
TWENTIETH-CENTURY ECONOMIC METHODOLOGY

PETER J. BOETTKE

George Mason University

The birth of the discipline of political economy is often dated to the eighteenth-century Scottish Enlightenment philosophers such as David Hume and Adam Smith. Of course, there were recognized antecedents dating back to Aristotle, the Spanish Schoolmen of Salamanca, and the French Physiocrats. Even the core idea of how private interest can be reconciled with public benefit through competition had a predecessor in Bernard Mandeville. But it was the Scottish philosophers who provided the foundation of classical political economy in the eighteenth century.

The systematic study of political economy begins with the recognition of two seemingly contradictory observations about commercial life. The first observation is that individuals pursue their self-interest and do so as effectively as they are capable of doing. The second observation is that commercial society exhibits a strong tendency to produce outcomes that enhance the public welfare in terms of material progress and betterment of the human condition more generally. Squaring these two observations is how the discipline was born.

The Methodology of Economics and Political Economy: An Overview

Before we go further, I think it wise to stop and reflect on something unique about this disciplinary origin. Political economy and economics began with a reflection on an already existing set of practices in the world. It was, in this sense, in the quest to gain philosophical insight into the

mystery of the mundane life around them that led these thinkers to study the economic system. In other words, a human practice was in operation that needed explanation. Economists did not invent economic life—whether as evidenced by the organization of the household, the harvesting of crops, the rise of manufacturing, or the free trade of goods and services across borders. Economic life happens, philosophers try to understand the manifestations of it—the changes in prices, the life and death of enterprises, the complexity of the division of labor, and the wealth and poverty of nations.

From the beginning of the discipline there have been debates concerning the methods used by thinkers to gain philosophic insight into these matters. One way to reconstruct Adam Smith's critique of the mercantilists is as a methodological critique of their understanding of the wealth of nations. Following the twentieth-century economist Fritz Machlup, this chapter will make a distinction between methods and methodology, where methods refer to the various techniques that economists employ in thinking about a problem and offering an explanation, and methodology refers to the philosophic study of those methods and their epistemological status. Methods of analysis have constantly evolved throughout the history of the discipline, and methodology has shifted as well with changes in epistemology. In other words, the positivism of the Vienna Circle placed criteria on what constitutes science that were different from the criteria that were understood during the age of British empiricism. As the criteria shift, so does the understanding of what is a good question to ask as well as what would be a good answer. Methods

of analysis are adopted if they help the explanation meet the currently fashionable criteria and discarded as relics of an older unscientific age if not.

Beginning in the late nineteenth and continuing throughout the twentieth century, the discipline of political economy was transformed into the science of economics as the methods employed by economists to study the economy more closely approximated the methods employed by those in the hard sciences, such as physics. Whether the methods developed in the sciences of nature were appropriate for the sciences of man was hotly contested throughout the twentieth century and continues to be the subject of intense debate into the twenty-first century. But it must be stated that most work-a-day economists do not see this as a debatable issue. Science is measurement, and the tools employed must satisfy that goal if science is to be done. The philosophical reflection on contemporary practice, let alone the entire enterprise of economics and political economy, is found in a specialized community of academics in philosophy, intellectual history, and economics who study the history of economic thought and methodology, as well as sometimes among the elderly of elite economists as they reflect back on their careers. In the discipline of economics proper, it is the very rare case (and professionally ill-advised) that a younger scholar will venture into the field of method and methodology.

But this does not mean that the methods of economics are stagnant either in past century or today. No, they are constantly evolving as the problems that attract the attention of economists shift. However, the central disciplinary puzzle remains of explaining how through the self-interested behavior of individuals a social order can result that serves the public interest. The assessment of the truth value of this statement shifts with the times, as well as the normative assessment of economic exchange and the market economy. But every economist who has practiced the discipline since the eighteenth century would recognize the proposition that the market economy was self-regulating as central whether they agreed with it or not.

Joseph Schumpeter (1945), in his *History of Economic Analysis*, makes a distinction between “vision” and “analysis” and argues that “vision” is a necessary component of the advancement of scientific analysis. The simple reason is that “vision” is a preanalytic cognitive act that provides the raw material for the scientist to analyze. As a mere matter of description of the way the human sciences operate, the economist must have a “vision” (a set of eyeglasses) that helps to clarify the questions that are to be raised. Visions are not neutral, however, with respect to the methods one uses to analyze a problem in the social world. Science may indeed be measurement, but the scientist has to know first what it is that must be measured and possess the measuring devices required for that task. Without either an idea of what to measure or the means for measuring, the intellectual enterprise can devolve quickly into nonsense rather than science.

Vision and analysis are both important parts of the narrative on the evolution of economic method and methodology in the twentieth and twenty-first centuries. The historical experience of economic disruption due to technological change, the devastation of war and depression, and the consequences of ideologically inspired revolutions also shaped twentieth-century economics. Just as the experience of the collapse of socialist ideological aspirations of the twentieth century shaped economic thought, the tragedy of less development, the fear of manmade global disaster such as irreversible climate change, the tensions of globalization, the fear of terrorism and religious fanaticism, demographic trends toward aging populations and the unsustainable public economic obligations that were made in the past, and the global financial crisis are in the process of shaping twenty-first-century economics. So we must be mindful of how visions frame the questions asked, how ideas of what constitutes science frame both what is considered a good question and good answer, how methods chosen will be a function of the question asked and the form an acceptable answer is expected to take, and, finally, how all of this is subject to change due to shifting philosophies of science, empirical puzzles that are thrown up in the world, and innovations in techniques of calibration that appear to permit measurement where it was seemingly impossible to get measurement before.

The toolkit of general competitive equilibrium, for example, that was developed in the late nineteenth century (Walras) and throughout the twentieth century (culminating in the Arrow-Hahn-Debreu model of the 1960s–1970s) sought to provide mathematical rigor to Adam Smith’s “invisible hand” proposition. Smith’s proposition came to embody in the mind of the economist a claim about the self-regulating nature of markets through relative price adjustments; the complex interdependency of economic life as evidenced by the division of labor, specialization, and exchange; and the efficiency of the market economy in production (least cost technologies) and exchange (gains from trade realized) through the guiding function of relative prices and the lure of pure profit and the penalty of loss. The incentives and information provided by clearly defined and enforceable private property rights; free movement of prices to reflect changing circumstances of tastes, technology, and resource availability; and profit and loss accounting, which induces entry of promising enterprises and weeds out failed enterprise, are enough to ensure that the market economy will satisfy the welfare criteria established by the theory of general competitive equilibrium. At least that is what elementary economics taught in the first chapters of Marshall’s (1890/1972) *Principles of Economics*, as well as in the first chapters of Stiglitz’s (1993) *Economics*, and for the most part every major textbook in between.

From at least the time of John Stuart Mill’s (1909/1976) *Principles of Political Economy*, economists always have admitted that there were ample situations where the

“invisible hand” of the market would be hindered in its operation. The problem of monopoly was mentioned throughout the classical literature (though the source of monopoly was not seen in the natural tendencies of the market by many). The problem of common-pool resources was also mentioned, as were examples of what later would be termed externalities, asymmetric information with certain commodities, inequalities in distribution, economy-wide business fluctuations (theory of general glut or economic crisis), and public goods. The *laissez-faire* presumption that Mill laid out nevertheless had grounds for exception from the *laissez-faire* principle that was quite large. Many economic debates about method were in fact debates about how persuasive that case for the exception from the *laissez-faire* principle was. As analytical tools evolved, the answer to that question changed. Pigou had one answer, Coase had another, and Buchanan had yet another. To be clear, it is important to remember that if the *laissez-faire* principle stands, then the role of the economist is limited to that of a scholar and teacher, perhaps social critic, and the role of the government is mainly seen as that of a referee to the economic game. But if there are grounds for rejecting the *laissez-faire* principle, then the economists’ role in society is potentially transformed into that of a policy engineer, and the government’s role is transformed from a referee to an active player in the economic game. The method and methodology of economics are not invariant with respect to the policy aspirations of economists and political decision makers. It has been argued that the intended audience of Adam Smith’s (1776/1976) *An Inquiry Into the Nature and Causes of the Wealth of Nations* was the enlightened statesman, and one could just as easily argue that this was true for the major works of Mill, Marshall, Pigou, and Keynes as well.

The theory of market failure developed by Paul Samuelson in the middle years of the twentieth century attempted to show under what precise conditions Smith’s “invisible hand” proposition broke down. Ideas such as positive and negative externalities, free riders, excludability, nonrivalry, and so on became part of the everyday language of economists due to Samuelson’s efforts both as a theoretical economist and as the leading textbook author for at least two generations of college students of economics. Samuelson’s impact was in providing the latest reasons to doubt the veracity of Smith’s proposition, and the form of argument in economics that he championed transformed the way economists must present their work for assessment among their peers. To eliminate ambiguity in argument, Samuelson argued, the rigor of mathematical formalism must replace the literary vagueness of an earlier less scientific age of economic analysis. The casualty of this transformation in method, Samuelson insisted, would only be the losing thinking of previous generations—losing thinking that produced an unfounded “faith” in *laissez-faire* and the invisible hand of the market economy. Samuelson spearheaded the neo-Keynesian synthesis in

macroeconomics and the transformation of the welfare properties in microeconomics. Every area of economics, circa 1950 and 1960, was touched by Paul Samuelson. After Samuelson, the method and methodology would be unrecognizable to the previous century and a half of economic and political economy thinkers since Smith in a way that was not the case for, say, the history of the discipline from Smith to Frank Knight.

The Keynesian revolution and the development of macroeconomics in general offered an alternative vision that argued that in the context of the modern money-using economy, the classic link between private interest and public benefit had been severed, and thus the market could not be relied on to self-correct. Rather than self-correcting, the capitalist economy was said to be inherently unstable. Both the original Keynesian income expenditure model and the later neo-Keynesian IS-LM model were developed to demonstrate how once the link between savings and investment was broken, the classical vision of a self-regulating market economy that steered the self-interested behavior of individuals in such a direction that the public benefit was served could no longer be sustained. This model, not without challenges from thinkers such as Milton Friedman, dominated economic thinking in the post–World War II era.

Simultaneously with the lost faith in the central proposition of classical economics, economists also developed models that demonstrated that the market economy was prone not only to macroeconomic instability but also to monopolistic abuse and other microeconomic inefficiencies caused by various market imperfections. The model of general competitive equilibrium could no longer be said to mimic the outcomes of a free-market economy, but the model could serve as a tool of policy. The new approach to economics promised that government correctives would ensure that the welfare properties of the competitive equilibrium model would in fact be achieved even though the market economy could not achieve them when left to its own devices. The irony of this should not be lost. A model that was developed to represent what the market economy achieved without any central direction (“invisible hand”) was transformed in the writings of economists such as Abba Lerner and Oskar Lange into a guiding tool for state direction of the economy (the visible hand of government planning). Economics was transformed from a discipline of philosophic reflection on the empirical reality of commercial life to a tool of social control by enlightened policy makers. Abba Lerner’s (1944) book has the appropriate title, *The Economics of Control*; Samuelson, along with others, introduced linear programming into economics; William Baumol further developed the applications of operations research into economics; and some of the top minds in the field of economics, such as Leonid Hurwicz, would devote themselves to a field entitled “mechanism design.” In the 1940s to 1970s, the entire discipline of economics was transformed into a tool for social

control, and the methods and methodology of economics of that age fit that new purpose. Methods and methodology that did not fit the purpose of prediction and control were rejected as relics of a bygone era—a past era that was less scientific than the modern age.

In the mid-1970s, amid an economic reality of high unemployment and high inflation, the classical proposition concerning the “invisible hand” was resurrected with the work of Thomas Sargent and Robert Lucas and the new classical economics. Added to the lexicon of macroeconomics were rational expectations, time inconsistency, and the invariance proposition in policy design. The basic idea was that economists could no longer continue to model economic actors as completely passive actors that are to be manipulated by public policy decisions (as they were during the Keynesian hegemony) but had to be modeled as capable of anticipating the consequences of policies and therefore will behave in a way to put themselves in the best situation to take advantage of the policy change. This response, unfortunately, will potentially dampen the effectiveness of the proposed policy. A classic illustration of this was the Keynesian proposition concerning the trade-off between inflation and unemployment. The Keynesian consensus argued that as unemployment ticked up during a downturn, policy makers could stem this by engaging in inflationary policies. The inflation would drive down real wages, without affecting the nominal wage. In effect, workers will experience a wage cut, but due to “monetary illusion,” they do not realize this, and so policy makers can keep unemployment in check through inflation. But this Phillips curve relationship breaks down if the workers recognize that their real wages are being cut and thus demand pay increases. Rather than inflationary monetary policy keeping unemployment in check, we get instead both inflation and unemployment rising. By the mid-1970s, the empirical reality of “stagflation” was the exact opposite of what was predicted by the Keynesian model of macroeconomics. Keynesian theory was empirically questionable, and theoretically incoherent was the judgment since it lacked microfoundations, and new classical macroeconomics filled the intellectual void.

At the same time, classical market theory reasserted itself against the market failure theories of the previous decade, and ideas such as the efficient market hypothesis (Fama), competition for the field (Demsetz), and contestable markets (Baumol) were added to the lexicon of microeconomics. Ronald Coase and James Buchanan pointed out that traditional Pigovian welfare economics was logically either redundant (actors within the economy would bargain away conflicts themselves) or nonoperational (if private actors cannot bargain away the conflict, then under the same assumptions neither could public actors accomplish the policy goal). Coase and Buchanan spearheaded a revolution in economics to do comparative institutional analysis in law, politics, and the market. The conceptual framework of economics changed in the 1960s to 1970s, but many of those changes represented the resurrection of many of the themes

found in the classical writings of David Hume, Adam Smith, David Ricardo, J. B. Say, and John Stuart Mill.

But it is important to stress for our present purposes that the changes of the 1960s and 1970s were not accompanied by a change in the methodology, so the methods were not so much transformed but applied consistently and persistently and into areas that previously were deemed as out of bounds. In fact, one way to understand the new classical revolution was as a response to a dual intellectual inconsistency evident in the preceding economics literature: (a) a conflict between what was taught in microeconomics and macroeconomics in terms of core economic theory, thus requiring a search for microfoundations, and (b) cutting short the story of market adjustment in microeconomic and macroeconomic narratives of imperfection and instability, such that when the economists opened up the analysis to account for agent learning and allowed for all the accommodating changes to take place in the market economy, the claims to imperfection and instability faded away. In other words, while there may be macroeconomic questions (e.g., inflation, unemployment, growth), there are only microeconomic answers, and those answers are provided through the examination of relative price effects and their impact on the behavior of individuals as they adjust through changing circumstances through time.

The development in the last quarter of the twentieth century of property rights economics, law and economics, public choice, the new learning in industrial organization, the economics of organization and new institutionalism, new economic history, entrepreneurial studies and market process theory, and new classical economics all represented efforts of one sort or another to analyze beliefs, behaviors, institutions, and situations that previously had been treated as either beyond the scope of analysis or as part of an unexamined framework. But again it is important to stress that while economic theory evolved and applications were found in new areas, the fundamental practice of economic methodology did not change as a result. In fact, the new methods were judged against the methodological conventions of formalism and positivism (at least the understanding of positivism among economists), and to the extent that the new methods failed to fit into those self-understandings of economic *science*, they would be dismissed as potentially interesting questions that were not operational. Until the set of questions being raised by new thinking in economics could be represented in a formal model subject to empirical test via sophisticated statistical analysis, they would have little impact on the practice of economists.

The Fracturing of the Neoclassical Hegemony

I have argued that Paul Samuelson initiated the formalistic revolution in economics in the 1940s and 1950s. Samuelson’s justification for this was simple—ambiguity

in thought emerges whenever we use the same words to mean different things or different words to mean the same thing, but by forcing economic arguments to be stated in a common formal language, assumptions would have to be made explicit (not hidden) and ambiguity would be avoided. In the 1950s, Milton Friedman also persuasively stated for economists that assumptions in theory construction did not really matter provided the construction was subject to empirical test. It is the submission to falsification that demarcates science from nonsense—an economist's rendering of logical positivism, instrumentalism, or whatever balled into an operational appeal for a simple formula of economic hypotheses subject to empirical test using statistical techniques. A philosophical statement of the positivist position with respect to the development of economics was actually made in the 1930s by T. W. Hutchison, but while recognized as a classic in economic methodology, the work did not persuade practicing economists. And it was not as if economists never made explicit methodological pronouncements—both descriptive and prescriptive prior to Hutchison. Lionel Robbins and Ludwig Mises defended the a priori and deductive logic nature of economic theory in the 1920s and 1930s. Mises, in particular, was adamant in his presentation of the a priori (purely deductive) nature of economic theory and built his argument on the earlier methodological work of N. Senior, J. N. Keynes, and C. Menger. Despite how Mises's statements have been interpreted by critics ever since, Mises did not claim originality for his position but argued instead that this was in fact the way that classical and neoclassical theorists of economics had in fact always done economics: deduction from self-evident axioms, combined with subsidiary empirical assumptions and aided by imaginary constructions (including the “method of contrast,” where a world without change is constructed so we may understand the implications of change). In addition, Mises (following Weber) insisted on the positive nature of economic science against claims of ideological bias. Positive analysis prior to the philosophical development of logical positivism consisted of an argumentative strategy and was linked with Mises's consistent subjectivist stance. Treating ends as given and limiting analysis strictly to means-ends examination, Mises argued (as did Weber), would ensure the value-free nature of economics. Robbins (1932) picked up on this argument in the first edition of *An Essay on the Nature and Significance of Economic Science*. From a more continental philosophical tradition, Mises's student Alfred Schutz (1932/1967) made a similar argument in *The Phenomenology of the Social World*. However, the arguments of Mises and others that attempted to justify both methodological dualism (i.e., that economics was a science, but a science whose epistemic procedures were wholly different from those of the natural sciences) and the positive nature of economic theory proved to be ineffective in the wake of the empirical events of the 1930s and 1940s. The Great Depression and the grand ideological debates

that were played out in World War II simply demanded an economics that was technical and analogous to physics—a form of social physics or better yet engineering. The discipline bent to this demand as young economists, motivated by the momentous events of the day, pursued advanced study of economics and went to work to solve social problems. Economics had to become a discipline capable of prediction and control and not endless disputes in social philosophy if progress was to be made and the shortcomings of the laissez-faire system were to be overcome through judicious public policy.

The change in the way economists do things that took place in the 1940s and 1950s was not led by philosophers of economics but a group of economic superstars who communicated to the rising generation how you are supposed to engage in the science. In this sense, the practice of economics in the second half of the twentieth century was dictated by Samuelson and Friedman (despite their disagreements), not by the Vienna Circle, Karl Popper, or Imre Lakatos. The methodological statements of Mark Blaug or Lawrence Boland or Bruce Caldwell in the 1970s and 1980s did not dictate practice in the discipline, just as the biting criticisms of Frank Knight or Ludwig Mises or Phil Mirowski (from the 1940s into the 2000s) have not curtailed the advance of the economists' self-understanding of the discipline as both formalistic and positivistic.

Both formalism and positivism came under intellectual assault in the 1960s to 1980s in the philosophy of science literature. Formalism resulted in unrealistic and sterile presentations of human life that missed as much as they captured, and positivism worked on an assumption that empirical tests were unambiguous. Without the empirical grounding provided by clean and unambiguous statistical tests, formal abstractions were prone to become free floating. Critiques of the modernist vision of science of an analytical form in the hands of Willard Quine (whether the falsifying result addresses the main hypothesis or the network of statements that led to the main hypothesis) or of the sociological variety in the hands of Thomas Kuhn and Michael Polanyi (paradigms and the notion of progress in science) or the continental form found in Richard Rorty (that all knowledge is contextual and framed by perspective) were embraced by various heterodox thinkers in economics, such as institutionalists, post-Keynesians, Marxists, and Austrian school economists. In addition, as formalism and positivism dominated practice in economics, there were always leading thinkers in the field who admitted the difficulties of carrying out the official methodology and questioned the current practice as failing to live up to the standards set or that the standards set were unrealistic. Ed Leamer's “Let's Take the Con Out of Econometrics” (1983) was one such critique of practice, as was Donald McCloskey's (1998) *The Rhetoric of Economics*. There was, parallel to this, philosophers who challenged the economists' scientific pretensions, such as Alexander Rosenberg, who argued that economics was either mathematical politics or science of diminishing

returns, but it was not an enterprise experiencing scientific progress. Dan Hausman has even described economics as an inexact and separate science. James Buchanan argued repeatedly that economics was indeed a science but a philosophical science—which actually was a position staked out by R. G. Collingwood in the first decades of the twentieth century and was influential on Mises. Positions were stated by prominent figures, but they did not change practice or self-understanding.

During the 1980s and 1990s, survey articles on economic methodology were published in high-profile professional outlets such as *Journal of Economic Literature* and *Journal of Economic Perspectives*, and books on the subject were reviewed and discussed in a variety of traditional outlets. New journals were established such as *Economics and Philosophy* and the *Journal of Economic Methodology*. The critiques of traditional economic methodology led to a rise in heterodoxy, or at least a more self-confident and vocal heterodoxy. But ultimately, the actual methodological practice of elite economists changed little. McCloskey was a major advocate of change, but the criticisms offered in works such as *The Rhetoric of Economics* and *The Cult of Statistical Significance*, while widely read, did not have the force to change practice. What occurred instead was that attempts to justify practice by appeals to philosophy stopped among economists. Demarcation efforts were not a philosophical exercise but a complete embracing of scientific conventionalism. Economic science is what economists do, not what philosophers claim is scientific. And to do economics, one must think in terms of simplified models (parsimonious yet elegant mathematical representations) that are subject to sophisticated statistical tests. Critiques of the ability of statistical tests to answer fundamental questions (e.g., Greg Mankiw's critique of economic growth statistics) did not lead to a broadening in the notion of the evidentiary burden that must be met by contributions in the field but instead to renewed interest in finding better statistical instruments. Methods evolved, but the underlying methodology remained.

But the heterodox critique did not go completely unheeded. Questions concerning the behavioral foundations of economics led to a renewed appreciation of psychology and even neuroscience. Similarly, the institutionalist critique of economics led to a renewed examination of the legal-political-social nexus and its impact on economic life. Scholars such as John Davis have pointed to these intellectual developments among economists as evidence of a breakdown in the hegemony of the neoclassical mainstream. But one should be careful here because while psychological and institutional factors are now prominent in economic research, and the evolution of methods has led to a rise of laboratory experiments, computer simulations, and natural experiments that in a previous generation may have been viewed with suspicion, the form in which an argument must be stated in the top research journals to

be considered “scientific” has not changed all that much from the days of Samuelson and Friedman.

The Absorptive Capacity of Formalism

It is important to realize that I am not making a normative assessment of these developments (and lack of change) but instead providing a description of the intellectual landscape in economics from 1950 to today. Methods are constantly evolving but guided by a methodology that more or less has been fixed by scientific convention mid-twentieth century. Methodology not only determines to a large extent the questions that can legitimately be asked by a discipline but perhaps more important limits what would be considered a good answer to those questions. During this period, there have always been slightly out-of-sync economists who have been more or less nonconformists. Think of Nobel Prize winners such as F. A. Hayek, James Buchanan, Ronald Coase, Douglass North, Vernon Smith, and Thomas Schelling. Or broad-ranging economic thinkers such as Kenneth Boulding and Albert Hirschman. Or even recognized masters of the craft of thinking like an economist such as Armen Alchian. The economics profession during the twentieth and into the twenty-first centuries has had significant dissenters with respect to the prevailing consensus on method and public policy, but to dissent methodologically with respect to formalism and positivism was the quickest way to be utterly dismissed. And this was true even after developments in the philosophy of science literature questioned the modernist understanding of science. As McCloskey has repeatedly stressed to readers, economics is the most modernistic of the human sciences. And when the philosophy of science literature no longer justified those modernist ambitions, rather than rethink those ambitions, economists simply appealed to conventional practice. Economics is, in this understanding, simply what economists do. While previous generations of students were at least required to read Samuelson and Friedman on the methodology of economics during their first term in graduate school, the current generation of graduate students is expected to practice economics as they are taught without any serious study of the philosophical justification of the conventional methodology of economics.

Formalism has proven to be an amazingly absorptive of heterodox ideas, especially after techniques and methods were developed that broke the taboo of multiple equilibria. Once the demand for models with determinate equilibrium results (i.e., single exit models) was relaxed, different paths could be explicated in models and so many heterodox ideas could be incorporated. One must remember that economists in the 1970s and 1980s struggled to gain acceptance for game theory, computer simulations, and laboratory experiments among economists. But once it was demonstrated that these methods could be used in a way consistent with the underlying methodology of model and

measure, they found wide adoption among economists for addressing questions that more traditional methods proved to be wanting. As Paul Krugman has pointed out in his discussions of the evolution of ideas related to economic geography and economic development, many nontraditional thinkers raised questions of increasing returns and location economies, but they lacked the tools to communicate those ideas in a way that economists could find useful. The usefulness criteria, I should point out, are provided by the methodological presumptions that were enforced. Useful, in other words, not as a tool of understanding but rather as a vehicle for forming testable hypotheses.

What is true for economic geography is also true for numerous other fields in economics, such as the study of politics, law, family, extended relationships, and norms. Many of the ideas being heralded as revolutionary are in fact the restatement of ideas held by an earlier generation of economists and political economists but previously deemed as relics of an unscientific age of economics. Hume and Smith, for example, did not have a myopic view of humanity but instead believed in a behavioral model that included not only self-love but other regarding as well. The past 50 years of economic research and education have seen the placing of “old wine” of the classical school and early neoclassical writers into the “new bottles” of formalistic modeling and statistical testing empiricism.

The reports of the breakdown of orthodox hegemony by David Colander and John Davis have looked only at the method and policy dimensions, whereas the significant margin to look at that ultimately determines the character of economics is the underlying methodology. And on that margin, despite all the philosophical shifts, the basic justification of the enterprise of economics as a formalistic and positivistic science has remained unchanged. Unless an idea can be absorbed under this rubric, it will meet intellectual death. Methodology is the ultimate judge, jury, and executioner in economics, although it often lurks in the background unstated. As McCloskey has put it, when one’s intellectual range is limited to M–N, when you get up close and look, it seems as if a wide range of topics are on the table and that all those around the table are fair and open-minded contributors to the enterprise, but when you step back, you realize that the intellectual span from M–N is quite narrow and misses the entire range of issues from A–L and O–Z. This is the fate of economics in its high modernist form, and little has changed in practice except that economists no longer appeal to high modernist philosophy to justify what they do.

Where Is Economics Going?

With what I have said about the relationship between method and methodology firmly in mind, let’s look at developments in economics as we entered the twenty-first century. First, during the last decade of the twentieth century, two empirical realities became of overriding

concern to economists—the collapse of communism and the transition from socialism and the failure of development planning and foreign aid programs to lift the less developed world into a position of greater freedom and prosperity. Second, as we prepare to enter the second decade of the twenty-first century, two other empirical realities become overriding concerns to economists—the tensions of globalization and threat of international terrorism and the financial crisis and threat of worldwide depression.

One way to think about this is to envision the discourse in economics as following the shape of an hourglass. In the eighteenth and nineteenth centuries, economics was part of a larger discourse in political economy and moral philosophy. As the discipline self-identified with a more technical (less philosophical) and scientific approach to economic questions in the twentieth century, the scope narrowed. By the 1950s, the discipline of economics was narrowed to the midpoint on the hourglass. Since the 1950s, we have seen the broadening of the discipline again to take into account questions that once preoccupied the minds of the “worldly philosophers.” By the turn of the twenty-first century, economists were once again tackling questions that could be recognized by the likes of Adam Smith and John Stuart Mill, let alone Max Weber. Indeed, the “worldly philosophy” seemed to be back en vogue. Amartya Sen tried to explain this shift in intellectual focus using the language of modern economics rather than imagery such as an hourglass. To Sen, there is a production possibility frontier for economic research, with economics as engineering on one axis and economics as philosophy on the other. During the twentieth century, economics moved toward a corner solution of economics as engineering, but during the last quarter of the century, economics began to move along the frontier away from the corner solution and once again pursue economics in a more philosophical manner. In Sen’s writings, this shift relates to questions of ethics that must be raised for welfare judgments to be passed.

What I have suggested, however, is that while the questions have broadened once again, they have done so only to the extent that they can be restated in a form that conforms to the methodology that actually led to the narrowing of the hourglass (or the move along the frontier to the corner solution). I will leave to another time the question of whether this form constraint distorts the substantive content of the conversation. For now, the point is simply to identify that the methodological constraint that produced the transformation of economics in the twentieth century (Samuelson-Friedman) asserts that despite the debates (internal [e.g., over the realism of assumptions] and external), this constraint is still binding on disciplinary discourse. Economics is a model measure discipline; it is a discipline that advances through journal articles, not books, and it is a discipline whose scientific status while constantly questioned by outsiders is never questioned by those who occupy the commanding heights of the profession.

That much said, one would be blind not to see the important changes in research focus and methods of analysis that have taken place. In one sense, the empirical puzzles of the collapse of communism, the transition to capitalism, and the failure of development planning led to a renewed appreciation for the underlying institutional context of economic life. Economics in this sense is conceived of as a science whose subject is exchange and the institutions within which exchange takes place. On the other hand, the transplanting of institutions from one environment to another has proven to be a very difficult policy task, and both the transition to capitalism and the elimination of squalor and poverty in the Third World have proven to be more difficult than imagined. The contemporary empirical puzzles of the tensions emerging from globalization and the threat of international terrorism have focused economists' attention on "mental models," including ideology, religious beliefs, and cultural value systems in general. Directed by these pressing issues, economists are reexamining the cognitive foundations and behavioral assumptions of the discipline. In other words, in the upper echelons of the professional hierarchy, both the institutional and behavioral assumptions of conventional models have come under examination and a required modification.

As a question of public policy, many economists saw the link between the puzzles of the 1990s that resulted in examining the institutional assumptions and the puzzles of the 2000s that resulted in examining the behavioral assumptions. Solving the problems of transition and of Third World poverty has captured the imagination of leading economic thinkers such as Douglass North, Bob Lucas, Joe Stiglitz, Jeff Sachs, Esther Duflo, Abhijit Banerjee, Andrei Shleifer, and Bill Easterly. Many of these economists rejected the traditional neoclassical depiction of individual decision making and the market economy, as well as the free-market policy recommendations associated with the "Washington consensus." Others argued for a more sophisticated understanding of the traditional model and offered a more nuanced defense of the basic message of the "Washington consensus." These debates will continue. But the basic message from the economics of the 1990s and 2000s is that institutions matter, and while individuals respond to incentives, they are also prone to suffer delusions and other error-inducing cognitive limitations in those responses. Learning is context dependent. Behavioral economics, in particular, argues that individuals often are mistaken in their beliefs and expectations.

Actors are not perfectly rational, information is imperfect, markets are not atomistic, and resources are not always channeled to their highest valued use. While these admissions of imperfection open the discipline to new areas of research, those new areas can be pursued only via the conventional methodology. That is the dilemma of economics in the twenty-first century. Kenneth Boulding once remarked that the problem of economics (circa 1970) was that the discipline was asked to address twentieth-century

problems (depression, war, cold war) with the mathematical tools of seventeenth-century physics (Newton). Hayek made similar remarks at the time—which should not be that surprising since both Boulding and Hayek were early adherents of general systems theory and respectively influenced in their thinking by Ludwig Bertalanffy and the idea of complex systems analysis. One could argue that an analogous critique could be offered to today's economics and does in fact get voiced in the discussions of complexity theory and economics. While models of social complexity and both agent-based and complex adaptive systems are not uncommon in the literature, they have not affected practice to the extent expected by the adherents of these models. In other words, the core theory of neoclassical general equilibrium theory remains the foundation of economic analysis. In a different context, Frank Hahn once described the criticism of the edifice of neoclassical theory as a bombardment of so many soap bubbles. In other words, the critiques are offered, but they ultimately bounce off.

Still, several method shifts in economics must be reckoned with in any discussion of economics and political economy in the twenty-first century. First, there has been the rebirth of political economy independent of the Marxist tradition. This goes back to the point raised by my hour-glass metaphor or Sen's discussion of the movement along the production possibility frontier of economic thinking away from engineering and more toward philosophy. Positive political economy and constitutional political economy are intellectual developments in both the disciplines of economics and politics that have brought back the serious discussion among social scientists of the structure of government, the role of rules (formal and informal) in political and economic interactions, and the political-economic and legal-economic nexus. In one respect, post-1960 political economy can be accurately described as asking the fundamental questions of political theory from Thomas Hobbes, John Locke, David Hume, and U.S. founding fathers such as James Madison with the analytical tools of modern economics. When James Buchanan was awarded the Nobel Prize in 1986 for his development of public choice theory, political economy was well established again in the curriculum on economists and political scientists, and this has only continued in the decades since that award.

Second, there have been changes in the conceptual perspective of economists. There seems to be a willingness among economists to challenge the core ideas of rationality, self-interest, and equilibrium. But this willingness should be viewed with some suspicion because as I have argued, this new openness has been purchased by abandoning a commitment to substantive propositions in economics while steadfastly affirming the commitment to the form in which arguments must be made to be considered contributions to the economics. The criticisms associated with heterodox traditions of neoclassical methodology

have not won the day, and thus the enthusiasm one reads in Colander and Davis for the fracturing of mainstream is overstated. Instead, the criticisms must be stated in a manner that conforms to those older Samuelson/Friedman notions of formalism and positivism. Roger Koppl has argued that the cutting edge of the mainstream (he refers to it as heterodox mainstream) is now occupied by work that employs the methods of bounded rationality, rule following, institutions, cognition, and evolution. And Koppl is certainly accurate in his description, but the argument that often accompanies this description of the current state of play in the discipline and the emerging alliance between various heterodox schools of thought such as post-Keynesian, old institutionalist, new institutionalism, complexity economics, Austrian economics, and post-Walrasian economics that will effectively challenge the prevailing orthodoxy is overstated. Instead, as I have stated earlier, the orthodoxy has tremendous absorptive capacity, and the evolution of methods of analysis such as evolutionary game theory has aided absorption. Heterodox arguments that can be restated in formal terms and tested using conventional statistical techniques can get a hearing among the professional elite, but those arguments that cannot quite be presented in that form (however interesting) will not get that same hearing, let alone influence economic research. This is one possible explanation as to why leading representatives of heterodox schools of thought are rarely published in the highest impact professional journals and are often unable to obtain teaching positions in the most prestigious departments. This is not an argument about discrimination and unfair barriers to entry in the field of economics. Economics is actually a very fluid discipline, and the culture at the top departments (e.g., Chicago) is notorious for the ruthless commitment to argument and not established status of individuals. But the judgment of what constitutes a good argument is not invariant with respect to the prevailing methodology. Model and measure rhetoric was used by Samuelson and Friedman to dismiss opponents, and the same can be seen today as the challenges of heterodoxy are absorbed into the orthodoxy—whether those challenges come from the lab, magnetic resonance imaging machines, computer simulations, history, anthropology, or philosophy. Still, there can be little doubt that the methods economists are employing in their work are evolving, and this evolution enables them to tackle many questions about the dynamic nature of economic life and the complex interdependencies that previous economic thinkers were unable to ask in a way that would produce acceptable answers as judged by the methodological strictures of formalism and positivism. Consider the work in this regard of the most influential economic thinkers in the 1990s and 2000s: Andrei Shleifer, Ed Glaeser, and Daron Acemoglu. These three have explored legal origins, the nature of regulation, and colonial heritage and the origins of democratic government. The questions are broad and

the methods are creative, but the form in which the argument is stated is very conventional.

Third, there have been significant changes in the empirical techniques that economists employ in testing hypotheses. Developments in econometrics, such as nonparametric estimations, as well as instrumental variable approaches, have enabled economists to pursue empirical research on topics that previously had appeared elusive. In addition, there has been an acceptance of experiments across the board as providing not only useful but essential empirical information. Economists such as John List engage in natural experiments and field experiments. Of course, laboratory experiments have long been used by economists such as Vernon Smith to advance economic knowledge in the fundamental theory of choice, market theory, public goods, voting, and booms and busts. Smith's Nobel address is one of the most profound statements of the nature of rationality in economics, the context-dependent nature of choice, and the contingency of social order. Smith and his colleagues have studied trust relationships, cooperation in anonymity, conflict, and market efficiency. Also, developments in programming have enabled economists to do computer simulations that illuminate important economic ideas, such as the work on the interaction of zero information traders still able to generate market clearing. The focus on institutions has led to a renewed appreciation for the field of economic history among economists and political economists. The analytic narrative approach to political-economic history enables the rational choice theorist to combine the argumentative structure of economics with the compelling narratives of historical case studies (or comparative case studies). The bottom line: As the analytical methods of economics have broadened, they have been matched by new methods of empirical examination of the world around us. But note again that while the methods have evolved, the scientific aspirations of the intellectual enterprise have not—that aspiration is to provide a parsimonious model that generates testable hypotheses that are then subjected to empirical refutation.

One final change to the landscape of economics in the twenty-first century that is notable is the renewed interest in both the application of economics to unusual topics in everyday life and the popularization of economics among the public not as part of policy discourse but simply as a way of thinking about the world. This movement can be captured under the label “Freakonomics” and is mainly associated with Steven Levitt. Levitt employs a natural experiment method to tackle everyday economics and make sense of statistical anomalies that are found in an examination of the data. Tyler Cowen's forays into “freakonomics” are more conceptual than Levitt's and attempt to walk his readers through the logic of choice, whereas Peter Leeson's work is focused more on explicating the mechanisms of social organization and explaining the operation of these mechanisms in unusual social environments.

As we finish the first decade of the twenty-first century, there should be little doubt that economics is a vibrant and diverse discipline. The methods of economics are constantly evolving as the technology of analysis changes. As I have discussed, in the twenty-first century, it has become commonplace for economists and political economists to tackle questions concerning the cognitive limitations of man and the institutional contingencies of exchange relationships. Koppl is right: The cutting edge of the profession is now occupied by researchers working on questions that were previously viewed as the domain of heterodox thinkers. But the narrative provided here disagrees with the assessment provided by Colander, Davis, and Koppl that a heterodox mainstream is emerging within economics that represents a fracturing of core scientific enterprise of orthodox economics. When we look closer, what we see is that while the methods are evolving and the policy disputes are ongoing, the fundamental question of methodology and the conception of economics as a science are unchanging (and unchallenged). Economists are stuck in a world where the discipline attempts to mimic the methodology of the natural sciences. The new methods introduced (often imported from disciplines perceived as more scientific than economics to begin with) are always judged against this formalistic and positivistic standard. As long as this self-understanding and its corresponding standards of acceptance and rejection remain intact, then frameworks of analysis that focus disciplinary efforts on understanding rather than prediction will continue to be dismissed as unscientific. The challenges of the interpretative turn in the human sciences, as summarized by philosopher Richard Bernstein, are completely ignored. But today so are the admonitions by philosophers such as Alexander Rosenberg that economists must more faithfully follow the methodological prescriptions of positivism ignored. The formalistic and positivistic nature of economics is the product of scientific conventionalism, which actually proves to be a more elusive target in methodological disputes than explicit references to the philosophy of science.

Economics is what economists do, and what they do is build models and test those models against data sets with statistical tools. There are always exceptions to the rule, but the exception proves the point. Michael Polanyi once described how new contributions to science in general have to balance scientific plausibility, intrinsic interest of the community, and originality of the contribution. Economics is no different than physics in this regard. Conservative forces are weighed against revolutionary innovations in the practice of science to provide discipline so that wishful conjectures in truth seeking are channeled in a productive direction. Research efforts overlap, and the work of one scientist becomes the productive input into the scientific production process of another to form a dynamic orthodoxy. There has not been a revolutionary shock to the methodology of economics since the mid-twentieth century. There has been a broadening of topics and even the emergence of

new and exciting methods in contemporary economics, but the basic notion of what it means to be doing scientific economics has not changed much since Paul Samuelson set the standard for theory and Milton Friedman explained what it meant to do positive economics. Methods of analysis are constantly changing and policy disputes are ongoing, but the underlying methodology of formalism and positivism has not been effectively challenged since it came to define the self-understanding of economics in the post–World War II period. So far, nothing in the twenty-first century practice of economics suggests that change to this self-understanding of scientific economics will come anytime soon.

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