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**University of
California, Davis**

**BOETTKE'S AUSTRIAN CRITIQUE OF
MAINSTREAM ECONOMICS:
AN EMPIRICIST'S RESPONSE**

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**Department
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Abstract

Many of Boettke's criticisms of formalist economics are justified. However, he defines it so broadly that it becomes practically synonymous with mainstream economics. Yet he blames it for the specific sins of formalist economics more narrowly defined. And since he treats Austrian economics as the only viable alternative to mainstream economics he incorrectly awards victory to Austrian economics. It has some valuable ideas to contribute to mainstream economics, but is not a good replacement for it, since it has serious deficiencies.

Boettke's Austrian Critique of Mainstream Economics:
An Empiricist's Response

Thomas Mayer*

Since the 1970s economics has undergone a major revolution, a revolution at least as much in methodology and "scientific taste" as in substantive doctrine. All along economists, like other scientists and serious scholars have treated rigor as an important virtue. But now the value attached to rigor relative to the other desirable characteristics of a theory, such as empirical confirmation and relevance to practical problems, rose sharply. The leaders of this formalist revolution seem to classify all economic analysis into mathematically formalized models and "mere talk". One might be excused for gaining the impression that they consider the dichotomy of "mathematically formalized" and "mere talk" more important than the dichotomy of empirically confirmed and empirically disconfirmed.

Despite its insistence on explicit statements and rigor the formalist revolution was not itself founded on a detailed and rigorous analysis of why formalism is superior to the older way of doing economics. In place of detailed reasoning its proponents offered mere assertions, often vehemently stated and treated as though they were self-evident. (Cf. Montgomery, 1997, Backhouse, 1997) Its great success should therefore be attributed not to superior logic, but, in part, to its advocates being brilliant scholars, highly respected for their substantive work. Another reason is that - at least to those who confuse mathematics and empirical science - formalism fits the image of economics as a science that economists want to project. Moreover,,

because - within the confines of the model - formalist analysis provides a high degree of certitude, many economists find formalism comforting. That it sharply differentiates the professional economist's product from cocktail-party chatter is also satisfying. In addition, it allowed the young, who generally had better mathematical training than their elders, to achieve dominance.

But although the formalist revolution succeeded in establishing rules that economists must adhere to if they want to publish on mainstream topics in the "respectable" journals, it did not capture the hearts and minds of all economists. It is not certain even that it commands the adherence of half of all the academic economists, and it is likely that outside of academia only a distinct minority of economists accept it. But the tone of economics is set by those who publish in the leading journals and teach in the major research universities. There it dominates.

But by now counter-revolutionaries are active. Peter Boettke is one of them. Being a counter-revolutionary myself I applaud his attack on formalism, though defining it in a narrower way than Boettke does. (see Mayer, 1993, 1995). But counter-revolutionaries, like other revolutionaries, are a quarrelsome lot, so I will criticize some aspects of Boettke's critique of what he calls formalism, and not discuss the many points on which I can only say "right on." In summary, I criticize Boettke mainly for confounding formalism with broader mainstream economics, and with thus posing a false dichotomy between formalist and Austrian economists, for going too far in his criticism of formalism, as well as for overstating the contribution of Austrian economics.

But before coming to these criticisms one should note two of the strong points of his essay. One is his distinction between the idealizations used in economic theory, and the criteria that should be used to judge the functioning of an economic system, a point discussed below. Another is his insistence that when discussing broad issues of economic policy, we have to look beyond what economic theory can tell us. Institutions do matter.

Since Boettke gives a prominent role to Abba Lerner as a formalist and proponent of market socialism, who gets it wrong because he ignores institutions, it may be appropriate to digress with a story Lerner once told. He said (private conversation) that after he wrote the *Economics of Control*, he believed that he had shown that it does not matter much whether a country is capitalist or socialist, as long as its managers follow the correct optimization rules he had set out. But, he added that he preferred socialism because (if I remember correctly) of its more equal distribution of income. However, subsequently he changed his mind - because of false teeth. On a visit to Israel he noticed that it did a thriving export business in false teeth. This, said Lerner, was logical because making false teeth is a business that requires much skilled labor and little capital. But, Lerner added, no government planning agency charged to increase exports would ever think of false teeth. Since Boettke stresses the importance of innovation under capitalism, there is therefore much less disagreement between him and Lerner than he suggests.

I. Boettke's Critique of Formalism

Boettke (1997) writes that in describing economic behavior in

mathematical language formalists drained the real world of its complexity. Hence formalism swept away "historical work on the complex web of institutions that undergird capitalist dynamics" (p. 21). Formalists, such as Samuelson, have "drained economic theory of institutional context. ... Parsimony won out over thoroughness." (p. 22) "[T]he real problem for economics was that the medium was becoming the message as the strictures of formalism denied scientific status to realistic *theory*. ... Ideas that defied the techniques of formal analysis came to be considered unworthy of serious consideration." (p. 21, emphasis in original.)

Boettke's delineation of what he calls formalism thus focuses on two separate characteristics. One is its extensive abstraction from institutional context. He is right in believing that this is required for and fostered by mathematical modeling. But not only by mathematical modeling. All thinking requires abstraction from a wealth of detail. When Austrian economists talk about entrepreneurship they, too, are abstracting from the real world, ignoring, for example, "irrelevant details", such as whether the firm (entrepreneur) is a closely held corporation, or one with widely dispersed stockholders who have little power over its management.

Hence Boettke's complaint that formalism abstracts from real world institutions is too sweeping. What matters is whether it is the important or the peripheral characteristics that are being abstracted from. But how do we know which are the important ones? When Austrians complain that formalist economics abstracts from the fact that historical time is irreversible, formalist

economists can reply that time being irreversible is an irrelevant characteristic that does not affect the insights and predictions generated by their theories.

Friedman (1953, Ch. 1) has argued that until we know whether a theory's predictions are accurate we cannot say whether its unrealistic assumptions, that is its abstractions, matter. An alternative criterion stresses explanation in place of prediction. According to this criterion, the right abstractions yield a theory that makes us understand the phenomenon, that provides what Fritz Machlup (1978, p. 145) called a sense of "Ahaness." Though philosophers of science and methodologists argue about the choice between these criteria, most of us pay some attention to both, though we differ in the relative weight we give them. Even if a theory seems to explain well, in the sense of linking a particular phenomenon smoothly to our prior beliefs, we do not accept it if its predictions are consistently falsified. Conversely, we usually reject as a mere spurious correlation a hypothesis that predicts well, but "makes no sense." Under neither criterion do we reject a theory merely because it abstracts.

One might perhaps respond that some abstractions are so obviously wrong that even without knowing how well a theory based on them predicts or explains, we can say with confidence that this theory must be wrong. But even though there are instances where this is correct (e.g. abstracting from self-interested behavior and assuming firms are driven only by altruistic motives) are there many such cases in economics? Boettke is able to say yes, only because he takes it as a given in his essay that

Austrian theory is correct. Hence, if formalist economics ignores some variable that play a significant role in Austrian theory the formalist theory must be wrong. But why assume a priori that Austrian theory is correct?

However, a more nuanced version of Boettke's criticism of the formalists' abstractions is correct. This is that often mathematical modeling not only requires more abstraction than verbal analysis, but also that it seems harder to be mindful of some of the abstractions that have been made when we read a mathematical analysis, than when we read a verbal analysis (see Keynes, 1936, pp. 297-98). Moreover, it is tempting to decide what part of reality to model and what part to abstract from more by the criterion of what is mathematically tractable, than by the criterion of what is important for the problem at hand. What makes this problem worse is that modelers usually do not point out that, since they are abstracting from some characteristics that may be salient, their conclusions are of limited value. To be sure, they may seem to guard against this possibility by showing that their model gives a good fit to the data, but econometric testing, too, is subject to much criticism (See Mayer, 1993, Ch. 9)¹

What is important here (and incidentally is also basic to Friedman's 1953 essay) is that we are using the theory and its abstractions to deal with a particular problem or question. And what is a valid abstraction when addressing one question can be an invalid one when addressing another. When trying to determine whether there is a stable relation between changes in bank reserves and the money supply we can safely abstract from the

fact that the actions of government officials are influenced by their self-interest. But we can not abstract from the fact that the ratio of the public's demand for currency relative to deposits varies. Conversely, when we ask, as Boettke does, whether market socialism could work effectively, we can abstract from the stability of the public's demand for currency, but not from the motives of government officials.

Boettke's criticism of formalism for its heroic abstractions is therefore too general. He would have to show that these abstractions result in theories that neither predict well nor foster understanding. To be sure, he does discuss why abstracting from the motives that would drive government officials under market socialism invalidates the formalists' advocacy of such a system. And although he does not provide any hard evidence, I find his argument plausible. But market socialism is hardly a central topic in the work of formalists, and it was originally presented without formal models by Oskar Lange (1939), and developed further by Abba Lerner (1944), whose use of mathematics generally consisted of simple geometry. Formalists might therefore readily concede that Boettke's criticism of market socialism is correct, and yet go about most of their work undisturbed.

The second part of Boettke's criticism of formalism, that the medium tends to become the message, is valid. Formalists tend to evaluate ideas by their suitability for modeling, and to judge models much too much by their technical sophistication and elegance, and much too little by the insights they provide into economic behavior and by their predictive success. Deidre

McCloskey (1985) is right in objecting that economics departments have appropriated the criteria that are proper for a mathematics department, rigor, generality and elegance. Thus a leading mathematical economist and mathematician, Gerard Debreu (1991, p. 5) wrote:

In the past two decades, economic theory has been carried away further by a seemingly irresistible current that can be explained only partly by the intellectual successes of its mathematization. Essential to an attempt at a fuller explanation are the values imprinted on an economist by his study of mathematics. When a theorist who has been so typed judges his scholarly work, those values do not play a silent role: they may play a decisive role. The very choice of the questions to which he tries to find answers is influenced by his mathematical background. Thus the danger is ever present that the part of economics will become secondary, if not marginal to that of judgment.

The issue is therefore not the fact that formalist economists use mathematics, but what they use it for, and the role mathematical techniques play in the criteria by which they evaluate work. Someone might use a mathematically sophisticated and complex model to solve a problem that is central to the "study of mankind in the ordinary business of life", Alfred Marshall's (1947, p. 1) conception of economics, without abstracting from those institutional details that are relevant for the applicability of the conclusion, and thereby produce good economics. Someone else who uses no mathematics might tackle a trivial problem, or use too restrictive abstractions, and thus produce bad economics.

To be sure, mathematics is in a way not well suited for economics, because mathematics is a tool for rigorously deriving correct conclusions from axioms (i.e. "assumptions") that are

beyond question, hardly a good description of the role of assumptions in economics. But the other tools available to economists also have their shortcomings. So Boettke's strictures against the use of mathematics are off target. The dispute about the feasibility of using mathematics to produce good economics is over.² Trying to re-fight it merely confirms formalists in their belief that their critics can be safely ignored.

There is a third aspect of formalism that Boettke does not touch on. This concerns the purpose for which models are constructed and relates to a definition of formalism that is narrower than Boettke's (see Mayer, 1993, Ch. 3). As an ideal type this is the insistence on deriving conclusions to every problem explicitly from first principles, which in economics means the assumptions of utility maximization (in practice often narrowed to mean income maximization) and rational behavior, along with a minimum of other assumptions. This type of formalism takes as its model not the natural sciences, but mathematics and logic with their reliance on demonstrative reasoning. By contrast, there is what can be called empirical-science economics, again an ideal type. It is more concerned with predicting or explaining empirically observable characteristics of our economy, and less with rigor, parsimony and elegance.

In practice formalist economists also want their models to tell us something that is applicable to the real world, for example, they do not work with models that assume that agents are completely altruistic. On the other side, empirical economists may sometimes start with the same propositions that formalists use. The difference is that formalists are much more likely to

treat these propositions as axioms, while empirical economists are more likely to treat them as working assumptions that are usually, but not necessarily always, correct.

Such a distinction is alien to Boettke's framework because he divides economists into Austrians, (old) institutionalists, Marxists, and formalists. Since he quickly dismisses institutionalists and Marxists, he only has to show the fallacy of formalism to award victory to the Austrians. But that is too facile. It tars the substantial majority of economists with the same brush, despite the major methodological differences among them. Those who take an empirical approach - and there are many - are thereby made to share responsibility for the extreme degree to which abstraction is carried by those who are formalists by my much narrower definition. But to accuse economists such as Milton Friedman, Charles Goodhart, Joseph Stiglitz, or Paul Krugman, to name only a few leading mainstream economists, of ignoring "the complex web of institutions", or of putting the medium above the message is surely wrong.

Boettke's discussion of the "Chicago School" illustrates this tendency to see all mainstream economists as alike since they differ from the Austrians. He does not distinguish between the first Chicago School (e.g., Knight, Simons and Mints) which paid much attention to the institutional setting, the second Chicago School, lead by Friedman and Stigler, which paid somewhat less attention to certain institutions, but did focus on empirical issues and on certain other institutions (see Hirsch and de Machi, 1990), and the third Chicago School (led by Robert Lucas) which is formalistic in my narrower sense of the term. Friedman's

(1953) famous methodological essay is often read as a justification for formalism, but that is a mistake. (See Friedman 1953, pp. 11-12, 24-25, 277-300; Hirsch and de Machi, 1990. See also Hammond, 1996, Ch. 2)

But even if one were to interpret Boettke's criticism of what he calls formalist economics only as criticism of formalism on my narrower definition, it still goes too far. Like most methodologists Boettke is a monotheist - there is only one true methodology. No trade-off at the margin is allowed. This might be appropriate if economics had only one purpose. Now in a sense it does: to provide an explanatory and predictive apparatus that ranks extremely high on the criteria of rigor, elegance and parsimony, as well as on the criteria of accuracy and applicability to many real-world situations. But a theory that fully satisfies all of these criteria would be a rare find indeed. So there is usually need for a trade-off, or for multiple theories operating at different levels of abstraction.

One can therefore justify some formalist economics on *l'art pour l'art* grounds because, like any other piece of rigorous and elegant reasoning, it provides intellectual satisfaction. If we support research in pure mathematics and in art history for the "glory of mankind," we should also support some research in formalist economics.³ Moreover, some formalist models can be justified, not as the end product of economic analysis, but as an intermediate product. Allan Gibbard and Hal Varian (1978) have argued that many economic models are "caricature" models, intended to highlight some particular feature of the economic process, even though this gives a distorted picture of the

economy. Others can then combine the lessons learned from such caricature models to build a more balanced model of the economy. The trouble is that putting these caricature models together into a realistic description of the economy seems less attractive or more difficult than building additional caricature models, so that too little of the former and too much of the latter gets done.

The question is therefore not whether some formalist economics should be done, but how much. The glib answer that some economists give, let the market decide, will not do. The academic economist's "market" consists of other academics, and not consumers who pay with their own money for what they demand, or institutions that are held closely held accountable by the general public or students, the ultimate customers of this research. And academics tend to treat as interesting problems those that are technically difficult, even if they do not have much bearing on how the economy functions or on policy choices. Thus, in academia the interests of producers tend to outweigh those of consumers. (Are there many other industries with as much market failure as academia?) I therefore agree with Boettke that there is much too much formalist research (on my narrow definition), but I would not like to see all of it eliminated.

II. Markets versus Planning

Another problem with Boettke's analysis is his emphasis on the problem of free markets versus planning, an issue on which the Austrians have focused much of their attention. This is obviously an important issue for economics. But it is only one of many issues. Most papers in economics journals either deal with

completely different issues, or if they do deal with an issue pertaining directly to free markets versus planning, they do so in a specific and narrow context, such as flexible versus fixed exchange rates. Boettke's general strictures about the efficiency of market processes have relatively little to contribute to such discussions, and hence to most of what economists do.

Mainstream economics devotes much of its effort to small questions rather than system-wide basic questions. For example, the first paper in the latest issue of the *American Economic Review* (September, 1997) deals with an empirical model of international specialization, and the next three papers have the following titles: "The International Transmission of Financial Shocks: The Case of Japan," "A Political-Economic Analysis of Free-Trade Agreements," and "An Empirical Assessment of the Proximity-Concentration Trade-off between Multinational Sales and Trade". Even the two papers in that issue that do compare economic systems ("Privatization in Eastern Germany: Management Selection and Economic Transition," and "Competition or Compensation: Supplier Incentives under the American and Japanese Subcontracting Systems,") do so in narrowly circumscribed ways. This narrow focus has proved fruitful. Normal-science research is how a mature field, which economics has become by now, makes its day-to-day advances. Boettke and Prychitko (1944) tell us that in recent years Austrians have moved away from their traditional emphasis on ideology. But even so, their research agenda is still too strongly influenced by the "big" issue of markets versus planning.

In addition, the issue of planning versus free markets is a

problem that belongs only partly within economics. As Boettke rightly points out it does little good to contrast an idealized picture of market socialism with capitalism as it works in practice. But determining how market socialism would work in practice raises some exceedingly difficult issues on which an economist's expertise is not sufficient. For example, would the state, presumably influenced by pressure groups, allow firms that should fail to do so and thus cause unemployment? In this respect the Chinese experience, at least so far, is not encouraging. Deeper sociological questions also arise; what elites would replace the capitalist elites, and what effect would that change have?

We know by now that social engineering is fraught with uncertainty and danger; the law of unintended consequences rules. There is a saying in the military: "no plan survives contact with the enemy." Something similar applies to economic policy.

What economists can do much better than evaluate how market socialism would work in practice is to compare the actual workings of a market system with the workings of an idealized market system. And what one can do well, that is what one does, particularly if one is an academic. This creates a natural bias against the free market, though one that may perhaps be fully or more than fully offset by biases in the other direction. Boettke is fully justified in complaining about this bias. Where Boettke is less justified is in rejecting the work of those who, like Stiglitz, have analyzed the implications of factors such as asymmetric information. Such analyses can be used as illustrations of how the actual functioning of a market system falls

short of its ideals. Stiglitz generally does not quantify the losses from these shortfalls, and perhaps they are minor. But even so, they exist. Students of market processes, such as the Austrians, should welcome rather than deplore any work that tells us more about how markets operate, even if it creates a bias in favor of planning on part of those who fail to reflect sufficiently on the shortfalls of actual from ideal socialism.

III. Ideology

Another problem is Boettke's treatment of rival schools as driven by ideology. In one sense ideology is the metaphysical core of a research program, and therefore unobjectionable.⁴ In another sense, used by those who complain about the ideology of their opponents, ideology is a tendency to accept or reject evidence on the basis of whether it fits one's preconceptions, and often a tendency to select theories on the basis of their policy implications, instead of the other way round. As such it is an instance of obtuseness, or lack of intellectual honesty at least with oneself, if not with others. To accuse someone or a school of being ideological is therefore to make a serious charge, albeit in relatively polite language. Unless one has evidence to back up that type of charge it is better not to make it. To be sure, if an intelligent and well-informed person rejects my arguments I am tempted to attribute this to willfulness rather than to the unconvincing nature of my argument, which, after all, I find utterly convincing. But this temptation should be resisted.

However, there is still another way of looking at ideology, to treat it as in undesirable in many cases, but also as natural

and as sometimes justified. It is natural to give more credence to evidence that supports one's position than to evidence that rejects it, if only because this eliminates an unpleasant feeling of cognitive dissonance. Moreover, in some cases it may be justified. Suppose I have much evidence that a proposition is correct. I am now presented with evidence that it is false. Hence, I usually must reject either all the old evidence that supports it, or else the new evidence.⁵ Suppose upon careful reflection I cannot find any errors in either the supportive evidence, nor in the new evidence. One possibility is to say that I do not know whether the proposition is true. But suppose I have to make some decision, or that the supportive evidence is very strong. It may then be reasonable for me to adhere to my former belief, and to treat the new, contradictory evidence as an anomaly that will sooner or later be somehow resolved, even though this makes me seem ideological.⁶

For all of these reasons it is better not to attribute ideology to one's opponents. Moreover, it is far from clear that one should blame, as Boettke does, an anti-market ideology for the rise of formalism. Institutionalism can also be used to justify interventionism, and, indeed, has been the traditional source of interventionist arguments in economics. Moreover, the hegemony of formalism can also be explained by self-interested behavior by economists, so that an ideological explanation is not needed (see Mayer, 1993, Ch. 2)

IV. New Keynesians and Post-Keynesians

Boettke's treatment of the new Keynesians does not do them justice. New classical economists had attacked Keynesian theory

for lacking micro-foundations for its basic assumption of wage or price inflexibility. In response new Keynesians have shown that such inflexibility is consistent with rational income maximization, thus refuting the new classical criticism. To be sure, they have not succeeded in measuring the absolute or even the relative importance of the various sources of wage and price inflexibility that they analyze (among which the efficiency wages that Boettke stresses is not necessarily the most important), and hence have not shown that we do live in a Keynesian or monetarist, and not a new classical world. But their primary task was not that, but merely to demonstrate that building models with wage or price inflexibility is consistent with a belief in rational utility maximization, and does not require any implausible ad hoc assumptions. Boettke does not give them sufficient credit for that.

The criticisms of mainstream economics that Boettke and other Austrian's make from a right-wing perspective have much in common (both substantively, and in style of argument) with the criticisms made by post-Keynesians, who represent the left-wing of Keynesianism. They, like Boettke are critical of formalism and its focus on equilibrium, and like the Austrians they want to re-introduce historical time into economics. They also stress the prevalence of uncertainty, and the inadequacy of treating it with the tools applicable to decisions-making when the probability distribution is known. Suppose one accepts Boettke's arguments about the inadequacy of mainstream economics in these respects. One does not then have to draw the same conclusions as he does; one could become a post-Keynesian instead. Again, Boettke's

tendency to see methodological disagreements in economics as a dichotomy between the Austrians on one side and everyone else on the other, muddies the waters.

V. Austrian Economics

Boettke's paper is at least as much a pro-Austrian tract as it a criticism of formalism. It is useful to distinguish two roles that Austrian economics could play. One is to supplement mainstream economics, and the other is to replace it.

Viewed as a supplement to mainstream economics Austrian economics has something to contribute. Mainstream economics has narrowed its vision to a limited set of ideas that modelers find tractable. For example, the modern revival of Walrasian economics pays little attention to the work of Schumpeter (who, ironically) was a great admirer of Walrasian economics), because his vision is too sweeping to be confined within the constraints of mathematical models. The Austrians therefore have something valuable to contribute when they insist that the entrepreneur is more than some nondescript graduate of an MBA program, who mechanically grinds out the profit-maximizing solution to a standardized problem. Similarly, their insistence that price competition is only one aspect - and not necessarily the most important aspect - of the competitive process adds a valuable insight that game-theoretic models of oligopoly are likely to miss. Insistence on entrepreneurs being innovators operating in a fog of uncertainty, as well as insistence on irreversible historical time, are other Austrian ideas that may deserve much more attention than the mainstream gives them.

One might liken the difference between mainstream and

Austrian economics to the difference between a flashlight and a lantern. The former illuminates sharply along a narrow beam. The latter provides all-round but less sharp illumination. Which one is preferable depends on one's purpose, and neither should be rejected because the other does some things better. Consider, for example, Boettke's criticism of the equilibrium concept used in mainstream economics. He is right in saying that its beam misses much of what we should see, such as the entrepreneur's creation of new goods and new markets, so that some of the criticism of product differentiation is simplistic. On the other hand, the insight that markets tend towards equilibrium provides us with a powerful tool for predicting how they will respond to certain shocks. If economists were to relinquish equilibrium analysis they would lose much. But if they fail to see that there is much more to market behavior than a tendency towards narrowly defined equilibrium, they also lose much.⁷ Both mainstream economists and Austrians have something to learn from each other.

Unfortunately, the lines of communication between them are frayed. I doubt that many mainstream economists read any Austrian economics, though this situation may have improved recently. Some economists who are not identified as Austrians now taking Austrian ideas seriously (see Caldwell, 1982; Hoover, 1988; and Montgomery, 1996). But communications still need much improvement. One factor hindering it is the somewhat arrogant belief of many mainstream economists that: the writings of heterodox economists, or for that matter just about all economics written prior to the 1970s, have nothing to teach them.

But another reason is the tendency of Austrians to address their discussions primarily to each other, to spend too much time questioning the purity of each other's doctrine, and debating minor deviations among the elect; in other words to behave like a stereotypical school.

It is still the case, though perhaps now to a lesser extent, that an Austrian discussion of a mainstream proposition often consists in large part of showing that it is inconsistent with Austrian economics, perhaps even that it has been explicitly rejected by one of the Founding Fathers. Given the disdain with which most mainstream economists treat Austrian economists such introversion is understandable - nor is it necessarily worse than the introversion of the formalists (on my definition), who tend to treat economics as the study, not of the economy, but of other economists' models. But such introversion by either side does not facilitate progress.

And even when Austrians go beyond condemning some work as being un-Austrian, their criticisms tend to deal in generalities, such as a failure to recognize the existence of historical time, or the use of illegitimate aggregate concepts, instead of getting down into the trenches. It is easy for mainstream economists to ignore such broad criticism and say: "yes, alright, but our theories work despite all this." It would be more difficult to ignore the Austrians' criticisms if these would more often consist of evidence (or more specifically of what mainstream economists consider evidence) that a particular mainstream theory or model fails to predict or explain because it ignores a specific point made by Austrians.

More generally, few mainstream economists are likely to become converts to Austrian economics in its entirety, but more might take specific Austrian ideas seriously if these were shown to be relevant to their specific concerns, and were reformulated in way compatible with mainstream methodological criteria. Granted that Austrian economics is built on philosophical foundations that differ sharply from those of mainstream economics, it would "sell" better if it were sold separately. Similarly, a more politically neutral version might find additional markets. Since Austrians are in the distinct minority it is up to them to build the needed bridges.

Austrians may, of course, object that they want to do more than bring about a few changes in mainstream economics. But aiming at an attainable goal is better than aiming at one that is out of reach. Moreover, a series of small changes may add up to a large change. One should not exaggerate the incompatibility of paradigms. Austrian economics needs fewer generals making grand plans, and more privates fighting the war a hill at a time.

Considered as a substitute for - rather than an addition to - mainstream economics, Austrian economics is not likely to be successful. And for good reason. Mainstream economics does have many shortcomings, particularly what I have called formalist economics. But it has also has many successes, especially when accompanied by an empirical mindset, as it is in the work of economists such as Atkinson, Akerlof, Friedman, Modigliani, Solow, and Tobin, to mention just a few modern masters. It has produced testable - and confirmed - hypotheses on important aspects of economic behavior (see Mayer, 1995, Ch. 11). We would

be the poorer if this work were to cease, or to become only a minor tributary to the stream of economics.

This is not to deny that over the years Austrian economics has also made major contributions. Its contribution to the debate about the feasibility of rational resource allocation under socialism is an outstanding achievement, and so is Hayek's work on the role of decentralized information, as well as his earlier work on the conditions for neutral money that won him the Nobel prize.

Moreover, since there are many fewer Austrian than mainstream economists, when evaluating the fruitfulness of Austrian economics one should look at per capita contributions. But even so, its already discuss concentration on "big" problems has meant that Austrian economics has paid too little attention to the day-to-day problems that constitute the work of a normal science. In Lakatosian terms, too much of its research concentrates on the metaphysical core, and too little on the protective belt.

Boettke's (1994) *The Elgar Companion to Austrian Economics* has a section of short papers on applied economics, presumably intended to illustrate the fruitfulness of Austrian economics when applied to specific problems. I do not think that he succeeds. Many of these essays do make valid points, but too often these are points originally made by economists who are more closely identified with mainstream than with Austrian economics. Showing that they are also consistent with Austrian economics certainly does not discredit Austrian economics, but it fails to make a case that Austrian economics should replace mainstream economics. When these papers do cite specific Austrian contribu-

tions to the problem at hand, these are usually contributions made a long time ago, and that, too, does not suggest that Austrian economics is a progressive research program.

Despite its intellectual ferment in the 1980s (see Lavoie, 1994a), Austrian economics has not been very productive with respect to those problems that interest mainstream economists, and also, I would argue, that interest the public in general. Part of this is due, of course, to Austrian theory not providing the rationalizations for government intervention that much of the public and many economists welcome. But that is not the whole story. While in the hands of an intellectual giant like Hayek Austrian economics is a powerful tool, in the hands of lesser economists it is not as powerful a tool as is mainstream theory. And that is a serious drawback. Inevitably, most researchers are not giants, and a productive methodology is one that provides ways in which they, too, can make useful contributions. We should evaluate methodologies not only in the context of verification, but also in the context of discovery.

The lesser fruitfulness of Austrian economics in the hands of most economists is due not only to its focus on "big" problems, but also to the much smaller role that Austrians give to that great source of work for many economists, empirical work. Many Austrians consider econometrics to be useless. Leading Austrians reject on methodological grounds the validity of aggregates, such as the price level. Thus Hayek (1935, p. 5) wrote: "from the very nature of economic theory, averages can never form a link in its reasoning." Austrians therefore reject as inapplicable to the study of society the instrumentalism that is used so much in the

natural sciences. This position strikes me as mistaken, but not being a philosopher I will not attempt to discuss it.

Instead of econometrics Austrians could use economic history as a testing ground, particularly if they are willing to use averages. Although some have done so, for example, to argue that an unregulated banking system has worked well in past (for a summary see Schuler, 1994), on the whole, Austrians do little historical work. Perhaps this is due to their traditional rejection of historicism. But economic history can be used both to support and make use of economic theory, and not as a substitute for it. So it is not surprising that some Austrians, Boettke among them, are now urging Austrians to make more use of economic history, and to test both the correctness and explanatory power of their theories. (see Boettke, Horwitz and Pritchko, 1994) Indeed, Boettke (1994, p. 5) has argued that to say that Austrians reject empirical research is: "a misreading of the Austrian tradition", though he adds that Austrians also believe that: "the significant debates in the social sciences are theoretical debates."

All the same, perhaps because they want to engage in the more significant debates, Austrians do less empirical work than mainstream economists do. Another reason might be that many Austrians adhere to a mentalist subjectivism that suggests that empirical work is not likely to be successful (see Lavoie (1994b). By rejecting econometrics and making only very limited use of economic history Austrians have denied themselves the opportunity to test their theories, though this has changed a bit in recent years (see Lavoie, 1994a).

Austrians might respond, along with some mainstream economists (see for instance Robbins, 1932), that empirical testing is not needed, that a valid logical chain from axioms we know from introspection and casual observation to be true suffices. But while that may allow us to argue that a certain effect does occur, it usually cannot tell us whether it is significant or not. For example, Hayek (1935) attributed business cycles to the changes in the relative prices of various goods induced by changes in the money supply. Skeptics might respond that, while such relative price changes may contribute to business cycles, it is possible that they explain only a trivial proportion of the fluctuations we experience as business cycles. Similarly, for a long time Austrians have talked about the importance of complementarity among various types of capital. But how much complementary is there? Only recently has an economist (and one who has only tenuous links with the Austrians) provided empirical evidence that such complementarity does matter empirically (see Montgomery, 1996).

VI. Conclusion

In summary then, Boettke's criticism of what he calls "formalism" goes too far. He defines formalism too broadly because he sees methodological disputes through the lense of Austrians versus everybody else, thus ignoring some basic distinctions among the "everybody else". In particular he overlooks the important empiricist tradition in economics.

All the same, though I have not discussed these points, there is much in his criticisms of formalism (if it is narrowly defined) that is correct and well argued. Moreover, his essay

does serve a useful function in drawing attention to Austrian economics, which has some important ideas to contribute to a synthesis. But substituting Austrian economics for what Boettke calls formalism, or awarding it the main role in such a synthesis, would be a mistake.

ENDNOTES

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¹ For other problems with modeling see Mayer (1996).

² Those who criticized the use of mathematics in economics made a bad case by arguing that it is of little use. There is by now massive evidence to the contrary. They should have made their case, not against the use of mathematics, but against the over-emphasis on mathematics, and made their case on two grounds. One is McCloskey's (1985) charge that the values appropriate for a mathematics department threaten to overwhelm economics, and the other is opportunity cost. Obviously, other things being equal, an economist who knows more mathematics is better equipped to do research than one who knows less. But other things are not equal. The more time students spend learning mathematics, the less time they have available to learn economics, though, admittedly the shift from language requirements to a math requirement was clearly beneficial. Similarly, given the amount of time economists spend on research, and given the available space in journals and books, the number of hours an economist spends on a paper is fixed. The more of this time is spent polishing the mathematics, the less time is available for checking the correctness of the analysis in other ways.

³ In Bergamo (Italy) I saw just below the dome of a church a fresco that can be seen - and seen only faintly - from the top of a certain towers. Presumably it was painted "for the glory of God", not for people's enjoyment or instruction. One can justify some papers in *Econometrica* that almost nobody can read in the same way. Given how few readers even most not very technical papers in economic journals have, and how unlikely it is that they will have any influence on policy or economists' thinking, one might argue that most of them are best justified as "for the glory of God or mankind."

⁴ One might, however, object that in the social sciences knowledge is so precarious that one should limit as much as one reasonably can those propositions that are privileges as

uncontroversial. Many ideological propositions would then not qualify for the core.

- 5 But that is not always so. In some situations it is reasonable to hold two conflicting views (see Foley, 1979).
- 6 Here is a concrete example. Phillip Cagan (1965) found that being covered by a corporate pension scheme induces households to save more on their own. This implies an increasing marginal utility of wealth, and therefore conflicts with the well established belief that marginal utility decreases as one obtains more. I therefore did not accept Cagan's findings, though I could find no fault with his analysis. Subsequently, when Cagan's data were re-analyzed, it turned out that they were wrong.
- 7 Boettke (1997, p. 30) recognizes this when he writes that: "Equilibrium theorizing is not to be rejected, according to Hayek, but its real purpose must be constantly kept in mind. Formal modeling can be a very good servant, but a poor master." However, the general impression that Boettke gives is of a strong condemnation of equilibrium theory.

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