Introduction: A Sociological Interpretation of the Modern History of Economics

The Purely analytic scientist becomes so accustomed to seeing matter as a demonstration of certain verifiable or falsifiable principles that he lives at one remove from it. Between him and the real world springs the law, the explanation, the necessity to categorize. Everything Midas turned to gold, everything this kind of scientist touches turns to its function in his analysis. . . . The Complexity of the modern sciences is such that specialization is essential; not only in the interest of scientific or industrial efficiency, but in the nature of the mind's capacity. The scholar in many fields is extinct; not because the desire to be such a scholar is extinct, but because the fields are too many, and too complex.


Overview

This book analyzes a struggle between two schools of economics in the period between the world wars. The two schools are the neoclassical school, which emerged at the last third of the nineteenth century, and the institutionalist one, which had started with the works of Veblen and Commons at the end of the nineteenth century and had enjoyed a short period of prosperity in the interwar period before rapidly declining after the Second World War. Current historians of economic thought usually ignore the institutionalists or consider their movement to be an ephemeral and inconsequential episode in the history of economics. My story, however, reveals that the rise of the institutionalist school was an important chapter in the history of economics and has had lingering effects on the practice of economics to the present.

By analyzing in detail the neoclassical-institutionalist conflict, I wish to achieve three goals. First, I hope to expose a forgotten chapter in the history of economics. Historians of the period commonly assume that pre-Keynesian economists were strong supporters of *laissez-faire* policies, who continued to oppose fiercely any intervention of the state in the man-
agement of the economy, despite the protracted Great Depression. It is commonly argued that it was the Great Depression which proved the inadequacy of neoclassical theory, and that it was Keynes who managed to find a better theory (e.g., Lekachman 1966; Heilbroner 1972, chap. 8; Hall 1989; Blaug 1990b). As we will see in chapter 2, this presumed gap between pre-Keynesian neoclassical economics on one hand, and Keynesianism on the other hand is grossly distorted. But my focus is on another aspect of pre-Keynesian economics, which is completely missing or seriously distorted in many works on economic thought during the first half of the twentieth century.

This study shows that the institutionalist economists, who were quite numerous at the time, strongly attacked that economic theory which proscribed state intervention. Marshall and his students had already criticized the notion of laissez-faire, but the institutionalists went much farther. The support of most American economists in intervention preceded the Great Depression.² The experience of a managed economy during World War I strengthened the belief of many economists that coordination and planning could increase economic productivity.³ The fact that many of the American economists in the interwar period espoused institutionalism may change our conception of the intellectual history that led to the New Deal and to the emergence of the American welfare state. At the same time it makes problematic some phenomena which have looked “natural” so far. These include the support of many economists for later conservative economic policies and the highly abstract nature of economic science after the Second World War. The current so-called “neoclassical school” cannot be conceived of as the natural outgrowth of the prewar Marshallian neoclassical school. From certain perspectives these “neoclassical” schools appear more antagonistic to each other than Marshallian neoclassicism and the institutionalist school that fought over the discipline before the Second World War.

Important as these historical theses are, I hope that this analysis of interwar conflicts in economics has important implications for a broader set of theoretical issues concerning the nature of scientific practice and the forces behind changes in scientific knowledge. My second goal in this work is to use the case of the struggle between institutionalism and neoclassical economics to demonstrate the merits of the constructivist approach in the sociology of science, and especially the actor-network analysis of Bruno Latour, for understanding the nature of scientific practice in economics. Although I am not suggesting here an explanation of the changes in economics, the story I tell is relevant to models of “progress” in scientific knowledge. In economics as in other fields, historians and methodologists have used Kuhnian and Lakatosian perspectives to make sense of the history of their field. These reconstructions of history, I will
INTRODUCTION

show, are based on selective reading of the history and on arbitrary definitions of the boundaries of economics. Furthermore, they impose obsolete conceptions of science on the practice of economists and use value judgments instead of impartial analysis of the history. Although the constructivists are conscious of the fact that no historical account is free of biases, they seek to explain the development of science not on the basis of what present-day practitioners consider “good science.” I will argue that my constructivist account is more compatible with the full history of economics and that it avoids many of the problems that Kuhnian and Lakatosian accounts have met.

Instead of explaining change solely by the accumulation of problematic anomalies (Kuhn), or by the exhaustion of the ability of a research program to generate innovations (Lakatos), the cause of change implicit in Latour’s scheme is conflict and negotiation among various players, economists and noneconomists alike. The neoclassical-institutionalist struggle is a good illustration of this. The rise of institutionalism after World War I could indeed be attributed either to anomalies which made it difficult to accept neoclassical theory, or to the depletion of new, interesting things to say from that perspective. The constructivist view, however, would not deem these explanations sufficient. It would inquire as to (1) why certain phenomena were perceived anomalies by some, but not all, economists, (2) why certain anomalies were considered important enough for some economists to invalidate neoclassical economics, and (3) why some economists stopped believing in the potential of neoclassical economics to yield more important findings. The answers to questions of this sort are to be found in the beliefs of individual scientists, the negotiations among them, and their capabilities to mobilize various resources and to forge alliances to promote their opinions. This view does admit the cardinal role of cognitive factors in decisions scientists make, but it reminds us that cognitive factors—such as contradictions between theory and empirical findings (“anomalies”), the validity of evidence of various sorts, or the priority of different standards—are mediated through and realized in social, hence contingent, processes. The same cognitive principles and ideals can be actualized by various practitioners in different ways. The question, then, is whose perspective is more powerful, and which direction gains enough momentum to sweep the majority of the discipline. The answers to these questions cannot be deduced from the cognitive factors alone: they must be sought after in the social relations among practitioners.

For historians and methodologists of economics, this book offers a new way to look at the history of their discipline. For the wider audience of students of science the book provides an application of the constructivist approach to the description of a broad historical process, which
encompassed a whole discipline and several decades. This is the third goal of the book. Most constructivist research has been preoccupied so far with analyses of practices in single laboratories (e.g., Latour and Woolgar 1979), with the tactics and strategies of individuals or small teams (e.g., Latour 1988a; 1988b), and with similar “micro” studies. This “bias” is not accidental. The constructivist approach is suspicious of “macro” concepts such as “disciplines,” “paradigms,” and “research programs.” It is more interested in exposing the dynamics of scientific practice by thick descriptions of how concrete scientists use instruments, handle materials, communicate with each other, and negotiate with non-scientists (see Pickering 1992a).

Entities, such as “neoclassical economics,” “institutionalism,” “Cambridge school,” and so forth, are indeed not objects that stand independently of human action and interaction. The decision to lump together a large number of economists, whose works are diverse and heterogeneous, is always somewhat arbitrary and disputable. The boundaries of such entities often, if not always, change over time. It is not unusual that the “schools” are “created” only after their members had disappeared. Similarly, bitter enemies, who conceived of their views as diametrically opposed to each other, can be pigeonholed as peers of the same camp by later-day practitioners. All these facts renders my decision to interpret the struggle between a “neoclassical school” and an “institutionalist school” more than a bit suspicious. Many sociologists would prefer my story to revolve around a concrete group of economists or a specific moment in the history of economics.

And yet, categorization of individuals into paradigms and mapping the terrain of disciplines are important practices both for practicing scientists and for historians who wish to present a coherent reading of history. Various labels were used by the interwar economists themselves, as we will see in chapter 3, and the current historiography of economics is replete with references to neoclassical, institutionalist, historicist, and many other schools. This might be the cause for much of the confusion among historians and economists concerning the works of individual practitioners and the relations among them. Nonetheless, I thought it would be easier for current readers to follow the historical story if I use the common ambiguous labels. I do so with utmost care. The next two chapters tell the history of neoclassical economics and institutionalism—mostly in the United States but with unavoidable digressions to its British sources of influence—as I see it. This narrative, I hope, will dispel much of the confusion caused by the conventional nomenclature. But I have to warn the reader again: when I use the terms “neoclassical economics” and “institutionalism” I do not refer to two clear-cut camps of economists. The terms are used to illuminate the variety of views of economists in regard to the
nature of their field. Some economists stood closer to what one can call
the institutionalist pole, while others occupied the space near the neoclas-
sical one. But each economist stood in a unique position of his or her
own, and many were somewhere between these two poles.

The next sections provide a background for each of the main goals and
a summary of my arguments. First, the conventional history of economics
is presented and its Kuhnian and Lakatosian reconstructions are critically
examined. The following section presents the constructivist approach in
the sociology of science in general, while the fourth section concentrates
on the actor-network analysis and on how scientific changes are ex-
plained by that specific variant of the constructivist approach. Finally, the
last section takes us back to interwar economics and previews the neo-
classical-institutionalist struggle, which will be described in detail in
chapters 4 to 8.

A Textbook Version of the History of Economics

The history of economics as commonly presented in economics introd-
cutory textbooks, or as told by historians of economic thought, is quite
straightforward (Mirowski 1994, 68). First, Adam Smith (1723–90) cre-
ated the new science of economics. His The Wealth of Nations, published
in 1776, is considered to be the cornerstone of the discipline, and, com-
bined with the works of David Ricardo (1772–1823), Thomas Malthus
(1766–1834), and others, constitutes classical economics. In the 1870s
this tradition was elaborated on by the introduction of marginal calculus
by several scholars simultaneously: William Jevons (1835–82) in En-
gland, Carl Menger (1840–1921) in Austria, Léon Walras (1834–1910)
in France, and, a bit later, John Bates Clark (1847–1938) in the United
States. Menger’s work laid the foundations for the Austrian School, and
Walras’s work is the basis for the Lausanne school, where Walras and his
successor, Vilfredo Pareto (1848–1923), taught. The new marginal eco-
nomics, systematized and organized by Alfred Marshall (1842–1924),
continued to entertain the basic assumptions of hedonism and rationality,
held by the classical tradition. And like the classical tradition, it sup-
ported laissez-faire policy of government nonintervention. Hence, it be-
came known as neoclassical economics.

This theory had dominated the discipline until the Great Depression
challenged its maxims and threatened its preeminence, if not the very ex-
istence of the discipline itself. Fortunately, John Maynard Keynes (1883–
1946) appeared with a solution to the paradox of the Great Depression
and saved the discipline. Post–World War II economics saw the synthesis
of Keynesian and neoclassical teachings and the application of mathemat-
tical and statistical tools to this “neoclassical-Keynesian synthesis.” This development, it is argued, led economic theory to its heights, and made it the most developed social science. Later writers added a new chapter: the so-called failure of economics in the 1970s—they argue—led many to doubt the Keynesian Revolution, and the neoclassical approach has found new supporters in what some call “the new classical economics.” The current situation is depicted as a struggle between Keynesians (or neo-Keynesians) and new classicist theories (monetarism, rational expectations, real business cycle), which is a new chapter in the development of the theory, which had started with Adam Smith, and progressed continuously through the works of Ricardo, Marshall, and Keynes. All these economists are thus presented as links in a chain. Each one of them built on the theory of his immediate predecessor, corrected some of his or her mistakes, and endowed to his successors theory in a better shape (cf. Leijonhufvud 1976, 67–68). Each theory is replaced by a more powerful one, and economic knowledge grows and improves with each generation of economists.

The textbook version of the history of economics was used as a basis for Kuhnian and Lakatosian historiographies. Historians of economics have debated whether the development of the discipline was achieved through neoclassical and Keynesian revolutions, according to the Kuhnian model, or through a gradual succession of degenerating research programs by progressive ones, according to the Lakatosian model. I will argue that both views are seriously inadequate and misleading. They reconstruct the history to fit the models, ignore important episodes and ideas that played significant roles in the history, and leave unanswered deep conceptual problems. In what follows I discuss the Kuhnian and the Lakatosian applications offered so far and the problems with these applications.

Kuhn’s main idea is that scientists in each field share the same paradigm. A paradigm is an exemplar of how to work in the field. It is usually based on a major success in the past and is acquired by practitioners during their professional socialization. The paradigm defines for practitioners what is worthwhile investigating, what methods are valid, and what kinds of solutions are acceptable. Most of the time, Kuhn claims, scientists accumulate more knowledge and solve puzzles within the framework of such a paradigm. This is what he calls “normal science.” But alongside the accumulation of knowledge, anomalies accumulate as well. Scientists find more and more phenomena and problems which cannot be explained or solved by the theories and methods of the existing paradigm. With the accumulation of such problems, more and more scientists feel uneasy. This is when revolutions are bound to happen. A revolution means that a new paradigm is adopted, which allows scientists to solve the most disturbing anomalies. This involves a profound shift of
research focuses and styles of work. Older practitioners often find it difficult to make the change and the revolution is carried primarily by younger scientists. Kuhn’s work has contradictory interpretations. Relativist philosophers and sociologists of science interpreted the idea of revolutions as negating the idea of progress (e.g., Barnes 1982; see discussion in Laudan 1990). Kuhn himself, however, was not comfortable with such an interpretation and in his later work sought to preserve the notion of progress.

During the 1960s and 1970s, Kuhn’s model was much in vogue. It also appealed to social scientists despite Kuhn’s explicit statement that social science had not yet reached the stage of paradigmatic science (Baumberger 1977). Kuhn’s model was employed by sociologists (e.g., Wiley 1979), psychologists (e.g., Weimer and Palermo 1973), political science (Almond, 1966, 875), and others. Many historians of economics joined that trend and reinterpreted the history of economics as a succession of paradigms (e.g., Blaug 1975: 410–11; Redman 1991, chaps. 7–9). I will present only a brief summary of this reading.

The rise of classical economics following Adam Smith’s The Wealth of Nations is presented in this historiography as the moment of transition from a pre-paradigmatic stage to the stage of “mature science.” During the pre-paradigmatic stage, there is no single paradigm to guide the work of scientists. Individual scientists work according to their whims and intuitions. Then one of the scientists makes a discovery, or suggests an explanation that impresses many fellow workers and becomes an exemplar for future work. The Wealth of Nations was such a momentous achievement. It was the first time that someone offered a comprehensive framework to analyze economic problems of various kinds. Smith’s successors adopted his framework and worked on problems which developed out of it.

The rise of neoclassical economics has several properties which resemble Kuhn’s model of revolutions (e.g., Bronfenbrenner 1971; De Vroey 1975). First, it introduced marginalist calculus as a standardized method of economic analysis. Second, it changed the focus of economists from macro-questions of national income to micro-analysis of firms and consumers (Birken 1988). Third, there is some evidence that there was a “crisis” in economics in the 1860s. The fact that marginal economics was suggested simultaneously by three economists appears as a reaction to this crisis. That marginalist analysis was actually suggested much earlier by Thünen (1783–1850), Gossen (1810–58), Cournot (1801–77), and others only corroborates the Kuhnian model, which includes the appearance of forerunners before a revolution takes place.

Not all historians of economics agree that economics experienced a revolution in the end of the nineteenth century. Stigler (1973) admitted, for example, that marginalism was part of economic teaching, but he
insisted that very few economists actually employed it in their research (cf. Howey 1973, 35). Other historians emphasized that Walras, Jevons, Marshall, J. B. Clark, and Menger held widely different views on methodology, the scope of economics, human behavior, economic policies, and more, thus rendering the notion of one “marginalist paradigm” inaccurate and misleading (Blaug 1973, Coats 1973, 38; Jaffé 1976). The disagreement about the dating of the supposed revolution poses another problem. Finally, it is argued that some of the pioneer “revolutionaries,” including Marshall and Menger, actually claimed that they merely refined earlier teachings (Blaug 1973, 11).

While there are problems with the Kuhnian view of neoclassical economics, the case of Keynesian economics seems, at least on the face of it, much less problematic. If revolutions occurred in economics, it is widely accepted, the emergence of Keynesian economics must be regarded among them (Blaug 1975, 411–12). Keynes, it is commonly argued, combined the two branches of economics, price theory and monetary economics, which had been practiced separately (Harcourt 1987, 6). He demolished Say’s Law, which supposedly dominated economic thought from early in the nineteenth century, and showed that the market may reach equilibrium below full employment. The so-called Keynesian revolution, it is almost universally assumed, revived macroeconomics and provided it with new concepts and tools (Stanfield 1974). Saving and consumption propensities, multiplier, effective demand, liquidity trap, and liquidity preference were all introduced to the economic discourse by Keynes and have become common terms since then. Expectations and other psychological factors have been given a bigger role in Keynes’s theory (Shackle 1967). Investment has replaced savings as the crucial variable in accounting for growth and development. Keynes “legalized” state intervention in the economy and undermined the doctrine of laissez-faire, which, the Kuhnians say, had dominated economic doctrines until then. Deficit budgets, an anathema prior to Keynes, became a common tool of economic policy. Frugality which until then had been regarded by economists as a virtue, suddenly became a vice.

Coats, who is among those who argue that the rise of Keynesianism “possessed many of the characteristics associated with Kuhn’s scientific revolutions,” adds the sociological aspects of a revolution. “There were,” he says, “unrecognized precursors of Keynes, a growing concern about the inadequacy of existing theory, and a change of psychological outlook on the part of many economists virtually amounting to a ‘conversion experience.’” In accordance with the Kuhnian model, “the revolution was led by a band of youngsters who encountered fierce resistance from their elders.” And, also compatible with Kuhn, “within a remarkably short time the new paradigm had won an almost complete victory.”
(1969, 293). Seers writes that many economists “resisted changes in the syllabus to accommodate Keynesian economics and even appointment of Keynesians to economics faculty” (quoted in Routh 1975, 25; cf. Galbraith 1987, 238; Backhouse 1985, 333; J. M. Clark 1947, 1).

In spite of these apparently convincing facts, there are many who contend that Keynesian economics did not break so much from the old neoclassical theory. This line is usually carried by critics of orthodox economists, for whom the reform of the economic thought by Keynes was not sufficiently off the beaten track. Routh, for instance, maintains that Keynes continued to employ the same deductive reasoning and abstract analyses of neoclassicists (1975, 286–93). Interestingly, this view is shared by those who support the Keynesian teaching but perceive it as continuation of the past (e.g., D. F. Gordon 1965). Oser writes that the Keynesian system “arose out of the neoclassical, or marginalist, school, and Keynes himself was steeped in the Marshallian tradition.” “Although Keynes sharply criticized certain aspects of neoclassical economics,” Oser explains, “he used many of its postulates and methods. His system was based on a subjective, psychological approach, and it was permeated with marginalist concepts, including static equilibrium economics” (1970, 390; cf. Deane 1978). Canterbury tries to synthesize the conflicting views concerning Keynes by arguing that “Keynes’ theory was not as revolutionary as it appeared, which is not the same as saying that it was not revolutionary” (1976, 140; italics in original).12

Keynes was not an unknown or a peripheral economist. On the contrary, he stood in the center of the establishment: a son of a prominent neoclassical economist, a pupil of Marshall, and an acquaintance of powerful officials at the Cambridge administration, the Treasury, and the business community. Moreover, when The General Theory was published, Keynes held the most prestigious chair in political economy, the one at Cambridge, and edited the most circulated professional outlet, The Economic Journal. Keynes’s contemporaries listened very carefully to anything that he uttered. His earlier book, Treatise on Money (pub. 1930), already received a great deal of attention, and when economists heard that The General Theory, which they had already been awaiting for several months, was finally published, they lined up in bookstores to get a copy (Tobin in Breit and Spencer 1986; S. Weintraub 1988, 41; Samuelson 1966, 4:1517).

Other historians, who emphasized the continuity of Keynes and his predecessors, used this continuity to support a Lakatosian view of the history of economics as progressing not through revolutions but through gradual shifts from “degenerating scientific research programmes” to “progressive” ones. The notion of “scientific research program” (SRP) resembles Kuhn’s notion of “paradigm” but carries other implications. A
scientific research program is composed of a “hard core” which is surrounded by a “protective belt.” The hard core includes fundamental axioms which are based on metaphysical beliefs and are taken as given. Research is done by constructing theories to reconcile the hard core with observations of the real world. Researchers follow the “positive heuristic”—the principles derived from the hard core that dictate what should be studied and the ways it should be done—and observe the “negative heuristic”—the research topics, and methods precluded by the logic of the hard core. The theories are called “a protective belt” because their manipulation enables scientists to retain their hard-core beliefs. According to Lakatos, a hard core cannot be refuted. If it is abandoned eventually it is not because of a “crisis” caused by accumulation of anomalies. The reason is rather that scientists move from “degenerating SRPs”—programs that construct theories only to explain ad-hoc facts which are already known—to “progressive SRPs”—programs that lead to the discovery of new facts. Lakatos says very little on the process of change itself, but historians of economics tend to interpret such a transition as a smooth and gradual transformation in contrast to the extreme change implied by the concept of a revolution (e.g., Blaug 1994; Backhouse 1994c).\(^\text{13}\)

The Lakatosian historians admit that economics has been changed during the 1930s but perceive this change in a different manner. Mark Blaug, the leader of this approach, argues that the idea of a “Keynesian Revolution” is based on a “Walt Disney version of interwar economics”: “No American economist,” he says, “advocated [the neoclassical] policy of wage cutting” between 1929 and 1936. On the contrary, “the leaders of the American profession strongly supported a [“Keynesian”] programme of public works and specifically attacked the shibboleth of a balanced budget.” Hence, he continues, there is no need to resort to the notion of a Keynesian Revolution which creates an “image of a whole generation of economists dumbfounded by the persistence of the Great Depression, unwilling to entertain the obvious remedies of expansionary fiscal and monetary policy . . . and finally, in despair, abandoning their old beliefs in an instant conversion to the new paradigm” (1975, 414–15; see also id. 1990a, chap. 4; Backhouse 1985, 275–76; Weir 1989, 55; Lee 1989). Blaug thus suggests that “the history of a science is more fruitfully conceived, not as a steady progress punctured every few hundred years by a scientific revolution, but as succession of progressive research programmes constantly superseding one another with theories of ever-increasing empirical content” (1975, 409–10).\(^\text{14}\)

The Lakatosian view has its own problems. Blaug ignores all too easily many manifestations of resistance to Keynesian innovations. His argument that economists “were united in respect of practical measures for dealing with the depression, but utterly disunited in respect of the
theory that lay behind these policy conclusions” (ibid., 415), is at least exaggerated, if not altogether wrong. J. R. Davis’s seminal research (1971; see also Barber 1985) shows, indeed, that the support of public work and deficit budget was much wider than one could expect based on the “Walt Disney version.” But that there was some resistance is hardly disputed.15

In the late 1970s, the Lakatosian view became popular among historians and methodologists of economics and constituted the “orthodoxy” in the field (Redman 1991, 142–4; Hausman 1992, 87; Backhouse 1994c, 173). Recent debates have been concerned with examining various theories or research traditions to examine whether they have followed the methodology of SRPs. The results were mixed; some have argued that economics passed the test successfully; others have claimed that it failed. In a conference dedicated to Lakatosian methodology held in Capri in October 1989, most participants viewed the Lakatosian framework with suspicion or even with unabashed objection. According to Blaug, the veteran Lakatosian, twenty-five out of the thirty-seven participants were inclined to abandon the Lakatosian approach altogether because they found the concept of a scientific research program too vague and were unable to agree on the contents of hard cores (Blaug 1991, 500; see also Backhouse 1994c, 176–77; Hands 1993).16 More important, the attempts to apply the Lakatosian explanation to changes in economics failed to show unequivocally that winning SRPs, however defined, had excess empirical content. Blaug tries to save the Lakatosian model by listing a series of improvements in economic theory and concludes that “there has been much theoretical progress in twentieth-century economics. There has also been some empirical progress which, however limited, is perhaps enough to refute the extreme pessimists” (1994, 121).

In Blaug’s view the constructivist positions of McCloskey and Weintraub imply that there is no real progress in economics (Blaug 1994, 130); that is why he calls them “pessimists.” But the constructivist approach in general, and the Latourian actor-network analysis that I will present below in particular, do not imply anything of this sort. The opposite is true: for Latour, it is the very nature of science that it constantly produces “new agencies and hybrids” (1987). The fact that science progresses does not mean that we must choose between Kuhnian and Lakatosian models. As we will see below, there are other possibilities to perceive such progress.

When Blaug brought the Lakatosian framework into economics, he claimed that “certain puzzles about the Keynesian Revolution dissolve when it is viewed through Lakatosian spectacles” (1975, 415). I believe that many puzzles which have remained thereafter dissolve when they are viewed through “constructivist spectacles.” The constructivist sociology
of science, I suggest, can account for the revolutionary *image* of some episodes in the history of science, including economics, and to the contrary *image* of other episodes. Moreover, it helps us understand why the same episodes may appear both as revolutionary and as traditional. It also helps us understand why the evaluations of "progressiveness" of SRPs are never conclusive. In general, it provides us with a framework in which we can incorporate all the elements of the history of economic thought, not only those that the current mainstream has selected as "significant." And it is a framework which shows that many enduring problems with which historians of economics have grappled for many years are constructed only by artificial conceptualizations that problematize natural aspects of scientific practice.

My effort in this direction is not the first such effort. In the Capri conference in October 1989, the Lakatosian reconstructions were challenged by two prominent sociologists of science, Harry Collins and Karin Knorr-Cetina, and by a reputable philosopher of science, Nancy Cartwright. Apparently the other participants were "not really comfortable with the reconceptualization these individuals offered about the enterprise of economic science" (Weintraub 1991a, 101) and chose to ignore it and not publish it in the conference proceedings (De Marchi and Blaug 1991). The omitted papers were later published in *History of Political Economy*, but most economists seem to have paid little attention. As Weintraub notes, "there are not many historians of economics who have an interest in the sociology of the economics profession" (1991b, 9). Weintraub's own book (1991b) is one of the very few attempts to apply the views of Collins, Knorr-Cetina, and other sociologists of science to specific episodes in the history of economics, and I hope that this book helps to attract more attention of economists to views which are seriously considered and discussed by sociologists, historians, and philosophers of other sciences.

The Construction of Scientific Knowledge

Scientific knowledge, as any other kind of knowledge, is produced by human beings working together. Philosophers have debated for centuries the possibilities of the human mind to know Nature. Sociologists have added to this the recognition that the human mind is a social mind (Amann and Knorr-Cetina 1989; Knorr-Cetina 1980, 1991). The language in which we think, the concepts we use, the principles of logic we employ—all our basic tools of thinking are acquired and shaped in a long process of interaction with other people. Furthermore, whatever the possibilities of knowing are in theory, the outcomes of scientific inquiries
depend on the practices of research and on the social process that accompany the production of scientific knowledge. "The world is out there," Rorty admits, "but descriptions of the world are not"; they are human-made (1989, 5). The constructivist approach has not originated, however, in philosophical reflections and is not directly related to any school in philosophy (cf. Fuller 1990). It is rooted in historical studies and observations of working scientists that show how scientific knowledge is actually constructed.

The essence of constructivism is the attempt to study science "as it is" (Latour and Woolgar 1979; cf. Klammer 1990, 27). Its advocates have refused to accept conventions that privileged science and set themselves to watch scientists working in their laboratories, to listen to their conversations, and to follow rigorously the historical development of machines, methods, theories, and inventions (Collins 1991a). It is somewhat ironic, but not a coincidence, that this "positivist" approach yielded results that challenged traditional positivism. Constructivists have drawn attention to the fact that the content of scientific knowledge is being shaped in a complex social process. This applies to their own findings, a fact which is often brought as a "proof" of self-refutation. The constructivists do not deny that their findings are as shaky, or as stable, as the findings of other empirical scientists (Latour 1988a, 266n; McCloskey 1994b, chap. 15; McCloskey 1995, 1322). Constructivists do not say that empirical findings are valueless but rather that their value, their meaning, is not given; it has to be negotiated among competent scientists (i.e., persons who are regarded competent by their peers) who hold different interpretations. The empirical findings are therefore contingent, which is by no means tantamount to saying that they are of no importance (Smith 1993).

Those who are familiar with the growing tradition of the rhetoric of economics would undoubtedly identify the similarity between that tradition and constructivism. McCloskey, Klammer, and their fellow rhetoricians refuse—like the constructivists—to take what economists say about how they practice economics at face value (McCloskey 1985, 1988a, 1988b, 1990a, 1990b, 1994a, 1994b; Klammer 1987, 1990; Klammer et al. 1988). Like constructivists, they think one should go and see how economists actually persuade each other (McCloskey 1994a; see also Lind 1992, 1993). If science works well and reaches some goals (a matter for another discussion), it is because of that unseen know-how gathered on the job and not due to the official rules one learns in school.

Both the constructivists and the rhetoricians of economics are accused of being sheer relativists who oppose the use of empirical evidence in science. McCloskey (1994b) labors to dispel this charge and, like the constructivist sociologists of science, claims that empirical evidence is crucial
in science, but its meaning is never objectively given (Latour 1987; see also Smith 1988). Roy Weintraub—a constructivist influenced mainly by Stanley Fish’s approach to literary texts—insists that constructivism “does not mean that anything goes” (1989, 488). Texts, like empirical findings, can be interpreted in many different ways, but texts, like empirical evidence, limit the range of possible interpretations (on Fish, see Hoover 1994, 290). Defending the traditional view that perceives the outcome of experiments and observations as unproblematic runs against what we know on the practices of scientists. It is self-refuting to defend the value of empirical research and, at the same time, to deny the empirical findings of constructivists!

The finding that knowledge is socially constructed was mistakenlly interpreted as meaning that knowledge is determined by interests external to the scientific practice, such as political and ideological beliefs, religious convictions, egoistic pursuits of fame or power, and so forth. To prevent this mistake, some constructivists have recently cut the word “social” out of “social constructivism”; they prefer to be called “constructivists” rather than “social constructivists” to avoid the erroneous identification as sheer externalists and to emphasize that “there seems no warrant for assigning causal priority to the social in understanding scientific practice and culture” (Pickering 1992a, 14). This step is not intended “to deny that science is constitutively ‘social’” (ibid.). It is intended only to underline that scientific knowledge is the result of dialectical relations among social, institutional, conceptual, material, and other elements of science in various combinations. The old debate between internalists and externalists (Shapin 1982; 1992) in the study of science is obsolete, because the elements of the outside world do not appear as “purely internal” or “purely external.” “Internal” factors—what is a fact, how the validity of a fact has to be established and reaffirmed, the priority of various logical requirements—are themselves the result of negotiation and resource mobilization in the “external” world (Johnson [a.k.a. Latour] 1987).

Shapin and Schaffer’s study (1985) of the conflict between Hobbes and Boyle over what constitutes a valid way of knowing is one of the best demonstrations of the constructed nature of scientific knowledge. Boyle promoted experiments as the only reliable method of determining the truth. He conducted his experiments in public and argued that the agreement of all viewers validated the outcomes and safeguarded against idiosyncratic errors and perceptual deceptions. Hobbes, in contrast, believed in the more traditional view that the only valid knowledge was the knowledge based on infallible mathematical proofs. Experiments, he believed, were not reliable because our senses often misled us. Amidst the seventeenth century, both positions were considered viable, and only due to Boyle’s victory—a victory contingent on the specific social circum-
stances of seventeenth-century England—experimentalism has become so pervasive and "reliable" in modern science. Shapin and Schaffer describe the rise of experimental ideology that has become a core belief of almost all disciplines.

Economics, in which frequent references to the scientific method of Boyle are abundant, is among the few fields in which the Hobbesian approach still takes precedence over Boyle's experimentalism. According to several surveys, empirical papers in professional literature are much less frequent in economics than any other discipline (McCloskey 1994b, 172). Theoretical papers that use no empirical evidence are considered more prestigious and empirical tests are devised based on theoretical considerations. From the constructivist perspective, this feature of economics is no reason for alarm. One of the findings of the laboratory studies of constructivists is the large heterogeneity of science (Watson-Verran and Turnbull 1995). Karin Knorr-Cetina (1991), for example, had documented the emergence of epistemic cultures in different fields. Epistemic culture is composed of know-how techniques, rules of thumb, and other informal, practical guidelines for how to do various things; "untidy goings-on" in Knorr-Cetina's language. The emphasis here is on the "disunity of science": there are different cultures, because each field has its own history, its own dynamic of personal relations, and its own subject matter. We cannot make a distinction between a unified scientific method and different contexts, because the core of science, its epistemic foundations, are themselves constituted by the context (ibid., 107). From this point of view, Dudley-Evans's finding (1993) that economics articles constitute a different genre than scientific papers in biology is not surprising; each field, not only economics, has its own "genre." The constructivist emphasis on the diversity of sciences dissolves the traditionally made distinction between natural and social sciences. Both the so-called "natural sciences" and "social sciences" are conglomerates of various practices that differ enormously. The desire of many economists to imitate the physicists and their pride that their science is the most similar to physics are therefore based on an utter misunderstanding of what science is (Mirowski 1994, 55).

The recognition of the contingency and plurality of science led constructivists to the "principle of symmetry." According to this principle students of science should treat in the same fashion scientific theories that have triumphed and those that have lost and disappeared. Traditional interpreters of the history of science have tended to explain victories of scientific approaches by their superior quality. They problematize only the resistance to theories which have later been recognized as better. The assumption is that no explanation is needed for the reception of Newtonian mechanics, because rational actors would naturally prefer it.
given the evidence available at the time of Newton. In contrast, the constructivist approach treats all scientific theories in the same manner, regardless of their eventual destiny (cf. Weintraub 1991b, 5–6). The goal is not to show how “good” theories win, but to document how the view of what is “good” is being constituted and then used to resolve debates about Nature.

The traditional view takes the position of the winner and interprets history from the winner’s perspective (Weintraub 1991b, 5). Whoever wins is the right winner. This is why this approach produces Whig histories, which are, asMcCloskey says, “too easy” (1994b, 90; see also her comment on p. 103; Mirowski 1994, 65). Of course, all historians are familiar with cases in which a scientist who had appeared as a winner in the beginning was later declared wrong. There are also numerous cases of “losers” who were crowned as “winners” many years after their works had been rejected. The Whig historians simply dodge the problem such cases pose by taking the position of the last winner. They explain her previous defeat as caused by social—that is, “unscientific”—causes and celebrate her eventual victory as the triumph of reason. The constructivist approach, in contrast, does not perceive history as a “Greek tragedy,” to use Elkana’s potent image (1981). We cannot assume that what has happened was the only way it could have happened.

The critics of constructivism accuse it of undermining the noble quest for truth that has guided scientists for centuries. If the sanctity of the Scientific Method is challenged, they imply, the most invidious and preposterous theories, such as Nazism or astrology, would claim the same social recognition and public support that science receives today. This critique is based on a deep misunderstanding of constructivism.26 Constructivism does not seek to challenge the validity of science nor to challenge its methods. On the contrary, it opts for symmetric treatment because of its deference toward science and its recognition that philosophers and methodologists cannot legislate for scientists (Callon and Latour 1992). Saying that truth—claims are contingent does not mean that everything goes, nor does it mean that any two views are equally plausible. There can be bad and good arguments (Smith 1988; Gerrard 1993; McCloskey 1994a). But when two camps of scientists make opposite claims, there is no outside arbitrator who can determine which camp is right. If a group of researchers—trained and socially recognized as competent—accept in good faith a certain theory as convincing, we, as outsiders, must assume that it is a reasonable theory.27

The criticism of constructivism might be based on the realist notion that there is one truth against which scientific theories should be measured. A certain theory might be either true or wrong, and if it is true, than all rival theories are wrong (Mäki 1995). The constructivist ap-
approach is different. It does not deny that some claims are true and others are false, but it highlights the fact that at the frontier of science the known evidence usually gives rise to more than one reasonable theory, that is, a theory that competent practitioners deem reasonable in light of the known evidence. Maybe there is one superior method—the philosopher’s stone or the holy grail—to rule which theory is the correct one, but unfortunately we are offered contradicting “philosopher’s stones.” How can we decide which one is the “true” one? This is decided only by the negotiation, alliances, and rhetoric of “stone merchants,” as illustrated, for example, by the historical study of Shapin and Schaffer and by the empirical studies of Knorr-Cetina, studies that show that what scientists have perceived as the ideals of science have varied across disciplines, times, and places (cf. McCloskey 1995, 1320–21).

In an attempt to save Popperian/Lakatosian methodology of economics, Backhouse (1994) argues that empirical progress as evident in predicting novel facts should still be considered a universal principle of methodology. This defense fails to grasp the nature of the constructivist critique. Constructivists would have no argument with either the positive observations that many scientists pay attention to successful predictions, or with a normative argument that they should. Successful predictions, neat explanations, and practical utility are evidently very persuasive in science, which may account for the victory of approaches that appear to perform these tasks. But unlike other approaches, the constructivist approach calls attention to the fact that “success,” “neatness,” and “usefulness” are not given by Nature but constructed in a process of negotiation and conflict. Moreover, the question of what relative weights should successful predictions, neatness, and usefulness be assigned is not answered by traditional methodology. Indeed, it is answered differently by various communities of scientists.

Unlike the traditional views of science, the constructivist approach does not entail that all sciences work well. For positivists, if a scientific field is shown to have failed, it is immediately excluded from the domain of science. There is no such thing as “bad science.” For an extreme relativist, no scientific project is better than others, nor is it better than nonscientific enterprises of knowledge production. The constructivist approach is different. It acknowledges the unique features of scientific practices that might endow its products unique qualities. But the actual outcomes depend on many factors and vary substantially from one field to another. The constructivists themselves, as outsider observers of science, are not interested in evaluating the sciences they study. This is not because such evaluation is unimportant. From their perspective, evaluation is an integral part of the practice of working scientists, who must make decisions concerning the direction of their field. The findings of historians and
sociologists may influence such decisions, and historians and sociologists may express their own views concerning the direction the field they study should go. But constructivist accounts do not purport to entail clear conclusions about the suitability of various methods. It is compatible with all methods and approaches and refuses to deprive the status of science from a given practice because some powerful gatekeepers feel that its method are improper.

Accounts of Scientific Development: The Actor-Network Analysis

The constructivist approach emphasizes the diversity of science. This is an empirical finding, as well as a logical implication of the recognition that there is no absolute standard to judge scientific enterprises. This view can settle many barren and inconclusive debates among scientists and students of science. But at the same time it raises new questions. If many possibilities are open for scientific enterprises, how should we explain the options actually selected? Traditionally, students of science implied that the development of scientific knowledge is determined by the quality of contending theories and their use of the proper methods. The only thing that was left open for explanation was the tempo of science, that is, its rate of progress. This pace was explained by the degree of financial support and institutional freedom given to scientists. But if the question is what kind of theories are considered adequate and what scientific methods are used we need a new kind of conceptual framework. The actor-network approach (ANA) developed by Bruno Latour and Michel Callon offers us such a framework.28

The actor-network approach perceives scientists as involved in attempts to promote their own contributions and turn them into “black boxes”—that is, into knowledge which is accepted and used on a regular basis as a matter of fact. Scientists are involved in what Latour calls “trials of strength” at which their claims about the validity of their findings and the usefulness of their research have to withstand challenges made by competing colleagues. A successful trial means that an ongoing concern has incorporated the contribution into its institutional set of practices. It is not enough to be recognized as “valid” and then put aside. The new contribution has to become part of a larger apparatus which can be used regularly without any need to justify the use in order to become part of the ever-growing and ever-changing stock of knowledge. The contribution might be a theoretical principle which enters introductory textbooks or is referred to regularly in the reporting of experimental results (i.e., the “law of diminishing returns”). Or it might be a method of measuring a certain variable in order to test theories or for
some practical reasons (price index, for instance, which is used both for comparing monetarist with Keynesian theories, and for deciding on pay raises and social security adjustment). The contribution might also be a part of a machine without which the machine cannot function properly. In short, any component that enters into the work of scientists, and which may be disputable, can become a "black box" once an agreement has been reached. In any case, the contribution has to become an obligatory passage point for some concerns, that is, something that cannot be dispensed with.\textsuperscript{29}

In order to succeed in trials of strength, scientists, who compete among themselves, have to marshal various "allies" in order to harden their cases and make them more defensible. "Allies" can be anything that bears upon the strength of the contribution in question, including, of course, other scientists or people who support the contribution, either financially, or by bestowing their authority upon it, or simply by using it. But also included are various instruments and practices that embody the contribution and arguments that justify it. The authority of respected practitioners in the field, examples from neighboring fields or from other prestigious disciplines, the views of philosophers and methodologists—all can become part of the network that support the contribution. "Facts" are also allies, of course, and in most disciplines, they have a considerable weight. But facts do not speak for themselves! They need scientists as mouthpieces, and the scientists who summon them up must interpret them, convince others in their factuality, and explain how they support their arguments.\textsuperscript{30} To achieve this they have to array the "facts" along with other allies, such as the interpretations of other famous scientists, other black boxes, or the way other established facts have been interpreted (Latour 1987, 94–104).

The idea is not that scientists are like crafty lawyers who do not care about "the Truth" and manipulate the facts to advance personal interests, as Latour is often, and mistakenly, interpreted. The point is that nobody knows what "the Truth" is before the trials of strength are concluded. Thus, scientists who believe that they have revealed a piece of "the Truth" must do their best to convince others. If we want to extend the metaphor of the court, we can think about it as a trial in court in which nobody (not even the plaintiff and the defendant!) knows what happened, and the role of lawyers is to present the best case for both sides; the jury—the scientific community in our case—has to decide on the basis of these presentations. As rhetoricians of law have noticed, one cannot separate between "substantive" arguments and "rhetorical" tricks. There is no argument without rhetoric. If a certain fact seems to be so unambiguously supportive of the other side, scientists often admit it and surrender, as happens occasionally in court. When they stick to their position in spite of this "obvi-
ous” fact, we, as observers from the sideline, cannot just say that they are pigheaded. After all, if it is not “obvious” for them, who are we to say that it is “obvious”?

Again, this approach is quite similar to the rhetoric of economics. Klamer, for example, presents economists as participating in conversations. They argue to persuade each other and, occasionally, to convince noneconomists. For that purpose they construct a variety of arguments; use analogies, metaphors, labels; reconstruct intellectual history; and make claims concerning the status of their arguments (Klamer 1987, 164). What Klamer calls “variety of arguments” is what Latour calls “allies.” My use of the Latourian framework is partially due to my disciplinary training but also reflects my belief that it carries the rhetoric enterprise farther. McCloskey and Klamer deal with speech acts; what economists say and write. The sociology of science deals with what scientists do, and where—in which institutional and technical environments—they do it as well. The persuasiveness of claims depends not only on arguments, metaphors, analogies, and so forth; it also depends on financial resources, personal ties, and organizational skills. Whether McCloskey and Klamer would like to accept this, I am not sure. In any case, their defense on rhetoric is identical to the constructivist insistence that “facts do not speak for themselves.” The relative weight of arguments versus practices and organization can be left for empirical study.31

All allies—facts, people, money, methodological principles, theories, instruments, machines, practices, organizations, and so forth—constitute a network which upholds and ratifies each element of it. It is difficult to undermine any single link of the network without undermining the others, and therefore the ability to connect a new element (method, theory, instrument, etc.) to a strong network is likely to ensure its success in ensuing trials of strength. If the new element is supported by many older elements that are already perceived as valid, it will be hard to dispute its own validity. It would be accepted by those who have accepted the whole network and become part of it. Such a success should not be interpreted, however, as a proof of the veracity of the new element. It is absolutely possible that the whole network is based on shaky foundations, but such a claim can be made only by other scientists and scholars who must base their claim on another network, stronger or weaker.

The concept of network overlaps in some degree institutional bodies such as disciplines, schools, paradigms, and research programs. An academic “school” is a group of scholars who make frequent use of each other’s work. By using the conventions of their own group, its members convince each other, and if the network can mobilize enough resources (most importantly, money) it can be intellectually self-sufficient. But in order to convince outsiders they often have to connect their own school
to other schools, other disciplines, or to cultural conventions of the society at large. A network is therefore wider and much more complex a unit than a school. A "paradigm," an exemplar of how to do research in a specific field (Kuhn 1970a) could be interpreted in the Latourian scheme as part of the network which many researchers try to get connected to. Unlike Kuhn however, the ANA does not assume that scientists necessarily use the "paradigm" in the same fashion, or that they mean the same thing when they refer to the underlying assumptions (cf. Gilbert and Mulkay 1984). Weintraub provides a fine example of this by showing us how "equilibrium" has been differently perceived even within the same group of early general equilibrium theorists (1991b, chap. 5).

Kuhnian historians have attempted to find boundaries among paradigms, as if there were some absolute boundaries independent of human agents. Yet researchers who study the histories of various disciplines, often come up with conflicting descriptions of the basics of paradigms, quarrel about when revolutions actually happened, and even find it difficult to categorize individual scholars and ideas. It is the same with "hard cores": "Attempting to apply Lakatos's view of the structure of research programs to economics creates unnecessary and unhelpful questions" (Hausman 1992, 88). It is not a surprise that those who have attempted to apply the Lakatosian model "have disagreed concerning what the hard core of neoclassical theory is" (Hausman 1992, 86; see further discussion, pp. 87–88; Backhouse 1994c, 176–77). Axel Leijonhufvud has noticed that "controversies may rage within as well as between research programmes" (1976, 66; see also Mäki 1994). Although he believes that the "first order of business" in the history of economics is to "characterize the essentials of the two contending programmes," he admits that "there is no—can be no—'canned programme' for how is it [sic] to be performed," and therefore attempts to do so would be controversial themselves (ibid., 69). Attempting to solve these difficulties, Mäki distinguishes between "antagonism" and "family quarrels." Antagonism is a dispute over "core assumptions," and family quarrels are disputes over "peripheral assumptions," apparently within the same camp (1994, 237). Mäki acknowledges the difficulty of deciding what is "core" and what is "peripheral" (ibid., 247; see also Blaug 1991), but he underestimates this difficulty. Any assumption can look either central or peripheral, depending on the social context in which the dispute is conducted.32

The ANA avoids the pitfalls involved in all the attempts to create indisputable and clear-cut maps of the discipline. Under the ANA we do not have to assume that all so-called neoclassical economists, for instance, shared the same metaphysical beliefs, an unlikely fact. It is enough to say that neoclassical economists often referred to similar allies—e.g., Alfred Marshall, supply and demand curves, or the "utility" of the consumer.
We also are not required to decide who is “in” and who is “out.” It is absolutely conceivable that some scholars would make a more frequent use of the “neoclassical allies,” that over time some elements of the “neoclassical network” would become more or less popular, or that some scholars would tie their works to more than one “hard core.” “Paradigms” and “hard cores” are not real objects “out there” that we, the students of science, have to reveal. The boundaries between them are shaped by negotiations and struggles of scientists who are involved in trials of strength, similar to the way “facts” are constructed (Dean 1979, 212). There is no one “correct” way to map a field and classify its practitioners. It is therefore fruitless to argue whether a certain practitioner “really” belongs to this or that paradigm, or whether a certain idea “really” constitutes a part of a certain paradigm or deviates from it. The task of the historian or the sociologist is to locate the various social and ideational connections and follow how the practitioners themselves have defined the various schools and approaches. It is not our job to impose our perceptions on the history.

This was a brief summary of the constructivist approach and the actor-network analysis. It was meant to whet the interest of those who do not know about this approach, but it is obviously not enough to provide all its details and defend it against its many critics. I can only hope the readers would be interested enough to consult the works of constructivist sociologists in general, and the works of the actor-network theorists in particular.

The Goals of This Study

The struggle between institutionalists and neoclassical economists during the interwar period is an example of a competition between two networks vying for the same space and the same resources. In the situation analyzed in this book the struggle was over the question “What should economics be like?” This type of question often leads toward prolonged disagreements. When the destiny of a whole field is at stake, many people have an interest in the outcomes of the dispute. Furthermore, the question is too general and ambiguous to allow any side to easily compose an unassailable network, and therefore in many disciplines we find networks that have consolidated around different answers to similarly broad questions. Within each school the fundamental principles are taken for granted—as a black box—and used in the production of more black boxes. But at the same time at least some advocates of each school are preoccupied with a major “trial of strength” in which the whole structure of their black boxes—from the fundamental principles, assumptions, and methods to
the most specific factual and theoretical propositions—is at stake. Some resources have to be devoted to this struggle over fundamentals. Nonetheless, and in contrary to what Kuhn and many others seem to believe, this is not necessarily a bad thing: as in economic competition, a rivalry between two (or more) scientific procedures may involve some waste, but it also constitutes a stimulus for the competitors to examine their “products” and improve them.

The competing methodologies of neoclassical economics and institutionalism were two black boxes that coexisted together and were employed simultaneously in producing further knowledge. But since the two camps fought over the same territory (i.e., nominations in economics departments, space in major journals, public attention) and made contradictory assertions, they had to channel at least some resources toward the continuing effort of asserting the validity and fruitfulness of their proposed methodologies. The bulk of this book is concerned with this controversy. We will see that economists promoted their methodological approaches by weaving and meshing together elements of many sorts, similar to the way scientists and engineers construct new facts or fabricate new machines. The proposed methodologies have to be “valid,” they must fit the accepted “canon,” they should be compatible with knowledge which is deemed sound, and they need to be regarded as useful and fruitful. Some of their properties might be less satisfactory than others. But as a whole, the approach has to be attractive enough relative to its competition in order to pass the trial of strength. Rivals are likely to challenge various elements of the arguments made in favor of the methodology and advocates must respond by mobilizing further allies, strengthening existing elements, and jettisoning weak allies that cannot be defended.

Given the unique subject matter of such trials of strength, the kinds of allies which are likely to be mobilized in such cases are quite different from the allies marshaled into concrete trials. I have identified five main types of allies which seem to me typical of conflicts over methodologies: (1) methodologies of prestigious disciplines, (2) theories from neighboring disciplines, (3) well-known features of the economy, (4) relevance to practical problems, and (5) respected authorities from the discipline’s own past. Chapters 4 through 8 analyze how these allies were marshaled and employed by institutionalists and neoclassical economists in their conflict during the interwar years.

The analysis will employ another important concept that Callon and Latour brought into sociology, namely, translation. Borrowing from Michel Serres, Callon and Latour use the term to refer to the way actors take on themselves the task of speaking in the name of other entities (Callon and Latour 1981; Callon 1986; Latour 1987, 108–32). This is evident
in the case of leaders of social groups who speak in the name of the group, thus defining its identity and interests. But, Callon and Latour maintain that this is also the case with scientists who speak in the name of Nature, which cannot speak for itself. It needs a mouthpiece, and scientists argue over the question who is the “authentic translator” who represents Nature most reliably (Latour 1987, 94–100). The same process of translation took place in the institutionalist-neoclassical struggle: various economists took on the task of speaking in the name of philosophical principles, recognized methods, theories, public interests, and the dead economists of the past, and interpreted for their colleagues what those mute entities meant for the question of “What should economics be like.”

My goal is not to account for the outcome of the struggle but to analyze the case of inter-paradigmatic struggles and to identify some of their unique features. This limitation may disappoint many of the sociologists of science who will read this work, as well as some of the historians of economics who are interested in the social configurations that channeled the evolution of economics (Coats 1984). However, an account of outcomes requires the analyst to pay attention to all the components of the pertinent networks. In the case of economics that means that one has to study the businesspersons and government officials with whom economists were in touch, the positions economists held in government and corporations, the funds they were able to solicit, and the tools and the economic plans they constructed for their various clients. This is a project well beyond the capacity of this book. Although there are few sociological analyses of interwar economics, these are too few and two sporadic to be used as a basis for a comprehensive account of the developments in a field as rich and vast as economics, and in a period as eventful and fertile as the period between the world wars. I chose, therefore, to analyze scholastic and cognitive arguments only, and in this sense my study is not a full-blown ANA. What it does is to elucidate the structure of methodological controversies, and the logic of the actor-network approach serves this goal well.

A full sociological account must also take much more notice of the distinctions and disputes within each of the two camps of institutionalism and neoclassical economics. So far I have talked about institutionalists and neoclassical economists as if they belonged to two clear-cut groups with diametrically opposed views, incompatible methods, and unbridgeable approaches to economic problems. This picture is very far from the truth as we will see in the next chapters. It also contradicts our constructivist view of “paradigms”/“SRPs” presented earlier in this chapter. I nonetheless adopted these terms (“institutionalism” and “neoclassical economics”) to simplify the presentation and make the main thesis comprehensible. Furthermore, contemporary economists were aware that
there was a "fault line" within the discipline between what was often referred to as "orthodoxy" and a younger group challenging that orthodoxy. The latter was labeled differently by various writers, including "young economists," "recent thought," and even "institutionalism" (see chap. 3). Given this awareness by contemporary actors, and in order to keep this project manageable, I followed the traditional division into "institutionalism" and "neoclassical economics," but the readers are warned that our understanding of the development of economics would not be full until we investigate the divisions within each group.

The texts analyzed in this book are all the articles which were classified under the title "Methodology" (Category 1.1) in the Index of Economic Articles, 1924–1939 (American Economic Association 1961, vol. 2). In contrast to Backhouse’s statement that “before the 1970s the literature on economic methodology was very limited” (1994b, 1), the interwar period was at least as prolific as the era after 1970, and the number of articles under the heading of "Methodology" was eighty-four. This was at a time in which there were only five general journals in economics in the U.S. and Britain and very few other minor journals. I augmented these materials by the essays in The Trend of Economics, a book edited by Rexford Tugwell (1924b) that included meta-theoretical essays of many promising young economists of the time, and that was frequently referred to by various writers during the interwar period. The importance of the book is indicated by the fact that eight out of its fifteen participants served later as presidents of the AEA, and at least four of them are considered among the most famous American economists ever (Wesley Mitchell, J. M. Clark, Frank Knight, and Paul Douglas). I also used primary and secondary sources of later years when those sources helped clarify the nature of the arguments of the contending parties.

The use of publicly published texts only has, of course, its limitations. But at the same time, it has “the advantage of being accessible, portable and static” (Backhouse et al., 1993, 17n). In order to dispel current misconceptions of interwar economics, which are based on an even more limited and biased selection of texts, the current analysis should be enough.

The interpretation of texts is not a straightforward practice. The way texts are understood depends on the readers and their prior knowledge and on the contexts in which the texts are read. As it was argued quite frequently recently, the very meaning of “meaning” is not clear; does “meaning” refer to what the author was trying to say? Is it the meaning ascribed to the texts by their intended or original readers? Or does each reader construct its own meaning of the text (Lavoie 1990a, 1990b; Gerrat 1993)? These fundamental questions are critical in any study based on the reading and analysis of texts. Since these issues have been elabo-
rated on by many writers, there is no need to repeat the arguments and I will simply state my position.

Nowhere I assume that my interpretation of the analyzed texts is objective and precise. My reading and analysis have been undoubtedly influenced by my motivation and in the context of establishing a general thesis about the history of economics. They were also influenced probably by my prior knowledge of sociology and economics and by my ideological inclinations and professional preferences as a sociologist. Yet my interpretation was done in good faith and offers—I believe—a plausible way to read these texts. It is by no means a final interpretation, but, as McCloskey often argues, it is an invitation for a conversation. It is a suggestion of an interpretation that has not been voiced so far and which will hopefully engage those who disagree in a conversation over the proper interpretation.