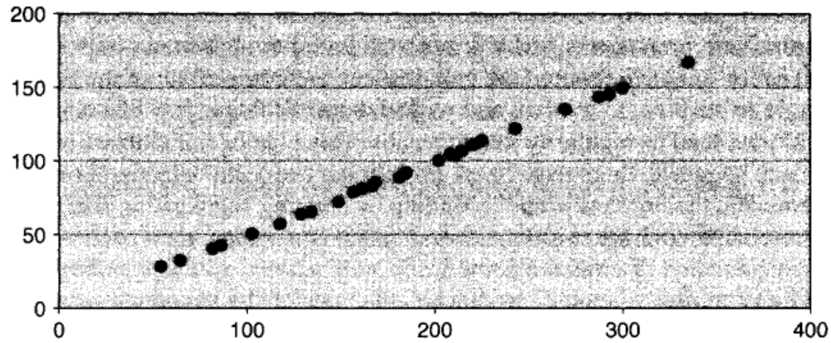

Figure 7.2 Linear transformation with a singular matrix

Then, if a singular transformation is applied to this distribution, we have the result illustrated in Figure 7.2.

The practical implication is that, given the outstanding evidence of a statistical correlation between the y_i (r =1), if one is tempted to consider y_i as the cause of y_j and to measure this relation as the non-zero slope of the segment, the result is quite conclusive. Yet it is wrong: "The 'cause,' however, is not a manifestation of something intrinsic in the distribution of the x_1 and x_2, but it is only a human *figment,* a human *device,* due to the special form of the transformation used" (Frisch 1970:217, his emphasis). The statistical evidence is then a trick of the imagination. Furthermore, Frisch proves that it is always possible to find a non-singular transformation that provides whatever strong correlation one may desire.

The reverse problem also poses a similar difficulty. If we just know the y_i, eventually "with a false correlation," how can we be sure that our observation (Figure 7.2) is not derived from an empirical distribution similar to that of Figure 7.1? The answer lies in the second part of Frisch's argument. The next step has in fact a broader philosophical implication, since it relates to a specific theory of knowledge presented as a paradox.

If the impression of regularity and correlation is merely derived from the imposition of a specific transformation to real data, then the orderliness of nature is suspect. Indeed, the perception of regularities or the definition of covering laws may be just a human artifact: the transformation is the decisive feature, whatever the nature of the empirical distribution of data. It may be, then, that the epistemic primacy of laws in science is just a tautological consequence, a necessary but not necessarily correct self-confirming conception of Nature as being ordered. In that case, laws exist simply because we look for them with tools that impose spurious evidence of their confirmation.

But Frisch then proposes another possible interpretation. It may also be that the singular transformation allowing for regularity, lawless order and its scientific interpretation is just a product of social evolution, an imposition over reality—not a figment of the imagination, but a true effect of the human struggle to survive, to evolve, and to dominate. Biology, just like science, would then tend to regularity and to impose this type of transformation: or, as Frisch suggests, there may be an evolutionary process selecting both the systems and the humans prone to finding or creating regularities, that is, to generate a Lamarckian evolution rather than a Darwinian one:

> If the "ultimate reality" is chaotic, the sum of the evolution over time—biological and scientific—would tend in the direction of producing a mammoth singular transformation which would in the end place man in a world of regularities. How can we possibly on a scientific basis exclude the possibility that this is really what has happened? This is a crucial question that confronts us when we speak about an "ultimate reality". Have we *created* the laws of nature, instead of *discovering* them? Cf. Lamarck vs. Darwin.
>
> (Frisch 1970:219, original emphasis)

Curiously enough, Frisch did not try to resolve this challenge. He adds then a general reflection:

> What will be the impact of such a point of view? It will, I believe, help us to think in a *less conventional way.* It will help us to think in a more advanced, more relativistic and less preconceived form. In the long run this may indirectly be helpful in all sciences, also in economics and econometrics.
>
> (Ibid.)

This is paradoxical. First, the author plays with the idea that "ultimate reality" is chaotic and that regularity is imaginary. Then, he argues that order may be introduced not just by intellectual artifacts, but also by the process of social action over Nature, and even that the selection may act in such a way as to pair Biology and Science in the choice of "transformist" agents. Finally, he concludes that, whatever the reality really is, this may help us think in a less conventional way. That is the least one can say, since realistic and relativistic assertions are mixed up in the argument and it becomes impossible to distinguish between fact and fiction.

But the ending is even more puzzling, since Frisch suggests that what is needed is just engagement for the betterment of society. And he finishes the section of his paper by quoting an Indian friend, a former ambassador: "Understanding is not enough, we must have compassion." There is science, but there is more knowledge than just science.

And the paper proceeds in the subsequent sections to a "brief survey of the development of economics in the last century," to a detailed history of the foundation of the Econometric Society, then to a discussion of measurement of the preference function, and of national and local plans and specific techniques for planning.

Complexity, chaos, and randomness in early econometrics

Because the early generation of the founders of econometrics was so deeply engaged in positivistic reasoning, the Poincaré lecture is quite exceptional since it introduces an interesting philosophical question about the nature of determinism. It even suggests a relativistic conclusion: there is nothing in science itself that provides a scientific foundation for the truth of the scientific assertions. Regularities, expressed in science as laws, may be a figment of reality or constructed reality—through a Darwinian process of selection of regularity builders—and there are no means to discriminate between these two opposite conjectures.

But the Poincaré lecture and its follow-ups are also outstanding documents since they highlight Frisch's radical distance in relation to the introduction of the probabilistic approach in economics. For what he terms "complete or pure chaotic distribution" is nothing more than "perfect randomness," in modern parlance. No chaos whatsoever is involved in this demonstration (Figures 7.1 and 7.2). Yet the very use of this term—in 1933, in 1951, in 1970!—emphasizes Frisch's mistrust of the role of randomness in economic and statistical explanations.

In 1933, shortly after the Paris lectures, Frisch concluded his major work on cycles: the paper for the Cassel *Festschrift* which much later won him the Nobel Prize: "Propagation Problems and Impulse Problems in Dynamic Economics" (Frisch 1933). The paper is a discussion of a mixed system of differential and difference equations representing a damping and equilibrating mechanism that mimics cycles, to which Slutsky random shocks are then added for the sake of realism. Nevertheless, this rocking horse, with a stabilizing system and kicks to maintain the oscillations, does not explain the nature and the origin of these decisive sources of exogenous energy which account for the movement.

This was a marginal question even for Frisch, who stated that: "The concrete interpretation of the shock e_k does not interest us for the moment" (Frisch 1933:200–201). A brief discussion of the problem is provided at the very end of the paper, and Frisch, who had had lengthy discussions with Schumpeter on this topic, suggests that one interpretation of this energy is the Schumpeterian innovations. But Schumpeter was all but satisfied with this representation of his model of cycles, and there is an incongruent jump between the small, irrelevant, and unexplainable Slutsky shocks and the major, systemic, and explanatory Schumpeterian shocks

(Louçã 1997b). Lacking an explanation, Frisch just ignored the fundamental question.

The same paper provides a hunch about another answer to the Poincaré puzzle. Discussing Slutsky's concept of the spurious cycles generated by the summation or averaging of random shocks, Frisch suggested a new interpretation: The economic damping mechanism is the real counterpart to the intellectual process of summation of the multitude of exogenous variables. Cycles exist, therefore, because they are the result of the social system (represented by the deterministic system of simultaneous equations) absorbing the exogenous shocks. Since he was at almost the same time engaged in the preparation of the Poincaré lectures, this suggests that Frisch's normal answer would be that regularities indeed exist, even if they are the product of social action against the "absolutely chaotic distribution" of events.

One way or the other, Frisch maintained his distance in relation to the development of statistical inference based on the properties of the random term, which did not play any role whatsoever in his work. Two years later, when a student he had met at Yale in 1930, Paul Hoel, wrote him in order to check the possibility of coming to Oslo to study econometrics, Frisch replied in an extensive answer presenting his views on the probabilistic approach to time series and suggesting a parallel investigation to that of Koopmans:

> At the present time Mr. Tjalling Koopmans, of Amsterdam, is here working on a doctor thesis in mathematical statistics. He is particularly interested in building a bridge between the approach in my book "Confluence Analysis" and the R.A.Fisher sampling approach. The difference between these two points of view is this. In sampling theory, in order to test the significance of a statistical observation, one puts up the fiction of a "universe," that is some big collection from which the actual observations are "drawn" in a more or less "accidental" manner. Whatever assumptions one makes are made in the form of *assumptions about this universe*. This point of view is fruitful, it seems to me, in problems concerning experiments that *can be controlled*. For instance, agricultural or biological experiments. But this theory is very inadequate when it comes to applications in economics, or in social sciences in general, where we most of the time have to accept observations that are presented to us without our being able to influence the results to any considerable extent. In these cases all the problems of confluence analysis crop up, and these can, it seems to me, be better treated by another type of analysis, namely, an analysis where the assumptions being produced *are assumptions about the sampling itself*. For instance, one may assume that each observation is a sum of a systematic part and a

> "disturbance," and then introduce assumptions concerning what has been the connection, or lack of connections, between the disturbances *in the sample*. In this way one arrives at identities, exact upper and lower limits, etc., not results which are formulated in probability terms. One does have a means of investigating how a particular constellation of assumptions entails a particular consequence for the result obtained. This analysis of the effects of *alternative assumptions* is very important for applications to economics.
>
> This is of course a very rough outline of the difference between the two approaches. If I should give a fuller statement, I would have to explain that, in some sense, the notion of probability comes in my approach and that, after all, there may be some points of contact between the two approaches. But it would lead too far to go into this in a short letter. I mention it in order to suggest to you a field of research, which, I think, is particularly important and very intriguing.
>
> (October 15, 1935, Letter to Hoel, Oslo University Library; original emphasis)

This indicates quite clearly Frisch's attitude in relation to the probabilistic approach for the statistical analysis of time series, and therefore his severe opposition to the extension of the role of randomness in applied economic models. The notorious insistence on this point of view for thirty-seven years also proves that he did not change his conception.

The same may be said for Tinbergen, who shared the same concept of the primacy of deterministic models—the rationale for naming randomness as "absolute chaos," suggesting its unknowable character. One simple example is given in order to prove that such was the case, and that for Frisch as well as for Tinbergen this stance did not alter over the decades. Even though a new concept and a rigorous treatment of chaos was already available, as was also and obviously the case for the concept of randomness, Frisch and Tinbergen stuck to their early idea.

During the last year of his life, Tinbergen answered a challenge from Kurt Dopfer, who criticized his views and argued for a complexity approach in economics, and wrote that he did not know the question well enough to argue about it. And then came the phrase which is crucial for the case of this chapter: Tinbergen added that, unlike him, Frisch had come across chaos:

> Finally, some words on chaos theory. Here I simply have to admit that I never studied or applied it and that I increasingly have become aware of its potentialities. It is a relatively new area, although Ragnar Frisch came very close to it some decades ago …It is a subject for the future.
>
> (Tinbergen 1992:256)

Even in 1992, as with Frisch in 1970, Tinbergen did not distinguish between chaos and randomness: both were outside the scope of his theorizing. Yet evidence was found that Frisch and his colleagues had considered at least once a non-linear formulation in order to represent a complex system of market interrelation between two agents, and that system implied chaos. This system was conceived of as a result of a challenge by Tinbergen and (mostly) Koopmans against Frisch's views as expressed in his 1934 famous paper on "Circulation Planning," published in *Econometrica*.[6] But, at that time, Frisch merely expressed in a mathematical form the non-linear system and did not attempt to solve or to simulate it, although he was still able to understand that its behavior depended crucially on the initial conditions and the values of the parameters. Indeed, the system is chaotic, but they could not recognize it (Louçã 1997b).

The problem, of course, is not that of the understanding of chaos: it is the misunderstanding of randomness. Both concepts are ill defined, yesterday as today, one may argue. And that is true. At least, one must agree with Tinbergen's very last remark: all this "is a subject for the future." Indeed, it is.

Notes

1 Frisch had lived in France while studying and preparing his PhD, which was originally written in French. He was quite aware of the scientific landscape of France, which was one of the countries with a better mathematical school,

2 von Neumann and Ulam published the first account of the tent map in the unit interval in the *Bulletin of the American Mathematical Society,* 1947:53.

3 In spite of that, the manuscripts of the seven lessons were never published. This was not uncommon in Frisch's scientific career: quite often, after finishing a piece of research, it was put in a drawer rather than published or even informally circulated. But, in what concerns the last lesson, evidence points to the hypothesis that it was never written down.

4 Of course, this lecture was not available to anyone but to his students and is written in Norwegian: like the other lectures, this was never published.

5 Here again is one of the coincidences that make the wonders of the numerologists: it is usual and quite fair to consider that Frisch was awarded the Nobel Prize for his 1933 path-breaking paper on cycles, and when receiving the prize, thirty-seven years later, he chose to refer back to another topic which he had discussed in the same year of 1933: chaos. This topic he presented again in 1951, eighteen years later, and then in the speech prepared for the acceptance of the 1969 Nobel Prize, another eighteen years later. The latter is the most substantial evidence there is about the content of the Poincaré lecture, and furthermore of Frisch's fidelity to the same idea.

6 It became famous not only for denying the self-equilibrating property to capitalism, therefore predicting the collapse of the free market, but also because, and more trivially, it is the largest paper ever published in *Econometrica.*

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Part III

BROADER VIEWS ON COMPLEXITY

8

WILL COMPLEXITY TURN ECONOMICS INTO SOCIOLOGY?

Alex Viskovatoff

Introduction

Complexity is a seductive concept, and not just for economists. The view of the world provided by the physics that inaugurated the scientific revolution—with classical dynamics' notion of a deterministic world governed by timeless laws, in which there is no reason in principle why the motion of a physical system could not be viewed the same backward as well as forward, so that all change is reversible—seems now one more vestige of a distant, much more rigidly structured, age. Laplace's demon, who, given Newton's laws and a description of the position and momentum of each particle in the universe, could predict and retrodict every event in the history of the universe, has been replaced by the famed butterfly of the lore of the popular literature on chaos theory, whose wings flapping can make the difference between a hurricane occurring or not occurring. After chaos theory demonstrated the extreme brittleness which characterizes many causal processes, the question of how order is possible (despite the chaos one finds in the universe) was bound to come to the forefront, with investigators searching for general principles for how this "order out of chaos" might come about. Corresponding to this shift there has been a rise in the prestige of biology relative to that of physics, helped along by the ubiquity and seemingly endless potential of the digital computer, which allows one to think of life as consisting of nothing so mundane and inelegant as gooey liquids, but as being produced at bottom by computer programs written in the genetic code.

It was inevitable that the new scientific *Zeitgeist* would have an impact upon economics. It had been clear for some time that the two main classes of models which were consecutively taken to embody the most "fundamental" economic theory—general equilibrium theory and game theory—suffered from the same basic problem: they could not demonstrate the stability or uniqueness of the equilibrium which their logic seemed to require. The new "science of complexity" such as done at Santa Fe seemed like the obvious place to look for a way out of the impasse in which orthodox theory found itself. The image of the market as a place where competing desires come into an equilibrium when matched up with given costs of production—in the same way (as Walras [1909] 1960 argued) that weights of two different masses can balance each other if placed on opposite sides of a lever at appropriate distances from the fulcrum—is to be replaced by the image of the market as a population of expectations, with the incidence of rival expectations in the population being governed by the same principles of natural selection which drive the process of biological evolution. It was thus reasonable to believe that a means for economics to deal adequately with the complexity that undeniably characterizes the economy lay at hand. At the same time, it was clear that this new understanding would be undeniably *scientific:* while the time-worn notion of *homo economicus* was to be replaced with something more contemporary (and arguably more realistic), the "artificial agents" with which he would be replaced would still bear no resemblance to concrete human beings, so the impression could still be maintained that to understand the economy one need merely follow the more mathematical practices of the natural sciences.

In this chapter I will compare the appropriation of the complexity concept within Neoclassical economics with the way it has been used and developed by another social scientist who attributes much importance to the concept: the German system-theoretic sociologist Niklas Luhmann. As we shall see, while economists use the concept merely to suggest ways in which new types of models might be developed, without fundamentally changing their orientation or theory, Luhmann uses it to construct a new systematic body of social theory which in effect "re-maps" the whole domain of the social in terms that it develops out of itself, as opposed to "inheriting" many of its terms from common-sense, folk terms, in the way that terms such as "consumer" or "agent" are inherited by Neoclassical economics. The general moral will be that the complexity concept holds much more potential for reconstructing economic theory than is currently being exploited by economists.

Economic theory's current confrontation with complexity

Certainly, the group or "school" most associated with the idea of complexity is the Sante Fe Institute. This holds true both for the natural sciences, specifically physics and biology, as well as for economics. Although the Institute was initially founded in the 1980s by physicists associated with the Los Alamos Laboratory in order to serve as a catalyst for the development in the natural sciences of a new interdisciplinary movement that would have as profound an influence as the study of chaos did in the previous decade, economists got on board quite early on, essentially in order to encourage the president of Citicorp to provide funding.[1] Since the work of the SFI is the most visible work dealing with complexity in economics, I will restrict my attention to it in this chapter.

The volume Arthur *et al.* (1997a) is meant to serve as a report on the current state of economics research at Sante Fe, so I shall take it as representative of that research. The editors of the volume state that they "believe that a coherent perspective—sometimes called the 'Sante Fe approach'—has emerged within economics," and that it is meant to go beyond the "two conceptions of the economy that underlie standard, neoclassical economics," which can be called the "equilibrium" and "dynamic systems" approaches (Arthur *et al.* 1997a:2–3).[2] These approaches run into difficulties because of certain properties of real economies:

1 The interaction of agents is dispersed: agents interact with only a subset of other agents.
2 There is no global controller.
3 The system is hierarchically organized.
4 Agents continually revise behaviors and strategies as they acquire experience.
5 New markets, technologies, behaviors, and institutions are continually appearing.
6 Because of perpetual novelty, the system is never in equilibrium.

The computer scientist John Holland, who has had a very strong influence on the SFI economics research program and on Brian Arthur—the economist in residence at Sante Fe—in particular, has coined a name for systems with such properties: *adaptive non-linear networks*. As the editors of the volume state:

> unfortunately, the mathematical tools economists customarily use, which exploit linearity, fixed points, and systems of differential equations, cannot provide a deep understanding of adaptive nonlinear networks. Instead, what is needed are new classes of combinatorial mathematics and population-level stochastic processes, in conjunction with computer modeling…This conception of the economy as an adaptive nonlinear network—as an evolving, complex system—has profound implications for the foundations of economic theory and for the way in which theoretical problems are cast and solved.
>
> (Arthur *et al.* 1997a:4–5)

Orthodox economic theory has departed from a (i) universalizing conception of rationality; (ii) a viewpoint according to which the economy in effect has no structure, with all agents interacting indifferently with all others; and (iii) a notion of explanation that stresses the finding of unique outcomes in unchanging situations. If one is to follow the adaptive nonlinear networks paradigm, all three of these centerpieces of Neoclassical theory have to be abandoned. First, one must introduce *cognitive foundations* into economics, allowing that rationality cannot be understood homomorphically, as a combination of utility maximization and Bayesian updating, but must be understood as consisting of a large variety of cognitive strategies, in which a significant problem for the agent is simply how it should *interpret* the environment. Second, one must introduce *structural foundations:* every agent does not interact with every other agent, but *networks of interaction* form in which each agent interacts primarily with a small sub-set of the other agents; furthermore, it is necessary to recognize that "economic action is structured by emergent social roles and by socially supported procedures—that is, by institutions" (ibid.: 5). And third, it must be recognized that "in a world of perpetual novelty… outcomes cannot correspond to steady-state equilibria, whether Walrasian, Nash, or dynamic-systems-theoretical": what must be studied is "process and emergence," although precisely what that comes down to in practice is not yet clear (ibid.: 6).

How far do the readings collected in Arthur *et al.* (1997a) go towards realizing the complexity manifesto? If one looks over the various contributions, one finds that they can be grouped into three categories:

- papers which model a problem, suggested by orthodox theory but not of overreaching practical import, from a complexity angle (the papers by Arthur *et al.,* Brock, and Darley and Kaufman);
- papers which use a model from the natural sciences to "interpret" an economic phenomenon, with the connection between model and reality being as unclear and problematic as it is in the case of more orthodox models (the papers by Padgett, Durlauf, Ionnides, Kirman, Tesfatsion, and Lindgren);
- papers which do not present a mathematical model, but simply provide a kind of institutionalist description of economic processes or a critique of some aspect of orthodox theory without the use of a model (the papers by North, Leijonhufvud, Lane and Maxfield, and Blume).

I cannot provide comprehensive summaries of the papers here, but must restrict myself to mentioning two representative examples. To begin with a model of the first type, in what is clearly meant to be the flagship paper of the volume (since it appears first and contains more programmatic statements than any of the other contributions) Arthur *et al.* (1997b) present an "artificial stock market" in which agents in the form of computer programs obtain their expectations of prices not by deductively solving for equilibria, but by using an "inductive" strategy in which they observe what types of price movements are associated with what patterns of previous price behavior:

> we assume that each agent acts as a market "statistician." Each continually creates multiple "market hypotheses"—subjective, expectational models—of what moves the market price and dividend …As it becomes clear which expectational models predict well, and as poorly predicting ones are replaced by better ones, the agent learns and adapts. This type of behavior—coming up with appropriate hypothetical models to act upon, strengthening confidence in those that are validated, and discarding those that are not—is called *inductive reasoning*. It makes excellent sense where problems are ill-defined. It is, in microscale, the scientific method.
>
> (Arthur *et al.* 1997b:22)

It is worth noting precisely what form this "induction" takes in the model. Following the work of John Holland, the agents are modelled as classifier systems following genetic algorithms (Holland *et al.* 1986:102–126). In this artificial intelligence (AI) paradigm, a system follows a given set of rules; each rule specifies what condition most hold for it to be triggered and what action to take when it is triggered. How classifier systems differ from the more widespread production systems that are used in AI and wherein Holland's innovation lies, is that in classifier systems, the rules are specified as simply as possible: as strings of 0s, 1s, and an "either 0 or 1" sign. This allows the strings specifying rules to be recombined with each other, in a process analogous to that of genetic recombination, to produce new rules: hence the term "genetic algorithms." Given the availability of a function specifying the effectiveness of a rule, rules can be assigned weights and used for the production of new rules in proportion to their effectiveness. By thus selecting its rules on the basis of their effectiveness, a classifier system is capable of adapting to its environment, and thus of "learning" or "practicing induction."

I will note only two points about genetic algorithms. First, although one of their main virtues is supposed to be their ability to learn about their environment despite having a very simple architecture, it is nevertheless true that a classifier system has a considerable degree of specification of its environment built into it by the programmer implementing it: specifically, what environmental event is associated with a 1-bit in a given position in the condition part of a genetic algorithm and what action is associated with a 1-bit in the action part. Thus it cannot really be said that classifier systems are able to autonomously represent their environment in the same way that human agents and (in a lesser sense) other kinds of organisms do. Second, it is not at all clear how much, if anything, the "induction" produced by genetic algorithms has to do with induction as actually practiced by human beings. Holland stresses how classifier systems are able to learn without having large amounts of representational knowledge built into them by the programmer and without logically manipulating this knowledge (as is done in the more conventional symbol-system approach followed by traditional AI). However, it would seem that, despite not being deductive in the narrow sense, *human* induction does nevertheless operate by performing (if informally) logical operations upon symbolic representations of the phenomena of interest—and is thus different from the kind of "cognition" "produced" by classifier systems.

Take the example of my noticing that all the people present at an economics seminar are men, a piece of knowledge that I certainly obtain inductively. I would suggest that the process of inference involved functions in the following way. First, through a process that occurs below my level of consciousness, I notice either that "There's an awful lot of men present in this room, given how many people there are in it altogether," or that "There don't seem to be very many women here." Second, I posit the hypothesis that there are no women in the room and decide to test it. Third, I test it by glancing at every person in the room and confirming that he is not female. All three steps were involved in the production of this inductive knowledge. The first step, which occurred unconsciously, involved some form of pattern matching, in which objects in the visual field are simultaneously matched with general descriptions; the fact that all the objects seem to match the same description becomes so striking that it enters my conscious awareness. The second step involved a form of reasoning that seems to require language, first to produce a universally quantified proposition, and second to realize that there exists an algorithm for testing it. The third step required implementing the algorithm as a sequence of operations carried out one after the other. It is highly unlikely, to say the least, that either the first unconscious step or the other two conscious steps are implemented in any way that resembles genetic algorithms. Therefore, it is hard to avoid the conclusion that what Holland, and following him Arthur, are dealing with is not really induction, but merely a metaphor (as opposed to a good, scientifically productive model) for induction, in the same way that the optimization of a utility function serves as a metaphor for choice in orthodox economics. Thus, in making the argument that agents in financial markets ought to be viewed as following processes of inductive as opposed to deductive inference—which is the main point of the paper—Arthur *et al.* (1997b) are simply substituting one bad metaphor for another. The problem of explaining, in terms of processes of reasoning that traders actually follow, the behavior of financial markets still remains. On a different level, so too does the quintessential problem of economic methodology: how (if at all) can models in which agents bear little or no resemblance to actual human actors explain how the economy works?

Let us consider one more example, this time of the second type of paper—Padgett (1997). Padgett departs from Eigen and Schuster (1979), who provide an explanation for the origin of life by introducing their idea of the hypercycle—a sequence of autocatalytic reactions which ends by producing the first element of the sequence; according to Eigen and Schuster, it was only with the appearance of such a hypercycle that the emergence of a stable genetic code mapping nucleic acid sequences into the amino acids constituting proteins could develop. Padgett's paper explores viewing the formation of skills in the firm by analogy with the emergence of specific nucleic acid sequences in hypercycles. As he notes:

> The metaphorical leap from RNA to firms is obviously enormous. But the benefit of this particular operationalization is that it will offer an extraordinarily minimalist set of assumptions within which to investigate the emergence of collective order. Not only are no assumptions about hyper-rationality required, no assumptions about consciousness of any sort are required.
>
> (Padgett 1997:200)

In the paper, Padgett conducts simulations that focus on "two central ways in which hypercycle 'games' can be varied": the length of the hypercycle; and the mode of skill reproduction, that is, whether only the skill of the initiator of the interaction constituting a skill is reproduced, or only the skill of the recipient, or both. And indeed, Padgett does get some results from his simulations, which he describes.

What is the purpose of such simulations and such a model? Padgett claims that the fact that "no assumptions about consciousness of any sort are required" in his model is a virtue of his approach. But this is a virtue only according to what Deirdre McCloskey has called math values, as opposed to science values (McCloskey 1996: ch. 3). What is the value of being able to model skills as "evolving" in the firm under quite "general" assumptions, so that we do not need to view them as developing with the participation of any consciousness, when we know that *in fact* the founders of a firm are always conscious of what they are doing, that the workers in a firm learn certain skills because it was decided by means of deliberation that those skills are useful for the production process, and that, when a new skill is learned, it is first practiced consciously, and then with decreasing levels of awareness as one becomes more adept (Fitts 1964)? (Just think of learning how to play a musical instrument.)

There is something that corresponds to nucleotide sequences. There isn't, however, anything that corresponds to the skills the author talks about: there are either declarative procedural rules that are *interpreted* (so that consciousness cannot be abstracted from), or patterns of motor activity produced by neuronal networks below the level of consciousness. The point of the hypercycle model is to find how it is that particular basepairs came to have the role they have in the machinery of the cell. In the case of skills in firms, there is no corresponding question. Thus this model cannot lead to an empirical research program.

It is not clear what models of this kind are supposed to contribute. It is possible to understand why one might want, for instance, to be able to describe the bond between two kinds of molecules in terms of a quantumtheoretic model. If the model is correct, one can predict what new molecules can be produced without being forced to do many time-consuming experiments. One cannot see the molecules directly, and the models make up for that. In the case of individuals following rules, however, things are different. One can infer what rules agents are using by means of the old method of *Verstehen;* and again by means of *Verstehen,* one can guess what actions these rules might lead to. It is as if we had an in-built cognitive facility that enabled us to "guess" what molecules look like, as well as guess fairly well which molecules will combine with which, and how. On the other hand, the models of interaction presented here cannot lead to such inferences about which rules will be followed and to what effect: the models are built at too general a level, with no sensitivity to the particularity of the rules in question, that is, to their meaning. Thus, while the quantum-theoretic models have a clear scientific utility in that they can yield predictions, models of the kind developed by Padgett do not, since they do not properly model cognition. The upshot is that it is not clear why one should construct models of this type, as opposed to just doing case studies of business and other social practices, as informed by *Verstehen*.

In sum, one is left skeptical of the ability of the Sante Fe approach to produce a research program that will be notably more successful at being able to maintain a productive, empirical contact with its object domain than is the more conventional variety of Neoclassical economics. The models presented in the Arthur *et al.* (1997a) volume either attempt to relax the Neoclassical notion of rationality, but in a way that does not bring it closer to the rationality of actual human beings; or they are based upon physicalist metaphors which are no better than the original Neoclassical metaphor of preferences as a potential function. In those papers in which no models are developed, whatever insights are produced do not derive from the kind of mathematical and computational modeling techniques which the Sante Fe school is attempting to develop, but from the more conceptually oriented theorizing that is explored in the rest of this chapter, so they cannot be taken as supporting the viability of the Sante Fe approach.

Niklas Luhmann's theory of social systems

In some respects at least, it is hard to imagine a theory that is more diametrically opposed to Neoclassical economics in its basic point of departure than is Luhmann's theory of social systems (Luhmann 1988, 1995, 1997; Hutter 1994; Viskovatoff 1998, 1999). Neoclassical economics takes for granted almost all the institutions upon which the economy depends: the law, norms of proper behavior, and markets themselves. In contrast to mainstream economists, sociologists like Luhmann, see these institutions as being in need of explanation. Almost all economic explanations center around the choices that individuals make; and here again, economists start off from the easiest possible situation: either there is complete certainty, or all the relevant contingencies, as well as their probabilities, are known. For Luhmann in contrast, a "system's" environment always contains more complexity than the system can possibly hope to master, and the system's fundamental problem is to produce strategies for "reducing" this complexity in its internal operations so that some kind of action is possible (Luhmann 1995:23–28). The starting point for the analysis is simply that *there are* systems with an over-complex environment: aside from that, everything is contingent and in need of explanation. In providing such explanations, Luhmann follows the method of "functional," rather than "causal," analysis; in the social domain, as a rule many causes are intertwined, mutually producing many effects which can themselves be causes, in such a way that it is hard to isolate simple cause-and-effect relationships. Thus, rather than looking for such relationships, one is more apt to learn something by asking what functions various institutions and structures of interest serve, and making comparisons between different ways that the same function can be performed (ibid.: 52–58).

Social Systems

According to Luhmann, sociology studies not human actors and their interactions, but *social systems*. Social systems do not consist of human beings, or even actions, but of *communications*. To adopt the concept of "autopoiesis" of Maturana and Varela (1980), social systems are autopoietic systems, that is, systems which produce themselves by producing the elements out of which they consist; hence social systems themselves, rather than individuals, produce communications. We thus see another example of Luhmann adopting a perspective quite opposite to that of Neoclassical economics: he develops a very strong variety of methodological holism. While of course not denying that the minds of individual actors "participate" in the production of communications, Luhmann argues that it is nevertheless sensible to view the relevant social systems as being at least as "responsible" for their production as the individuals involved. For example, a formal organization such as a bank consists of communications which take the form of decisions; these decisions are reached, however, not on the basis of factors relating to the "individuality" of the individuals involved in making the decisions, but rather on the basis of rules (or, as Luhmann calls them, programs) which the organization has adopted. Thus what is critical for a particular decision having been reached is not that certain individuals were involved in it, but that it is a decision made within a particular organization. In addition, the decision derives its "identity" from how it will impact upon further decisions made in the organization after it is made. Once it is made, it may be of no further relevance or interest to the person directly involved in making it, but it will affect the decision making of others within the organization.

There are three kinds of social systems: interaction systems, organizations, and society. An *interaction system* comes into being when individuals interact with each other in each other's physical presence. In the case of *organizations,* what is significant is not that individuals be physically present, but that they be—by means of a decision—formally declared to be members of the organization. That obviously allows organizations to "contain" many more individuals than can an interaction system; and that in turn allows organizations to be much longer-lived, since if a few individuals are replaced in an organization it will remain the same, whereas if the same happens in an interaction system one in effect gets a new interaction system. Both of these factors allow organizations to build up much more complexity (in the form of an "organizational memory," for instance) than can interaction systems; in addition, because of their formal quality, institutions are highly programmable, and programming is one of the principle means that social systems have at their disposal for producing complex, organized behaviors. One thus finds that organizations are responsible for most of the accomplishments produced by modern society. Almost all manufacturing is carried on by industrial corporations, and the days when scientific discoveries were made by private individuals, such as Robert Hooke or Gregor Mendel, who could work at their leisure without institutional affiliation, are long gone. (In view of this "primacy" that organizations seem to have in modern society, it is odd that economics should not accord them a fundamental position within its conceptual framework—the "theory of the firm" being instead a recent afterthought.) *Society* consists of all the human communications that occur in the world. The communications produced by all other social systems, whether interaction systems or organizations, thus occur within it and contribute to its autopoiesis.

Functional Differentiation

An important characteristic of society is the form of differentiation it takes. According to Luhmann, there are four principal forms societal differentiation can take: segmentary differentiation, in which the societal sub-systems are similar or "equal" to each other and are distinguished either by descent ("clans") or by location ("tribes"); differentiation along the lines of center and periphery, where there is inequality between the two, but each of the two is divided up into equal segments (households); stratificational differentiation, in which sub-systems are distinguished by rank and status (for example, nobility vs. peasantry); and functional differentiation, in which sub-systems are unequal in that they perform different functions but equal in that none is accorded higher "rank" than any other (Luhmann 1997:613).

As society "evolves," one sees a progression from segmentary differentiation to functional differentiation, with the other two forms being adopted in between; in this progression, society develops in the direction of increasing complexity. It is a central premise of Luhmann's thought that what is constitutive of modernity is functional differentiation, and Luhmann sees as one of the main merits of his theory that by stressing the centrality of this form of differentiation as the "principle" of modernity, it for the first time provides a description of modern society that is sufficiently complex that it can do justice to this form of society. And according to Luhmann, it is only system theory that can adequately describe modern society. This is because the newer system theory, instead of concentrating on problems of maintenance of system equilibrium, stresses the *difference* between system and environment, emphasizing the facts that the system maintains its identity by employing this difference in its internal operations, and that different systems have different environments—just because the environment, and not just the system, is the "result" of the "imposition" of this system/environment difference by the system (Luhmann 1995:16–18).[3] System theory is thus able to recognize that in functionally differentiated society, in which each functional sub-system can manage much more complexity than society can as a whole (without relying on its functional systems), society has no center from which a privileged, comprehensive view of its various problems would be possible. Instead, problems will be addressed by each of the various functional systems, each in its own way, and each with its own view of the problems. At the same time, since Luhmann's theory does contain the *general* concept of a (functionally differentiated) social system, it is able to conceptualize society as a whole, as well as its functional sub-systems. This makes it unique as a social scientific theory: it can analyze the various functional systems in their particularity while still having adequate conceptual machinery to represent the whole. This is to be contrasted, for example, with the new political economy which, when it observes operations of the political system, sees the same thing it sees when it observes the economy: utility-maximizing individuals.

The most important functional systems of society are the economy, the political system, the legal system, science, and the education system. The advantage of functional differentiation over other forms of differentiation is that the former allows each sub-system to specialize in solving problems specific to its function, while ignoring problems or perspectives more suitable for other sub-systems. Thus the economy can specialize on its function, which is to ensure that the future needs of individuals will be satisfied, while the science system can specialize on its function, which is to produce new knowledge. The functional systems are highly autonomous; what determines whether a claim made by a scientist will be accepted as correct will be the internal operations of the system—exposing the claim to critical scrutiny and empirical test—and not the scientist's ability to mobilize the coercive machinery of the state on his or her behalf, to spend vast sums on a public relations campaign, or to make use of his or her unusual physical attractiveness and pleasant personality. Despite this autonomy, the functional systems are highly dependent on each other: the economy would not have its present effectiveness if it could not depend upon the legal system to maintain the institution of property and enforce contracts and upon the science system to continually make discoveries which can lead to new technologies which can be used to improve production processes and develop new products.

Symbolically Generalized Communications Media

The operations of each functional system are facilitated by a *symbolically generalized communications medium* associated with it. For the economy, the medium is money; for the political and legal systems, it is power; and for science, it is truth. There are also symbolically generalized communications media which have no developed functional system associated with them. For example, the medium love supports the building up of intimate relationships, which are interaction systems, but because of its stressing of the particularity of the beloved, it has not led to the development of a corresponding functional system.

The symbolically generalized communications media serve two main purposes, which become apparent on the level of interaction and of system, respectively. On the level of interaction, they serve to combine selection with motivation. What is meant by this is that within their particular domains they allow arbitrary selections, while at the same time providing motivation to all parties involved to accept the selection. We can illustrate this by considering the example of money. When money is used, a good or service available immediately is exchanged for a sum of money which will allow the person receiving it to obtain a good or service at a later time. The motivation of the person buying a good or service is easy to understand: by making the purchase, s/he is able to enjoy the use-value of the service or good. The motivation of the person selling is somewhat more complex, since that person is merely receiving an ability to pay *(Zahlungsfähigkeit)* for something at sometime in the future. The seller's motivation thus obviously depends on the seller's trust in the communications medium, that is, in his or her belief that in the same way that s/he was willing to give up a good or perform a service in order to obtain an ability to pay, people will be willing to do so in the future. Under the assumption that the medium will work in the future, what the person obtains with this ability to pay is then the option to obtain an arbitrary good or service at an arbitrary time in the future.

This covers the respective motivations of the two people directly involved in the exchange. But why should the rest of society, which did not participate in the transaction and had no say in it, accept this reallocation of resources? Because this reallocation is legitimated by money, and money is of value to and hence accepted by everyone, since it fulfills its function as a motivator of exchange so well:

> Differently than the conventional way of viewing things in the economic sciences, we thus see the social function of property not in the immediacy of the appropriation of goods or services and the social function of money not in the mediating of transactions. As a state of affairs and as a historic-genetic motive that is naturally undented. But the function of the correspondingly generalized communications medium lies elsewhere; it lies, as always, in the overcoming of an unlikelihood-threshold. *Everyone* must be made motivated to *passively accept extremely specific selections* made by someone else: from the furnishing of their own living room and from the purchase of a particular bolt all the way to the "taking over" of a multinational corporation by another. Otherwise, already long ago, and certainly under contemporary demands, the economy could not function.
>
> (Luhmann 1997:349–350, original emphasis)

On the system level, the symbolically generalized communications media give closure to their corresponding functional system and make clear where its boundaries lie. It is easy to tell what communications occur within the economy: it is precisely those that involve the use of money, that is, those communications that are *payments*. Non-payments are also elements that make up the economy, since they are also communications which involve the use of the medium money, together with its device of prices. Payments, however, are less trivially involved in the production of the economy than non-payments, since payments actually reproduce the economy by transferring liquidity from one person or organization to another, in a never-ending process of "circulation." According to Luhmann, then, the economy is an autopoietic system consisting of payments.

It can thus be seen that the distinction pay/do not pay is of particular interest to the economy. Another such distinction, upon which the former actually depends, is the distinction own/do not own. Such binary distinctions are called *codes* by Luhmann:

> Symbolically generalized communications media require a *unified code* (central code) for the whole medium domain. A code consists of two opposed values and excludes on this level (naturally not "in life") third and further values. In this way, the indefinite, gradually increasing possibility of the rejection of the communicated proposed meaning is transformed into a hard either/or, that is, an "analog" situation is transformed into a "digital" one, and what is gained thereby is a clear decision-question which is the same for Alter and Ego.
>
> (Luhmann 1997:359–360, original emphasis)

The code associated with money reduces the openness exhibited by any social situation by providing a context which makes it clear that what matters in a given interaction is whether a payment will be made or not. (Corresponding codes for other functional sub-systems are true/false for science, legal/illegal for law, loves/does not love for intimacy, and beautiful/ugly for art. Another binary code is moral/immoral, but this does not have a functional system associated with it.) However, this code by itself is not sufficient to actually lead to a decision to pay as opposed to not pay. For that, *programs* are required:

> Coding ensures the ("outward") differentiation and specification of a medium in distinction to other media, and programming can for that reason occur only in relation to specific codes. For the truth-medium, for example, programs take the form of theories and methods; for lawfully-coded power the form of laws, court decisions which create precedents, and contracts; the medium money is respecified in the form of investment-programs and consumption-programs, which are then controlled by means of balances or budgets, respectively.
>
> (Ibid.: 377)

It can be seen that a particular code is everywhere the same, so that the code is what gives a particular kind of communication, for example a payment, its relevance for the corresponding functional system and ties that communication to the system's operations. Programs can, however, vary highly across the systems that employ them. A further point is that programs are what allow, within a given functional system, considerations to be introduced that are normally excluded by the functional system's binary coding: "An artwork must satisfy its own code of harmonious/ disharmonious or, traditionally put, beautiful/ugly. But in the choice of subject one can 'politicize' or consider sales possibilities" (ibid.: 378).

An important "construct" associated with the economy is the *contingency formula* scarcity. Consideration of this concept will also allow us to specify more precisely the function of the economy:

> Formally viewed, all economic activity orients itself around *scarcity.* The reference to scarcity however does not suffice as a specification of function… Scarcity is really just a "contingency formula" which, viewed as a constancy of sum and as the rule that every use costs something, simplifies the transformation of the given problem into operations and acts of regulating. That could justify defining the *object* of the economic sciences (and especially the object of the self-reflection of the economic system) as the disposing over scarce goods and resources.
>
> Contingency formulas are however always already reductions which stand in for the function when it is a matter of the system orienting itself toward itself. At least social-theoretic analysis must hence go back to the actual function, and this lies precisely in the creation and regulation of scarcities for the purposes of ameliorating the problems involved in the future satisfaction of needs. The problem with which the economy is concerned is, in other words, the future that is present at any particular time; one could also say—the excitability of the present by the future; or—the social problem of the present affliction with scarcity caused by others.
>
> (Luhmann 1988:64–65)

An interesting feature of modern, monetized economies is that they actually involve *two* scarcities: the "natural" scarcities of goods and services and the internally produced scarcity of money. Once an economy is monetized, the latter scarcity can be used to regulate the former, so that the scarcities of goods and services also become internally produced.

To conclude this section, I will make an observation (admittedly one with which Luhmann himself probably would not agree) that will be useful in the next section. When two or more individuals get together, there are two functionally equivalent ways they can achieve successful communication. One is to rely upon societal communications media to motivate acceptance of a communication. The other is to build up complexity in the interaction system itself, which will give participants a sufficient amount of shared knowledge to allow them to achieve a certain level of mutual understanding and hence possibly agreement. In the latter case, it is fairly clear that the more culture that is common to the participants and the greater the complexity of the culture (I mean by culture selections such as values and world-interpretations, not schematisms of interaction like the communications media), the easier the task for the interaction system will be, since many of the necessary selections will already have been made for it.

Explaining productivity growth

It is widely known that conventional economic theory is not very good at explaining variations in productivity growth, either across countries or across time. Clearly productivity growth is related to technical change, but the Neoclassical theory of technical change states merely that, in order to produce a particular product at a particular price, firms alter their choices of technologies—combinations of specific quantities of capital and labor inputs with given combined productivities—according to changes in the relative prices of these inputs. Unfortunately, the factors determining productivity growth appear not to be restricted to such narrowly economic mechanisms, and Paul Krugman (1990:23), for example, has noted that "the sources of the [recent US] productivity slowdown…lie more in the domain of sociology than of economics." Taking this point to its logical conclusion, the economic historian William Parker (1990) has argued, that for an adequate understanding of productivity growth, a search for quantitative relationships among time series of aggregated variables—as is routinely practiced in cliometrics—is not adequate. Rather it is necessary, in the manner of more traditional history, not only to investigate qualitative changes in the way that production and other aspects of the economy are organized, but also to investigate the influence upon the economy of social factors lying outside it, since it is largely from outside the economy that the innovative ideas which drive economic growth originate.[4] Since economics is not simply history, some kind of theoretical framework to underpin historical narrative is required. The theory sketched out in the previous section is one candidate for such a framework.[5] An adequate treatment of any episode of productivity growth is a major task which involves constructing a historical narrative using social-theoretic concepts. I am of course not able to embark upon any such undertaking here. Instead, in this section I will use the problem of productivity growth as an example to illustrate what very different explanatory strategies a theory taking a sociological approach to complexity can give rise to, as compared to encounters with complexity of more mainstream character as considered in the second section. I shall do this by discussing what I believe to be one significant factor responsible for the low level of productivity growth that has persisted in the US since 1970, and showing how it fits quite neatly into our social theoretic framework.

A very interesting explanation for the disappointing performance of the American economy was given by Robert Hayes and William Abernathy (1980) in an article in the *Harvard Business Review.* While they do not discount more traditional explanations, such as the oil price shocks, poor government fiscal and monetary policies, and over-regulation, they argue that these cannot be the whole story: corporate management must also be partly to blame. Over the 1960s and 1970s, one saw a shift in the background of the majority of high-level managers, from one that reflects hands-on experience in solving the specific problems that arise when running a particular manufacturing firm—which involve technological issues and production—to a background in law, finance, or marketing. Accompanying this shift, there has been a steady trend to increasingly make decisions about whether to undertake a given investment or introduce a new product on the basis of quantitative decision rules which use measures such as expected profitability or rate of return on investment (ROI), abstracting from the specific, qualitative features of a contemplated investment. Hayes and Abernathy argue that the use of such rules leads to a reluctance on the part of managers to take the kinds of risks that are unavoidable when introducing innovative, new products. The continual introduction of such products, however, is the only thing that can lead, in a dynamic global economy, to competitive success in the long run. This excessive risk-aversion is exacerbated by the well-known pressure that American companies are under to maintain high short-term profitability.

The use of decision rules based on ROI leads to a bias against introducing innovative products because of a kind of "framing effect": If one is appraising a prospective project in terms of a quantitative, abstracting description of it, what is salient is the risk and the short-term cost. The positive aspects of a project will be less apparent in a quantitative description, since they are longer-term and involve making assumptions about the feasibility of incremental improvements and the reception by the market of a (perhaps fundamentally) new product: all factors which are difficult to appraise on objectively compelling grounds, especially if one is a subordinate who is penalized for mistakes. The decision to undertake a risky and innovative project must ultimately come down to a hunch; and to have much of a hunch, one must know the details, not just a parameter summarizing their expected implications. In addition, what Hayes and Abernathy call "management by numbers" implies that, since descriptions of a project are made in increasingly schematic form as they are passed up the organizational hierarchy, there will be no place in the organization in which all the aspects of the introduction of a product, such as product design, design of the production process, marketing, and finance, are examined in an integrated way: a situation which indeed might make extreme caution a sensible strategy.

As I indicated at the close of the previous section, there are two ways in which the acceptance of a communication can be motivated. The sender of the communication can give specific reasons why the communication gives a valid description of a given state of affairs, relying upon knowledge common to the sender and receiver (the interaction system) and also upon knowledge contained in the larger culture; or the sender can invoke a communications medium, in which case the state of affairs is described in a schematic manner. The use of expected profitability measures to make the decision of whether or not to undertake an investment is thus an instance of the latter, medium-based strategy. Hayes and Abernathy note that this strategy is used for making investment decisions much less in Europe and Japan than in the US. Now, it appears that the same, mediumbased strategy is used in other functional sub-systems in American society to a significantly higher degree than in other modern societies; it is worth exploring this in order to demonstrate the power of our framework to make comparisons between the economy and what is happening elsewhere in society. I shall accordingly now argue by means of two examples that there is a general strong tendency toward the *mediatization of communication* in the US which does not exist in other societies.

The first example concerns the legal system. The difference between the American and European attitudes toward law is well conveyed by the following remark of the French sociologist Michel Crozier, who considers French attitudes in particular:

> European, and particularly French, tradition put the king beyond the law. He might abuse this privilege, but then again he might also live up to popular expectations and use it to correct any law whose literal application, in a given case, would be unjust. France overthrew the monarchy but did not abolish privilege. I once spoke (in 1959) with the prefect of a French *département,* whose career would later take him to the very top, and who bluntly told me: "Sir, a prefect is there to break the law. If there was no need of breaking the law from time to time in order to protect the innocent or to assure the public welfare, there would be no need for prefects." Coming from an official specifically entrusted with the job of making people respect the law, this comment, even if strictly confidential, would strike Americans as utterly scandalous.
>
> (Crozier 1984:100–101)

French culture thus appears to have the effect that French legal officials view law as a necessary mechanism for regulating interpersonal relations, but one which is occasionally too blunt; in those cases it is desirable for an official to disregard the law and examine a situation in more complex terms than the law is able to provide. This is not at all true of the US, where the law is perceived to have a kind of objective validity which entails that it must be applied literally, as if it were a kind of machine, no matter how the results it produces may appear from a common-sense perspective.[6]

Our second example deals with science. Science uses the medium of truth, which is based on the difference empirically true/empirically false. As is well known, a central tenet of the scientific doctrine called behaviorism was that any talk using words like "minds" or "beliefs" is totally unscientific and should be banished from science, since these things are not intersubjectively observable. Now if there is one thing that we assume in our daily interactions, it would seem to be that people have minds. Behaviorism thus went against a fundamental cultural idea (indeed, so fundamental that interaction without it is impossible) as a consequence of applying the communications medium truth in an uncompromising, mechanical fashion. It does not appear to be a coincidence that behaviorism was a specifically American research program (Mandler 1984:19).

If these examples are at all suggestive, it would thus appear that the tendency pointed out by Hayes and Abernathy of American managers using investment criteria based on ROI is an instance of a general predisposition in American society to base communication heavily on symbolically generalized communications media, which are extensively programmed, rather than on the knowledge and values contained in shared culture, which is more diffuse and complex. One would like to explain this predisposition. A reasonable place to look is at general characteristics of American society that have been present essentially since its inception. The natural source to turn to here is de Tocqueville ([1835–40] 1981). Tocqueville noted two general traits of American society that are of relevance here. One is a high valuation *of quickness:* people wished above all to make something of themselves by taking advantage of the apparently limitless possibilities that a wide-open continent had to offer, and to do so in a quick, pragmatic way, without being slowed down by the kinds of constraints on interaction that European societies had developed along with their tradition-bound cultures. The other was *democracy:* this was a country where privilege did not exist, or at any rate was illegitimate; everyone here was free and had a right to an equal say. It is not hard to see how these two traits would lead to an unusually high reliance by society upon symbolically generalized communications media. The precise function of these media is to enable a fast and efficient acceptance of communications without reliance upon the internal complexity of interaction systems. By using the communications media money and law, people could make important trades and agreements without needing to build up an extensive interaction history. While the preference for speed in this way limited the complexity of interaction systems, the preference for democracy limited the complexity of the general culture: this was because, as Tocqueville observed, in a democracy, for an idea to be good, it must be understandable by everyone—so it must not be too complicated. This constraint which is placed upon the degree of complexity of the general culture means that if a disagreement between two parties is sufficiently fundamental they will not be able to understand each other, and so they will have to appeal to an outside party capable of making binding decisions, that is, to the courts or the state.

It is interesting to make here another cross-cultural comparison, this time with Japanese society. In marked contrast to other industrialized societies, most daily interactions in Japan occur with the interaction system "in the foreground" and, indeed, with a sizable emotional investment being placed in the interaction system—traits which are characteristic of archaic rather than modern societies (Hendry 1987:202–203). Nakane (1970) has described Japanese society as being constituted at every level on the model of the Japanese house/family or *ie*. Thus, co-workers in a Japanese company will develop intensive, long-term relationships with each other, socialize together in their free time, and develop the dependency relationships and the distinction between insider/outsider that are characteristic of families and intimate relationships. Similarly, companies have longterm relationships with their suppliers, customers, and banks. These kinds of relationships exist at other levels as well: employees will not only think of their work group as "their family," but also of their company as a larger family containing their group. And in villages, neighbors think of themselves as forming a cohesive community.[7] It is easy to see how this high valuation of "local solidarity" by Japanese society allows Japanese companies to avoid the short-term-oriented behavior described by Hayes and Abernathy. Within the organization, the fact that employees usually stay with the firm for life and that high-level managers have risen from a low-level position within the firm means that a rich organizational culture (which contains knowledge not only about the organization but also about technology) can develop, and that high-level managers will be in a position to understand the fine points of projects proposed by their subordinates, and so be able to make a more informed (and hence confident) decision. Furthermore, the long-term and emotional nature of the relationships between superiors and their subordinates means that there will be a high level of trust, that it will be easy to think in long-range terms, and that the group will not be unduly afraid of taking risks, since they are "all in it together."[8] At the inter-organizational level, the fact that companies in a group own each other's stock means that they will be relatively free of pressures for short-term profitability.

Despite these many apparent advantages enjoyed by Japanese business people relative to their American counterparts, at the time of writing the US economy is outperforming the Japanese in terms of the rate of growth of output at least, having exhibited an annual growth rate of 2.1 percent as opposed to 1.6 percent over the period 1990–6.* One might conclude from this that whatever the merits of the explanation for the slowdown in US productivity growth just sketched out, the account given is of purely historical interest, since the problems described would seem to have been recently overcome. However, if one considers the precise manner in which the relatively high US growth rate has been brought about, it can be seen that the account may be as relevant as ever. The means for producing the "outstanding" US economic performance have consisted of three components:

1 a virtually complete lack of wage increases (US wages increased on average by only 0.15 percent per annum over the period 1985–95, compared with 2.9 percent in Japan and 2.85 percent in Germany;
2 a sustained secular devaluation of the dollar relative to the currencies of the US's industrial competitors;
3 maintenance of aggregate demand, in the face of the stagnant consumer spending that would follow from 1, by means of an increase in consumer and business debt relative to GDP.

The case can thus be made that rather than having overcome its relative economic disadvantage due to its high reliance on programmed means of decision making—which leads to a lower level of productivity-enhancing investment and hence, *ceteris paribus,* a lower rate of productivity growth—American society has merely adapted to it, by offsetting its lowered technologically induced labor productivity growth in manufacturing with a progressive downsizing of the amount of labor employed and by lowering its labor costs relative to that of its competitors by keeping wages low and repeatedly devaluing the dollar.[9] But it is doubtful whether this strategy is sustainable in the long run: there are grounds for thinking that labor productivity is enhanced by having relatively large amounts of labor employed at relatively high wages (the high cost of labor induces producers to search for technological improvements to make labor more productive, and the labor which is employed becomes more productive simply from learning by doing); the low wage growth aggravates problems of low aggregate demand and hence necessitates increased indebtedness; and if the dollar is devalued past a certain point, foreigners will no longer be willing to hold debt denominated in dollars, thus removing a primary means for the financing of the US trade deficit and raising the danger of a sharply deficient aggregate demand at the global level. Of course the resolution of these questions will require detailed empirical research. (Certainly, the fact that despite all the downsizing, average US manufacturing productivity growth was slightly lower than that of Japan, Italy, and the UK between 1985 and 1995 and between 1990 and 1995 is no grounds for jubilation.) But the very fact that these questions come up suggests that the considerations raised in this section continue to have profound significance for the long-term well-being of the US economy.

* The US still lagged behind Japan in terms of the rate of growth of productivity, with Japanese productivity increasing by 1.0 percent, while US productivity increased by only 0.7 percent over the same period.

Conclusion

Taking the SFI research program and Niklas Luhmann's theory of social systems as our paradigms for the responses of economics and sociology respectively to the problem of complexity, it does not appear that economics will be turning into sociology anytime soon. The SFI school is still working very much in the tradition of orthodox political economy, according to which one causal factor can be analyzed by theory in isolation from all others, to be—ideally—combined with them once all theorizing is complete in a subsequent stage of practical application.[10] Arthur can therefore write about complexity in economics as follows:

> given sufficient homogeneity of (unbiased) beliefs, the standard equilibrium of the literature is upheld…As the dial of heterogeneity of individual beliefs is turned up, the market undergoes a phase transition and "comes to life". It develops a rich psychology and displays phenomena regarded as anomalies in the standard theory but observed in real markets… [I]t displays complex, pattern-forming, nonstationary behavior. We could therefore rename the two regimes or phases *simple* and *complex*. We conjecture that actual financial markets live within the complex regime.
>
> (Arthur 1995:25)

Complexity for Arthur is hence not a characteristic of how causal processes are connected to each other, but a characteristic of the behavior of the time series of a given variable. If this view is accepted, then economic theorizing can proceed essentially in the same way as before, with one model being introduced after another, each being intended to shed light on one putative causal mechanism of interest by being able to mimic it through a carefully chosen set of assumptions; these assumptions are chosen, *à 1a* Milton Friedman, so that they have as a logical implication the phenomenon of interest, not so that there should be grounds to believe that they are actually instantiated in some real economy. The mode of theorizing exemplified by Luhmann's work suggests a different response to complexity. According to this view of what complexity signifies for social and economic theory, the fact that economic phenomena are complex means that they cannot be analyzed one by one, as held by John Stewart Mill: one must instead respond to the presence of complexity by recognizing that economic phenomena can only be understood in their concreteness, by examining in any particular case how the various causal structures and processes interact over time to produce the final result. In such an approach, 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.

By design, Luhmann's theory possesses this ability. In this theory, the world is not populated simply by atomistic agents, where the question of whether complexity is present turns around whether or not agents are able to acquire mutually consistent beliefs. Complexity is always present in society by definition, so the theoretical problem is before all else to produce a set of concepts that can adequately mirror this complexity. Thus, the economic world does not consist of atomistic agents (or of a "collection of beliefs, anticipations, expectations, and interpretations," in Arthur's 1995:20 refinement), but of actors, rules, and systems (with the systems not being reducible to actors, since the systems consist of (in addition to communications) rules, which are largely produced by the systems themselves, independently of the actors' intentions). Furthermore, many of these rules make up rather complex "institutions," such as the symbolically generalized communication medium money, which in turn predetermine the interpretations placed on given situations, through such "schematisms" as scarcity, profitability, or the difference able to pay/not able to pay. In addition, as we have seen, while "tending" to adhere to the perspectives generated by the communications media, actors—being conscious agents—are able to set aside these interpretations, substituting their own more locally generated ones. And lastly, all of these entities interact with each other over time, and the economic system interacts over time with the other functional subsystems of society, to produce the high-level phenomena in which we as economists are interested. It is quite unlikely that any sufficiently striking economic phenomenon will be understandable simply in terms of one cause, as one tries to understand, for example, the US productivity slowdown when one attributes it to oil price shocks or inflexible wages. A satisfactory economic explanation will not resemble one from classical mechanics, where (to mention the kind of explanation that Mill took as his model for economics) the direction and velocity of a body that was hit by another body is explained by adding the vectors representing the momenta of the two bodies before the impact. (Even if the Sante Fe school considers mechanisms which produce "complex" behavior, it still views the way in which mechanisms interact with each other on the model of mechanics: either agents have consistent expectations, in which case the price behavior will be simple; or they will not, in which case it will be complex.) It will instead resemble the explanations of another science that deals with complex phenomena, evolutionary biology, in which one explains why a particular organism came to have the features that it has by considering the relation of the species from which it evolved to its ecological environment, the epigenetic constraints placed on the original species by its genetic makeup, and how all of these interacted with each other over time to produce the evolutionary history which gave rise to the new organism.

Citicorp supported the Sante Fe Institute's research on complexity because it was unhappy with the fact that:

> the existing neoclassical theory and models based upon it simply did not give him [the president of Citicorp] the kind of information he needed to make real-time decisions in the face of risk and uncertainty… [N]one of the models really dealt with social and political factors, which were often the most important variables of all.
>
> (Waldrop 1992:93)

In this chapter, I have contrasted two approaches that economics can take to the problem of complexity. Time will tell which is the more fruitful for generating knowledge about the kinds of problems which interest Citicorp.

Notes

1 A popular account of the genesis of the SFI is provided by Waldrop (1992). Mirowski (1996) provides a useful (skeptical) summary.

2 The former is the practice in most of micro-economics and game theory to find equilibria produced from the rational choices of individual optimizers; the latter is the practice in macro-economics of building models which attempt to reproduce observed patterns of behavior over time of economic aggregates.

3 Luhmann thus adopts an extreme perspectivism, and indeed constructivism: within the conceptual world of his theory, objectivity is not possible (it is in fact an incoherent notion), since all observations are made by an observer, and the observer "creates" his "world" by employing his own system/environment difference or, more generally, makes an observation possible by employing some other distinction. What is novel about Luhmann's perspectivism is that social systems, and not just human beings, can have perspectives. Since the theory "observes" systems which themselves observe, it is a theory of "second-order" observation.

4 The recent "endogenous growth" literature does recognize that since such a large portion of economic growth is attributable to technical change, the latter must be treated endogenously in the model, but, as William Lazonick has observed, "the dominant tendency [there] is to portray technological advance in a simplistic manner in which new 'ideas' are generated as a function of the inputs of labour and capital," without recognizing as Moses Abramovitz has suggested that "something called 'social capability' is responsible for the ability of some national economies to so develop their productive resources that they 'catch up' and even in some cases 'forge ahead' of other national economies that 'fall behind'" (Lazonick and O'Sullivan 1996:9).

5 Parker (1990) sketches out his own framework, basing himself largely on Talcott Parsons' sociological theory.

6 As the liberal legal philosopher Ronald Dworkin has put it: "We live in and by the law. It is sword [and] shield, our abstract and ethereal sovereign. We are subjects of law's empire, liegemen to its methods and ideals, bound in spirit while we debate what we must therefore do" (Dworkin 1986:vii).

7 The following example gives an especially clear illustration of the difference between the relative importance of interaction and communications medium in Japan and in the US. When a family sued another family in their village for the accidental death of their son, this made national news because it was so unusual, led to the ostracism of the suing family by the entire village for its turning to outsiders for resolving a dispute which should have been dealt with internally within the village, and eventually resulted in the chagrined family withdrawing their suit, when it was already too late (Hendry 1987:190–191).

8 Indeed the level of trust is so high that high-level managers may not have really to review decisions made by their subordinates at all: the real job of the former is often seen as managing the relations among their subordinates, who are the ones who make the actual decisions.

9 For a detailed argument to this effect, see Brenner (1998), from which the above statistics were taken.

10 To quote Mill: "The method of the practical philosopher consists of two processes; the one analytical the other synthetical. He must *analyze* the existing state of society into its elements, not dropping and losing any of them by the way. After referring to the experience of individual man to learn the law of each of these elements, that is, to learn what are its natural effects, and how much of the effect follows from so much of the cause when not counteracted by any other cause, there remains an operation of *synthesis;* to put all these effects together, and from what they are separately, to collect what would be the effect of all the causes acting at once… [M]ankind can never predict with absolute certainty, but only with a less or greater degree of probability; according as they are better or worse apprized what the causes are and have *summed up* the aggregate effect more or less carefully" (Mill, 1836:336, last emphasis added).

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9

MARSHALL AND THE ROLE OF COMMON SENSE IN COMPLEX SYSTEMS

Flavio Comim[1]

> The “principles” of economics must aim at affording guidance to an entry on problems of life, without making claim to be a substitute for independent study and thought.
>
> (Alfred Marshall, *Principles of Economics*)

Introduction

While complexity theory is something relatively new in economics, it is interesting to note that complexity in economic theory is a familiar and well-known topic in the history of the discipline. Many current research themes on complexity, such as autopoiesis, non-linearity, path-dependence and lock-in, were previously addressed—in different contexts—by economists interested in the complexities of real-life economies. Indeed, Alfred Marshall’s famous appeal for “economic biology” as the “Mecca of the economist,” Maynard Keynes’ emphasis on the instability of equilibrium positions and the role of expectations, Piero Sraffa’s and Nicholas Kaldor’s criticism of the partial equilibrium method due to the existence of increasing returns and non-linearities in economic processes, and Joan Robinson’s argument for the asymmetric consequences of time in economic theory are just a few examples of the importance of the themes relating to complexity in the intellectual history of economics. The writings of many others, such as Joseph Schumpeter, George Shackle, Friedrich Hayek and Richard Goodwin, could be mentioned as examples of the importance of the idea of complexity in the history of economics.

Current ideas on complexity have been largely influenced by work carried out at the Santa Fe Institute. Research on complex systems claims to have provided a new *Zeitgeist* for the doing of science, the main relevance of which consists in expressing a dynamic picture of basic real-world structures. Synergism between the realistic and the artificial-world approaches in economics is pursued and justified on the basis that, on the one hand, scientific activities cannot be reduced to mathematical simulations without concern for reality and, on the other hand, commitment to realistic settings without some technical guidelines is not enough to generate organized knowledge. What started in 1987 as a very speculative dialogue between economists and natural scientists—with a clear emphasis on chaotic and non-linear dynamic processes in complex macro-economic problems—has now been called by Arthur *et al.* (1997) the "Santa Fe Approach" (hereafter SFA) to economics, an approach which they claim has profound implications for the foundations of economic theory.

Although an investigation of the historical precedents to the contemporary discussions on the SFA would be of interest *per se,* the case for this investigation is stronger on the basis that any contextualization of the future significance of the SFA must address old challenges in the history of economics on the issue of complexity. In a certain sense, scientists are molded in the history of their disciplines, and acceptance of new approaches and paradigms might be related to and constrained by past common understanding of problems and challenges within the discipline. In the case that concerns us here, in order to highlight the relevance of the alleged SFA's synergism, we shall attempt to investigate the historical roots of complexity in economics. Because of the vastness of the task, we shall narrow the focus of the discussion in two ways: by focusing on Marshall's views on complexity, and thus providing a concrete illustration of a historical precedent for the SFA; and by addressing the specific problem of context definition and context transition in complex systems, and thus isolating one of the central issues in the complexity literature through which a contextualization of the SFA is attempted.

According to the SFA, complex systems are expected to represent processes where there are no attractors or where attractors may change. From this perspective, the main theoretical challenge raised by this approach concerns the definition of an idea of context (or cluster) and an explanation of how the transition between different contexts takes place. It is argued here that this issue is not discussed in any depth in the literature and that, when discussed, there is an almost uniform tendency to justify the context transition in terms of the biological metaphor of evolution, which not only may fail to provide a mechanism for transition but also ignores ethical aspects related to the definition of contexts in economics.

The aim of this chapter is to examine how Alfred Marshall's views on the practical aspects of economics and the specific problem of context definition in complex systems might help to illuminate the use of complexity theory in economics. The essay focuses on Marshall's understanding of common sense and his references to the complexity of the economic systems rather than on an objective comparison between Marshall's and SFA's notions of complexity. The main point made here is that Marshall's discussions of common sense and complexity may offer a good illustration of the importance of practical aspects for any synergistic effort in economics.

The chapter is organized into three sections. The first outlines the main features of the SFA, focusing on the issues of context definition/transition and ethical aspects. The second describes Marshall's views on complexity and his appeal to common sense as a way to apply economic reasoning (on complex systems) in practice. The last section argues that Marshall's views on complexity, rather than just being an historical precedent for the SFA, consist in an original illustration of the role of common sense and judgment in the use of economic theory. It suggests that the SFA should address some of the issues raised by Marshall concerning the conceptualization of complex systems.

The Santa Fe approach to economics

As Arthur *et al.* (1997) point out, there is no single vision able to express the meaning and significance of complexity in economics. Yet they identify a family resemblance among interrelated sets of themes that pervade the current meaning of complexity in economics or the "Santa Fe Approach" to economics. Among these themes we find: applications of non-linear dynamics to economics and data analysis; the theories of path-dependence and lock-in; and the emergence and evolution of self-organized systems.[2] The main purpose behind this approach is the study of what is called *complex adaptive systems* or *adaptive non-linear networks* (ANN). These networks or systems—which might refer to proteins, ants, or economic agents—are characterized by different properties which define a logical realm where many relatively independent parts are highly interconnected and interactive (Cowan 1994). In its latest version (in Arthur *et al.* 1997), these ANN are characterized by six properties:

1 *Dispersed interaction* Global results are produced by the interaction of many diverse and dispersed agents acting in anticipation of other agents. It is important that agents are not only numerous but also diverse, because it is this property which helps to produce "perpetual novelty" in aggregate behaviour.
2 *No global controller* Interactions among agents are produced by mechanisms of competition and coordination, such as legal institutions.
3 *Cross-cutting hierarchical organization* Global organization happens at many different levels, where units at any level may serve as "building blocks" of units that are at a higher level.
4 *Continual adaptation* Internal states of agents change in response to changes in the environment, where individuals constantly adapt to their accumulated experience.

5 *Perpetual novelty* New behaviours and new structures may stimulate the creation of new behaviours and new structures, producing an ongoing state of perpetual novelty.
6 *Out-of-equilibrium dynamics* Given the state of perpetual novelty, the economy does not operate close to any optimum equilibrium.

The main relevance of complex systems is that they consist in an interdisciplinary attempt to work with theoretical structures which are expected to reflect the external and dynamic conditions of the world. This means that because the networks are adaptive they are able to internalize empirical information from the world in terms of states of a system (see Martin 1994:266). Because the networks are subject to novelty they are able to reflect the dynamic nature of structures that emerge and evolve. In a way, one of the reasons for the appeal of this approach is its acknowledgement of the shortcomings of neoclassicism in economics. Arthur has provided a good illustration of the relevance of complex systems for the doing of economics.[3] According to him:

> The problem with all theory is that it tends to portray the system you are looking at—say, the economy, or the biosphere—in terms of the dominant *Zeitgeist* metaphor of the time. For Adam Smith's time, in 1776, for 50 years—or longer—the dominant metaphor was systems in stasis, systems that operate in a kind of clockwork fashion, that are highly deterministic, and are in some sort of equilibrium balance. That was the notion of the Enlightenment, partially inherited from Newton and others. We are entering a different *Zeitgeist* at this stage in the twentieth century, where we're more interested in things that are in process, and pattern change. So I'm hoping that new theories of economics can reflect that the economy is in process, that the economy is always developing. The development never necessarily stops. The question is how do we talk about that? How do we think about it? What are the mechanisms? And above all, does that give us a feeling that we're closer to reality?
>
> (Arthur 1994:80)

Thus, it seems that the main relevance of the ANN for the doing of economics is that they are a motion picture or a metaphor of the basic dynamic real-world economic structures. As noted before, because the changes in the internal modes are assumed to be a result of a reaction to environmental conditions, these systems are expected to learn from their interaction with reality. For this reason, as discussed by Anderson (1994:10–11), mathematical theory is not the best way to follow in order to understand complex systems, due to the fact that its "lemma-theorem-proof" structure is only concerned with exact unique solutions which do not provide good approximations for analyzing real world complexities. Another aspect of this issue is that, as Holland (1994:332) puts it, "our traditional mathematical tools rely on linearity and equilibria—fixed points, bases of attraction, and the like—features mostly missing from complex adaptive systems." Hence, standard mathematical approaches—based on equilibrium—cannot capture complex behavior which is in "perpetual novelty," and new mathematical tools, based on combinatorial analysis and population-level stochastic processes, need to be developed.

From this perspective, economics is not seen as an isolated discipline but as one more integrated entity embedded into a larger cultural system. To conceive of economics as an autopoietic system (Zeleny 1996) is to say that the scientific strategy of looking for attractors is meaningless. The focus of attention in studying ANN must shift from the search for equilibrium points to ever-changing trajectories. Arthur (1994:646) has argued that "all economics has been able to do for the last fifty or one hundred years is to look at systems with very strong attractors, not even talk about how an equilibrium point is reached but simply point out that there is an equilibrium and that if we were there, there would be a tendency to stay there." By contrast, the SFA argues for a representation of realistic and genuine processes of change, where perhaps there are no attractors or attractors may change, meaning that processes have no definable end states. Thus, the representation of the notion of change is at the core of the SFA.

The main alternatives discussed in the literature of complex systems and artificial intelligence point to two main organizing concepts—schemata and histories—that provide a basic description of the relevant features or aspects of reality which are in permanent change. In so far as these concepts represent the idea *of context* (or cluster), that is, of tight connections within a certain group, they serve the purpose of individuating and identifying reality.

The first of these two organizing concepts is that of *schemata* (Martin 1994). In social sciences, schemata may represent a set of habits, customs, institutions, and so on, or any other sort of regularities perceived by the elements of the system. According to Martin, schemata have two main characteristics: summarization and internalization. Summarization means that one of the functions of schemata is to produce a reduced and organized description of the relevant aspects of an object or event. Internalization means that schemata can internally represent external real-world features as states of a system. However, once the information is summarized and internalized, schemata become a representation of real processes that follow the organizational principles established by the particular system. Martin (1994:276) remarks that schemata "might provide the kind *of probative guide to intuition* that can always be of use in understanding the behavior of extremely complicated phenomena."

Because the entire emphasis of the SFA is put on the dynamic aspects of complex systems, the main question to be asked here concerns the transition between different schemata (or contexts). How does transition take place? As indicated earlier, this point has not received the attention it deserves in the literature, and when it is discussed there is a tendency to justify the schemata transition with evolutionary metaphors (see Khalil and Boulding 1996). However useful as tools for creative thought, biological metaphors must be handled with prudence. Louçã (1997: ch. 5) provides a balanced assessment of the use of biological metaphors in economics. On the one hand, he shows how evolutionary metaphors based on Lamarckian biology and Darwinism have committed the obliquity error,[4] because logical relations of causality cannot be translated from biology into economics. He points out that:

> The balance sheet of the precise metaphors taken from biology is rather poor: for most of the cases, the metaphors amounted to extreme versions of the obliquity error, not only translating similarity into causality but also implying wrong relations of causality.
>
> (Louçã 1997:97)

On the other hand, he argues that biological metaphors constitute an invaluable source of creative thinking free from positivist vices. Evolutionary systems present characteristics, such as mutation, heredity, and selection, which provide through the concepts of self-evolution, self-sustainability, autocatalytic development and self-reference better alternatives for the study of processes of change than the physicist metaphors of equilibrium, maximization and conservation. His conclusion is (ibid.: 104) that the "evolutionary metaphor is useful and necessary, but it is also limited."[5] Evolutionary ideas may be transferred to economics but only if we respect the specificity of social and historical domains, where purposeful human action must be taken into account.

The upshot of this discussion is that schemata evolution and the transition between different contexts may be illuminated by biological metaphors, but one must exert prudence in the interpretation of the metaphors due to the idiosyncratic features of social systems. Indeed, the faculties of prudence, judgment, intuition, and discrimination are the ultimate arbiters of the use of metaphors and, hence, ethical aspects are an intrinsic part in the use of biological metaphors.

Bearing in mind the consequences of the use of physical and mathematical metaphors in economics, the issues of prudence and judgment in the use of biological metaphors acquire a double significance. What is at stake here is not merely the question of choosing the best type of metaphors for the doing of economics but also the question of how one exercises prudence and judgment in the choice and use of metaphors. With the development of the "lemma-theorem-proof" way of doing science, economists' concern with prudence and judgment in the doing and using of economics has virtually disappeared (see Krueger *et al.* 1991). Now, interest in complexity theory and evolutionary metaphors arises from the fact that they might provide not only a better metaphor through which to understand the processes of emergence, evolution and self-organization of social systems but might also provide scope for a logical inversion of the way of doing economics: from a situation where theory determined the choice of the problems to be studied to a situation where the nature of the problems under study may determine the choice of the most appropriate theory, resulting in a more realistic economics.

The second organizing concept put forward as a way of describing the features of reality is the concept of *histories,* elaborated in the literature of artificial intelligence. Hayes (1985) defines a history as a particular spatio-temporal entity "in which something happens." In other words, a history is a spatio-temporal isolation of an event. It allows qualitative description of real-world phenomena since it provides factual descriptions of situations that are extended through time and are bounded spatially. A history is subdivided into *episodes* that are the expression of the different particular kinds of things that happen in that history. The basic criterion for determining the identity of histories is their spatio-temporal continuity. Some examples of histories would include a lecture, an economic recession or even a particular set of speculative acts on the financial market.

The concept of history results from the need for "conceptual closure" when attempting to express a wide range of intuitive[6] concepts in the description of certain phenomena. Because we want explanations of reality with breadth and depth we need to create clusters where we are able to relate a certain concept to many others. Hayes (1985:15) argues that: "It is exactly this, being tightly caught in a dense web of inferential connections to other parts of the theory, which gives a token [that is, a formal symbol] meaning, by cutting out unwanted implausible models." Thus, the meaning of the representations depends on the circumstances (that is, the nature of the boundaries) in which the heuristics of the problem is created. The transition between different histories or contexts may be subject, in addition, to a change in the circumstances but also to a change in the heuristics of the problem. This might generate histories that diverge among themselves because they are defined taking into account different dimensions. Then, the main question, known in the artificial intelligence literature as *the frame* problem (see McCarthy and Hayes 1969) is: How do we know which facts remain true and which facts change when something happens? Thus, the acknowledgement that there is no intrinsic mechanism able to explain the transition between different histories means that we are back to the same basic problem which we identified earlier when discussing the evolution of schemata. Now, the main conclusion to be drawn from an examination of the notions of schemata and histories is that it seems implausible that the rich variety of the everyday world could be represented without addressing the complexity involved in the comparison of different contexts.

The outcome of this argument is that the SFA's *use of* evolutionary metaphors and complex systems—if meant to bring realism into economics—needs to be complemented by an understanding of how economists conceptualize change in real social systems, because on this perception depends the prudent and proper use of complex systems in economics. This is the issue raised by Louçã of prudence in the use of biological metaphors. But how does one define the economists' notion of context definition/transition that is needed as "the ultimate arbiter in the use of metaphors?" It is argued here that this is itself a complex issue, the answer to which must be sought in the common-sense reasoning involved in the practical affairs of ordinary life. Otherwise, the meaning of the SFA 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.

Marshall, complexity and common sense

Marshall believed that economics was a discipline with a dual nature. On the one hand, he considered it to be a fruitful field for intellectual and analytical speculations. On the other hand, he trusted economics to play an essential role in the betterment of the conditions of mankind. This dual nature of Marshall's approach to economics has been widely acknowledged in the literature. For instance, J.M.Keynes ([1924] 1925:11–12), in his bibliographical tribute to Marshall, remarked that "Like his two colleagues, Henry Sidgwick and James Ward, in the Chairs of the Moral Sciences at Cambridge during the last decades of the nineteenth century, Alfred Marshall belonged to the tribe of sages and pastors; yet like them also, endowed with a *double nature,* he was a scientist too" (italics added). Marshall's double nature influenced his conception of economics and its relation with ethics. As J.K.Whitaker (1975:9) noted: "More than most economists, Marshall remained aware of the broader context of his subject and its wider ramifications. In particular the boundary between political economy and ethics always remained, not so much blurred…as disregarded." More specifically, the dual character of Marshall's work was manifested in his concern with economics as a useful guide for practical issues.[7] A.C.Pigou, in his *In Memoriam* to Marshall ([1924] 1925:85), observed that "economics for him was a handmaid to ethics, not an end in itself, but a means to a further end: an instrument, by the perfecting of which it might be possible to better the conditions of human life." Thus, it might be said that Marshall, as a result of his double nature, imprinted a dual nature on his economics.

A straightforward consequence of this dual nature is his emphasis on the distinction between the "pure," "narrower," "theoretical," and the "applied," "wider," and "proper" aspects of economics. In the first sense, economics was for him a purely abstract science, concerned mainly with broad general propositions and hypothetical "if questions. In the second sense, economics was seen by him as a set of principles, applicable to concrete economic conditions, which take into account local and temporary elements of particular situations. Although for Marshall the contrast between these two aspects of economics is a question of degree, he considered that ultimately (Marshall 1961, 2:109 from third edition 1895), "the whole of economics is an applied science." This did not mean that theory was not an important part of the whole economics, but that ultimately theory was at the service of practice. What Marshall called *economics proper* was, then, a combination of theory and practice. He makes this very clear in a letter of 28 August 1902 to F.Y.Edgeworth:

> In my view "theory" is essential. No one gets any real grip of economic problems unless he will work at it. But I conceive no more calamitous notion than that abstract, or general, or "theoretical" economics was economics "proper". It seems to me an essential but a very small part of economics proper: and by itself sometimes even—well, not a very good occupation of time… a combination of the two sides of the work is *alone* economics proper. Economic theory is, in my opinion, as mischievous an impostor when it claims to be economics *proper* as is mere crude unanalysed history.
>
> (Marshall, in Pigou 1925:437)

It is such reasoning that enabled Marshall to contextualise the *statical method* within a wider framework of analysis. Because the theoretical and analytical aspects of economics were not the whole of economics for Marshall, he did not expect them to resemble the real world closely enough (see his letter of December 15, 1902 to J.B.Clark, in Pigou 1925). This meant that the statical method was relevant for Marshall in so far as it was able to provide coherent and useful guidelines for subsequent application of the theory. From this perspective, the statical method, with its mechanical analogies, was as important to economics as the dynamic and biological analogies.[8] Marshall's remark (1898:39) that "economic reasoning should start on methods analogous to those of physical statics, and should gradually become more biological in tone" should not be understood as a statement of absolute superiority of biological metaphors over physical ones.[9] Because of the dual nature of Marshall's economics, the statical method might be as serviceable to practice as any other method. As Marshall observes:

> the statical solution has claims of its own. It is simpler than the dynamical; it may afford useful preparation and training for the more difficult dynamical solution; and it may be the first step towards a provisional and partial solution in problems so complex that a complete dynamical solution is beyond our attainment.
>
> (Marshall 1898:38)

As far as the use of biological metaphors is concerned, he introduces a conditional clause when he remarks (ibid.: 43) that "biological analogies are to be preferred to mechanical, other things being equal" but that "Other things may not be equal"; and his advice (ibid.) is that "wherever helpful it [mechanical analogy] should be used." Now, we are emphasizing here the usefulness Marshall saw in the statical method, with its mechanical analogies, not because of the qualities of this method *per se,* but because of the attention its *use* demands. The idea behind it is that the stronger the limitations of a method, the more attention and prudence must be exercised in its application to wider contexts. To re-emphasize, mechanical analogies are "preliminary devices" (see Marshall, 1898:52) to be subsequently used and framed within a wider and applied context. From this perspective, both mechanical and biological analogies fall short of providing authoritative statements if not complemented by the *human element of the problem.* Consequently, even if Marshall had written a second volume of the *Principles,* based on biological analogies, he would still have had to contextualize those results within *economics proper.* It is interesting to note that Marshall criticized Sidgwick for his ambitions of providing in his *Principles* an authoritative guide to the art of political economy (see Coats 1990, Marshall 1961, 2:154 from the third edition 1895) because he did not believe that the ethical aspects of economics could be described in a complete and self-contained way. Similarly, Marshall was well aware of Comte's emphasis on the intricacies and complexity of social phenomena but was critical of his proposal of a unified science. The human element involved in science is intrinsic to the dual nature of Marshall's economics. In *The Present Position of Economics* he writes:

> It is vain to speak of the higher authority of a unified social science. No doubt if that existed Economics would gladly find shelter under its wing. But it does not exist; it shows no signs of coming into existence. There is no use in waiting idly for it; we must do what we can with our present resources. The only resources we have for dealing with social problems as a whole lie in *the judgment of common sense.* For the present, and for a long time to come, that must be the final arbiter. Economic theory does not claim to displace it from its supreme

> authority, nor to interfere with the manner nor even the order of its work, but only to assist it in one part of its work.
>
> (Marshall [1885] 1925:163–164, italics added)

Marshall believed that because the world of man's actions as a whole is too wide and complex, it could not be analysed by an *a priori* unified intellectual effort. To sum up, the keynote to understand the dual nature of Marshall's economics lies in (i) giving a limited role to theoretical studies in economics as a whole and (ii) resorting to the judgment of common sense to attain a wider and applied economics. Outcomes (i) and (ii) are, as we shall argue, a result of Marshall's perception of the complexity of the real world. Had he believed otherwise, the balance between theory and practice would have been otherwise.

The theme of complexity was pervasive in Marshall's writings. It can be found, for instance, in his main books, such as the *Principles* in its several editions, and *Industry and Trade;* in papers, such as *The Present Position of Economics* (1885) and *The Old Generation of Economists and the New* (1897); and in his correspondence with other economists, such as Edgeworth (see Pigou 1925). To start with, it should be mentioned that Marshall argued that the aim of the work of economists was "to disentangle the interwoven effects of complex causes" ([1902] 1925:437). Complexity for Marshall was found not only in the object of study—the natural and social processes—but also in the knowledge used to analyze those processes. He puts considerable emphasis on the way through which the progress of knowledge might become more complex in itself. He observes that more complex studies are bound to be more realistic ([1920] 1961, 2:175), and that "Every year economic problems become complex" because they are studied from many different perspectives ([1920] 1961, 2:291). However, complexity and variety of knowledge are, according to Marshall, best handled with simplicity. He is echoing Isaac Newton's and Adam Smith's principle of explaining a wide diversity of phenomena based on a small set of principles when he argues ([1897] 1925:298) that "there has been a growing readiness among economists, as among students of physical science, to recognize that the infinite variety and complexity of nature's forms is compatible with a marvellous latent simplicity of her governing principles." Although Marshall does not discuss the applicability of this principle, it is possible to deduce from other comments he makes that simplicity of governing principles applies mostly at the level of foundations of theories and in the short run.

Complexity was an important feature of economic systems for the Marshall of the *Principles* ([1920] 1961). This importance is manifested in (i) his references to the complex nature of some economic phenomena; (ii) his direct statements concerning the complexity of economic systems; and (iii) his use of some categories currently employed in the study of complex systems. In what follows we elaborate on each of these items.

There are many references to complexity in Marshall's characterization of economic systems. For him several things are complex: the mutual interactions between supply and demand are complex (ibid.: xv); the "industrial organism" is complex (ibid.: 47, 248); forms of the modern money market are complex (ibid.: 72); the scale of manufactures is complex (ibid.: 256); "the good and the evil effects of the action of speculators" are complex (ibid.: 293); problems of large joint-stock company administration and governmental business are complex (ibid.: 306–308); the problem of value is complex (ibid.: 366); incidents of the tenure of land are complex (ibid.: 413); questions which relate to long periods are complex (ibid.: 456); cumulative effects are complex (ibid.: 559); the idea of "organic whole" is complex (ibid.: 582), relations of debtor and creditor are complex (ibid.: 650); relations between industrial efficiency and the hours of labor are complex (ibid.: 693); the causes that govern the limits of production are complex (ibid.: 849) and "the heterogeneous forces of supply" are also qualified by him as complex (ibid.: 852).

Marshall's acknowledgement of the complexity of economic systems is also illustrated by statements such as that "Nature's action is complex: and nothing is gained in the long run by pretending that it is simple, and trying to describe it in a series of elementary propositions" (ibid.: x); that "The science of man is complex and its laws are inexact" (ibid.: 32); that our economic problem "is too complex to be focused in a single view" (ibid.: 545) and that "the socio-economic organism is more delicate and complex than at first sight appears" (ibid.: 712). Moreover, Marshall develops in his analysis several elements that refer to concepts current in today's complexity theory. Based on his perception of "the variety of human nature," Marshall (ibid.: 14) argues that "economics cannot be compared with the exact physical sciences: for it deals with the ever changing and subtle forces of human nature"; more precisely, as he discusses in Chapter VIII, Book IV, these forces are not only "changing" but they are "evolving." Meaningful changes are the ones that are "appropriate" in the sense that they potentiate "progress" (self-organization). As observed by Viner ([1941] 1982:250), Marshall drew from biology "a live sense of the complexity and variability of the interrelations between economic phenomena." Drawing heavily on Darwinian biology, Marshall puts forth a stylized form of autopoietic[10] and morphogenetic[11] argument in order to justify the evolution of industrial organization:

> the development of the organism, whether social or physical, involves an increasing subdivision of functions between its separate parts on the one hand, and on the other a more intimate connection between them. Each part gets to be less and less-sufficient, to depend for its

> well-being more and more on other parts, so that any disorder in any part of a highly-developed organism will affect other parts also. This increased subdivision of functions, or "differentiation," as it is called, manifests itself with regard to industry in such forms as the division of labour, and the development of specialised skill, knowledge and machinery: while "integration," that is, a growing intimacy and firmness of the connections between the separate parts of the industrial organism, shows itself in such forms as the increase of security of commercial credit, and of the means and habits of communication by sea and road, by railway and telegraph, by post and printing-press.
>
> (Marshall [1920] 1961:241)

Individuals are seen as members of the social organism and it is within this context that their actions should be interpreted (ibid.: 25). He also discusses the possibility of non-linearities resulting from the presence of increasing returns which affect the use of the *ceteris paribus* clause. It seems that as long as there are no cumulative effects one can use shortperiod analysis to represent economic phenomena, but that when these effects are allowed to operate then the method needs to be changed. Marshall (ibid.: 379, no. If) points out that "violence is required for keeping broad forces in the pound of *Ceteris paribus* during, say, a whole generation, on the ground that they have only an indirect bearing on the question in hand. For even indirect influences may produce great effects in the course of a generation, if they happen to act cumulatively; and it is not safe to ignore them even provisionally in a practical problem without special study." When discussing the peculiarities in the action of demand and supply with regard to labour, he remarks (ibid.: 560) that cumulative effects, such as the indirect effects of custom, "exert a deep and controlling influence over the history of the world." It might be argued that his logical division between short- and long-period analyses respects the differences between linear and non-linear effects.

But Marshall's views on complexity go beyond what was described above as similarities with the SFA's main properties because they address the question of how economists' sense of reality is associated with the conceptualization of change in real social systems. Thus, Marshall's views constitute a very particular "whole," the examination of which must include three important interrelated features of his economic analysis: his appeal to common sense as a way of applying economic reasoning (on complex systems) to the real world; his notion of "normal" as an expression of tendencies in complex systems; and his answer to the context problem. These features follow a logical sequence which defines the framework for the analysis of complexity which Marshall bequeathed to later economists.

The Role of Common Sense

The substance of economics was for Marshall intrinsically related to human conduct in the ordinary affairs of life and to the practice of ordinary discourse. As Marshall put it in the *Principles* ([1920] 1961:1): "Economics is a study of mankind in the ordinary business of life." Implicit in this definition is the existence of two distinct realms: the real-world realm of the ordinary business of life and the theoretical realm representing the ordinary business of life. Thus, present in Marshall's discussions is the notion that economics is a representation, an expression of a "sense of reality" already available—at least for the economist. Marshall reiterates this view many times in the *Principles,* when for instance he claims in the first page of the preface to the first edition that "Economic laws and reasonings in fact are merely a part of the material which Conscience and Common sense have to turn to account in solving practical problems, and in laying down rules which may be a guide in life." But does Marshall's reference to conscience and common sense explain the formal ethical doctrine behind his economics? According to Coats (1990), it does not. He argues that Marshall did not contribute to the debate on the ethics-economics connection and that "He steadfastly refrained from engaging in philosophical controversy or committing himself to any formal ethical doctrine, preferring instead to rely on 'our ethical instincts and common sense'" (Coats 1990:155–156). He interprets Marshall's reference to common sense as a way to avoid explaining his ethical beliefs and to choose the alternative of adhering to the conventions and standards of his time. There is strong evidence that Marshall incorporated into his economics conventions and standards of his social and intellectual group. Yet should this be interpreted as meaning that Marshall was reluctant to explain his ethical beliefs and that he refrained to commit to any (formal) ethical doctrine? Perhaps not. It might be suggested that because so many of Marshall's commentators have been unable to identify in common sense an ethical doctrine, they have interpreted his references to common sense as reluctance to explicate his ethical beliefs. It seems that Marshall was well aware of the existence of the Scottish Common Sense Philosophy, through Sidgwick—who followed the basic tenets of Thomas Reid's philosophy—and Kant—who discussed the role of common sense argued by James Bertie. It is also likely that he knew of the writings of G.E.Moore—his contemporary at Cambridge and one of the most important common-sense philosophers of this century. Now, it is not being suggested here that Marshall engaged in philosophical discussions concerning common sense, but that probably he was aware of common sense as an ethical doctrine (and not appealing to it as a way to avoid referring to ethics) and that he used it, as argued below, in this sense.

According to Marshall, economics follows the practice of ordinary discourse because economists examine mental states and their manifestations like everybody else does "every day in ordinary life." As he puts it in his *Lectures to Women* ([1873] 1995:87–88): "There is nothing peculiar to science. All that the scientific man does is done by all in ordinary life; he has nothing more to teach in principle than what you may gain from observing the events of ordinary everyday life." The difference, as he discusses in his *Principles,* is that economists are more patient, thoughtful and prudent in their analyses than ordinary individuals are (Marshall [1920] 1961:16). As a result, "economics takes man just as he is in ordinary life" (ibid.: 20) and not as an abstract and fictitious economic man (ibid.: 27). The restrictions Marshall imposes on this very generous principle, which allows economists to use their own experiences and judgment as part of the scientific activities, is that economists must be concerned with those aspects of life that can be observed and can be verified by results. Also, it could be said that because economic systems are complex, economists might use their common sense as a way to approximate their theories to realistic economic processes and applying economics. Marshall argues that:

> The work to be done is so various that much of it must be left to be dealt with by *trained common sense,* which is the ultimate arbiter in every practical problem. Economic science is but the working of *common sense* aided by appliances of organized analysis and general reasoning, which facilitate the task of collecting, arranging, and drawing inferences from particular facts. Though its scope is always limited, though its work without the aid of *common sense* is vain, yet it enables *common sense* to go further in difficult problems than would otherwise be possible.
>
> (Marshall [1920] 1961:38, italics added)

It is worth noting here that for Marshall not all notions of common sense qualify as *trained common sense*. Rather, what he calls *untutored common sense* or public opinion ([1885] 1925:164) is most likely to produce the wrong consideration. Untutored common sense is for him that body of knowledge based on surface phenomena, unconcerned with the complexity of the manifold mutual determination of actions, that denies a careful and thoughtful investigation of experience. Yet a most interesting point in Marshall's references to common sense is that he saw untutored common sense as a knowledge in progress, evolving from errors and containing the right "seeds" for future extension of the boundaries of knowledge. Commenting on the growing importance of public opinion as an economic force and the need to educate public opinion for its new responsibilities, Marshall argues that:

> thus public opinion has a very responsible task. I have spoken of it as the opinion of the average man; that is, of an average member of one of those classes of society that is not directly and immediately concerned in the question at issue. But he is very busy, and has many things to think about. He makes great mistakes; but he learns by all of them. He has often astonished the learned by the amount of ignorance and false reasoning which he can crowd into the discussion of a difficult question; and still more by the way in which he is found at last to have been very much in the right on the main issue.
>
> (Marshall [1890] 1925:287)

Therefore, what Marshall called trained common sense might be seen as a middle ground between untutored common sense and reasoning on strict logical lines ([1897] 1925:297); a knowledge that becomes more complex as it evolves. As argued here, common sense is important for Marshall not only because it enables economists to handle complexity but also because it incorporates the faculties of reason and judgment that are needed for organizing facts, developing theories and applying them to practical issues. He reminds us in the *Principles* ([1920] 1961:39) that "facts by themselves teach nothing," they need interpretation (theory) in order to be made intelligible as theory needs them in order to have any practical use. Without theory it is difficult to know what kind of effects to expect from a study of facts. Criticizing the historic school of economists, Marshall remarks that:

> facts by themselves are silent. Observation discovers nothing directly of the actions of causes, but only of sequences of time. It may find that an event followed on, or that it coincided with, a certain group of events. But this gives no guidance except for other cases in which exactly the same set of facts occurs over again, grouped in just the same way. And such repetitions never occur in the life of man; nor indeed anywhere save in physical laboratories: history does not repeat itself. In economic or other social problems no event has ever been an exact precedent for another… Experience in controversies…brings out the impossibility of learning anything from facts till they are examined and interpreted by reason; and teaches that the most reckless and treacherous of all theorists is he who professes to let facts and figures speak for themselves.
>
> (Marshall [1885] 1925:166–168)

To elaborate, for Marshall, any inference based on facts, performed either by custom or by formal reasoning, involved a two-stage process:

the first stage consisted in passing from particular facts to general propositions; the second, in passing from generals to other particulars. By doing so, the main purpose of the inference was to provide a grasp of the situation to be assessed; a sort of intuitive guide rather than a precise documentation of the future.[12] It might therefore be difficult to characterize Marshall's view of science within the contemporary scientific paradigms. It might be the case, perhaps, that Marshall followed "an essentially positivist view of science," as argued by Pratten (1998), but there are problems in explaining Marshall's concern with recording constant event regularities and with adhering to an account of being constituted uniquely by the category of experience (see Pratten 1998:124–125), if Marshall's remarks on factual inference and common sense are considered. Indeed, as Coats (1967:711) observes: "Marshallian economics possessed important semi-intuitive, extra-logical attributes" that were manifested, for instance, in the demand for the analyst's skill in distinguishing relevant from irrelevant variables in a given problem—attributes which do not fit well in a constant conjunction view of science. Thus, this characteristic of Marshallian analysis complicates a positivist labeling of Marshall.

Returning to the main line of argument, it might be said that, according to Marshall, economic theory needs common sense, which in turn needs economic theory in order to "go further in difficult problems." The final outcome of the interaction between economics and common sense is, then, what he called trained common sense—the important tool economists need in order to handle complexity. This is reminiscent of a remark by Thomas Reid—known as "the father of Common Sense Philosophy"—that in the tree of knowledge (Reid [1785] 1983:186) "common understanding is the trunk, and the sciences are the branches."

One might object to common sense on the basis that it is restricted to phenomena at the level of appearances; but Marshall when discussing in the *Principles* the effects of a "standard rule" of keeping wages artificially high in any trade ([1920] 1961:44–45), compares an approach which privileges "surface results" with another which stresses "the scientific use of imagination" (an alternative designation for trained common sense), showing how he believed that common-sense reasoning is able to go beyond the simple appearances of the problem. Similarly, when arguing that wages might influence the hours of labour (ibid.: 696), he observes that "immediate effects are no guide as to ultimate"—which adds a temporal dimension to the ability of common sense to avoid being deceived by surface phenomena. This issue was emphasized in the "Preface to the Eighth edition" of the *Principles* in which Marshall points out (ibid.: xvi): "the well-known fact that those effects of an economic cause, which are not easily traced, are frequently more important than, and in the opposite direction to, those which lie on the surface and attract the eye of the casual observer." It is then clear that for Marshall common sense is not a "straitjacket" which prevents economists from having access to counter-intuitive and hidden aspects of reality. On the contrary, because common sense depends on the complex features of reality it is a major tool in the acquisition of counter-intuitive knowledge. But how does common sense generate this knowledge? According to Marshall, an important role of common sense is dealing with a complex problem by breaking it up into its (simple) several parts and, later, gathering them back together. It is only by focusing on the component parts of the problem that the human mind is able to acquire counter-intuitive knowledge (see Marshall [1885] 1925:164). Here, the key to complexity seems to be the flexibility with which a particular problem might be broken up. Then, by gathering all parts relevant to a particular situation, common sense allows a great variability in the explanation of the economic phenomena. This role of common sense was translated by Marshall into the principle of *the one in the many, the many in the one*. In his *Industry and Trade* (1923:5–6), Marshall explains that, on the one hand, what might be seen as a unique phenomenon is, in reality, a combination of many influences—the one in the many—and on the other hand, what is realized as the product of a variety of factors is indeed based on very few principles—the many in the one. This last half of the principle also assumes that there is a concept of normality represented by the confluence of different tendencies (discussed below). The principle that Coats (1967:711) qualified as "surely vague enough, even semi-mystical" and Whitaker (1975:110) said to sound "strange and half mystical," was best explained by Scott:

> In the *Philebus* Plato makes Socrates say that a true understanding of the One and the Many was a gift of heaven to men, and Marshall's elucidation of his second principle of "the Many in the One and the One in the Many" shows him as the newest Prometheus who thereby threw a blaze of light upon economic relations. Starting from the conception that the work of the economist is "the disentangling the interwoven effects of complex causes," he develops a species of dialectical movement, by which, on the one side, through searching analysis he shows that what seems simple, uniform, or one—sometimes in experience, sometimes in previous economic theory—is not so in reality, but must be resolved into its constituent elements. From this tendency it follows that an apparently simple statement in Economics is rarely true. Then there is the reverse tendency, namely, that, as the multiplicity of established fact is examined and the ideas latent in it are drawn out, the principles which explain it are found to be fewer than was supposed and several of these may be resolved into one still more comprehensive. It was always Marshall's double aim to get greater reality by pushing his analysis through many fields of inquiry

> in order to exhaust every thing relevant: while, no less, by tracing the causes of causes he tracked them to their point of interaction.
>
> (Scott 1924–5:449–450, also quoted by Whitaker 1975: 108–109)

It is clear from Marshall's comments on economic methodology that he believed that economists should contextualize economic problems within the wider and complex frame of everyday life. Another important aspect of this contextualization is the method used to communicate and apply the results of economic theory. Marshall argues in the *Principles* ([1920] 1961:50) how useful it is to keep the use of terms as much as possible in accordance with ordinary usage and with traditions of the past. He advocates—and follows—a writing style reminiscent of Smith's *didactic style*. As Keynes ([1924] 1925:46) puts it, the *Principles* "is elaborately unsensational and under-emphatic. Its rhetoric is of the simplest, most unadorned order. It flows in a steady, lucid stream, with few passages which stop or perplex the intelligent reader, even though he know but little economics." Marshall clearly appeals to the use of ordinary language as a guiding principle in the choice of terms in economics. Interpreting the complexity of ordinary language on lines very similar to the later Ludwig Wittgenstein's language-games,[13] Marshall points out that in economics:

> Its reasonings must be expressed in language that is intelligible to the general public; it must therefore endeavour to conform itself to the familiar terms of everyday life, and so far as possible must use them as they are commonly used. In common use almost every word has many shades of meaning and therefore needs to be interpreted by context.
>
> (Marshall [1920] 1961:51)

The idea behind Marshall's support for the use of familiar terms of everyday life in economics is that a knowledge that may not be understood is not of much use or, perhaps more importantly, a knowledge that is not flexible enough is not a knowledge that can be applied to a variety of particular situations.[14] Because, according to Marshall, no practical problems can be settled by appeal to pure and general theories, there is a need for translating pure into applied theory, or in other words, a need for contextualizing economics within different particular social settings. Having this relation between familiar terms ("the didactic style") and the use of economic theory in perspective, it might thus be explained why Marshall ([1897] 1925:297) argued that "the growing perfection of scientific machinery in economics, so far from lessening the responsibilities of common sense, increases those responsibilities." The argument is that an increase in the technicalities and authority that accompanies the growth of scientific knowledge puts increasing pressure upon the economist's abilities to translate those pure aspects into practical guidance to particular situations.

To sum up, it must be noted that Marshall not only seemed to be aware of common sense as an ethical doctrine but he also claimed it as the body of knowledge to which economists must turn when addressing practical problems. While he differentiated between untutored and trained common sense, he emphasized that common sense is an evolving knowledge. Main roles (that might be mentioned) include: to handle complexity; to provide counter-intuitive knowledge; and, most importantly, to contextualize and apply pure economic theory to everyday life economic affairs. In order to handle complexity, Marshall argues in the *Principles* that common sense appeals to vagueness ([1920] 1961:394, 638),[15] to context (discussed below) and to practical conveniences (ibid.: 39, 53 no.1). A tacit assumption behind this reasoning is that the degree of complexity involved in ordinary affairs of life is higher than the degree of complexity of theoretical problems (ibid.: 628, 722). Coherent with his view is that diverse activities generate different intuitions and distinct levels of apprehension of the complexities of the real world.[16]

As mentioned above, another tacit assumption behind Marshall's use of the doctrine of common sense is that there are notions of normality to be discovered by the investigation of facts. As he has proposed ([1893] 1961, 2:501–502), "Science must study facts, ascertain which of them are representative and normal, and then analyse, and reason about normal conditions, at first within a narrow range; and afterwards, as knowledge increases, giving a wider range to these normal conditions, and thus becoming at once more complex and nearer the actual facts of life." It thus seems that there are two senses in which the notion of normality is associated with common sense and complexity. An investigation of the concept of "normal," for Marshall, might thus provide further information on the role of common sense in complex systems.

The Concept of "Normal"

The idea that there are concepts such as normal values, normal profits, normal wages, normal efficiency, long-period normal values, is central to Marshall's theories of value and distribution (see Harcourt [1981] 1992:253). Whereas the presence of these "normal" values in Marshall's theory has been justified as a result of the influence of the classic economists on his work (O'Brien 1990), not much attention has been given to the relation between normal values and ordinary people's notion of the "normal"—interpreted as a precondition to conduct in the ordinary affairs of life.

There is a close link between Marshall's theory of normal prices and his theory of long-period equilibrium prices. Guillebaud tells us, as reported by Robertson (1956:16), that Marshall had in mind two different concepts of "the long period," "one in which it stands realistically for any period in which there is time for *substantial* alterations to be made in the size of the plant, and one in which it stands conceptually for the Never-never land of unrealised tendency." Because his theory of long-period normal prices was a result of the combination of a succession of time periods—which could be short or long, actual or potential—defined according to the particular purpose in hand, it is not that clear how Marshall coordinated these two different concepts. As observed by Harcourt:

> [Marshall] seemed to vacillate between whether they [time periods] are actual or potential, sometimes naming periods of calendar time as illustrative of what he had in mind, wishing, I suspect, to have it both ways. That is to say, he wished his long-period normal prices, etc., to be real centres of gravitation—to help make sense of actual observations in actual time.
>
> (Harcourt [1981] 1992:256)

The problem is that Marshall, once committed to the stationary state analysis, expressed the long-period normal equilibrium as averages of observations which were not easily translated into ordinary discourse. Parsons ([1932] 1982:239 no.1) criticizes Marshall for having used the phrase "stationary state" in different contexts.

As noted above, Marshall's definition of the term "normal" and "normal action" has two sides. The first side is associated with the concepts of tendency and context. Because the normal action is that course of action (Marshall [1920] 1961:34) "which may be expected *under certain conditions*" it is related to a statement of tendencies that are inexact and faulty. Normal is what is to be expected given the complexity of a certain situation. It is the result of a probable judgment based on the evidence provided by the context in which the situation or action takes place. As Marshall puts it:

> Illness is an abnormal condition of man: but a long life passed without any illness is abnormal. During the melting of the snows, the Rhine rises above its normal level: but in a cold dry spring when it is less than usual above the normal level, it may be said to be abnormally low (for that time of year). In all these cases normal results are those which may be expected as the outcome of those tendencies which the context suggests; or, in other words, which are in accordance with those "statements of tendency," those Laws or Norms, which are appropriate to the context.
>
> (Ibid.: 34)

Marshall illustrates what he means by "normal" with many examples from ordinary life and shows how different contexts may give rise to different notions of what is normal or not. The point is that there is no absolute a-historical standard of normality in this use of the term "normal." It is a relative term (relative to the context in which it is used) which merely expresses the balance of tendencies that might be expected for a given context. As argued by Whitaker ([1977] 1982:479): "Normal action is always to be viewed as the consequence of all motives, not the economic one alone." From this perspective, normal is related to what Marshall called *economics proper.*

The second side of Marshall's notion of "normal" refers to it as an analytical concept. According to this notion, all particular features are ignored in order to emphasize the broad (and analytical) aspects that are shared by most elements of the problem. This does not mean that the particular aspects are eliminated from the problem, only that the notion of "normal" does not detect or address their presence (more precisely, the particular features are abstracted from, not isolated; ibid.: 619, 844). It is not a coincidence that Marshall uses this notion of "normal" when discussing the equilibrium of demand and supply (ibid.: 341). Yet, in this more restricted sense, the notion of "normal" needs ultimately to be complemented by those particular features that are non-stationary (ibid.: 347). He is well aware that situations of "equilibrium" are unable to represent "normal" situations in the first sense of the term.[17] This is an analytical device—one with many limitations—used to examine the complexity of the real world.

When discussing the notion that a market equilibrium might be stable, Marshall employs a physical metaphor in the form of oscillation of a pendulum (ibid.: 345). Using this abstraction—which could be said to represent the normal conditions of the market in the second sense of the term—he introduces the explanation of market deviations and price behavior. But after that, when he reintroduces the particular features that are needed in order to characterize the complexities of the real world, he is back to the concept of "normal" as an expression of tendencies for a given context. He argues that equilibrium could be seen as a pendulum:

> But in real life such oscillations are seldom as rhythmical as those of a stone hanging freely from a string; the comparison would be more exact if the string were supposed to hang in the troubled waters of a mill-race, whose stream was at one time allowed to flow freely, and at another partially cut off. Nor are these complexities sufficient to illustrate all the disturbances with which the economist and the merchant alike are forced to concern themselves.
>
> (Ibid.: 346)

These two sides of Marshall's notion of "normal" might be called, in the absence of better terminology, "c-normal" to designate the first sense, related to the expression of tendencies within complex systems, and "anormal" to indicate the second sense, associated with the analytical demands of theoretical systems. This classification corresponds to Harcourt's (1991) distinction between "period" and "run." Whereas "period" represented for Marshall an analytical concept, "run" corresponded to actual (historical) calendar time. The important point is that ultimately a-normal concepts need to be interpreted in terms of c-normal concepts, which are dependent on the meaning of context.

Time is one of the most important elements in Marshall's determination of context. The idea is that different periods of time allow the manifestation of different sets of causes, defining different concepts of normality, which characterize different contexts. Thus, by dividing the periods of time between short and long Marshall is expressing a context-clause:

> In this case, as in others, the economist merely brings to light difficulties that are latent in the common discourse of life, so that by being frankly faced they may be thoroughly overcome. For in ordinary life it is customary to use the word Normal in different senses, with reference to different periods of time; and *to leave the context to explain the transition from one to another.* The economist follows this practice of every-day life: but, by taking pains to indicate the transition, he sometimes seems to have created a complication which in fact he has only revealed.
>
> (Marshall [1920] 1961:363, italics added)

Marshall combined these two periods of time in his "period analysis," emphasizing the adjustments (of supply, knowledge) involved in the transition. While the idea of a classificatory device is a relatively simple one, Marshall was, according to Opie ([1931] 1982:164), "continually led into confusion, by making indefinite allusions to the continuity of clock-time, and was even driven to assert that the occasions when it is necessary to separate long and short periods sharply are 'neither frequent nor important'." Similarly, different sets of sales express different conditions of normality. This aspect has been hidden in Marshall's analysis because in his use of "stationary state" the term "normal" has a fixed meaning: normal equal to average (Marshall [1920] 1961:372). But because the stationary state is not the ultimate stage in economics for Marshall (ibid.: 497), "the language both of professed writers on economics and of men of business shows much elasticity in the use of the term Normal when applied to the causes that determine value." It is the elasticity and flexibility presented by the notion of c-normal that turns this notion into an expression of tendencies for a given context in complex systems. It is then of fundamental relevance to discuss the determinants of context in Marshall's systems.

The Context Problem

Defining context is the crucial epistemological device used by individuals in their ordinary affairs to cope with the complexity of facts of life. Context might differ according to variations in the area of space, and/or in the period of time, and/or in the particularities resulting from different circumstances. In ordinary life contexts are manifested as a diversified *continuum* of situations that given their complex nature are difficult to isolate with precision. As discussed above, boundaries change according to the evolution of particular histories and behaviors. Marshall's point is that if economists want to see their science producing results useful for (applicable to) complex economic systems, they must follow the practice of context definition/transition—as ordinary people do in their everyday affairs in life. Now, it is on this basis that Marshall defines the use of *ceteris paribus* in economics, because for him:

> This scientific device is a great deal older than science: it is the method by which, consciously or unconsciously, sensible men have dealt from time immemorial with every difficult problem of ordinary life.
>
> (Marshall [1920] 1961:xiv)

Ceteris paribus is interpreted and used by Marshall as an ordinary language device, an epistemological mechanism of apprehension of a particular reality. It says nothing about how the world really is—indeed Marshall emphasizes that a system cannot be studied in isolation from the rest—but rather, it deals with how individuals understand the world. It follows that the use of *ceteris paribus* clauses is coherent with the use of c-normal concepts and with a realist principle about how individuals use common sense to cope with real-life complexities.

As mentioned above, context defines an epistemological domain which explains what tendencies might be expected from a given situation. Often, in ordinary conversation conditioning clauses are omitted and then the common sense of individuals provides the background needed to convey the intended meaning during the conversation. But the same cannot be said of "scientific treatises". Marshall argues (ibid.: 76) that "ordinary conversation may pass from one point of view to another without any formal note of the change: for if a misunderstanding arises it soon becomes manifest; and confusion is cut short by a question or by a volunteered explanation. But the economist may take no risks of that sort: he must make prominent any change in his point of view or in his uses of terms." Marshall does not delve into the reasons for this need for clarity other than mentioning that it is a better way to scientific progress in the long run. However, he warns of the dangers of formalism in economics: a false feeling of security and the possibility of misrepresentation of meaning. "Context is a sort of unexpressed 'interpretation clause'," said Bagehot (quoted by Marshall ibid.: 52 no.1). But because context changes, interpretation clauses change and as a result theoretical terms should not be defined in a rigid way.[18] Vagueness is an intrinsic feature of context definition in complex systems. As Marshall points out (ibid.: 787): "Things relating to man's actions never can be classified with precision on any scientific principle."

This characterization of context is manifested in Marshall's constant preoccupation with situating his theoretical claims within the England of his time (see, for example, his characterization of England's economic conditions: ibid.: 633 n.1). The features of ordinary life may vary much from place to place and from time to time. Therefore, theories that might be applied to these particular realities must incorporate conditions that respect the peculiarities of different circumstances. This, according to Marshall, the Physiocrats (ibid.: 505), Adam Smith (177, 507) and David Ricardo (156, 508) understood very well. And Marshall tried to follow in their footsteps addressing the context which he called the "England *at the present time*" (his emphasis) (ibid.: 321), "modern England" (556), "England's present economic condition" (671) or "the new economic age" (675). It follows that, because different contexts give rise to different theories, history and geography must be respected within economics.

What Marshall understood by "respected" is something that warrants further consideration. In the structure of Marshall's method, context was not defined at an analytical level. There is no scope for discussing the influence of contextual diversity within the stationary state technique. However, context was defined at a practical level and was seen by Marshall as a "correction device" to be used "in order to bring these [theoretical] results into harmony with the actual conditions of life and work" (ibid.: 505). That meant, for Marshall, that the practical importance of different real-life elements determines the "context condition" to be set in the application of different theories. It is important to note that context—related to practical aims—implied for Marshall (i) an isolation of a particular situation and (ii) a conjunction of all the different aspects related to that situation. He argues (ibid.: 39) that "the direct pursuit of practical aims leads us to group together bits of all sorts of knowledge, which have no connection with one another except for the immediate purposes of the moment." Context definition is then explained by Marshall as part of the individual's practical reason: it is a transitory characteristic of action.

The issue here is how the practical nature of context definition influences the analytical side of economics. Marshall's initial answer is that science cannot be planned with reference to its possible uses because this would imply a waste of mental energy that would hinder progress in science. Conversely, there must be no "margin of debatable ground" (ibid.: 53) in the analytical side of economics. Thus, while Marshall maintains (ibid.: 40) that "the practical uses of economic studies should never be out of the mind of the economist," he argues that those economic studies must not be "planned" with reference to their practical uses. But this should not be understood as a denial of the influence of practice on theory. Rather, for Marshall, theory and practice must co-evolve in a symbiotic and not in a parasitic way. Exact scientific reasoning needs practice and vice versa. As he argues:

> And though it be true that economic causes are intermingled with others in so many different ways, that exact scientific reasoning will seldom bring us very far on the way to the conclusion for which we are seeking, yet it would be foolish to refuse to avail ourselves of its aid, so far as it will reach:—just as foolish as would be the opposite extreme of supposing that science alone can do all the work, and that nothing will remain to be done by practical instinct and trained common sense.
>
> (Ibid.: 779)

It is part of the tradition started by Marshall that these two different logical domains (the theoretical and the practical) must co-exist and co-evolve in a symbiotic way. For him, there is no trade-off between these two domains. In theory, economists are expected to break down complex questions and to achieve more precise results. In practice, economists are expected to combine isolated and narrow issues and to achieve more general results. When combined, both activities are expected to produce more reliable results because, while theory helps the elimination of mistakes, practice makes results less abstract and therefore more applicable (ibid.: 366). Therefore, simplicity in theory co-exists with complexity in practice and any attempt at inverting these relations will cause confusion and misunderstanding (ibid.: 459 n.1). In the real world simple analytic statements may be of no use and misleading.[19] A good example of what he means by this statement is provided by Marshall's discussion about the problem of distribution:

> It has now become certain that the problem of distribution is much more difficult than it was thought to be by earlier economists, and that no solution of it which claims to be simple can be true. Most of the old attempts to give an easy answer to it, were really answers to imaginary questions that might have arisen in other worlds than ours, in which the conditions of life were very simple. The work done in

> answering these questions was not wasted. For a very difficult problem can best be solved by being broken up into pieces: and each of these simple questions contained a part of the great and difficult problem which we have to solve.
>
> (Ibid.: 510)

Marshall argued that as long as the picture of the imaginary world is complemented by the picture of the real world, it is not only useful but ultimately necessary to solve—back to the real world—very complex problems. Simplicity is needed (i) to give definiteness to our ideas and (ii) to make the problem manageable. For this reason simplicity (and the statical theory of equilibrium) must be seen as an introduction to the discussion of complex economic problems (ibid.: 460–461). To a certain extent Marshall allowed the common-sense and practical notions of real life to influence his theoretical framework. Keeping theory at the service of practice provided Marshall with a reason for a sense of proportion in his arguments (ibid.: 770) and introduction of time in economic analysis.[20] It also provided a justification for avoiding "long chains of deductive reasoning" in science because (ibid.: 771) "they are seldom a sufficient guide for dealing with the heterogeneous materials and the complex and uncertain combination of the forces of the real world." Moreover, Marshall argues that:

> The function then of analysis and deduction in economics is not to forge a few long chains of reasoning, but to forge rightly many short chains and single connecting links. This however is no trivial task. If the economist reasons rapidly and with a light heart, he is apt to make bad connections at every turn of his work. He needs to make careful use of analysis and deduction, because only by their aid can he select the right facts, group them rightly, and make them serviceable for suggestions in thought and guidance in practice; and because, as surely as every deduction must rest on the basis of inductions, so surely does every inductive process involve and include analysis and deduction.
>
> (Ibid.: 773)

It is interesting to note that the complementarity between theory and practice and deduction and induction advocated by Marshall is also conceived by him as a characteristic of ordinary life in which "every thoughtful and observant man is always obtaining, from conversation and current literature, a knowledge of the economic facts of his own time" (ibid.: 778). Common sense is the product of experience and reasoning, facts and thought, because only by combining all information available may individuals cope with the complexity of the real world. As far as economics is concerned, Marshall comments, for instance, how it is "increasingly difficult to bring the test of experience" (ibid.: 702) to the issue of trade unions. His conclusion is that our understanding of experience is theory-laden and the significance of our (economic) theories is fact-laden. They might be considered as separate aspects of knowledge but they are in fact parts of the common sense of ordinary life—the epistemological background individuals have for coping with complexity. Economists must then be careful when interpreting empirical information because "the test of experience" does not produce clear-cut results. This point is in evidence when Marshall points out that:

> Adam Smith saw clearly that while economic science must be based on a study of facts, the facts are *so complex,* that they generally can teach nothing directly; they must be interpreted by careful reasoning and analysis. And as Hume said, the *Wealth of Nations* "is so much illustrated with curious facts that it must take the public attention." This is exactly what Adam Smith did: he did not very often prove a conclusion by detailed induction. The data of his proofs were chiefly facts that were *within everyone's knowledge,* facts physical, mental and moral. But he illustrated his proofs by curious and instructive facts; he thus gave them life and force, and made his readers feel that they were dealing with problems of the real world, and not with abstraction; and his book, though not well arranged, is a model of method.
>
> (Ibid.: 759 n.2, italics added)

Thus, an economics of complex systems cannot rely on simple and direct examination of factual evidence. It has to be based on more general and widely accepted knowledge. To the extent that this knowledge corresponds to common sense it qualifies as a flexible and useful knowledge, already in use by individuals dealing with the complexities of ordinary affairs of life.

Summarizing the previous arguments, the two questions to be answered are: First, how does the above discussion illuminate the issue of context definition/transition? Second, how can Marshall's legacy to the issue of complexity be summarized?

Marshall maintained that the notion of context definition is a practical issue. It is related to the practical circumstances (all different aspects) of a particular problem. Because theory and practice are closely related for Marshall, context should influence (at least indirectly) the analytical side of economics through, for example, short chains of reasoning, flexible and vague concepts and prudence in the use of mathematics. The notion of context transition is basically an epistemological problem to which the answer for Marshall is common sense. In other words, it is through (trained) common sense that individuals answer the frame problem and exercise their reasoning and judgment powers. But again this is a practical issue that only indirectly influences the "science of economics." Because economic systems are complex, economists are subject to epistemological constraints similar to those faced by individuals in their ordinary affairs. Economics must take this issue into account.

Marshall's emphasis on the use of biological metaphors is a topic that deserves more attention, in particular because he did not seem to have followed his own methodological advice when the context seemed to call for the use of biological metaphors but he chose physical analogies to describe economic phenomena. Apart from this, Marshall's legacy on the issue of complexity may be summarized in five interrelated points:

1 Complexity is an important feature of economic systems. Economic forces evolve, are cumulative, present non-linearities and are time irreversible.
2 Complexity is a problem closely associated with the practical usefulness of economic reasoning; it belongs to the realm of *economics proper.*
3 Complexity can be handled by *trained common sense.*
4 Complexity and common sense can be concretely related through, for example:
   (a) contextualization of economic problems within the wider frame of everyday life;
   (b) respect for historical and geographical differences in the use of economics;
   (c) careful examination of the role of empirical evidence in testing theories.
5 Complexity cannot be understood through long chains of deductive reasoning or purely formal analyses of economic processes.

Although tailored to Marshall's particular methodological needs, this list also provides a basic framework for subsequent contributions related to Marshall's work. It identifies the problem of complexity, relates it to the practical aspects of economic reasoning, and focuses on the role of common sense in the handling of complex systems.

Concluding remarks

The principal argument of this chapter is that the main danger in the adoption of the SFA is the replacement of a biological metaphor by a physical one without an ethical principle regulating it or a study of the conditions that justify the adequacy of one metaphor or another. The first part of the chapter outlined the main characteristics of the SFA and related them to the representation of the notion of context. The aim was to highlight the importance of the role of judgment in the selection of metaphors; that is, that ultimately the relevance of the SFA depends on its correspondence with economists' sense of reality about economic processes. If this—so claimed—synergism between theory and reality is not actually undertaken, the SFA could become merely another excuse for doing sophisticated mathematics. In general lines, economists' sense of reality depends (among other things) on how they conceptualize change in real social systems. It was then noted that economists, like everybody else, conceptualize change through contextualization of meaning. The second part of the chapter, concerned with Marshall's contribution, attempted to provide answers to the questions related to the role of judgment in combining theory and practice, and the problems of context definition/transition. It started by arguing that complexity was an important issue for Marshall and that in order to solve the problems associated with it he appealed to an elaborated notion of common sense: what he called trained common sense.

The dual nature of Marshall's economics, with its distinction between its theoretical and applied aspects, puts in evidence the need for *the human element of the problem*—which in his case takes the form of common sense—in contextualizing theory within concrete economic situations. It suggests, then, that we turn to the SFA in search of its "human element," or its ethical doctrine, which might help promote the synergetic effort that is so heralded by its advocates. By doing so, we would be using Marshall's insights to contextualize the SFA.

To re-emphasize, Marshall's most evident contribution to the issue of complexity consists in his commitment to an ethical doctrine as a way of making a bridge between the pure and applied aspects of economics. More importantly, perhaps, was his focus on context definition as a practical issue and its implications for theorizing. Also, his emphasis on the *use* of theory, so as to provide guide for action, focuses on complexity as a characteristic of systems as they are perceived by individuals. This epistemological emphasis on the complexity problem provides scope for understanding how individuals realize change in their ordinary affairs of life. As a result of his preoccupation with the practical dimension of economics, Marshall organizes scientific devices, such as *ceteris paribus,* or advocates principles, such as *the one in the many and the many in the one,* on the basis of common-sense procedures adopted by individuals to cope with complexity in the real world. By contrast, the SFA puts forward a concept of complexity in which characteristics are defined as properties of ANN and only indirectly related to the real world. SFA's theories have been much influenced by the new mathematical tools available, such as non-linear dynamics, and there has not been much discussion on whether theories on complex systems have developed as a result of mathematical innovations or of a legitimate synergetic effort to provide more realistic accounts of economic processes. Not surprisingly, the context problem has yet to receive the attention it deserves in this literature. Should the SFA become a new *Zeitgeist* for the doing of economics, it must address some of the issues raised by Marshall; in particular, the issue of how ANN relate to ethical principles in the choice of metaphors.

Notes

1 I am very grateful to Geoff Harcourt, Angels Varea and participants of the 98 HES Meeting at Montreal for their valuable comments. I would also like to acknowledge the financial support from Commissao de Aperfeicoamento de Pessoal de Nivel Superior (CAPES). This paper is part of my PhD dissertation, submitted to the University of Cambridge in 1999, entitled "Common Sense Economics: Essays on the Role of Common Sense in the History of Economic Thought"; a different version is forthcoming in F.Duchin, G.Erber, M.Landesmann, S.Nakamuro, A Vercelli and R Scazzieri (eds), *Structural Change and Economic Dynamics,* Amsterdam: Elsevier Science.

2 Broadly speaking, the idea of non-linearity means that we cannot deduce the aggregate behaviour by a mere summing of individual behaviors. Because agents are expected to interact, interaction is expected to influence the aggregate behavior. The notion of path-dependence means that previous results influence the subsequent trajectory of the system, and lock-in means that once individuals have adopted a certain strategy there are obstacles to leaving it. Finally, self-organization means that systems present morphogenic properties, out of which order is produced from disorder. For more on these concepts, see Anderson *et al.* (1988).

3 It is interesting to note that Arthur, in his misunderstanding of Adam Smith's method and the "notion" of the Enlightenment, does not recognize there the precedent for the new *Zeitgeist* he describes.

4 Louçã argues for Hesse's criterion of validation of analogies for a realist model. It all depends upon the extension of the positive analogy compared to the negative analogy (the properties of the secondary subject not existent in the primary one). As defined by Louçã (1997:66) the *error of oblique transfer* refers to "the illegitimate transposition of propositions defined in the space of the horizontal relations [formal or material analogies] to the space of the vertical relations [logical causal relations], causing an excessive inference which ignores the negative analogy."

5 A full statement of Louçã's argument is found in the Introduction of his book (1997:4): "Darwinian evolution represents essentially an allegory for economics: it provides a new vision, escaping from the mechanistic prison, but the attempts to generate precise biological analogies orienting the research in economics are doomed to fail. No economic analogue exists for the replication unit in biology and the discrimination between genotype and phenotype is not relevant in society, neither is social evolution identifiable by natural selection processes. Indeed, an excessive expectation attributed to the metaphor the power of selection of specific hypotheses and of defining models for analysis, but the results were scarce. Yet, the evolutionary metaphor is not to be dropped. The argument of this book is that it is even more useful than ever, if its merits are considered."

6 Hayes (1985:22) observes that: "Many mathematical intuitions at the basis of geometry and real analysis (from which topology is an abstraction) seem to be at

odds with the way we think about everyday space." For instance, he argues that we do not think in terms of infinite variables.

7 Coats (1990:157–158) discusses four main reasons why, according to him, Marshall thought that economics could not be completely separated from ethics. First, and following Keynes, he argues that because Marshall was "too anxious to do good" he undervalued the theoretical aspects of economics in favour of the more practical ones, meaning that economics was relevant for Marshall in virtue of its practical usefulness. Second, he points out, again based on Keynes' remarks, that Marshall viewed his career as "a species of secular priesthood," "a sort of religious work for the sake of the human race," meant to relieve poverty and to raise quality of life. Thus, economics for him might have been a means of somewhat compensating for the abandonment of his earlier religious beliefs. Third, Coats argues that because Marshall knew that ethical forces influence behavior, he tried to take them into account in the economic analysis of tendencies. Finally, there was a political motive behind Marshall's ethics-economics connection. He claimed an ethical dimension to his economics in order to align himself with the new reformist economic movement.

8 As Marshall ([1920] 1961, 2:48 from the fifth edition 1907) argues, referring to the statical and dynamic methods, "both methods of study are needed for the complete work of stripping away the accidental and temporary, and for concentrating attention on the essential and permanent in the action of those forces, which are fashioning progress now, and may be expected to fashion it in the future."

9 The literature which interprets Marshall's abandonment of the second volume of the *Principles* as an unfulfilled promise—of analyzing change from a biological perspective—seems to over-emphasize the importance of biological metaphors for the achievement of realism in economics. It seems to ignore the idea, mentioned before, that for Marshall *economics proper* was a combination of theoretical and practical elements and that therefore a failure to develop better metaphors did not necessarily compromise Marshall's attempts to provide a more realistic economics. Although there is no scope here to fully discuss the issue put forward by Thomas (1991) and Hodgson (1993) concerning Marshall's unfulfilled promise, some comments on their arguments must be made. First, when Marshall (1898) suggested that biological analogies might be more useful in an advanced stage of economic reasoning, there is no indication that this work would have been carried out by him nor evidence that he thought that this advanced stage would be achieved in his life-span. Second, Hodgson's (1993:407) claim that "The *Principles* deals primarily with economic statics," is contradicted by Marshall himself in the Preface to the fifth edition in 1907 and his 1898 paper in the *Economic Journal* where he argues that his book "is concerned throughout with the forces that cause movement: and its key-note is that of dynamics" ([1920] 1961:49) and that the chapters on the Growth of Free Industry and of Economic Science, the analysis of organic growth in Book IV, time in Book V and the influence of progress on value on Book IV must all be seen as a result of biological conceptions (Marshall 1898). Finally, there is evidence that Marshall's idea of writing a second volume of the *Principles* was not inspired as much by a wish to improve methods as it was motivated by his wish to improve his accounts of facts. This interpretation is consistent with Whitaker's account of "Marshall's omnivorous eye for detail," according to which: "Marshall had deferred the full publication of his theories for many years, because he feared that if separated from all concrete study of actual conditions, they might seem to claim a more direct bearing on real problems than they in

fact had" (Whitaker 1975:107). Be that as it may, it seems that the role of biological metaphors was over-emphasized by Marshall's critics, in particular, taking into account their tendency to interpret interchangeably the terms biological and dynamics.

10 Autopoietic systems are those in which each of their elements can emerge, persist, and reproduce only within a complex of relations and networks. For a discussion on the social nature of autopoietic systems, see Zeleny (1996).

11 Morphogenetic processes concern the development of form and structure in an organism which happens through the emergence of formation and differentiation of basic elements.

12 Whitaker (1975:107–108) observes: "What Marshall seems to have been aiming at was something different from a straightforward inductive enquiry" and that facts were not used by him as an integral part of the enquiry but that—following Smith—Marshall used facts "that were within everyone's knowledge" to convey a message that his theories were dealing with problems of the real world. In support of the notion that Marshall was not interested in facts for their own sake, Pigou ([1924] 1925:85) reports that "though his [Marshall's] main strength was undoubtedly on the analytical side, he was a tireless collector of realistic detail. He told me once that, in his early days, he had set himself to master the broad principles of all the mechanical operations performed in factories: that, after a time, when he visited a factory, he was able to guess correctly the wages that different workmen would be getting by watching them for a few moments, and that, when his guess was significantly wrong, there was always some special explanation. In the same spirit he eagerly welcomed the opportunity of serving on the Royal Commission on Labour, on which he came into close personal touch with many representative work-people and employers of labour. What he aimed at in all this was to get, as it were, the *direct feel* of the economic world, something more intimate than can be obtained from merely reading descriptions, something that should enable one, with sure instinct, to set things in their true scale of importance, and not to put in the forefront something that is really secondary merely because it presents a curious problem for analysis" (italics in the original). Also, in a letter to Professor Bowley (of March 3, 1901, see Pigou 1925) Marshall comments that he organized his *Old Red Curve Book*—where he put together important facts collected from the press—taking into account his careful correlation of different groups of facts. In another letter to Professor Bowley (of October 15, 1906), he mentions that in his investigation of the real wages in Germany, he relied more on his "field work" and conversations with ordinary people than on the statistics. It seems clear, then, that Marshall was interested in facts and experience because they could provide him with a sense of reality that history, statistics, or pure theory were not able to. While these statements do not imply that Marshall did not follow a positivist view of science, they make that possibility more unlikely.

13 In Appendix K of *Industry and Trade* (1923), Marshall argues that a boy after kindergarten (1923:819) "has more to gain from handling words than from any other exercise: for the materials for his work come to him gratis and in abundance; and in building with them, he is called on to exert the highest spontaneity of which he is as yet capable. Demands are made on his general intelligence, his judgment, his sense of proportion, his logical acumen, his perceptive sensibility and his taste; and in a greater or less degree he can rise to these demands. He is architect, engineer, and skilled artisan all at once." It is most interesting to note that Marshall, when praising Adam Smith in a letter to L.L.Price (of August 19, 1892, see Pigou, 1925:378–379) uses some of the above

adjectives to describe Smith's qualities, such as "sense of proportion." It might be speculated that these qualities Marshall sees exerted by children and admires in Smith are the qualities of common sense.

14 This argument is partly supported by Viner (1941) who contrasts Marshall's preference for the biological metaphor with the mechanical nature of mathematical reasoning. As he points out ([1941] 1982:251): "Devotees of the mathematical approach to economic problems frequently claim for that approach that the alternative non-symbolic method, or the 'literary method' as they too generously put it, is too imprecise and clumsy a tool for the exposition in all their complexity of the relationships between economic variables, with the implication that it is the complexity of economic problems, rather than their simplicity, which establishes a necessary and fruitful field for the use of mathematics. Marshall…seems to me to have taken exactly the reverse view…namely: that non-symbolic language and simple statistical methods alone had the elasticity to deal with the infinite detail and variability of concrete economic phenomena; that resort to mathematics, unless confined to a preliminary stage of economic investigation, involved a greater degree of surrender of this elasticity than it was wise to accept."

15 The issue of vagueness in Marshall's appeal to common sense is discussed here only with regard to the solution to the problem of context definition. Yet this issue warrants a separate study focusing on Marshall's discussion of vagueness as a historical precedent for Wittgenstein's notion of context. There are some similarities between them that when examined could illuminate many issues ranging from the characteristics of the Cambridge Tradition to Maynard Keynes' philosophical development. Marshall's quotation of Bagehot's *Postulates of English Political Economy* (78–79) (in Marshall 1920 [1961]: 52 n.1) illustrates the trade-off between precision and accuracy that is one of the trade-marks of the Wittgensteinean philosophy of ordinary language. According to Bagehot: "Any one who tries to express various meanings on complex things with a scanty vocabulary of fastened senses, will find that his style grows cumbrous without being accurate, that he has to use long periphrases for common thoughts, and that after all he does not come out right, for he is half the time falling back into the senses which fit the case in hand best, and these are sometimes one, sometimes another, and almost always different from his 'hard and fast' sense."

16 Marshall argues that practice allows individuals to cope successfully with complexity. He observes ([1920] 1961:252), that: "The mind of the merchant, the lawyer, the physician, and the man of science, becomes gradually equipped with a store of knowledge and a faculty of intuition, which can be obtained in no other way than by the continual application of the best efforts of a powerful thinker for many years together to one more or less narrow class of questions." Marshall ([1893] 1961, 2:501–502) compares theory to a product that needs to be finished off by handicraft, claiming that "each man's common sense, like his skill in handicraft, dies with him."

17 A.K.Dasgupta provides an interesting interpretation of the concept of equilibrium for Marshall. According to him, Marshall's period analysis allows him to qualify as "equilibrium" processes that are actually in disequilibrium without contradicting himself. For instance, he points out (1990:254) that "The so-called 'equilibrium' on a day is disequilibrium in relation to the short period, as a short-period 'equilibrium' is disequilibrium in relation to the long period. Each period leaves a trail linking it to its successor; the process is continuous and irreversible."

18 Marshall ([1920] 1961:81–82) argues that "the economist must forego the aid of a complete set of technical terms. He must make the terms in common use

serve his purpose in the expression of precise thought, by the aid of qualifying adjectives or other indications in the context. If he arbitrarily assigns a rigid exact use to a word which has several more or less vague uses in the market-place, he confuses business men, and he is in some danger of committing himself to untenable positions."

19 Again quoting Marshall ([1920] 1961:368): "In this world therefore every *plain and simple* doctrine as to the relations between cost of production, demand and value is necessarily false: and the greater the appearance of lucidity which is given to it by skilful exposition, the more mischievous it is. A man is likely to be a better economist if he trusts to his common sense, and practical instincts, than if he professes to study the theory of value and is resolved to find it easy" (italics added).

20 For instance, Keynes ([1924] 1925:43) claims that "The explicit introduction of the element of Time as a factor in economic analysis is mainly due to Marshall."

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10

COMPETITION, RATIONALITY, AND COMPLEXITY IN ECONOMICS AND BIOLOGY

Mark Picton[1]

Introduction

This chapter focuses on the exchange of analogies between biology and economics, particularly with regard to the use of the concepts of competition, selfishness, altruism, and rationality. Confusion over the level of analysis has obscured the conflict between modern evolutionary biology and the behavioral assumptions of mainstream economics. In the biological case, there is competition between selfish agents in almost the opposite sense to that in economics. In economics, competition is an outcome of the properties of the individual agents, who are thought to be selfish. In biology, the "selfishness" of the agents is an outcome of competition. The complex relationship between an individual organism and its environment suggests that altruism at the level of the individual is not inconsistent with selfishness at the genetic level. Furthermore, an examination of the way in which evolution reaches (locally) optimal solutions to problems yields a strong presumption against assuming ubiquitously rational behavior at the individual level for humans.

The closeness of the relationship between biology and economics has varied. Darwin's avowed debt to Malthus suggests that the starting point for modern biology owes something to classical political economy. Malthus' own thinking on population was itself influenced by Franklin's observations from biology.[2] The early Neoclassical economists were, however, more taken with mechanistic rather than biological analogies.[3] Prior to Alchian's (1950) application of the natural selection concept to the firm, few economists made much appeal to biological thought and some were hostile to it.[4]

Sociobiology and economics have had a closer relationship, with biologists using optimization and game-theoretic techniques sometimes borrowed from economics, and economists applying advances in game theory from biology. In addition the evolutionary biology treatment of altruistic behaviour in, for example Dawkins (1976), Trivers (1971), found favour with Becker (1976), Hirshliefer (1977, 1978), Schelling (1978) and Frank (1987, 1988). There was also great interest from both disciplines in Axelrod (1984), a chapter of which was written with the evolutionary biologist W.Hamilton. More recently, the emerging evolutionary psychology school (Tooby and Cosmides 1992; Pinker 1997) has much in common with economists concerned with the possibility of global rationality (Conlisk 1996) and the case for the importance of rule-following (Heiner 1983; Vanberg 1994).

The term "complexity" is used in a number of different, if related ways in economics. Some authors stress positive feedbacks and path dependence;[5] others emergent properties after turbulence and chaos;[6] still others self-organization or increasing returns generally.[7] The sense in which this chapter is concerned with complexity is with the nature of economic agents rather than with their interaction. This is in contrast to an approach which addresses the behavior of *Homo economicus* in a world where there are non-convexities or one which stresses the difficulty in reducing the operations of an economic system to individual behavior.

Complexity also enters into biological thought in a number of different ways. The "gene's eye" approach emphasized in this chapter is highly reductionist. Biologists such as Gould are critical of this approach and emphasize contingency, path-dependence and the role of history in addition to the mechanism of natural selection.[8] The difference is a matter of emphasis in that some, such as Dawkins, concentrate almost exclusively on the mechanism; whilst those such as Gould emphasize the capriciousness of the interaction between the mechanism and improbable and timely circumstance. The highly reductionist view is taken here to suggest that even a simple assumption of *as if* selfishness at the genetic level can produce complex and non-selfish behavior at the individual and social level.

The remaining sections in this chapter examine (i) the influence of classical political economy on Darwin; (ii) the treatment of altruism by biology and economics; and (iii) the challenge to the *Homo economicus* assumption from both economics and evolutionary psychology.

The influence of Malthus on Darwin

The influence of Classical political economy, and particularly Malthus' *Essay on the Principle of Population* (1970, first edition 1798) on Darwin's *On the Origin of Species* (1859) has been the subject of much debate. Whilst some historians of thought, both from biology and economics, contend that there was little or no influence (for example, Schumpeter 1954; Gordon 1989), others accept that there was substantial influence.

Part of the controversy about the influence of Classical political economy on early evolutionary thought concerns quite specific questions, such as whether Darwin read Malthus prior to forming his ideas about natural selection and whether Darwin's thoughts on the niches of different species were directly provoked by economists' work on the division of labor. In particular, there has been considerable debate about whether Malthus' ratios were taken by Darwin and directly applied to animal populations.[9]

Another feature of the controversy is the degree to which the emphasis on competition and self-interest as driving forces in Classical political economy influenced both the theory and rhetoric of Darwin and his early supporters. This approach is obviously an alternative based on the transfer of analogies from Classical political economy rather than of Malthus' particular rendition of the geometric nature of population increase.

A crucial step in Darwin's thinking was to recognize the role of competition between members of the same species under conditions where not all could survive as the mechanism by which more favorable characteristics would prosper and less favorable ones would be eliminated. The way this was expressed, both by Darwin himself and by his early supporters, still has a grip on the mind today and is a major reason not to *dismiss* Malthus' influence on Darwin as "that of a crystal tossed into a saturated fluid."[10] The "gladiatorial" view of natural selection is of Tennyson's "nature, red in tooth and claw," where individuals and species battle to survive in a war of all against all. Natural selection, however, does not entail competition by these means alone. Successful, adapted designs survive, whether as successful contestants in gladiatorial struggle or as successful cooperators in a joint venture. Competition is relevant in both cases, as selection pressure weeds out unsuccessful designs, but the struggle to survive and pass on one's genes is not necessarily a struggle which manifests itself only in terms of a battle where the weak are crushed by the strong.

It seems that the sense in which competition between essentially selfish members of a species came to be expressed by Darwin and his early supporters was influenced by the notions of competition and self-interest advanced by the Classical political economy: Darwin himself:

> all nature is at war, one organism with an another, or with eternal nature. Seeing the contented face of nature, this may at first be doubted; but reflection will inevitably prove it is too true.
>
> Nature may be compared to a surface on which rest ten thousand sharp wedges touching each other and driven inward by incessant blows.
>
> (Darwin and Wallace 1958:259, 118)[11]

and his chief early public supporter, Huxley:

> From the point of view of the moralist the animal world is about on a level of a gladiator's show. The creatures are fairly well treated, and set to fight—whereby the strongest, the swiftest, and the cunningest live to fight another day. The spectator has no need to turn his thumbs down, as no quarter is given.
>
> (Huxley 1888)

Part of the nineteenth-century controversy over Darwin's theory sprang as much from this way of presenting the argument as from the argument itself.[12] The analogy between competition among selfish economic agents and natural selection is, however, inexact and somewhat misleading.

Darwin himself, of course, was aware that natural selection did not operate solely through a gladiatorial struggle for survival. The "survival of the fittest" was a term used "in a large and metaphorical sense *including dependence of one being on another*."[13] Nonetheless, until recently, behavior which appeared to be other than competitive in the gladiatorial sense—such as altruism—was taken to require a group-selection explanation, or even as anomalous in evolutionary terms.

Altruism and selfishness in economics and biology

Both economists and biologists are skeptical about apparently altruistic behavior. It is usually supposed that behind an apparently altruistic act lies a more subtle selfishness. What is meant by selfishness is, however, not always clear.

A major difficulty for making sense of differing accounts within economics of the significance of altruistic behavior is that there is little agreement about what self-interest means. If altruistic behavior is supposed to be contrasted with self-interested behavior, this presents some difficulties. As Hirschman has emphasized, this problem has its roots in the political usage of the term "self-interest" and the role it played and continues to play in public and academic discourse. The term self-interest is used both as a euphemism for narrowly selfish behavior and tautologically.[14] Whilst in modern economics "self-interest" is generally identified with the latter meaning, the identification by Smith and Hume with the former is plain.[15] The "invisible hand" is associated with private vice, of avarice, and love of lucre. The canonical butcher and baker quote clearly presents a view of self-interest as a motivation *in contrast* to benevolence.[16] Smith's emphasis was on public virtue from private vice, not merely from decentralized interaction.

The tautological interpretation subsumes all motivations under that of self-interest. Self-interest does not consist merely of narrowly defined "economic" or financial motives, but is an encapsulation of a number of different, not necessarily base, motivations. Wicksteed, for example, refers to a householder's decision about what to serve guests for dinner as involving considerations of social status, and personal, social, and ethical judgments as well as financial and taste matters, all under the rubric of self-interest.[17] Indeed, according to Robbins, Wicksteed "shattered [the] misconception" that "the whole structure of Economics depends upon the assumption of a world of economic men, each actuated by egocentric or hedonistic motives."[18]

Not surprisingly, the existence of two quite different notions of self-interest, one allied with selfishness and one encompassing all motives, has been an ongoing source of confusion. In order to make a case for the relevance of non-selfish motivation in economic affairs, it is as though two cases have to made: one for the importance of altruistic motivations as opposed to self-interested ones; and one for the importance of a non-selfish component of self-interest itself. In reference to political behavior, Mansbridge remarks that:

> [s]ome rational choice theorists equivocate on this point, most of the time making the false claim [that political behaviour can be explained in selfish terms], and then backing off to the vacuous claim [that all motives are encompassed by self-interest] under critical fire.
>
> (Mansbridge 1990:20)[19]

A clear consequence of an all-encompassing view of self-interest is that an unrequited transfer need not be accompanied by a "warm inner glow;" that it need not be subtle selfishness. This is not to say that no apparently altruistic behaviour vanishes under skeptical scrutiny.

A "gift" from (say) a rich person to poor people may serve any or all of the following purposes in terms of the rich person's utility:[20]

- There may be a flow of commodities as compensation for the "gift": that is, the transfer may be a way of internalizing a real physical externality.
- The "gift" may be a roundabout way of achieving some other benefit, such as status.
- The agent may receive a "warm inner glow" from giving; that is, the agent's own welfare may be increased by giving to the poor. Here, the agent feels sympathy for others and feels better off by giving something to them.[21]

The agent may feel no better off or even worse off by giving, but still prefer to give.[22] Once a distinction has been drawn between the agent's own welfare and preferences or between his or her selfish pleasures and only partly selfish interests, this notion should not seem at all paradoxical. The utility-maximizing agent may quite consistently trade off own welfare for the perceived welfare of others.[23] That such acts involve a sacrifice does not make the act irrational: merely what is preferred is not necessarily what maximizes own welfare.

Altruism, Group Selection and the Selfish Gene

In evolutionary biology, the view that apparently altruistic behavior is best explained as a subtle form of selfishness is related to the "motives" of genes rather than organisms. An altruistic tendency in an individual is one that reduces that individual's chances of surviving to breed successfully and increases the chances of another. An example of this is where a particular member of a species emits a warning signal upon the approach of a predator which increases the survival chances of other members of that species, but decreases the survival chances of that individual. Whilst it is the case that mainstream biology explains this kind of apparent altruism in selfish terms, it does not discount the possibility of genuinely altruistic motivation at the individual or organism level.

The particular interpretation of altruistic behavior to which evolutionary biologists object is that the behavior is done "for the good of the species." In contrast to a "group selection" view, evolutionary biologists insist that the selection process occurs at the level of the individual or the gene. At the level of the gene, there can only be selfishness, even though this may involve individuals behaving altruistically.

According to this view, a particular behavioral pattern arises from mutation/s and becomes prevalent in the population because it survives by advantaging the organism/s which carries that strategy. This means that the criterion for the persistence of a genetic trait is individual, not group selection. That is, a mutation which would make all the members of a particular species "better off" if all or most carried it could only come to be prevalent in the population if the few initial carriers of the mutation benefited from it.[24] Using the example above, suppose that a particular species emits warnings when predators approach. Further, suppose that these warnings improve the survival prospects of the entire group and that a mutation which reduced the tendency to give warnings would not survive.[25] To explain the phenomenon would require an account of how the mutation survived as a novel trait amongst a very small proportion of the population (or just a single organism) and how it increased in frequency. This amounts to how it advantaged (or did not disadvantage) the initial carrier(s) of the trait and how subsequently those not carrying the trait were at a disadvantage and diminished as a proportion of the population.

The essence of the "gene's-eye" view of biology is that an organism may usefully be viewed as the means by which a set of genes attempts to survive, and both the physical and behavioral characteristics of the organism are the strategies employed by the genes in pursuit of that end. The genes themselves, of course, do not have actual goals or deliberately employ strategies. A particular mutation may improve or reduce an organism's chances of living to pass on its genes to future generations. The sense in which genes "selfishly design" organisms for their own replication is that mutations which *happen* to improve an organism's chances of living to pass on its genes increase in frequency in a species.

The notion of the "selfish gene" is not at all incompatible with non-selfish behavior at the level of the organism. "Just as blueprints don't necessarily specify blue buildings, selfish genes don't necessarily specify selfish organisms."[26] Genes are *as if* selfish because of the *outcome* of competition amongst different patterns of genes which manifest themselves as behavioral and physical characteristics. Behavioral patterns may be thought of as strategies designed to advance the selfish interests of genes, but this is an outcome of the process of evolution; and it neither implies that surviving organisms will behave selfishly nor that any unselfish or self-sacrificing behavior at the level of the organism is "really" selfish because it developed as a result of biological competition.[27] It merely implies that a mechanism was a successful strategy when it first emerged, in terms of biological fitness.

Reciprocal Altruism and Inclusive Fitness

Trivers (1971) developed a model of reciprocal altruism which demonstrated that mutual helping behavior is compatible with selfishness at the genetic level. Suppose there were a species of birds commonly afflicted with a harmful parasite. A mutation of the species which caused birds to groom other birds which groomed them would be capable of survival. Note that the mutation is viable not because the population would be "better off" if all or most birds groomed others, but that a small sub-set of birds engaged in mutual help would do better than average because they conferred advantages on each other and withheld assistance to non-groomers.[28]

There are obvious similarities to the prisoners' dilemma and to the tit-for-tat or similar conditional cooperation strategies here; although the genotype may "play" its strategy by providing the phenotype with the *emotional* rather than the intellectual equipment required for cooperation. Second, the spread of a cooperative behavior need not be complete: if it is hard to identify cooperators amongst a large population, then the behavior might not spread to the whole population. A mix of strategies—some individuals playing tit-for-tat and some being opportunistic and being groomed but never grooming—might be an equilibrium or evolutionary stable strategy.[29] This is akin to supposing that there are increasing costs to fitness of identifying and/or sanctioning opportunists. Third, whilst more complex strategies than something like tit-for-tat may be more effective in securing the survival benefits from grooming, they may well be substantially more costly in terms of the design of the organism. For example, if the benefits of cooperation are large (always being parasite free) and the costs of being exploited by an opportunist are small (five minutes wasted grooming them), then a simple behavioral rule is liable to outcompete some more complex process which might require more resources (a larger brain, with associated problems of carriage, birth, and so on).

There may be more than one evolutionary stable strategy. In a grooming situation, such as that in the preceding paragraph, a population of either all (or almost all) tit-for-tat or all opportunists is hard for other strategies to "invade."[30] There is likely to be a threshold proportion which a cooperative strategy must reach before it becomes viable in a population initially consisting of all cheaters. Once reached, tit-for-tat can prosper, but there is no particular reason to suppose that evolution will favor one initial situation over another. Populations which are lucky enough to reach the threshold consist of individuals which all do better, but since selection works by the individual promoting the genes' interests, not the group promoting the gene pool's interests, populations may well end up at the "all cheat" equilibrium, which is stable. "If a population arrives at an ESS that drives it extinct, then it goes extinct, and that is just too bad."[31] This leads to a kind of group selection argument which even Dawkins, who is generally scathing of such arguments, supposes "might actually work."[32] If there are two evolutionary stable strategies, and the cooperative one is better for all, then the population which has it is less likely to become extinct:

> [Although] the individual cannot be expected to make sacrifices for the group, which would mean that particular genes would be selected out within the group even if it would help the group …at the same time, the individual cannot have genes which help its development but put too large a strain on the society of which it is part.
>
> (Tullock 1994:68)

Dawkins suggests this is "a kind of higher level, 'between ESS' selection in favour of reciprocal altruism."[33]

Kin Altruism

On the face of it, a difficulty in accounting for reciprocal altruism lies in its original emergence. If there are already some individuals who will respond to altruistic behavior, it is clear that the mechanism can be adaptive. However, this leaves the question of how the trait could arise as a successful adaptation in just a single individual. According to Hamilton and Axelrod,[34] the answer lies in kin selection. That is, if the bulk of an individual's genes is shared by close relatives, the appropriate strategy from the point of view of the genes is one that advances the survival properties of both the individual organism and its relatives. This is inclusive fitness, or Hamiltonian altruism (although "nepotism" is perhaps a more appropriate term[35]). The closeness of the relationship determines the weighting of the importance of the survival of others in the behavior of the organism.[36] Altruistic behavior amongst kin will therefore improve *inclusive* fitness if the cost to fitness of an organism is exceeded by the benefit weighted by the degree of relatedness.[37]

Kin altruism from older to younger individuals, manifested in some emotional attachment between parent and offspring or between older and younger siblings, might be supplemented by younger individuals responding to this kindness.[38] That is, an adaptation which repaid kindness with kindness would increase inclusive fitness.[39] In such a circumstance, a mutation towards less focused altruism could prosper, particularly if the mental equipment—the urge to reciprocate, the capacity for guilt, and so on—were already in place.[40] If, as generally supposed, the ancestral environment for humans (see next sub-section) were one in which many neighbors were relatives, then this idea of reciprocal altruism taking root from kin selection seems plausible, particularly as there are very substantial gains from cooperative behavior amongst humans.

The Ancestral Environment

A crucial part of the evolutionary psychology perspective is to recognize that the circumstances in which humans evolved were some time ago and rather different to the environment faced today. The circumstances for which humans are adapted are referred to as *the ancestral environment*.

The ancestral environment or environment of evolutionary adaptation is generally supposed to have consisted of fairly small, somewhat isolated bands of humans living in a hunter-gatherer society. Given that the pace of environmental change has been rapid, and in recent centuries extraordinarily so, human evolution is likely to have lagged behind environmental change a great deal.[41] "Our minds are adapted to the small foraging bands in which our family spent ninety-nine percent of its existence, not to the topsy-turvy contingencies we have created since the agricultural and industrial revolutions."[42] According to this view our intellectual and emotional faculties persist today as a relic of solutions to problems found in the Pleistocene era: "genes give [people] thoughts and feelings that were adaptive in the environment in which the genes were selected."[43]

Selfish Genes, Genuine Altruism?

The gene's-eye perspective suggests that an organism behaves in a way which suits the selfish interests of its genes in the environment to which it is adapted. This means that an individual organism may sacrifice itself under certain circumstances if that behavior spreads its genes effectively. From the point of view of the gene, this is selfish behavior: everything is. From the point of view of the organism, the picture is not so clear. Admittedly, there does not seem to be much more to a self-sacrificing bacterium than a shell which serve its genes. Is it any different for humans? As noted (see note 24), Frank (1988) does not view reciprocal altruism as genuinely unopportunistic behavior.

There are two questions here: first, can this view account for genuinely unopportunistic behavior in Frank's sense?; second, is Frank's dismissal of these bases for genuinely unopportunistic behavior justified? Genuinely unopportunistic behavior in the Frank sense can indeed be accounted for from the evolutionary biology perspective. If our emotional and intellectual faculties are relics of the Pleistocene era, then we may well do things today which neither serve our own selfish interests nor our genes' reproductive or replicative interests. Given that we are not adapted to the modern environment, evolutionary misfiring on a fairly grand scale would be expected. This may well involve behaving altruistically in circumstances that would have enhanced fitness in the ancestral environment, but do not today. However, if this is not to be characterized as merely a mistake, it is not at all clear that altruistic behavior at the level of the individual should be considered to be somehow "really selfish" if it does serve the interests of the genes.

The origins of the thoughts and feelings of people may be explained by reference to the interests of genes, but this does not mean they are not real, at least for the purposes of social science. *We* are what counts for us, after all, and whether we do or do not effectively serve our genes' interest is neither here nor there to us. If humans can be said to have motives and experiences at all, then the fact that one feels bound to do some noble act that is due to what profited the genes of a distant ancestor does not sully the act:

> [C]onsciousness…can be thought of as the culmination of an evolutionary trend towards the emancipation of survival machines as executive decision-takers from their ultimate masters, the genes. Not only are brains in charge of the day-to-day running of survival machine affairs, they have also acquired the ability to predict the future and act accordingly. They even have the power to rebel against the dictates of the genes, for instance in refusing to have as many children as they are able to.
>
> (Dawkins [1976] 1989:59–60)[44]

Rationality and evolutionary psychology

The picture of the human mind which the evolutionary biology perspective provides is one where perception, reason, and emotion work together in a way which would have produced behavior which enhanced inclusive fitness in the ancestral environment. The usefulness of this for economics is that it provides a starting point for social science which is consistent with the natural sciences. Adopting a position which supposes that humans cannot be reduced to the maximization of some hypothetical utility function does not mean abandoning methodological individualism or even reductionism. On the contrary, supposing that a simple driving force at the genetic level may produce at the level of the organism mechanisms which are complex and irreducible *at that level* is perfectly consistent with a wholly reductionist stance. A methodological individualism which proceeds from a richer view of human psychology than *Homo economicus* may nonetheless be methodologically individualist.

In this section it will be argued that due consideration of the problems posed by information suggests that truly optimizing behavior is inconsistent with a view of human psychology which takes account of what could have evolved, if indeed it is possible at all. There are several major problems with information: the overwhelming amount of sense data; limits on the ability to process information; the costs involved in processing information; and the necessity of interpreting information so as to make it meaningful.

If decision making is costly, then even if the individual has the *ability* to process all information, it would not pay so to do. Given that making decisions is costly in terms of time, then at some point the benefit in receiving an additional unit of information must be exceeded by the costs of acquiring further information. The obvious "solution" to this problem is to collect the optimal amount of information: to collect information until the marginal benefit of collecting or considering an additional amount of information is equal to the marginal cost of acquiring and processing it. A problem with this idea is that the value of information is unknown until it is considered. This means that unless one has the information, the benefit of the information or the opportunity cost of failing to consider the information is unknown. This does not in itself mean that collecting or processing the optimal amount of information cannot be done: the agent could estimate the costs and benefits of information and then collect the appropriate amount.

This approach leads to the same difficulty: How much information should be collected about how much information to collect? Beyond some point there are presumably diminishing returns to collecting information to form an estimate of the usefulness of information for the decision problem at hand; but once again, either *all* information available for the purposes of the estimate must be collected or the amount of information for the estimate would need to be informed by a higher-level estimate of the costs and benefits of the (first) estimate. Even if collecting all the information were possible, the amount of information collected would be excessive (and therefore not optimal) in that some information would in hindsight only tell the agent that it was not worth collecting. The "information about information" approach—which does not require needing to know everything in order to find out whether you needed to know it—always requires an additional estimate, and thus falls into an infinite regress. At some level, agents just have to guess about the appropriate amount of information.[45] "It is evident that the rational thing to do is to be irrational, where deliberation and estimation cost more than they are worth."[46]

Heiner (1983) provides a criterion for judging the advantages of flexibility. Suppose that a person may follow a rule and ignore additional information or gather and use that information in order to make a decision. In a situation where there is uncertainty due to environmental complexity and perceptual variables, the use of additional information may lead to a better or a worse outcome.[47] Additional information allows a more desirable state to be reached, but there is also the possibility of error: additional flexibility increases the chance that an agent will exercise that flexibility in the right way at the right time, but it also allows more scope for the misapplication of that flexibility. Flexibility in behavior must meet minimum reliability conditions in order to promote an agent's interests. The question of whether flexibility to select an action will improve performance depends on the chance and consequences of selecting that action under the right conditions versus the chance and consequences of selecting it under the wrong conditions.[48]

A number of economists have emphasized the importance of rules as distinct from optimizing behavior.[49] There are logical problems with optimizing in a world where decision making is costly and/or where information is costly to acquire *even if* it were possible in principle to know enough to make an optimal decision. Research by evolutionary psychologists (amongst others)[50] suggests that the problem of informational and choice overload casts further doubt on the possibility of genuinely optimizing behavior:

> [W]ith each new dimension of potential variation added…the total number of alternative possibilities faced by a computational system grows with devastating rapidity. For example, if you are limited to emitting one out of 100 alternative behaviors every successive minute…after the second minute you have 10,000 different behavioral sequences from which to choose…and 10^{120} after only one hour…The system could not possibly compute the anticipated outcome of each alternative and compare the results, and so must be precluding without

> complete consideration the overwhelming majority of branching pathways.
>
> (Tooby and Cosmides 1992:102–103)

This means that even if an agent were fully informed, the computational task of evaluating all alternatives is impossible.

The meaning of information is not self-evident. Information is not itself knowledge. Faced with given data from the world, an individual must interpret them in order for them to be meaningful. Without some "innate assumptions" about the world, it is not possible to make sense of it. The importance of frames can be illustrated with reference to vision: "If perceptual mechanisms are trying to construct a three dimensional model of the local world from a visual array, there is an infinite number of different ways to do it that are all consistent with the array."[51] An information-processing system which imposes no structure on the problem cannot turn the visual stimulus into knowledge. The fact that eyes give us useful knowledge *is a consequence of assumptions about the structure of the world which are built into the brain:* certain parts of the visual system "look for" things that might be edges, and assume they *are* edges: "the visual system hates coincidences: it assumes that a regular image comes from something that really *is* regular and that it doesn't look that way because of the fortuitous alignment of an irregular shape."[52]

Given the volume of sensory information and the need to make sense of that information, specialized mental organs are likely to characterize the actual structure of the brain. A single, general-purpose mechanism which did not impose structure on the world by making assumptions about it would be unable to make sense of the information received. Furthermore, even if a general-purpose mechanism could make sense of the volume of data received, basing decisions on all available information would be very costly in terms of biological resources: "[T]here is a host of…reasons why content-free, general-purpose systems could not evolve, could not manage their own reproduction, and would be grossly inefficient and easily outcompeted if they did."[53]

On this view, the globally maximizing and wholly rational *Homo economicus* is inconsistent with the notion that the human brain consists of structures which have evolved to solve the problems faced by humans in the ancestral environment. The evolutionary psychology perspective suggests a modular brain, one with mechanisms which successfully tackled specific problems encountered by Pleistocene-era humans. According to evolutionary psychologists, "[o]ur mental life is a noisy parliament of competing factions"[54] rather than an integrated whole.

Conclusion

Part of the tumult surrounding Darwin's and Wallace's theory of natural selection included the realization that it involved the breakdown of the distinction between humans and the natural world. That does not mean that humans are "just another animal," we are unusual, and we are certainly special to us. That people are a product of evolution is hardly a radical proposition, yet the social sciences remain somewhat divorced from the natural sciences. Evolutionary psychology provides a picture of human behavioral proclivities which appears to be at odds with the core of mainstream economics. This inconsistency requires a resolution.

The genesis of modern biology in Darwin's *Origin* involved recognizing the importance of the consequences of the struggle of the individual. Whilst biology took some time to find an appropriate way of characterizing the individual nature of selection so as to account for cooperative behavior amongst individual organisms, the picture is now sufficiently developed to provide a clear alternative to mainstream economies' assumption of essentially selfish, asocial and rational agents.

That evolutionary biology's appropriation of selfishness and optimization applied at the genetic level should pose such a fundamental challenge to the key behavioral assumptions of economics is something of an irony, since those facets of the disciplines do seem to represent something in common.[55] Given that humans' mental capacities evolved as solutions to problems faced in a hunter-gatherer society, however, it does represent a serious challenge to the parsimony of behavioral assumptions in economics. The ecological niche occupied by humans is one of specialization in cognitive development: we "[h]umans have the unfair advantage of attacking in this lifetime organisms that can beef up their defenses only in subsequent ones."[56] That in the absence of formal institutions humans managed to find some way to hunt the stag can hardly be doubted.[57] This perspective suggests that the importance of informal institutions has been neglected and that standard policy prescriptions based on the primacy of explicit and market price-like incentives or sanctions as means to influence individual behavior may be seriously misleading.

Notes

1 I wish to thank Keith Jakee, Francisco Louçã and Yew-Kwang Ng for useful comments. I remain responsible for errors of fact or interpretation.
2 Franklin (1755).
3 See Hodgson (1995:xiii–xiv).
4 Marshall and Veblen are important exceptions to this. Even Schumpeter, whilst espousing an evolutionary approach, was of the view that "no appeal to biology would be of the slightest use" (1954:789). Perhaps this attitude contributed to his rejection of the Darwin-Malthus connection (ibid.: 445).
5 Arthur (1994).

6 Louçã (1997).
7 Yang and Ng (1998).
8 See, for example, Gould (1991, [1995] 1997).
9 Darwin's comments on the matter plainly suggest a debt to Malthus: "I happened to read for amusement Malthus on *Population,*…Here, then, I had at last got a theory by which to work" (1958:120) and "The final cause of all this wedging, must be to sort out the proper structure. & adapt it to change.—to do that, for form, which Malthus show, is the final effect, (by means however of volition) of this populousness, on the energy of men" (*Notebook D,* written *c.*1838). See Jones (1989) for these and other relevant entries from Darwin's *Notebooks.*
10 Mayr (1982:492).
11 Darwin's "Essay of 1844" reproduced in Darwin and Wallace (1958:259, 118).
12 See Gould (1991:325–339).
13 Darwin (1859:62, emphasis added).
14 Hirschman "The Concept of Interest: From Euphemism to Tautology," in Hirschman (1992:35–55).
15 "in contriving any system of government and fixing the several checks and balances, every man ought to be supposed a *knave,* and to have no other end, in all his actions, than private interest…and, by means of it, make him, notwithstanding his insatiable avarice and ambition, cooperate to public good" (Hume [1742] 1963:42).
16 Also a contrast between interest and other motivations, notably pride. See S.Holmes, "The Secret History of Self Interest," in Mansbridge (1990:267–286).
17 I.Steedman, "Rationality, Economic Man and Altruism," in P.H.Wicksteed *Common Sense of Political Economy,* 1989; reprinted in Zamagni (1995:189).
18 Robbins' introduction to Wicksteed (1933).
19 Mansbridge "Self Interest in the Explanation of Political Life," in Mansbridge (1990:20). "Vacuous" is perhaps a little strong.
20 See, for example, Brennan (1975).
21 Sen's term from "Rational Fools" (1977).
22 Sen's "commitment".
23 See Ng (1983: ch. 1).
24 This is true both for the organisms and the genes.
25 The example is not meant to be particularly realistic.
26 Pinker (1997:44).
27 This contrasts with Frank's view that "Tit-for-tat, reciprocal altruism, kin selection, and other conventional evolutionary accounts…for all their obvious value…do not explain genuinely unopportunistic behavior at all" (Frank 1988:27). See p. 202.
28 Note that a *single* such individual would not survive if the cost to fitness were non-zero. However, if there were kin selection (see p. 200–201) the mutation could gain a foothold.
29 A type of equilibrium known as an evolutionary stable strategy, developed by Maynard Smith. An example is the "hawk-dove" game, where the two strategies "fight to the last" and "run away if really threatened" tend towards stable fractions of the population for given payoffs. See Dawkins ([1976] 1989:72). Tit-for-tat is obviously a somewhat more complicated strategy than hawk or dove. The equilibrium may consist of individuals playing different strategies ("polymorphism"), or of individuals playing both strategies with a probability of playing each strategy equal to the equilibrium proportion for that strategy. See Dawkins ([1976] 1989:75), Wright (1994:79–81).

30 In simple situations, where cheating is unsubtle, tit-for-tat can drive other strategies to or near to extinction (see Axelrod 1984:49–53). In the previous paragraph, where it was suggested that a mixture of tit-for-tat and opportunism could be stable, more subtle cheating and increasing costs were assumed. Tit-for-tat is not quite an ESS, it can be invaded in certain circumstances. Nonetheless it is rather robust. See Dawkins ([1976] 1989:217), Axelrod (1984:169–191).

31 Dawkins ([1976] 1989:186).

32 Ibid.: 321.

33 Ibid.

34 Axelrod (1984:88–105).

35 Tullock (1994:15), Pinker (1997:435).

36 The "weighting" is *as if* and *ex post:* patterns of genes which include some degree of self-sacrifice at the level of the organism to advance the survival prospects of those with whom they share genetic characteristics will tend to spread in a population.

37 Where $c>br$, where $c(b)$ is cost (benefit) in terms of future reproductive success, and r is the degree of relatedness and equals 0.5 for siblings, 0.125 for first cousins, and so on (see W.D.Hamilton, "The Genetic Evolution of Social Behaviour" *Journal of Theoretical Biology* 7:1–16, 17–52. This notion was earlier expressed by J.B.S.Haldane as a willingness to sacrifice himself for two brothers or eight cousins.

38 "[A] gene counselling apes to love other apes that suckled at their mother's breast…might thrive. But what are younger siblings supposed to do?… [G]enes directing altruism towards altruists would benefit older siblings" (Wright 1994:201). See Hamilton and Axelrod, "The Evolution of Cooperation in Biological Systems," in Axelrod (1984:98–99), where it is noted that initial clustering of reciprocal altruists would allow invasion of an all cheater/defector population, and that kin do rather tend to be clustered. Once established, tit-for-tat is hard even for a cluster of individuals to invade. Thus "the gear wheels of social evolution have a ratchet."

39 This is not suggest that there is no conflict of interest between siblings, nor that either kin or reciprocal altruism will wholly obscure such conflict. See Wright (1994:165–169) for an example and Becker (1976) for an argument which suggests that at times in may pay to appear more altruistic than one is. Hirschliefer's (1977a) "Last Word" argument suggests limitations to Becker's "Rotten Kid" hypothesis.

40 Wright (1994:201).

41 Environmental change in the broadest sense. This includes the changes in the external environment—the climate, flora, and so on—which have occurred through forces external to humans and as a consequence of human activity; and the changes in relationships between humans which have occurred as a result of earlier evolution.

42 Pinker (1997:207).

43 Ibid. 208, see also 205–206; Wright (1994:38, 391–392).

44 Since Dawkins is a leading sociobiologist, this seems to give the lie to: "[Reasoning beings are not bound to do what makes evolutionary sense—a point that sociobiologists generally admit, only to give it little weight in their further speculations" (Singer 1981:131). However, it is true that sociobiologists sometimes seem to slip into the habit of referring to the phenotypic existence as mere shadow-play: see Wright (1994:324–326) for example.

45 Conlisk (1996:687). Conlisk cites Leif Johansen, *Lectures on Macroeconomic Planning,* Amsterdam: North Holland (1977:144): "At some point a decision must be taken on intuitive grounds."

46 Knight (1921:67). Information is both something which is a means to minimize costs and something which is costly and whose own cost must be minimized. Conlisk (1996) notes that this infinite regress problem for information has garnered curiously little attention.
47 "Environmental complexity" refers to the difficulty of the problem; "perceptual variables" to limits on the agent's competence in approaching the problem.
48 Heiner (1983:566); see also Vanberg (1994).
49 Ibid.
50 Notably cognitive psychologists, linguists, and researchers in artificial intelligence.
51 Tooby and Cosmides (1992:103).
52 Pinker (1997:216, emphasis in original). Other assumptions made by the visual system include: "Surfaces are evenly colored and textured…, so a gradual change in the markings on a surface is caused by lighting and perspective…Objects have regular, compact silhouettes, so if Object A has a bite taken out that is filled by Object B, A is behind B" and so on (ibid.: 217). These assumptions have a very strong grip on us, since optical illusions persist even when recognized as such. "[The] cheat-sheet is so deeply embedded in the operation of visual brain that we cannot erase the assumptions written on it. Even in a lifelong couch-potato, the visual system never 'learns' that television is a pane of glowing phosphor dots, and the person never loses the illusion that there is a world behind the pane" (ibid.: 29).
53 Tooby and Cosmides (1992:112).
54 Pinker (1997:58).
55 That optimization and equilibrium are common elements in mainstream economics and biology is emphasized by Krugman (1994).
56 Pinker (1997:190. See also 188–197). The "cognitive arms race" may well have as much to do with humans' relationships with (or against) other humans, than with the attacking of other species: "there's only so much brain-power you need to subdue a plant or a rock…but the other guy is as smart as you are and may use that intelligence against your interests" (ibid.: 193). It is highly unlikely that these factors *alone* propelled the cognitive development of humans, since a number of other species live socially, hunt in groups, and so on. The response of small organisms to our activities may vitiate that "unfair advantage": for example, bacteria (which breed very rapidly relatively to us) have effectively been selected for resistance to our antibiotics.
57 Rousseau's "Stag Hunt" is a prisoners' dilemma type scenario: each of a band of hunters may be distracted from encircling a stag (large collective reward) by the temptation of catching a hare (small individual reward).

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Part IV

ALTERNATIVE PERSPECTIVES ON COMPLEXITY

11

COMPLEXITY AND ECONOMIC METHOD

An institutionalist perspective

Robert E.Prasch[1]

Political economy was still in its infancy when the first discussions of method were articulated. Dugald Stewart, in his memorial of Adam Smith, presented us with the importance of "conjectural histories" in understanding the evolution of economic and social institutions (Stewart [1794] 1980:5). Not much more than thirty years later, Nassau Senior and Richard Whately offered their defenses of the method they considered appropriate for the investigation of the particular subject matter of political economy (Senior 1836; Whately 1832; Prasch 1996).

Indeed, political economy was not very old when discussions over economic method evolved into a debate over the relative merits of "inductive" versus "deductive" approaches to knowledge. This debate has continued throughout the history of economics. It was at the core of the contest between the English Historical School and the Ricardian mainstream (Leslie 1888; Ingram 1888), which was soon followed by the rivalry between the American Institutionalist school and the soon-to-be ascendant Neoclassical school (Veblen 1936; Yonay 1998).

By the early 1980s the field of economics had settled into a relatively stable equilibrium, with the Neoclassical school, in its formal and most abstract general equilibrium format, enjoying a comfortable hegemony over the profession. By that time, Friedman's positivist approach to economic method was no longer subject to sustained questioning. What dissent remained had been safely marginalized to underfunded and geographically remote university departments. Debate over fundamentals was virtually extinguished. Many prominent economists were beginning to claim that the only outstanding issues of economic research involved the estimation of various parameters within the dominant Neoclassical-synthesis model.

While this hegemony is still very strong, it is no longer as unchallenged as it once was. While marginalized schools, such as the Austrians, Marxists, Post Keynesians and Institutionalists, continue to struggle on, what is new is that the economics profession is beginning to see dissent emerge from within the citadel. Game theory, chaos theory, and discussions of rhetoric are beginning to whittle away at the once unquestioned verities of the Arrow-Debreu general equilibrium model. At least amongst economic theorists outside of the strict Chicago tradition, the idea that "assumptions don't matter" has already lost a good deal of its once-absolute sway.

The latest challenge comes from the Santa Fe Institute in the form of its steady promotion of the science of complexity across several disciplines including economics. Game theorists and economic theorists with established reputations such as Robert Axelrod (1997), Brian Arthur (1996, 1995, 1994a, 1994b), Duncan Foley (1998) and Peter Albin (1998) have all taken a sustained interest in this new method of mathematical modeling. These authors propose to improve, even revolutionize, economics as a discipline by examining models of "complex adaptive systems" in which the initial assumptions, the psychology of the economic agents, the process of learning, adaptation to a changing environment, and co-evolution become core components of dynamic economic models. The structure of these new models is conveyed by Nobel Laureate Murray Gell-Mann:

> [A] complex adaptive system acquires information about its environment and its own interaction with that environment, identifying regularities in that information, condensing those regularities into a kind of "schema" or model, and acting in the real world on the basis of that schema. In each case, there are various competing schemata, and the results of the action in the real world feed back to influence the competition among those schemata.
>
> (Gell-Mann 1994:17)

Predictions, and their verification, are relegated to a secondary role in this new approach to science. Adaptation, co-evolution, and the search for an emergent order are of primary interest. Since the structure and the exact qualities of the emergent order cannot be known to anyone in advance, and is not presumed to be globally stable anyway, it is unclear what role prediction would play in a complexity research program.

Economists such as Brian Arthur have observed that climatology, biology, and geology are sciences even though no one is concerned about our inability to predict the evolution of animal species or the formation of weather and rock formations. What these sciences do is conduct a search for and an assessment of the major forces at work, study the emergence of structure (hurricanes or species), and provide researchers with compelling explanations as to what has occurred and what can potentially take place.

In economics, prediction is closely associated with the method of equilibrium—which is one of the ideas that will, in all likelihood, have to be rethought if the notion of complexity were to take serious hold in economics research. After all, an evolutionary system does not arrive at a state of rest unless it were to stop evolving—which could only mean that it was dead (Arthur, 1994a: chs 1–3, 1994b).

Clearly, the science of complexity, if it were to be taken seriously, has important implications for economics as it is conventionally taught and practiced today. Although Brian Arthur wants to argue that his research into increasing returns and the resultant complex systems should be thought of as a complement to contemporary models of economics, mainstream economists are probably correct to be concerned and to assume, along with John R.Hicks, that a serious consideration of increasing returns could lead to "the threatened wreckage…of the greater part of general equilibrium theory" (Hicks 1946:84).[2]

Since Arthur desires to illustrate the properties of a system in which agents form expectations as they attempt to negotiate a changing co-evolving structure, he has called for a closer integration of psychology into the foundations of economics. He argues that people depend on induction when they interact. Indeed, he believes that, given the evolving structure of the economy and the characteristics of the problem that they are trying to solve, induction is the most rational method of learning that people can follow. Rather than imposing some *a priori* rule of expectations formation on economic agents, Arthur argues that we should perceive them as following something akin to the scientific method as they make sense of their economic lives (Arthur 1995, 1994b).

While important distinctions remain, Post Keynesians, and especially Institutionalists, should welcome the developments that are coming out of the Santa Fe Institute. While there will certainly be a tendency to ask "What took you so long?" we should focus on the bright side and celebrate the fact that the avant-garde of the economics profession are finally coming around to a sensible position on so many elements of economic theory. It is especially pleasing to see some very bright people drop the blinders of general equilibrium theory and positive economics in order to establish a more serious foundation for sustained research into the fundamental dynamics of the economy.

From a historical or comparative perspective, the Santa Fe approach represents the retrieval of several important trends within economics. Let us now take a look at some of the concepts that they have been thinking about, and contrast them with some of the work that has been circulating outside of the citadels of mainstream economics for some time.

The emergence of structure in the economy

The Neoclassical approach, especially in its general equilibrium format, takes the structure of the economy to be given prior to the interaction of economic agents within the market. This is a crucial aspect of the system's ability to make predictions and pronouncements about the "correct" economic policy to pursue under any specific economic conditions.

Picture "the theory of supply and demand" as it is usually presented in the textbooks. The graph is drawn and an equilibrium is determined. Given this structure, we "discover" what the proper price of the commodity is. The teacher then demonstrates to the class that no matter what the initial prices and quantities are, and no matter how the (necessarily historical) process of "higgling and bargaining" unfolds, "the" equilibrium price and quantity will be the only logical result of the economic process. It is crucial to the story that the process itself does not in any way feedback on the structure of the economy in the sense that price relations cannot be thought to feedback on either the demand or supply schedules (Levine 1980). Allowing for such feedbacks, in the form of evolving expectations or transfers of wealth through "false trades," would undermine the idea that there is one unique equilibrium point. Multiple equilibria diminishes the capacity of the model to generate clear predictions.

The understanding of these dynamics is not new to economic theory. For example, the potential for the actual sequence of trades to change the underlying equilibrium of the system was clearly understood, if ignored, by Hicks (1946:128–129). The mainstream of the profession continued to resist this insight even after it was revived in a more formal presentation by Robert Clower (1965).

Having a fixed equilibrium structure underlying all economic relations effectively removes market processes from the study of economics. Since the result of the process is presumed to be fixed and known in advance, it cannot be interesting to expend very much research energy in finding out how it is that this equilibrium is actually achieved. It follows that a study of the market as a process is banished from the profession's research agenda since it is defined as being outside of the boundaries of economic study. Given the structure of general equilibrium theory, market processes could not possibly affect any variable in a manner that could be of lasting interest. By implication, this methodological dicta (what Leland Yeager 1995 calls a "tacit preachment") devalues the research agendas of the Austrian school, Post Keynesians, and most investigators of economic history.

In an important sense, the relatively recent, and very rapid, emergence of theories of "rational expectations" is an artifact of the methodological principle of a fixed and immutable structure. If the market process is of no importance, why not cut to the chase and assume that we can rapidly converge on the postulated equilibrium? Arthur argues, correctly, that this approach is flawed since we get into a muddle in the event that every agent in the economy is working with rational expectations but has a different idea of what the "true" underlying model is. In addition, even in the event that everyone is in agreement on the underlying structure of the "game," Arthur points out that if one's actions or beliefs are partially predicated upon what others may or may not do, rational expectations can still unravel (Arthur 1995, 1994b).

The Santa Fe approach to economic studies perceives the economy as a "complex adaptive system." As such, they are interested in understanding how it is that a coherent economic structure can spontaneously evolve out of the individual self-seeking activities of persons and firms with limited knowledge, where no previous structure and few "rules of the game" can be supposed. Largely through the method of computer simulations, they have demonstrated that the system can develop a relatively stable structure in a remarkably short period of time.[3] Moreover, their models demonstrate true evolution in the sense that the structure that appears is dependent upon the specific relationships and networks that emerged. It follows that this structure is not reducible to a single "representative agent" that already has embodied within him- or herself the information and predilections necessary to bring about a previously specified equilibrium position. In this sense, the economic system is genuinely "complex." In addition, complexity theory has the advantage of enabling economists to contribute to, rather than dismiss or denigrate, the extensive literature that argues that the behavior of human beings in groups follows a logic that is different from, and cannot readily be reduced to, the behavior of rational individual persons taken in isolation (Le Bon 1952; Kindleberger 1996; Smelser 1962; McKay 1841).

Again, it must be noted, even emphasized, that in models of complex adaptive systems economic agents will change their strategies as their environment evolves (Arthur 1994b). This separates this kind of system from models of "bounded rationality" such as that of Oliver Williamson (1985) or others who go under the banner of the New Institutional Economics. In these latter models, the structure of the economy is still fixed; it is just that agents have a limited understanding of this structure. In a complex system rational expectations would not be adequate since the qualities of the structure are unknown (and unknowable in advance) and our beliefs and strategies are partially predicated on the beliefs and strategies of others. Our expectations and strategies are state-dependent in a world in which we have to induce the moves of others and the current structure of the system, even as allies and competitors are doing the same thing and acting on the partial information they have gathered. In such a world, the possibility for competition along "Schumpeterian" lines is a very real possibility (Schumpeter 1955: ch. 2).

Since many aspects of our economic system include the formation of expectations and strategies under the conditions they describe, the work of the Santa Fe school is an important addition to our knowledge of economic relations. I would point to Arthur's model of belief formation in the stock market as an example (Arthur 1995). This is a useful rearticulation of John Maynard Keynes' insights into the formation of expectations in financial markets (Keynes 1936: ch. 12). The Santa Fe approach, with its restatement of the necessary rationality of induction, helps to solve an important intellectual puzzle: the continued use of trend analysis in financial markets despite the almost unanimous rejection, even ridicule, of this approach on the part of the overwhelming majority of academic finance economists.

Positive feedbacks in economic life

The Neoclassical school of economics, particularly in its general equilibrium formulation, deals almost exclusively with models that feature negative feedbacks. These structures, in conjunction with a specific understanding of price competition, are thought to return the system to a unique equilibrium point once it has been disturbed from a state of rest. In the class-room, analogies to nature are often made, such as the force of gravity or the disturbance of water in a puddle—where negative feedbacks guide the system to a state of rest after it has been disturbed. In general equilibrium, the presumed universal presence of diminishing returns provides us with the assurance that the system will gravitate toward a unique and stable equilibrium. The emphasis on these relationships in general equilibrium theory is so strong that it has led one philosopher of science to ask if economics is not really a "science of diminishing returns" (Rosenberg 1992).

However, in a system that evolves, path dependence plays an important role. Once expectations are formed and decisions are made, they create a new set of facts that may be difficult or expensive to unravel or renegotiate. Indeed, the entire point of the law of contract is to ensure that decisions and commitments are kept, since tangible costs are imposed upon other parties in the event that economic actors change their mind sometime between making and acting upon a commitment. The existence of sunk costs and path dependence are the reason that contracts have evolved as an important institution in modern market economies.[4]

It is also trivially true that economic activities generate specific sets of skills, consumer goods, and capital goods that would not have existed had different decisions been made. For example, in my own case, it is much easier and less costly for me to teach an economics class next semester than it would be for me to take on a job as a mechanic. However, this might not have been the case if I had made other choices some years ago—there is nothing in my set of "natural" endowments that makes me a professor over a mechanic. Had I continued on as a mechanic after getting out of the Army instead of going to college, the "relative cost" between engaging in these activities next fall would have been very different than it now is.

The cost structure one faces is dependent upon decisions that multiple individuals have made over previous years. This observation is true for businesses, cities, and even nations. In saying this, I am making a rather minor claim: that our past activities determine the choice set that we face today. After all, this is what choice is all about if the term is to have any substantive meaning, and it is one of the reasons that choices can be difficult.

Economic history acknowledges and describes the relationship between past decisions and current opportunities. For example, the city of Dallas is located where it is because a railroad being built across Texas went bankrupt at that location. Dallas was, literally, the “end of the line.” In an important sense, some forgotten bank employee who declined to renew the railroad firm’s loan was the unwitting founder of one of America’s great cities. Only economic history can explain the location of Dallas. Moreover, only a theory of path dependence can explain the self-reinforcing aspects of the development of major cities over the years (Arthur 1994a: chs 4, 6, 7; Jacobs 1969).

Notions of path dependence are not new to economics. Allyn Young (1928), Gunnar Myrdal (1957, 1944: ch. 9 and app. 3), and Nicholas Kaldor (1970, 1966), along with numerous others, have explored various aspects of their properties over the years. Moreover, it is not enough to point to the state of mathematics for an explanation as to why these early leads were not further formalized and developed. Indeed, I would suggest that path dependence within the economics profession provides a much better insight into the emergence, and professional status, of certain schools of thought along with the comparative neglect of others. It is well understood that the economics profession features self-reinforcing mechanisms that guide young scholars along paths that have already been well trod, and generates few opportunities for teaching or research to those who wish to explore alternative formulations (Klamer and Colander 1990). I would suggest that had more professional rewards been the lot of those who investigated the economics of path dependence over the past few decades, we could reasonably speculate that enterprising researchers would soon have developed a mathematics to discuss it, while assembling numerous examples of such phenomena at work in the American economy. As it is, several interesting (and provocative) papers and books dealing with the economics of cumulative causation have been published in recent years (cf. Elbaum 1990; David 1985; Tyson 1992).

Thus far, the contemporary discussion of path dependence has focused on the issue of technology and its diffusion. Without a doubt this is an important policy matter, and one that has properly received increased attention since the industrial policy debates of the early 1980s. It has been rejuvenated with the beginnings of the government’s interest in the impact of Microsoft on the further development of the software industry. However, the focus on technology is a rather narrow conception of this idea of “lock-in.”

One can also look at norms and institutions for examples of historical evolution with eventual "lock-in." According to the Institutionalist school, norms and institutions are subject to historical lock-in (Mitchell 1969: ch. 20, 1936; Tilman 1996: ch. 4). What happens is that society develops a number of social norms that help to address very real and concrete problems, but then retains these norms and institutions well after the time that they have served their useful purpose. An example would be John Commons' argument that modern conditions of production and the dynamics of modern labor markets reduced the Supreme Court's "liberty of contract" doctrine to an exploitative relationship in practice that required modification through collective bargaining and legislation (Commons 1924: ch. 8). However, one should not take the position that only the Institutionalist school developed a theory of economic institutions and their evolution over time. Economists within the Austrian tradition such as Carl Menger (1883), Friedrich von Hayek (1948) and, more recently, Viktor Vanberg (1994) have all made important contributions to the literature on the formation of economic institutions.

Lately, many economists and others have argued that the American economy is subject to a form of "regulatory lag" in the sense that the economy is burdened by old rules and standards that may have been appropriate to an earlier era but now inhibit opportunity and progress. In its general form, it is difficult to find fault with this argument. Which particular institutions, regulations, and norms are the ones that are antiquated and should go is, of course, the puzzle. Nevertheless, the emergence of the Santa Fe approach may provoke us to take another look at the institutional structure of the economy. Perhaps it will provide, in some cases, a more constructive reconsideration of regulatory policy than that which is typically presented by a too-ready application of the "public choice" approach.

Economic policy

With the end of the Cold War and the rise to hegemony of the Neoclassical school in such institutions as the World Bank, the IMF, the Clinton Administration, and the economics profession, we have heard a lot about "market-friendly" reforms. Typically, these reforms are entirely focused on the injunction to allow markets to freely adjust so as to "Get Prices Right." This is unfortunate since it displaces the discussion that should be taking place: good economic policy is about "Getting Institutions Right."

In complex economies, government action is valuable to the economy to the extent that government facilitates the emergence and reinforcement of reasonable and stable institutions that enable economic agents to more effectively pursue their own ends. In a world where path dependence exists, a number of alternative steady states can emerge. Some have argued that the government can, in some cases, nudge the economic system onto a more optimal path (Arthur 1996). It can be expected that there will be a significant debate over such claims. It is clear that the fact of complexity should offer a general warning that the government's ability to possess important micro-knowledge of trends within the current economic situation is rather limited.

Regrettably, the "Getting Prices Right" crowd too often has a limited understanding of market processes and institutions. In the case of the former communist countries of Eastern Europe, they significantly under-estimated the ability, coherence, and capacity of the state that is required if a market system is to work at all. One often hears about the "night-watchman" state, but it has taken the implosion of the Russian economy to teach us that the enforcement of property rights and contract law are neither simple nor insignificant activities. Stated simply, judges must be able to judge free of threats of murder. Such issues may appear trivial, but it must be emphasized that in a viable market society, debts need to be collected by constables, not goons (Holmes 1997). These facts were neglected in the hurried "transition" to capitalism in Russia. The result of such miscalculations may soon become catastrophic as we watch that society dissolve. While Russia's former economic advisors, in their comfortable tenured jobs, can be expected to distance themselves from these results, I am convinced that a knowledge of path dependence, economic history, and the formation of norms and institutions could have provided the transition economies with higher-quality advice than what was in fact passed along during the early 1990s.

Conclusion

Santa Fe's expansion of the economic project will be good news to Institutionalists, Post Keynesians, Austrians, and perhaps even Marxists, since they are the schools that have continued to explore complex phenomena such as market processes and comparative institutions despite the disapproval, and often intense opposition, of their colleagues within the mainstream of the Neoclassical tradition. As such, the emergence of the Santa Fe approach represents a positive trend in the economics profession. Like so many new ideas, it builds upon concepts that were once well known and accepted yet have been discarded over time. It also broadens the study of economics by bringing in insights from some disciplines that have been neglected by economists over the past several decades—namely biology and modern physics. Modern theories of evolution have staked out a set of problems that are both interesting and, I would submit, very important to understanding our economy and economic policy. These include the formation of stable economic relations, and the role that norms and institutions play in molding sound economic relationships that feature progress and prosperity.

Notes

1 An earlier version of this chapter was presented to the Annual Meetings of the History of Economics Society in Montreal, June 1998. The author would like to thank Falguni A.Sheth and David Colander for their assistance with this paper.

2 Brian Arthur garbles this quotation of Hicks'. However, he does so in a manner that is most interesting. According to Arthur, Hicks states that allowing for increasing returns would result in "the wreckage of the greater part of economic theory" (quoted in Arthur 1996:102). A comparison of Arthur's presentation of Hicks with the correct quotation as produced in the text above, indicates that Hicks explicitly states that the threat is not to "economic theory." On the contrary, the threat is to "GENERAL equilibrium theory." Hicks understood that Joan Robinson, Piero Sraffa, Michal Kalecki, and Nicholas Kaldor had other ideas about the direction that economic theory should go in. As a result, he understood that they would be less inclined to view the possibility of increasing returns as undermining economic theory in general.

3 This result represents the revival of an older, and unreasonably neglected, insight. The idea that the market system generates its own internal structure is closely related to Friedrich von Hayek's discussion of the emergence of "spontaneous order" within market systems (Hayek 1948: chs 1–6).

4 As a thought experiment, ask yourself why economic agents in a world of perfect information, perfect competition, and/or zero costs to recontracting, would want to expend resources on the articulation and enforcement of contract law. After all, if someone breaks a commitment, one could always go back in "time" and agree to engage in the same task with someone else.

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12

COMPLEXITY THEORY

An Austrian perspective

Michael R.Montgomery

Complexity theory is the most recent movement to rise up as an alternative to general-equilibrium-based, Neoclassical mainstream economic theory. The following seven statements summarize my view of its main differences with Neoclassical theory. (1) Where Neoclassical theory is built on linearization (like the nineteenth-century physics from which it came), complexity theory stresses the essential non-linearity of economic phenomena (Brock 1997:387; Arthur 1990:99; Arthur *et al.* 1997a:2–5). (2) Where Neoclassical theory maintains that (or models as if) aggregate behavior can be fully understood by understanding the behavior of a "representative agent" *viewed in isolation,* complexity theorists hold to the fallacy of composition: aggregate behavior, they maintain, cannot be understood without recognizing the "deep" interactions ("networks" or "nets") that occur between actors within an aggregate (Brock 1988:82; Arthur *et al.* 1997a:3–9; Katz and Shapiro 1986; North 1997:227; Krugman 1997:241–242). (3) Where Neoclassical theory stresses equilibrium as the fundamental concept of economic science, complexity theorists emphasize disequilibrium (or, sometimes, inherently multiple-equilibrium) processes as their foundation (Arthur 1988:9–13, 1990, 1996; Arthur *et al.* 1997a: 4–6). (4) Where Neoclassical theory embraces "rational" expectations, complexity theorists emphasize theories of expectations-formation stressing the adaptive, evolutionary, inductive, "groping" processes by which agents in the real economy actually learn (Arthur *et al.* 1987, 1997a: 7–11; Lane 1997; Tesfatsion 1997).

Additional propositions, less methodological and more directly relevant to policy, are: (5) where Neoclassical theory is (largely) built on the assumption of diminishing returns (or negative feedback) to production processes, complexity theorists emphasize that increasing returns (or positive feedback) to many such processes are both common and economically important (if not dominant) (Kaldor 1970; David 1985; Arthur *et al.* 1987, Arthur 1988, 1990, 1996); (6) where Neoclassical theory seems to operate in an ephemeral world where past decisions don't constrain current decisions and where institutions are fluid, complexity theory emphasizes path dependence, adaptive evolution, and the vital importance of institutional structure (references above; North 1997); (7) where Neoclassical theorists often arrive at simplistic free-market policy prescriptions which seem to come already built-in by their starting assumptions, complexity theory's policy implications are more, well, complex. In particular, complexity theorists claim to have identified new classes of "market failure" scenarios (based on propositions 5 and 6) which invalidate long-cherished free-market cases for free trade and for free-market choices of technology (references under proposition 6). Proposition 7 has, predictably, generated enthusiasm among interventionist economists, and displeasure among free-market economists.

The above is, of course, only a brief summary of complexity theory. From an "Austrian" perspective, there is much to like, and much to dislike, about it. Here I will try to briefly indicate what I see as the links to Austrian economics, as well as suggest some insights (and criticisms) that I think Austrian economics can contribute to complexity theory.[1]

Things for Austrians to like about complexity theory

On the surface, the ideas of complexity theorists and those of Austrian economics seem to have much in common. Austrian economists have long been partial to many of complexity theory's implicit criticisms of Neoclassical theory. Austrians have consistently criticized the "representative agent" of Neoclassical theory as being thoroughly unrepresentative of actual actors in the real-world economy. They have even more strongly rejected the notion that such an agent's behavior can proxy for collective behavior. Austrians have consistently and sharply criticized the Neoclassical mainstream for its dependence on the notion of "equilibrium," pointing out that true market activity is a disequilibrium process occurring through time under limited-information conditions. Austrians also have been in the forefront of the attack on the strident unrealism of models built around the rational expectations assumption. Finally, while Austrian and free-market-oriented Neoclassicals often have agreed on concrete policy issues, Austrians frequently have found Neoclassical arguments for free markets to be overly simplistic and sometimes dangerously misleading.

To see the links between Austrian economics and complexity theory more clearly, it is useful to look at some cases where the two schools reach conclusions that are eerily similar. One is the Hayekian conception of "spontaneous order" (for example, Hayek 1948, 1960: chs 2 and 4). Key institutions, and indeed civilization itself, are "the result of human action but not the execution of human design" (Ferguson 1767, quoted in Hayek 1960:57). Institutions evolve in an environment of widely dispersed knowledge—dispersed not only over space but also over time (as earlier generations contribute to today's institutional structure). Market institutions are merely one example: others are law, culture, and language itself. Just as nobody "decides" what will be produced in a market society, and yet the problem is solved superbly, so do society's institutions evolve into highly efficient structures which reflect the input of the dispersed knowledge/experience of millions of individuals. Putting Hayek into complexity-theory language: Hayek sees society and its institutions as a "net"—a joint product produced by the decisions of millions of individuals, distributed over both space and time, each seeking to solve only his/her problems but through this process contributing to the creation of a society's "structure." The principle of the net in this sense is highly emphasized in complexity theory, which seeks to build meaningful interactions between many individuals into the core of economic theory. Complexity theory also emphasizes a conception of society as an adaptive, non-linear, evolutionary system with substantial path-dependence (see, for example, Arthur *et al.* 1997a:4–5), where such path-dependence stems largely from the existence of slowly evolving institutions as a constraint on rapid change. This is another much-emphasized Hayekian theme (for example, Hayek 1960: ch. 4).

Other trends in Austrian thought also are strongly echoed in complexity theory. One parallel arises in the treatment of expectations. Austrians have always objected (except as a tentative heuristic assumption) to the view that so-called "rational agents" can usefully be conceived of as walking about with a little model of the economy's structure in their heads, which they use to automatically form "the best possible" forecasts of the future course of key economic magnitudes. They have objected to the Neoclassical school's minimalization of the vast information-gathering and -processing problems facing the typical "agent" in the real economy. *How* do "agents" come to know "the model"? What happens if "the model" changes? Are there types of information (say, "the knowledge of particular circumstances of time and place": Hayek 1948:81) to which agents cannot reasonably be expected to have access; and if so, what are the consequences to conceptualizing the process of expectations formation? Austrians have thought long and deeply about these matters (for example, Lachmann 1943; Kirzner 1979, ch. 9; Vaughn 1994: ch. 7), and have rejected the Neoclassical vision of "rational" expectations as being thoroughly Utopian—*essentially* in error. Complexity theorists agree, and have sought to build and simulate formal mathematical models incorporating some of these ideas (see, for example, the several references above from Arthur *et al.* 1997). It is safe to say that no school of thought today is as interested in incorporating more accurate treatments of how agents learn and process knowledge as is complexity theory. Austrians have good reason to applaud this focus.

Finally, there is macro-economics: a field where one's views of expectations formation go far in determining one's views about the economy's aggregate behavior. Early rational-expectations macro-economics focused on information problems as a possible key to understanding the business cycle (for example, Lucas 1981). The analysis was arid, mechanical, and highly abstract, but Austrians could at least take heart that knowledge problems—which are central to their view of the economy—actually were being taken seriously at some level by Neoclassical macro-economics. This Neoclassical focus was, however, short-lived. By the mid-1980s, the ruthless "efficient (macro-)markets" logic of the strong rationalexpectations hypothesis had crowded these limited-information theories into the wings of Neoclassical (macro-)research, while a still more arid, mechanical, and abstract approach—full-information-based real business-cycle models—had come to dominate center-stage. From the mainstream's perspective, the problem with limited-information models was that, given the logic of the "rational" expectations approach, it was difficult to see how, say, confusion about the size of the money stock could last long enough to generate a business cycle. Answering this objection in a series of important papers, Austrian macroeconomist Roger Garrison showed how, in "rational" expectations macro-models, the key issue was not lack of knowledge of the *size* of the money stock. Instead, it was—in a decentralized, information-diffused market structure—the lack of knowledge by *all* actors of monetary policy's impact on the affairs of most of the other actors (for example, Garrison 1984, 1992). That is, to assume a known "structure" of the economy, as the rational expectations approach assumes, is to assume away most of the essential problems of macro-economics—which after all is best viewed as just another (admittedly vast) coordination problem in a world of limited knowledge. Such a perspective on macro-economics seems shared by complexity theorists: they too, it is clear, are wary of Utopian assumptions concerning information, its acquisition, and knowledge about its best use.

So what's the problem?

Austrians and complexity theorists thus seem quite close on a number of important "core" theoretical beliefs about what is important in the economy and what ideas therefore should form the basis of (conceptual or mathematical) economic models. Are these two schools then best seen as fellow-travelers? Are they close cousins in the attempt to shift mainstream economics back towards a more "true-to-life" perspective?

Before concluding that they are, consider the following: (i) excepting some perfunctory references to Hayek, Austrian economists are little-cited by complexity theorists (and vice versa): more intellectual cross-pollination might be expected of two so allegedly similar schools; (ii) policy conclusions identified by complexity theorists as emerging from their insights are primarily interventionist (or else directly imply the possibility of beneficial intervention by a central governing body), a position thoroughly at odds with the Austrian tradition; (iii) to judge by the papers in the two volumes *The Economy as an Evolving Complex System I* and *II,* complexity theorists see their methodological task as one of diverting the Neoclassical mainstream to a new class of formal models, rather than either moving the mainstream away from the mathematical-modeling paradigm or, alternatively, increasing its tolerance for non-mathematical analysis. Indeed (following chaos theory), complexity theorists have taken their inspiration from non-linear methods used in recent physics, just as the earlier postwar generation of Neoclassical economists took their cue from the mainly linear-based physics of the nineteenth century (regarding this last, see Mirowski 1988, 1989).

Let us start with physical science as a model for social science. Austrian economists have long criticized the mainstream assumption that models designed to enhance understanding of non-conscious, natural processes should be starting points for study in the social sciences. Hayek (for example, 1979) even coined (or at least used) a special term, *scientism,* to describe this tendency. From the Austrian perspective, one of the enduring flaws in scientism is that, in often subtly misleading ways, natural science assumptions manage to slip into social science applications, con-taminating social science analysis with an inappropriate context.

The policy conclusions emerging from complexity theory illustrate the difficulty of avoiding these problems. Complexity theorists emphasize so-called positive feedbacks (or self-reinforcing mechanisms) as fundamental to understanding the modern technology-based economy. Two young, competing, roughly equally impressive technologies (call them A and B) start out with roughly equal market share. Due to, perhaps, some early good luck, Technology A acquires a small lead over B. Interlocking technology effects (other technologies develop that depend on the leading technology) and network effects (consumers benefit from using the product that has the most other users) then propel lucky Technology A into an even greater lead, which serves to further reinforce the same effects and further increase its lead. Ultimately Technology A's victory is likely to be near-total. But—and here is the crucial point—if we ran the process over again, Technology B might get the early luck, and it would reap the self-reinforcing effects and just as thoroughly triumph over Technology A. Moreover, assume that the initially unlucky technology is the one whose long-run prospects are better. Self-reinforcement effects may well eliminate that technology despite its long-run superiority, injuring society's well-being. On these grounds, complexity theory directly implies market failure and a consequent open door to useful government intervention to "correct" the failure.[2]

The path-dependent *physical* process that Arthur *et al.* (1987; see also Arthur 1988) put forward as a model of the above *market* process is the non-linear Polya (or urn) process. Start with an urn containing one red ball and one white ball. Draw randomly from the urn. If the ball is red, put it back in the urn and add another red ball. If white, put *it* back in the urn and add another white ball. Repeat many times. Notice that this process immediately gives an edge to the ball-color that wins the first draw—an advantage that increases if it also wins the second draw, and so on. Depending on early draws, the series quickly converges to a number between zero and one. But the probability distribution is uniform: Starting the experiment over again, the next convergence is equally likely to be anywhere between zero and one. The luck of the (early) draws dominates the location of the process' convergence point. The mathematics of a more general process (which Arthur *et al.* treat shortly after the above is discussed), seems mainly to imply an increased probability that a process will be governed not by a uniform distribution, but rather by a convergence to a fixed proportion (or to a set of such proportions). However, the basic principle of dominant path dependency in response to early events remains.

Urn processes are a useful way to describe and illustrate self-reinforcing mechanisms and their accompanying path dependency. I would suggest, however, that they are a highly misleading way to approach a *market* process. In order for such a physical process to even begin to adequately proxy for a market process, its complexity (ironically) would have to be increased markedly. A modified "urn process" follows that, I would argue, better captures capitalism (and the entrepreneur's role in it). Take an urn process similar to the above, but now occasionally let a large number of new balls of one color be added to the urn. Clearly, such an event would have a significant impact on the final outcome. This, however, is precisely what happens in a market process, where periodically "the cards are shuffled" by changes in market structure, new technologies coming on line, global and national shocks, and so on, and a firm's mettle is tested by how it responds to these forces. Further, in a market process, of course, the supply process of new balls would be unlikely to be very random, but instead would be more like the following. Assume a puckish overseer takes several hundred red and white balls and cleverly hides them all over a vast overgrown yard. Imagine two players (one red, one white) who can add as many balls of their color as they find during any "round." One player sits on the porch fanning himself and drinking lemonade, using his binoculars and making occasional forays based on what he sees through them. The other player not only has outstanding eyesight, but is eagerly searching all over the yard, maybe even coming up with new, innovative ways to improve search efficiency. Now if you can put as many balls as you find in each "round" into the urn, who's going to win and control the process?—especially if you add the additional assumption that bad "search" decisions might be penalized by the *loss* of balls.

There is certainly path dependence here, but it is an *earned* path dependency, not an unearned one due merely to early good fortune. Due to ability and work ethic, the winner of the first round of the "ball hunt" is surely likely to win the subsequent hunts as well. The reason why, however, is not inertia, but rather simple ability and stick-to-it-iveness. Certainly the person with the strategic vision to make right decisions in the early "rounds" is likely to benefit also in later "rounds" from possession of such vision. This phenomenon, however, is quite different from "locked-in," deterministically generated path dependence as envisioned in the complexity theory policy discussions.

I am aware that Arthur's point with his urn model is to show how, even if two technologies were essentially equal at the start, one still might quickly develop a large advantage due to path dependence. And, so long as the model is used heuristically to illustrate just this point, I think it is an interesting teaching *tool,* one that certainly stimulates thought. But to jump from such a rigged model directly into in-depth policy discussion is, it seems to me, *methodologically invalid.* Such a strategy fails to focus on what aspects of the real-world phenomenon under study are not captured by the model: In this case (it seems to me), virtually all the important stuff.

The Arthur/David/Krugman "QWERTY" conclusions, which assume that market success and failure can be "front-loaded" at the start—like playing an equal opponent in chess while he spots you a rook—are certainly not consistent with the Austrian notion of the *market process.* Single decisions, and certainly single lucky events, are unlikely to determine the fate of a product (or a technology). The market is a process occurring through time: a successful business must make *sequences* of correct decisions in order to expand (or maintain) its market share. Correct decisions enhance market share; incorrect decisions reduce one's advantage. Entrepreneurial judgment, where economic actors "grope" their way to correct decisions more often than their competitors (cf. Kirzner 1985), seems to me to be a far more likely source of industry dominance than is the front-loaded self-reinforcement of early good fortune.

The uniqueness of "hi-tech"?

As the above discussion should make clear, I have some serious concerns with the policy conclusions that seem attached in the complexity literature to the twin ideas of path dependence and self-reinforcing mechanisms. My objections are largely the same as they would be if arguing with an economist from an earlier era who tried to convince me that "successful firms usually get bigger, which allows them to reap economies of scale and get more successful, which eventually leads to their taking over an industry (so let's prosecute 'em!)." On the contrary, big firms tend to become dinosaurs, not juggernauts, without significant virtues other than mere "bigness." Large firms are harder to manage as their sheer size raises managerial coordination costs to the point where decreasing returns to scale (increasing long-run average costs) begin to set in. Management culture changes from an offensive to a defensive mindset. Bad decisions are made, and management is one area where bad decisions tend to "feed back on themselves'—for example, a bad crucial hire promptly hires three more bad people who destabilize three previously healthy corporate sectors, which in turn…(Two can play at this feedback-loop game!)

In an industry where technology moves fast, all these problems are exacerbated by encroachments by new products and sharp-toothed minnowsized "niche" firms. And let us not forget that big firms have also "earned the privilege" of having the chance to make big mistakes. And they do: In the early 1970s Ford Motor Company built the huge Flat Rock iron-casting plant, which subsequently proved too large and helped put the company in a deep financial hole (cf. Koten 1981). In fact, American industrial history is replete with tales of large firms that make a series of missteps and find themselves drastically diminished accordingly.

I do not see how changing this example so that the economies of scale come from, say, interdependent technology networks, instead of the sheer size of, say, Carnegie Steel, alters the relevance of the above points very much. Arthur (1996) maintains that society's industry can, nowadays, usefully be split up into two fundamentally different "worlds": the "processing industries," where "roughly speaking, diminishing returns hold sway" (ibid.: 101); and the newer "knowledge-based industries," where "increasing returns reign" (ibid.). Arthur holds that "the two worlds have different economics" (ibid.), although at several points later in the essay he softens his distinction a bit, noting that "the two worlds are not neatly split" (ibid.: 103).

I would go further and say that there is no fundamental distinction. The "limitations" that "processing firms" run into when they try to expand is not so much something like "access to raw materials" (ibid.: 102), but instead, as intermediate micro books teach, increasing costs of coordination at the managerial level. Technology firms are as subject to these "constraints" as are processing firms. On the other hand, technology (that is, innovation) has played a remarkable role in the "processing" industries during the second half of the twentieth century. The postwar increase in US agricultural productivity, for example, is the wonder of the world. Innovations such as horizontal drilling and new detection technologies have revolutionized the petroleum industry. A meat-packing plant is a wholly different animal today than it was in 1980. Invention, entrepreneurship, "groping" toward innovation and its successful implementation—and the bearing of costs if these gambles go wrong—these are essential, vitally important parts of the market process in *all* industries, not just in hi-tech. The Arthur position that the world has been turned upside-down by the computer revolution may hold some water, but the related position that the *economics* of the world has fundamentally been altered seems to me dubious.

"Lock-in": technical vs. economic efficiency

I also take issue with what might be called the "QWERTY story," perhaps the most celebrated application of complexity theory (David 1985; Krugman 1994). "QWERTY" is the story of the evolution of the typewriter, whereby an inferior key layout is alleged to have gotten ahead of other layouts (specifically, the Dvorak keyboard) due to factors associated with the early years of typewriting. By the time these factors had disappeared, the keyboard layout was already standard, so that even though it was inferior to other layouts, its user base had expanded to the point where it has continued to dominate the market. Other QWERTY examples include the operating system DOS, the old English weight-measures in the US (pounds, ounces, and so on), and similar cases. Increasing returns and their accompanying self-reinforcing mechanisms are alleged to cause "lock-in" of a demonstrably inferior technology. Society thereby pays the price of this inefficient free-market decision: Recommended government action is intervention to correct the "market failure."

In evaluating the "case for QWERTY," I am reminded of another of the basic distinctions of micro-economics: that between *technical* and *economic* efficiency. Technical efficiency—the first love of engineers and other technologists—is achieved by acquiring the best-performing product technically. That new Porsche 911, that $8000 top-of-the-line laptop "screamer," that spectacular bridge made of the absolute finest materials no matter what the cost—these are technically efficient products. The economist has, from time to time, been charged with the unpleasant task of bringing the engineer down from the mountain-top and explaining that, while, yes, this is a wonderful product, it simply costs too much to build and so must be priced so high that it won't be bought (or more generally, won't be bought enough) no matter how great it is technically. Buying it would cost too many people too much of other goods: the technology gain, while desirable in the abstract, is too expensive when priced in opportunity cost terms.

I would suggest that the QWERTY keyboard and that DOS "clunker" (Arthur 1996:106) are examples of technically inefficient, but economically efficient, products—at least for the large majority of users. How is my worklife improved if I switch keyboards and my typing speed increases by, say 10 percent, but my typing speed accounts for only 1 percent of my total productivity? What do I, the typical word-processing, spread-sheeting, working stiff, stand to gain from enhanced capabilities that have an impact only on the very fringe (if that) of my working life? Computer engineers (and technologists) are contemptuous of that "kludge" DOS

(Arthur 1996:102), just as auto engineers are contemptuous of Fords and Chevys. That arguably suggests technical inefficiency, but not economic inefficiency.[3]

I suspect that the reason technologies like QWERTY and DOS have survived as long as they have is precisely that the costs of staying with these (technically) inferior technologies are relatively small, so that they retain substantial degrees of economic efficiency. By contrast, consider a counterexample. Suppose pathbreaking new speech-recognition technologies (allowing you to dictate directly into your computer) become perfected, but to use them effectively a Dvorak keyboard layout is (mysteriously) needed to set up the program and a brand new operating system is required to run it. How long would the QWERTY/DOS era continue under those circumstances? I would suggest the answer is: Not long at all.

Selective self-reinforcement

Another feature of complexity theory that is striking is the rather specialized application of the "self-reinforcing mechanism" idea in the literature on path dependency. I have already discussed above certain negatively self-reinforcing mistakes that can occur within the firm which would likely more than offset a technological advantage with which a firm begins a competitive process. A market process viewed from the firm's perspective is a long sequence of interlocking decisions: bad decisions often feed back into further bad decisions, leading to firms losing seemingly impregnably strong market-positions.

Another possible countervailing "feedback mechanism" which has received insufficient emphasis relates to the nature of government. Suppose that society, impressed with the dangers of technology "lock-in," asks government to assume the role of intervening in cases where free markets would be likely to "make the wrong choice" and lock society into the wrong (inferior) technology (Krugman 1994 is especially enthusiastic about the possibilities here).[4] We assume that government, armed with knowledge of the "new economics" of the technology-based increasing-returns world, is capable of making such an observation—a big IF, as the Austrians repeatedly remind us—and is capable also of acting promptly and effectively to correct the problem. We assume further—another big IF, as the public-choice school repeatedly reminds us—that government initially *wants* primarily to correct the problem and not (perish the thought) to engage in various wealth- and/or vote-garnering schemes under the cover of saving society from technological obsolescence (or from unfair trading nations).

How long before the resulting "net" of government technology regulators begins to meet, interact, exchange ideas, and convince itself of (i) its indispensability, (ii) its near-omniscience, and (iii) its pressing need to add staff, increase its budget, and expand its vitally needed presence into new dimensions of the technology question—or into new areas altogether? (See, for example, the recent activities of the Food and Drug Administration, e.g. Bovard 1994, *Wall Street Journal* 1998.) That is, government agencies also can be visualized as self-reinforcing "nets" of like-minded individuals: this is, after all, what people mainly mean when they talk about a "Washington culture." In several case studies, Twight (1993, 1995) has documented how new government agencies, once in power, aggressively move to control information flows and raise political transactions costs for those who seek to rein in their activities. These are merely the insights of the public-choice school, applied with a complexity theory twist—but they imply a very different appropriate regulatory regime than the benevolent and idealistic government model which seems to be the working paradigm of the more policy-oriented complexity theorists. Complexity theory, it seems, would benefit from more integration of Austrian and public-choice insights into its core.

Concluding thoughts

Since the last few sections have been largely critical, I want to close with some positive thoughts. First, it is evident that complexity theory is a potentially revolutionary development in economics, one that should dramatically impact twenty-first century economic theory, and very much for the better. While I do not accept the policy implications attributed to increasing returns in the complexity theory literature cited above, these are not an essential part of the movement. The broad theoretical themes stressed in this literature are its core, and these seem to be *"exactly"* what is so crucially missing from so much of present-day mathematical economic modeling—particularly macro-economic modeling. In particular, the idea of self-reinforcing forces associated with increasing returns, broadly defined, seems an important, long-neglected topic in the Neoclassical mainstream (largely neglected in macro-economics at least since the work of Wesley Mitchell 1913[5]). Complexity theory can help restore some long-needed balance and real-world relevance to a mainstream that has been all too willing to adopt diminishing returns as a bedrock principle due chiefly to its nice mathematical properties and general "tractability."

I accept the Arthur principle that, in an industrial society based on technology (that is, based on human innovation), the idea of diminishing returns as an operative constraint on business activity is of far less significance than one would conclude from mainstream models. I believe that, among other virtues, such a framework taken seriously would open the door to some fundamental Austrian insights. Among the most important of these is the idea that the pervasiveness of capital, and its *essentially* heterogeneous nature, matters both "micro-economically" and "macro-economically" (if that distinction means much any more), and must be fully incorporated into useful models and conceptualizations of the economy.

I define capital quite broadly as any stored-up stock of productive potential. Clearly, the acquisition of such a stock—any such stock, whether a machine, a new skill, or a joint product emerging from coordinated activity among many individuals—must give to its possessors a production range over which increasing returns can be reaped. I would suggest that the failure to fully recognize this fact and its weighty implications for economic theory is one of the primary reasons why the Neoclassical movement has lost some steam (or at least the intellectual high ground) in recent years. It is high time—especially in macro-economics—that we leave behind the tired diminishing-returns-based abstraction of capital as something one can instantly rent and just as instantly return when one is done with it, and recognize instead the *complex structure* that heterogeneous capital implies (and imposes) on industrial economies. These are Austrian themes *par excellence* (cf. Hayek 1975; Lachmann 1947; Garrison 1991; Montgomery 1995), and their exploration could have a decisive impact upon the economics of the twenty-first century. If complexity theory helps steer economists in *these* highly productive directions (as I think it must), then "let a thousand flowers bloom."

Notes

1 In associating my views with that of an "Austrian perspective," the reader should keep in mind that I write more as someone familiar with the Austrian view than as a representative of the Austrian school. For the views on complexity theory of one who is an authority on the Austrian school, see Vaughn (1997).

2 I suspect that this is one reason why complexity theory has been so well received by some Left-leaning economists, who see it as a new, "hi-tech" lever with which to pry the mainstream away from its perceived free market biases.

3 The half-empty glass can also be seen as half-full: for example, despite its well publicized weaknesses, it is remarkable how adaptable DOS has proved to be over its long life.

4 Arthur (1996:106), on the other hand, when commenting on the Microsoft antitrust case, warns against hastily naive policy applications of the pathdependence principle.

5 It is tempting to refer to Keynes' (1936) *General Theory* in this context, due to the multiplier principle. On the other hand, we must remember that the assumption of diminishing returns to capital pervades much of the text and directly accounts for much of the extraordinary (and, with the passage of time, clearly quite muddled) analysis and policy pronouncements in the second half of the book (for example, chs 16, 17, and 24).

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