

AUSTRIAN ECONOMICS AND THE MAINSTREAM: VIEW FROM THE BOUNDARY

ROGER E. BACKHOUSE

As a mainstream economist who is sympathetic to some, but by no means all, Austrian themes, I wish to offer an interpretation of the difference between Austrian economics and the mainstream that differs from some of the ones I have found in the literature. It is my view that mainstream and Austrian economists should pay more attention to each other, but to do so requires a clearer understanding of what distinguishes these approaches.

It is my view that the distinctive feature of mainstream economics, distinguishing it from Austrian economics, is model-building and the important point about models is that they are not the real thing. This may seem a trivial and obvious point but it has been ignored in much of the literature comparing Austrian economics with the mainstream even though, as I hope to show, it has great implications. As examples of this literature I focus on three main contributions: Vaughn (1994), Boettke (1996) and the methodological essays in Rothbard (1997). I choose these three because they are clear and each comes close, in a different way, to the position that I am putting forward.

DISTINGUISHING BETWEEN THE TRADITIONS

Conceptual

Austrian scholars who have sought to clarify the differences between Austrian economics and the mainstream have, entirely correctly, emphasized

ROGER E. BACKHOUSE is professor of economics at the University of Birmingham, United Kingdom. This is a revised version of the paper presented as the Hayek Memorial Lecture at the fifth Austrian Scholars Conference at the Mises Institute on April 16, 1999. I am grateful to Lew Rockwell and the Mises Institute for the invitation. The paper was written whilst holding a British Academy Research Readership and I am grateful to the British Academy for its support.

that the vulgar textbook claim that Menger did no more than offer a nonmathematical version of marginalist price theory, is far from correct. This has led many of them to focus on beliefs about the economic world as distinguishing the two traditions. Vaughn (1994, p. 2), for example, argues that methodological individualism and radical subjectivism are the assumptions shared by all Austrians. As constrained maximization is too narrow a framework within which to formalize these ideas, they imply a rejection of mainstream economics. Even Boettke (1996, p. 24), whose position is very close to the one I am offering here, argues that neoclassical economics can be identified with the Chicago program of maximization, stable preferences, and market equilibrium.

In contrast, Rothbard (1997, chaps. 3 and 7) locates the difference squarely in methodology. Austrian economics, he contends, is defined by the method of praxeology. Economics starts not with empirical laws but with explanatory axioms that are known with certainty. The fundamental axiom is that human beings act—they make choices in order to achieve their goals. It follows that the method of Austrian economics is to use thought experiments to derive qualitative laws. These laws are absolutely true, regardless of time and place, and need only to be applied to, not tested by evidence (pp. 130–32).

The thesis I wish to propose is that Rothbard is right to argue that the difference is methodological but that the methodological difference is not the one he proposes—praxeology does not capture the important issues. The key difference between Austrian economics and the mainstream is, I suggest, found in their different attitudes to models. Mainstream economics is about modelling in a way that Austrian economics is not. Though Vaughn (1994, p. 2) is right to see an aversion to mathematics as “a superficial identifying characteristic” of Austrian economics, attitudes toward mathematics point to the fundamental differences. Though this may seem close to the vulgar textbook claim that Austrian economics is simply neoclassical economics without the mathematics, it is, as I hope to show, very different. It is essentially a thesis about the role of models in mainstream economics.

Before developing it, however, there is an important historical point that needs to be made. That is in making my comparison between Austrian economics and the mainstream, I am referring only to the period that follows what Rothbard has called a “profound revolution in the methodology of the discipline” (1997, p. 28). This revolution, which took place between the 1920s and the 1960s, involved the “accelerated mathematization” of the subject to the extent that Rothbard could describe it as having become like a “third-rate subbranch of mathematics,” impenetrable to the layman. However, though economics did

become mathematized, the revolution involved even more profound changes in the structure of the discipline.¹

Historical

The change that took place between the 1930s and the 1960s is clearly illustrated by the career of Hayek. In the 1930s Hayek was part of the mainstream. By this I do not mean that his economic theories were not distinctive or that they did not form part of an “Austrian” tradition. What I mean is that the nature of the mainstream was such that a plurality of approaches could be encompassed within it. Hence, Hayek could view himself as “part of a broad scholarly community that had progressed beyond distinctions as to school or country of origin” (Vaughn 1990, p. 389).² Keynes and Hayek argued vigorously over the cycle³ and the degree of their mutual incomprehension was sufficient to suggest a Kuhnian paradigm conflict. However, they could not ignore each other. Surveys (e.g., Haberler 1936; Hansen and Tout 1934) took it for granted that both Keynesian and Hayekian theories had to be discussed and that they could usefully be discussed alongside each other.

In contrast, by the 1950s, virtually all discussions of the cycle took place within the framework of the second-order difference equation defined by the multiplier–accelerator model. Theories that did not fit within this framework, including Hayek’s, along with many others, were ignored. The diversity of prewar business cycle theory was completely lost. Similar changes took place in microeconomics, the changes here being perhaps best illustrated by the contrast between J.M. Clark’s discussion of “workable competition” and the perfect competition of the Samuelsonian “neoclassical synthesis.” Even before he moved to other fields, Hayek was no longer regarded as doing serious economics. His work on knowledge, the market, and competition, which could have found a place in the 1930s mainstream, did not fit in.⁴ Hayek may have changed but the mainstream had changed even more profoundly.

Enthusiasm for Keynesian economics was part of the reason but, in the longer term, the methodological revolution, with which the Keynesian revolution was

¹For a broader account of this transition, see Morgan and Rutherford (1998).

²Vaughn (1990, p. 389) argues that Hayek could take this view because discussions centered on limited problems such as “how one defined capital or what the role of bank money was in a trade cycle.” In the 1930s it was still possible for economists working within a variety of approaches to participate in a common debate. In contrast, by the 1970s the range of approaches that could be accepted into the mainstream discussion of even such limited issues had greatly narrowed. By the 1990s, the range had narrowed even further, excluding even those working with “old” Keynesian models.

³See Caldwell (1995).

⁴It was, of course, not just the Austrians who suffered this fate.

inextricably linked, was arguably more important. Hayek did not fall out of favor because he was not Keynesian (neither are Friedman or Lucas) but because he was perceived to be doing neither rigorous theory nor empirical work.⁵ Boettke (1996) views Samuelson's *Foundations of Economic Analysis* (1947) as crucial to this process, establishing the scientific way to do economics. Blaug (1999) places more emphasis on Arrow and Debreu's "Existence of an Equilibrium for a Competitive Economy" (1954). Mathematical economics like this was greeted with incomprehension by the older generation of economists, such as J.M. Clark.⁶ Mathematics did more than create the appearance of being scientific and provide an entry barrier—it narrowed the range of issues that could be explored. However, while this movement took hold of the profession in the 1950s, its roots go back to the 1930s. Though using only simple mathematical techniques, Robinson's *Economics of Imperfect Competition* (1933) exhibits many characteristics of the new approach: a narrowing of the questions, with real-world complications being assumed away to the point where the analysis becomes mathematically tractable. What Robinson candidly described as just "a box of tools" became the standard microeconomic fare of introductory textbooks alongside the Hicks–Samuelson interpretation of Keynesian macroeconomics.

CHARACTERIZING MAINSTREAM ECONOMICS

Substantive Assumptions About the Real World

A "static" comparison between two traditions is possible only if there is, common to each tradition, an unchanging set of assumptions—a Lakatosian hard core. The problem is that such hard cores are, as the literature on Lakatos's methodology of scientific research programs shows, very difficult to isolate.⁷ Given this venue, I can leave aside the problem of finding an adequate summary of Austrian economics and will focus instead on the problems involved in finding substantive assumptions to which all mainstream economists are committed.

⁵Friedman is an interesting case for he succeeded in remaining part of the mainstream despite objecting to prevailing methods. See Hirsch and de Marchi (1990) and Backhouse (1995, chap. 11).

⁶See Blaug (1999, pp. 257–58, n. 2) remembers Clark admitting to his class that he could not understand an article by Patinkin that we would expect today's undergraduates to read.

⁷De Marchi and Blaug (1991) is the major source for this. For a response, see Backhouse (1998a, chap. 3). Though it is not central to the present argument, I suspect it is difficult to offer a convincing Lakatosian reconstruction of Austrian economics. Rizzo (1982) outlines the hard core and heuristics of a possible Austrian scientific research program, but he fails to use this to provide a rational reconstruction of the evolution of Austrian economics, so it is not clear how far his characterization of the scientific research program can be defended.

1. Maximizing behavior: satisfying models and behavioral theories.
2. Stable preferences: there are models of endogenous preferences. Hildenbrand (1994) has even sought to derive the main propositions of demand theory without any of the traditional assumptions about preferences.
3. Equilibrium: game theory has transformed concepts of market equilibrium to the extent that it is not clear what it means to say that economists are, in general, committed to assuming equilibrium.
4. Perfect knowledge: countless economists have explored ways of modeling imperfect knowledge, even where probabilities cannot be attached to outcomes.

Mainstream economics has undermined views that previously seemed well-established.

Positivism

If anything holds mainstream economics together it is not specific assumptions about the real world but a methodological commitment. This is Rothbard's (1997, p. 29) claim. He argues that the reason for the methodological revolution of the 1930s and 1940s was a commitment to a positivist methodology. This explains why mainstream economists have become "locked . . . into . . . absurd" assumptions such as everyone in the market has *perfect* knowledge (1997, p. 122) or that the economy is always in long-run equilibrium (1997, p. 30).⁸ There are, according to positivism as portrayed by Rothbard, four steps in the development of scientific knowledge.

1. Observation of empirical regularities or "laws."
2. Construction of hypothetical explanatory generalizations from which these laws can be deduced and, hence, explained.
3. Deduction, from these generalizations, of further consequences that can be tested.
4. Rejection or modification of explanatory generalizations in the light of test results.

The starting point in this schema is a set of empirical laws, not the explanatory axioms that underlie Austrian economics. This approach to economics, Rothbard claims, is flawed for at least three reasons. No one has yet discovered any empirically robust laws (1997, p. 31). Theories can never be tested because it is impossible to undertake controlled experiments where all relevant variables

⁸Boettke (1996, p. 22) makes a similar point when he refers to natural language having been weeded out in the same way that Betamax was gradually pushed out of the market by VHS despite the former's technological superiority.

are held constant and hence *ceteris paribus* clauses will never be satisfied (p. 30). The act of making a prediction may change the forces at work (p. 31).

This is the point at which I part company with Rothbard. Though mainstream economics has been transformed in the way he suggests, and though “operationalism” (Samuelson) and “positive economics” (Friedman and Lipsey) were important and influential methodological precepts, to view mainstream economics as “positivist” not only oversimplifies but also seriously distorts it.⁹ Positivism, with its requirement that theories be grounded in observation statements, is a profoundly empirical doctrine, whereas much contemporary economic theory is profoundly unempirical.¹⁰ Models are not grounded on empirical laws about the economy. Many mainstream economists would be as skeptical about the possibility of discovering robust empirical laws as are most Austrians and would be suspicious of any models based on alleged empirical constants. In macroeconomics this emerges as the requirement that theories have micro-foundations. Furthermore, not only do many economic models not describe real-world situations, they describe situations that could not conceivably describe the real world. The classic example here is the Arrow–Debreu equilibrium model.

It follows that praxeology, as defined above, does not distinguish between Austrian and mainstream economics. Neoclassical economists frequently judge models not by their ability to survive empirical testing but by their conformity with individual optimizing behavior. Though the idea is developed in a very different way, this involves assuming that individuals have preferences and choose the action that leads to the outcomes they prefer. Like Austrians, they refrain from making any quantitative predictions.

Modelling

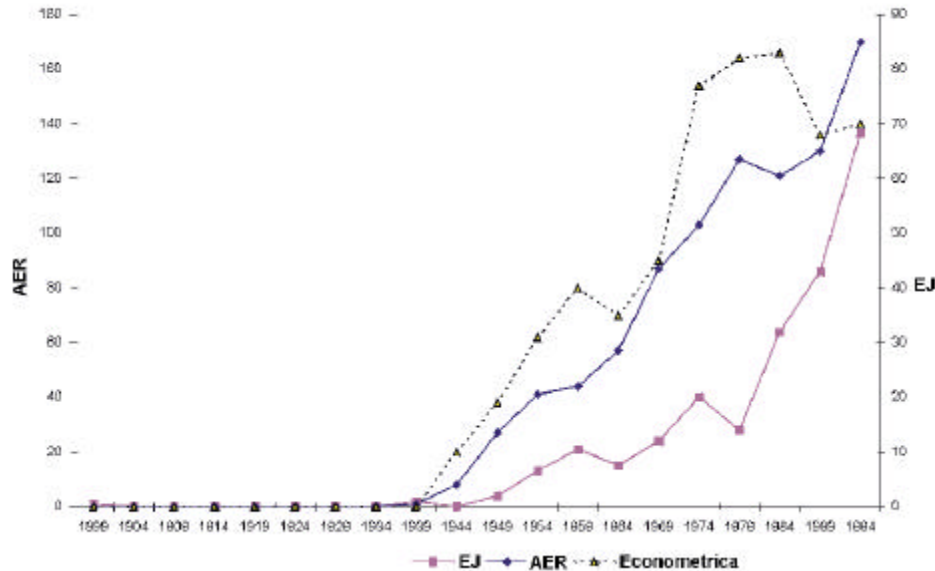
The main reason why these characterizations of mainstream economics fall down is that they pay no attention to what I suggest is the key feature differentiating contemporary mainstream economics from Austrian economics (and from certain other heterodoxies)—the use of models. Starting in the 1940s, economics became a discipline dominated by models. This is illustrated in Figure 1, which shows the frequency with which the term has been used in three leading mainstream journals. It was virtually unknown before the 1940s but since then has become standard practice. This is a phenomenon that, until recently, even the methodological literature ignored.¹¹

⁹I have a similar objection to McCloskey’s (1986) arguments about positivism even though he emphasizes different things.

¹⁰I owe this point to Terence Hutchison.

¹¹This does not mean that all the issues related to models were ignored. Many came up in discussing economic theories.

FIGURE 1
THE USE OF THE TERM "MODEL"
IN THREE LEADING MAINSTREAM JOURNALS



Note: This shows the number of articles in which any of the following phrases is used: economic model, mathematical model, empirical model, theoretical model. This method was chosen to avoid picking up articles that used the word model in unrelated senses (as in "the legislation of 1920 was the model for the new deal").

Perhaps the most important point about models is that they are not the same as the real-world phenomena to which they are related. A model railroad may be a working, exact scale model of a real railroad and it is even possible that the track layout and buildings may be faithful models of a specific real-world railroad. A successful model railroad will create the illusion of being a real railroad, and yet no one will ever confuse the two. No matter how accurate the modelling, the differences are clearly too numerous for this to be possible. In the same manner, economic models are different from any realities they might represent. The modeller makes decisions about which dimensions and aspects of the real world are to be represented in the model and which are not. The result is that it is a mistake to assume that economists necessarily believe the models they create. Models are created and analyzed even though their users know that they are, in important senses, unrealistic.

Why do mainstream economists do this? The main reason, which Austrian economists should recognize, is that the economic world is extremely complex.

General theories are impossible to obtain which means that more limited objectives must be pursued. In choosing the route of constructing models, economists are choosing to analyze artificial worlds that are simple enough for conclusions to be drawn. This, of course, raises the question of whether the models are sufficiently realistic for conclusions drawn from them to be of any use, the Austrian claim being that they are not. The conventional response to this charge is to argue that models need to be tested, which in turn raises questions about how they can and should be tested. The point I would like to emphasize, however, is that the realism of a model depends on the purpose for which it is being used. Take a model railroad. If we use this to ask whether the grocery store can be seen from the top of the station clock tower, it may be correct to assert that the model is realistic, for it can reliably be used for this purpose. However, if we wish to ask about how quickly trains can travel it is not realistic. The same is true of economic models: they may be usable to answer some questions but not others.

Another reason for working with models is a commitment to what is perceived to be rigorous theorizing.¹² Unless we define mathematics extremely broadly, this is not the same as saying, as some critics have, that mainstream economists are committed to using mathematics. It is arguable that economists' primary commitment is to rigor and that they have used whatever tools have been available to achieve this. In the past century this involved what we think of as mathematics, but there is no reason why this should always be the case. However, despite the emphasis on rigor and formal arguments, models have to be linked to reality and here informal arguments are involved. The testing of assumptions and theoretical results can never be completely formal, for judgments have to be made about the relation of theoretical terms to the real world and about the conclusions that should be drawn from the data.¹³ Just as important to understanding the way in which models are used, the relationships between different models involve informal arguments and economists' understanding of models and the concepts involved in them evolve over time.¹⁴

To illustrate the use of models, consider an example: Arrow's influential paper on the allocation of resources to research and development (R&D) (1962).¹⁵ In this paper Arrow constructs a model from which he draws what are

¹²Weintraub (1998) argues that conceptions of rigor have changed, even in mathematics, during the twentieth century.

¹³This is the problem of replication, discussed in Backhouse (1997, chap. 11).

¹⁴See Backhouse (1997, chap. 10; and 1998b).

¹⁵This example is discussed in detail in Backhouse (1999).

very clearly theoretical conclusions—theorems derived from the model. He assumes that investment in R&D produces knowledge; that knowledge, once produced, is a public good the benefits from which cannot be appropriated fully by its producer; and that as a result the production of knowledge will be suboptimal. The formal results refer to the model—to a hypothetical world—not to the real world. However, Arrow also draws conclusions about the real world—about what will happen in a free-enterprise or private-enterprise economy. It is at this stage that less rigorous arguments, inevitably, come in. Three assumptions are particularly important in deriving Arrow’s results: conceptualizing R&D as producing a public good (knowledge); interpreting free enterprise as perfect competition; judging efficiency in terms of comparison with theoretical ideal. Though these are clearly fundamental assumptions, they have received virtually no attention in either Arrow’s paper or in the large theoretical literature that builds upon this.¹⁶

To sum up, despite some of the optimistic rhetoric associated with claims for “positive economics” in the 1950s and 1960s, models are not representations of the economy that can be formally tested and applied to derive rigorously-established propositions about the economy. They are tools that have to be used and using tools involves making judgments and engaging in less rigorous forms of argument.

Suggesting that mainstream economists are primarily committed to rigorous theorizing rather than to positivism, or to specific beliefs about how one should model the economy may seem a minor point, but it is very important. It has great implications for trying to understand the way in which economic theory evolves. When economists are finding a strategy to cope with anomalous evidence it makes a big difference whether their primary commitment is to rigor or to the assumption of optimizing behavior.¹⁷ Thus, although mainstream economists may appear, at the moment, to be committed to the assumption of maximizing behavior, if a suitable alternative is offered, this assumption could be abandoned. In addition, if criticisms of mainstream economics are to be effective, it is important that they cannot simply be met with counterexamples that undermine the premises on which the criticisms are based.¹⁸

¹⁶This was raised by Demsetz (1969) in a widely-cited article. However, few of the citations to the article take up this point.

¹⁷For a case study of economists trying to cope with anomalous experimental evidence, see Hausman (1992, chap. 13).

¹⁸It is tempting to add a third point, that if this interpretation of mainstream economics is correct, it places a question mark over Boettke’s (1996) claim that “Austrian economics is historically a school within the broader tradition of neoclassical economics.” However, to answer this would take me into a discussion of how far the mainstream can be described as neoclassical and what that term means. Such issues, though interesting, are outside of the scope of this present article.

CONCLUSIONS

Why Austrian Economists Should Pay Attention to Developments in the Mainstream

Before turning to the question of why the mainstream needs Austrian economics, I wish, in the interests of balance, to offer some thoughts on the reverse proposition—why Austrians should pay attention to the mainstream.

The first reason is a rhetorical one. Austrian economists frequently criticize the mainstream and contrast the two approaches. As the mainstream is evolving, such comparisons need to take account of the latest developments if they are to be effective. Austrians may consider mainstream treatments of time and uncertainty inadequate, but the literature is growing and if it is not taken into account, criticisms will, with some justification, be dismissed. It is, for example, not enough to focus on the Arrow–Debreu model: if the charge that the mainstream does not deal adequately with problems of information and uncertainty is to be effective, the models of Stiglitz, Weiss, Akerlof, and others need to be considered.

The second reason is that even if we accept that economic theories cannot be tested in the way that optimists of the 1960s thought, Austrians have never, it seems to me, explained why models cannot be used for exploring the meaning, coherence and implications of concepts. Boettke (1996, p. 27) writes of Austrian economics pursuing the neoclassical project in “natural language.” He may be right to argue that mathematics alone is inadequate for the exploration of “social processes that defy determinate solutions” but no Austrian, to my knowledge, has ever explained why mathematics cannot be used alongside natural-language explanations. On this, my instincts are with Marshall who, though as aware as any Austrian of the problems associated with the use of mathematics in economics, nonetheless believed that mathematics was an invaluable input into the process of theorizing.

The third reason is that mainstream economics does include much work that is relevant to Austrian themes. Anyone interested in market processes should be interested in understanding how individual markets work and hence in, for example, Kirman’s analysis of the Marseilles fish market. The data on which such studies are based relate to the actual trades made by real individuals. Some Austrians (for example, Rizzo 1982, p. 58) take it as an axiom that markets are equilibrating. In common with many mainstream economists, I would wish this to be a result, not an assumption.

Why Mainstream Economists Should Pay Attention to the Austrians

Mainstream economics is based on the construction and analysis of models. However useful they are, because they are models, they should not be thought of as fully accurate descriptions of the real world. The problem is that

economists believe their models. Models should be used but they should not be believed. If mainstream economists were to pay more attention to the Austrians, they would remain more skeptical about their models. I cite three examples.

1. Arrow's analysis of research and development expenditure is important in that it shows one possible way to tackle the problem. The problem is that a literature has developed based on the assumption that it is the correct way to tackle the problem, ignoring other possibilities that may, in practice, be as important.
2. General purpose technologies: models have been developed to show that the introduction of new general purpose technologies may lead to a productivity slowdown. Economists have jumped to the conclusion that new GPTs must always have this effect even though there is evidence that this is not always the case.
3. Real business cycle theory: supporters of real business cycle have shown that technology shocks in perfectly-competitive continuous market clearing models could explain some important time-series properties of major aggregate variables. The conclusion has been drawn that this is the explanation, i.e., that the models are confirmed.

More generally, the problem is that models of competitive equilibrium are more tractable than other types of models, which means that such models are more highly developed than other types of models. Game theory is changing this rapidly, but the problem is recurring here in that models based on manageable solution concepts are being developed most rapidly. The result is that because economists have been able to create such models, there is a tendency to believe that they are an appropriate way to model the world. Economists have a tendency to jump to the conclusion that the models they can construct are correct.

The main reason why, at least at present, Austrian economics is particularly relevant is that it offers a strong challenge to some of the most basic assumptions underlying mainstream models—the assumptions that are so useful that they are most likely to be taken for granted. These include perfect competition, equilibrium, stable preferences, given technology and Pareto efficiency as a welfare criterion.

Lest this seem fanciful, let me give an example that relates to the work of F.A. Hayek. Hayek (1937) raised many questions about the concept of competition. Only two years later, Hicks (1939, p. 83), who must have been familiar with Hayek's work, shied away even from introducing monopolistic elements into *Value and Capital*, arguing that without the notion of perfect competition most

of economic theory would lie in ruins. He saw no choice but to assume perfect competition. It could therefore be argued that the imperatives of model-building provided the reason why Hicks developed the line of inquiry to which Hayek had raised such profound objections.¹⁹ This takes me back to my earlier point that model-building is the key to understanding the difference between Austrian economics and the mainstream. I remain on the boundary between the two traditions in the sense that while I see a greater role for model-building than do most Austrians, I share with many Austrians the view that there is a great danger from their misuse.

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¹⁹It should, of course, be added that Hicks not only made important contributions to other ways of doing economics (as in *The Social Framework* and *A Theory of Economic History*) but also repudiated much of his earlier work.

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