A History of Modern Economic Analysis

ROGER BACKHOUSE

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Preface

The aim of this book is to tell the story of how economic analysis has reached its present state. To do this it is necessary to devote most of the available space to developments which have taken place since the 1870s. One disadvantage of such an approach is that the coverage of eighteenth and early nineteenth century economics, which has traditionally taken pride of place in many treatments of the history of economic thought, must inevitably be compressed. If we wish to understand the nature of the discipline of economics as it exists today, however, this is a price worth paying.

Concentrating on twentieth century developments also creates two further problems. The first is that because both the number of economists and the quantity of economic literature have increased so rapidly during the twentieth century, especially since the war, it has been necessary to rely more heavily on surveys and on secondary literature than is the case with earlier periods. Thus, though I have attempted to refer back to the original sources as much as possible, this is a book which could not have been written but for the large quantity of secondary material on the subject.

The second problem is posed by the very technical nature of much twentieth century economics. When considering, for example, classical economics, it is reasonable to assume that students know enough economic theory, and enough mathematics, to understand many of the technical issues involved. When we consider on the other hand, topics such as post-war general equilibrium theory, or social choice theory, as we must do if we are to provide a broad perspective on modern economics, technical issues cannot be discussed in the same way. It is therefore necessary to try to explain what economists have done, or are doing, without getting into technical details any more than is necessary.

In considering twentieth century economics, a thematic treatment is essential, for it is the only way to show how ideas have evolved over time. This is particularly true of the period since 1945: the number of economists involved makes it inappropriate to tell the story in terms of a few leading individuals. This approach, though necessary, has the drawback that it becomes harder to get a picture of the work of individual economists who have contributed to a variety of fields. Samuelson's work, for example, is discussed in virtually every chapter in Part IV.

Though I have attempted to adopt a cosmopolitan outlook, the emphasis is, with notable exceptions, on English-speaking economics. This reflects, to some extent, the fact that whilst the "leadership" of the economics profession was, at the turn of the century, shared amongst many countries, it has increasingly become located in the United States. The exception to this attempt to adopt a cosmopolitan attitude is the two chapters on Economics and Policy, where I have confined my attention to Britain. The dependence of economic policy-making on the economic and political environment, in a way not true of economic theory, makes it necessary to concentrate on a particular country, and I have chosen to cover the one most familiar to me.

The emphasis of this book is on developments within the "neoclassical" mainstream of economic theorizing, dominant since the time of Marshall. This approach to economics, however, has not gone unchallenged, and so two chapters are devoted to alternative approaches. This organization of the material should not, however, be taken as implying that what lies outside these two chapters constitutes a monolithic orthodoxy. Not only are the boundaries of "mainstream" economics almost impossible to define satisfactorily, but even within them there has been enormous variety. Although I use the terms "mainstream economics" and "alternative approaches", this is to make the story easier to tell, rather than because any definite dividing lines exist.

In writing this book I have received an enormous amount of assistance. Published work to which I am indebted is, I hope, acknowledged in footnotes, or in the Bibliographical Note at the end of the book. In addition, numerous colleagues have suggested references for me to use, or explained various points for me. Thanks are due in particular to those who have read and commented on drafts of particular chapters: Peter Cain, John Cantwell, Mark Casson, George Catephores, Bob Coats, David Collard, John Creedy, Les Fishman, Rick Garside, Paul Grout, Stephen Hannah, Geoffrey Harcourt, Terence Hutchison, Jan Kregel, Stephen Littlechild, Prasanta Pattanaik, Douglas Rimmer, Somnath Sen. They have provided me with many ideas, and helped me remove many errors and ambiguities. The person who must be singled out, however, is Denis O'Brien, who read a draft of the entire book, and whose comments have enabled me to improve virtually every chapter. Neither he, nor any of the others, however, is responsible for any errors or inadequacies which may remain. Though others may be responsible for many of the book's good points, I alone am responsible for its shortcomings.

Finally, thanks are due to my wife, Merida, and my son, Robert, for putting up with my seeming to prefer my word processor's company to theirs.

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Introduction

1.1 THE HISTORY OF ECONOMIC ANALYSIS

Schumpeter, in what must be regarded as the classic work on the history of economic analysis (1954), defined his subject matter as

> the history of the intellectual efforts that men have made in order to *understand* economic phenomena or, which comes to the same thing, the history of the analytic or scientific aspects of economic thought.[1]

Though this defines a subject matter somewhat narrower than the history of all economic thought, it is wider than simply the history of economic theory: historical and statistical work, for example, are also included.

The history of economic analysis is important for several reasons, some applicable to any science, others specific to economics. Amongst the former there is what Schumpeter described as the highest claim that could be made for the history of any subject, "that it teaches us much about the ways of the human mind".[2] Of more direct relevance, however, is the need to place contemporary economics in perspective. Like most other disciplines, the structure of economics was neither planned nor rationally thought out. It simply grew and developed as economists pursued new lines of inquiry, dropped or modified old ones, developed new techniques, and so on. Even within particular branches of the subject we find different, and not always compatible approaches coexisting with each other (within microeconomics, for example, the theory of the firm and the theory of general competitive equilibrium). Studying the history of these ideas, seeing how and why they developed as they did, puts them into perspective.

The history of their subject is particularly important for economists, for two reasons. The first is that, unlike the situation in the natural sciences, the subject matter of economics is constantly changing. Not only are the issues with which economists are concerned changing in response to political and social changes, but the economy is itself changing. The structure of the British economy, for example, is very different now from the way it was at the time Adam Smith was writing. In addition, human behaviour itself cannot necessarily be assumed to be unchanging: as people become aware of new possibilities, (for example, when a new statistical regularity is discovered) they may alter their behaviour. Because of all these changes a historical perspective is more important in economics than in the natural sciences.

The second reason why the history of economics is so important concerns methodology. One of the characteristics of economics, *vis-à-vis* the other social sciences, is the large body of formal, abstract theory, much of it formulated mathematically. Despite this body of theory, however, there is substantial disagreement over its interpretation, and over the criteria which are to be used in deciding which parts of it to accept, and which parts to reject. Though most economists would subscribe to some notion of empirical testing, interpretations of this vary widely. The history of economic theorizing can be used to pursue some of these methodological issues. It is because of the importance of these methodological issues that some philosophical issues are considered next.

1.2 SOME CONCEPTS FROM THE PHILOSOPHY OF SCIENCE

Falsificationism and the growth of knowledge

Perhaps the most important question in the philosophy of science concerns the relationship between scientific theories and empirical evidence: how can empirical evidence be used to appraise a scientific theory? Here the most important contribution, as far as most economists are concerned, has been that of Popper.[3] Central to Popper's approach to science is the concept of *falsification*. His argument is that empirical observation can *never* establish that a scientific generalization is true, for, however much evidence we obtain in support of a theory, we can never be sure that the next observation will not turn out to be inconsistent with the theory. All that successful testing of a theory can do is fail to refute the theory. Such successful testing of a theory may be regarded as "confirming" the theory, in the sense that it increases our confidence in it, but this is not the same as proving the theory to be true. This impossibility of verifying a theory through collecting empirical evidence is the so-called *problem of induction*.

Popper's solution to the problem of induction involves arguing that although empirical observation cannot be used to verify a theory, it can be used to refute it. He thus argues that the crucial characteristic of a scientific theory is not verifiability, for finding evidence to confirm a theory is easy, but *falsifiability*. He thus uses falsifiability as the criterion with which to distinguish between science and non-science. Scientific statements, for Popper, are, at least in principle, falsifiable: there are certain events which, if they occurred, would be inconsistent with the theory. Non-scientific statements, on the other hand, are unfalsifiable, in that they do not rule out the occurrence of anything. Marxism is thus, for Popper, unscientific, for its adherents can always reconcile whatever happens with the theory.

In addition to providing a "demarcation criterion" for distinguishing science from non-science, Popper's emphasis on falsification leads him to stress the growth of scientific knowledge. Scientific knowledge, according to Popper, is not knowledge that has been established as true, but simply generalizations which have, so far, survived attempts to refute them. Science progresses by progressively eliminating false hypotheses, a point of view which stresses the growth of knowledge. Popper's contribution has been aptly summed up as being to replace

> the central problem of classical rationality, *the old problem of foundations*, [how we can know our knowledge is true] with *the new problem of fallible–critical growth*.[4]

The situation is, however, more complicated than the above account suggests, for falsification is always problematic. To see why, consider an example from economics: the hypothesis that the demand curve for bananas slopes downwards. If someone has produced empirical data to suggest that it slopes upwards, there would be many reasons why economists might refuse to accept that the theory had been refuted. (1) Doubts could be raised about the data – were price and quantity measured correctly? (2) The statistical procedures might be questioned – had a supply curve, rather than a demand curve been estimated? (3) Questions could be raised about the *ceteris paribus* condition – perhaps incomes changed, or tastes shifted for some reason? (4) Finally, there would be the question of whether or not the theory was correctly formulated. These details are less important than the general lesson that because it is *always* possible, in Popper's words, to "immunize" any theory against criticism, rejection of a theory becomes a matter of decision.[5]

From here the argument can be taken in several directions, two of which are relevant here. One direction is that taken by Popper, who suggested that scientists should adopt the methodological rule of refusing to adopt *ad hoc* strategems to save their theories. At the same time, however, he recognized that such a rule would have to be applied carefully, for if no one were protective towards theories, a new theory might be abandoned too soon, before it had had time to make its contribution to science.[6] The other line of argument is to investigate more closely the circumstances under which theories are rejected or accepted. If this approach is adopted, a much wider range of factors becomes relevant. For example, unanimity concerning refutation of a theory could be reached simply by "expelling" all those who disagree, by declaring them "cranks", whose opinion does not count for anything. The sociology of the scientific community is thus relevant. This approach underlies the work of Kuhn, whose ideas will be considered next.

Normal science and scientific revolutions

Normal science is the fundamental concept in Thomas Kuhn's account of the growth of scientific knowledge. He defined it to mean "research based upon one or more past scientific achievements that some particular scientific community acknowledges for a time as supplying the foundation for further practice."[7] As examples of such *exemplars* or *paradigms* he cites Aristotle's *Physica*, Newton's *Principles* and Lavoisier's *Chemistry*. For a scientific achievement to form the basis for further research in this way, it must have two characteristics: it must be sufficiently unprecedented to attract an enduring group of adherents; and it must be sufficiently open-ended to leave all sorts of problems for scientists to solve.

Normal science has several important characteristics, the main one being the abandoning of critical discourse in the sense that there is a set of assumptions which are not questioned, and a set of procedures which are followed. This is the *disciplinary matrix* within which normal science is carried on. In undertaking normal science, scientists are not following a series of explicit rules, but they are following an example. Provided the initial scientific achievement, and the results obtained, are accepted without question, rules are not needed.[8] Even if they were desired, suitable rules to govern the conduct of research might prove hard, if not impossible, to articulate.[9]

Far from such an uncritical attitude being a problem, as might be inferred from Popper's theory, it is only such an uncritical attitude which, according to Kuhn, permits the application of the theory to a large number of problems, enabling a large number of detailed aspects of the world to be investigated. If scientists spent all their time arguing over fundamentals, they would never manage to investigate many "small" phenomena. Within normal science, therefore, most research takes the form of "puzzle-solving". Puzzle-solving is research where the results are generally known beforehand, where it is known that there is a solution, and which operates within certain rules.[10] Kuhn divides such puzzles into three main areas: establishing facts (these being required either because they are interesting in their own right, or in order to help confirm the superiority of the paradigm involved over another); applying the paradigm to new areas; and reformulating the ideas involved in the paradigm, the first articulation of which may well have been clumsy, or difficult to apply to certain problems.

Such normal science has implications for the nature of the scientific community. Acceptance of a particular form of normal science leads to a more rigid definition of a field of research, those who do not accept its basic assumptions being excluded from the relevant scientific community.[11] Education in the subject becomes learning to solve the puzzles produced by the paradigm, and because of the shared assumptions within the group, textbooks can become important.[12] At the same time, professional competence is judged in terms of ability at solving the research puzzles produced by the paradigm, for failure to solve a puzzle does not discredit the paradigm so much as the scientist who fails.[13]

For much of the time normal science can, according to Kuhn, progress satisfactorily along these lines, but from time to time *crises* arise. The main element in a crisis is the discovery of *anomalies*: awkward facts which cannot be explained in terms of the paradigm.[14] Much of the time anomalies can be ignored: they are simply facts that the theory cannot yet explain. An anomaly produces a crisis either when it concerns something that is fundamental to the paradigm, or when it is particularly important for external reasons – it may be, for example, that scientists are failing to explain something that the public expects them to explain. This failure of a paradigm may, Kuhn argues, produce bewilderment amongst the scientists concerned, for they do not know how to put it right: they can no longer be guided by the paradigm.[15]

Alternatively, a crisis may arise, not because a paradigm cannot explain some awkward fact, but because the modifications required to the theory render it transparently unsatisfactory. The classic example of this is pre-Copernican astronomy, in which the movements of the planets could be explained, but only through introducing more and more complicated systems. The problem was that the complexity of the system was increasing much more rapidly than the accuracy of its predictions, thus making it clear that something was fundamentally wrong with the whole system[16] Finally, an anomaly may provoke a crisis simply because it has persisted for a sufficiently long time.[17]

The result of a crisis is a large number of *ad hoc* modifications to the theory concerned, and of divergent articulations of the paradigm.[18] There may be confusion as the basis for the subject is undermined. Scientists search apparently randomly for answers, even turning to philosophy, something for which there is little place in normal science.[19] Eventually, from these new articulations of the paradigm, a new exemplar emerges. This involves a reconstruction of the field from fundamentals, and a new period of normal science emerges.[20] For Kuhn, therefore, it is only in such periods of *revolutionary science* when the fundamentals of the science are questioned, that the Popperian idea of theories being tested through confrontation with empirical evidence is applicable.

A scientific revolution in Kuhn's sense involves the replacement of one paradigm with another. There is continuity in that it is the unanticipated novelty produced by one paradigm which provides the basis for the new theory and the new paradigm.[21] At the same time, there is a break with the past, a break which, Kuhn argues, involves more than simply the replacement of one theory by another. Not only does a change of paradigm involve a change of world view,[22] but it also involves decisions which cannot be made on the basis of logic and evidence alone.[23] The reason for this is the "nonsubstantive differences between paradigms".[24] Because normal science cannot provide the information needed to make a purely rational choice between two paradigms, crises in normal science "are terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch".[25] Because of the non-rational elements involved in the switch from one paradigm to another, it becomes difficult to speak of scientific progress except within a single paradigm.[26]

Scientific research programmes

Kuhn's response to the problem of determining the circumstances under which the incompatibility of a theory with empirical evidence led to the theory's being rejected led him to investigate the way scientists actually behave. Though he finds reasons for liking the pattern of scientific activity described by his term normal science, he is moving away from Popper's search for a normative theory, of how scientific activity ought to be conducted, into an investigation of how it is conducted. A different