CHAPTER 9

The case for pluralism

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Introduction, acknowledgments, and dedication

This is a revised version of a paper delivered at the conference, The Popperian Legacy in Economics, held in Amsterdam in December 1985 to honor the retirement of J.J. Klant.

I apologize in advance for writing what is bound to be perceived as a very egocentric chapter. In it I trace the development of some of my ideas on methodology, and I attempt to construct a case for pluralism. In constructing my case, I try to answer some of the objections I have heard most often concerning pluralism. Most of these objections were raised by a number of people who commented on earlier drafts of this chapter. I am indebted to Neil de Marchi, Wade Hands, Uskali Maki, Bob Coats, Roy Weintraub, Dan Hausman, Mark Blaug, Larry Boland, and members of Neil de Marchi's workshop in the history of economic thought at Duke University for their thoughtful comments and criticisms. Finally, I would like to dedicate this chapter to J.J. Klant. I suspect that he will feel some ambivalence about this, since many of the ideas expressed here will not meet with his full approval. However, it should be clear that I share his desire to understand better the "rules of the game" in economics.

My road to pluralism

As a good Popperian might, I begin with a problem. The problem may be posed as a question: How should we do economic methodology? When it is stated in this form, it should be clear that this question is a normative one and a meta-methodological one. Popper would not like the question because it appears to be an essentialist one. I therefore emphasize that I am asking it pragmatically.

In order to answer the question, we must first figure out what the purpose of methodological work is. Many answers suggest themselves.

I first became interested in economic methodology in graduate school because I thought that studying it would help me *to understand what economists were up to.* This, of course, was a fundamental mistake. The

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writings of methodologists, particularly in the positivist era, usually did not have much to do with the actual practice of economics. It is perhaps appropriate to note at this point that figuring out what economists are up to is not particularly easy. One can learn how to *do economics,* of course, by simply training to become an economist. But as Thomas Kuhn points out, this process is anything but a self-conscious one. It occurs through a sort of mental osmosis as one learns the paradigmatic solutions to various well-established normal science puzzles. By becoming an economist, one learns how to do economics, but this does not answer the question of what economists are up to.

Another naive notion that was soon dispelled was that methodology would tell me how to proceed; it would show me *how to do economics scientifically.* I now feel a little better about my naïvété, having read Neil de Marchi's contribution to this volume, where he describes the flirtation with Popperian thought that took place among economists at the LSE in the early 1960s. Apparently I was not the only one to make the mistake of thinking that philosophers held the key to how to do science, and that makes me feel better. Naïvété, like misery, loves company.

Later, as I read more deeply in the philosophy of science, I entertained two other possible answers to the question of the purpose of methodological work. One of these was that methodology tells us what science is; it allows us to distinguish science from nonscience; *it solves the demarcation problem.* The other answer was that methodology tells us how to choose among competing theories; it instructs us how to go about selecting the best theory; *it solves the theory choice problem.*

These answers were promising on one level, because they helped me to make sense of some of the methodological writings by economists that I encountered. Friedman's famous essay, "The Methodology of Positive Economics," for example, may be read as a prescriptive pronouncement on how to solve the choice problem: Choose the theory that predicts best and use simplicity as a tie-breaker. But on the philosophical level, these answers were problematical. Though there was no dearth of philosophers who claimed to have solved the demarcation problem (e.g., Popper, 1972; Lakatos, 1974), I found their critics to be more persuasive, particularly when I thought about applying their solutions of the demarcation problem to economics. Regarding the theory choice problem, I felt that Kuhn (1970) was closest to the truth when he argued that the search for a universally applicable, objective algorithm of choice is chimerical.[1] So, I did not find philosophy of science

[1] I review some of the arguments that led me to these conclusions in *Beyond Positivism,* chapters 5, 11, and 12. I should emphasize that I am now, and have always been, far less interested in these purely philosophical questions than I am in the pragmatic task of

to be very helpful in answering the question: What is the purpose of methodological work?

That the question has not yet been adequately answered has had an effect on the status of methodological work within the economics profession. Its position as a subfield is certainly an ambiguous one. Some economists openly disparage methodological work, because they consider it a waste of time. Yet many of the profession's most well-known members (e.g., among Nobel laureates, the list includes Hicks, Samuelson, Friedman, Myrdal, Hayek, Simon, Stigler, and Buchanan) have tried their hand at methodological writing. Certain groups within economics but outside of the mainstream (e.g., Austrians, Marxists, institutionalists, post-Keynesians) display an active interest in methodology. Their view of methodology is that it provides a framework for launching fundamental critiques of mainstream practice. However, most mainstream economists do not take these fundamental criticisms seriously. Indeed, the fact that heterodox economists engage in methodological work is taken as further evidence that methodological study has little importance. Paradoxically, even as they disparage methodological work, orthodox economists employ methodological arguments (albeit rather unconsciously) all the time. Arjo Klamer's recent book on the rational expectations revolution in macroeconomics, *Conversations with Economists,* makes this point very well. Note too that the usual dismissive argument made by mainstream economists against their heterodox critics (that the theories propounded by rival schools are "unscientific") is a methodological one.

It seemed to me that if methodological work was ever to be taken seriously within the profession, its purpose required clarification. But before such a task could be started, two other problems had to be addressed. My readings in philosophy convinced me that much methodological debate in economics was needlessly confusing. The first task was to acquaint economists with twentieth-century developments within the philosophy of science, with an emphasis on the current problems facing that field. The second was to use the philosophy of science to clarify various methodological debates among (mostly) mainstream economists. In these debates, economists usually viewed methodology as offering a set of prescriptions concerning legitimate scientific practice, and most had borrowed rather haphazardly from various philosophical positions (almost all within the tradition I labeled loosely as "positivism") in making their arguments. A knowledge of the philosophy of

trying to figure out how to apply them within economic methodology. I am not a philosopher; my demand for philosophical discourse is a derived one. This was my point when I subtitled my concluding section of chapter 5 on contemporary philosophy of science "A Dilettante's Review and Commentary."

science was helpful in clarifying language, in eliminating semantic debates, and in dismissing arguments from authority when economists had misread the philosophical authorities. These were my goals in writing *Beyond Positivism.* Of course, my insights were not unique. Both Mark Blaug and J.J. Klant approached the subject in much the same way in their books on methodology. That three independently executed attempts at surveys of the field should be so similar in approach is evidence that this work was needed and had to be accomplished first.

Of course, there are also differences in how we approached our subject. Without wishing to caricature their positions, I think it is safe to say that both Blaug and Klant adhere to some variant of sophisticated methodological falsificationism. This lends a coherence to their books that is absent in mine. Whether one is examining alternative methodological views (e.g., instrumentalism, a priorism) or particular research programs in economics (as Blaug did in the third section of his book), if one is a falsificationist one has a consistent set of criteria against which other positions may be judged. Of course, one need not be a falsificationist to be consistent. A good a priorist, or, for that matter, a good Marxist, or Freudian, or Christian, also has consistency on his or her side.

For me, *Beyond Positivism* is a very preliminary work. Its title is meant to suggest these questions: What lies beyond positivism? How are we to do methodological work in the postpositivist era? Obviously, this brings us back to the question of the *purpose* of methodological work. I tried to answer that question in the last section of my book, and the approach I took was labeled "methodological pluralism." In the remainder of this chapter, I develop that position more fully and try to show its strengths and weaknesses.

Pluralism defined

On the most general level, the pluralist believes that the primary purpose of methodological work in economics is to enhance our *understanding* of what economic science is all about and, with luck, by so doing, to *improve* it. I employ the term "understanding" in its everyday, commonsense usage, and I will speak more about the term "improve" later. To accomplish these goals, the pluralist undertakes *critical evaluations of the strengths and limitations of various research programs in economics and economic methodology.* In addition, our *understanding* of economics may be enhanced by various descriptive studies, for example, historical studies of the development of ideas, analyses of the sociological milieu in which a research program or discipline develops, and studies of the rhetoric of economics. Whether one wishes to consider such studies as methodological or not seems to me to be a matter of personal taste. Finally, both *novelty* and *criticism* are important to the pluralist. An aphorism that nicely captures the pluralist position is, "Seek novelty, and continually try to reduce it through criticism."

At this very high level of generality, there is little in my description of pluralism that should cause strenuous objection, but also little that distinguishes it from other views. The specific ways I would put my program to work are perhaps more controversial. In what follows, I will explore some of the details of pluralism and attempt to answer some of the more important questions raised by my critics.

Pluralism does not attempt to answer the demarcation question

One attractive feature of Popper's falsificationism, Lakatos's Methodology of Scientific Research Programmes, and certain other approaches in the philosophy of science is that they give clear answers to the demarcation question. One is able to distinguish scientific theories from nonscientific ones because only the former are capable of being measured against a well-defined set of criteria of theory appraisal. Sometimes, though not always, those that pass this test can be further evaluated, so that at a given point in time one theory may be chosen provisionally as the best one.

As we will see, the critical appraisal of theories plays an essential role in methodological pluralism. However, criticism is undertaken for the purpose of *understanding,* and with the hope that with understanding will come improvement. But criticism is *not* undertaken for the purposes of either discovering or applying some universal criterion of demarcation.[2] Of course, the majority of the profession may find the criticisms against a given research program to be compelling, with the result that few economists will work within it. That is a separate matter. The role of the methodologist is to discover the strengths and weaknesses of research programs. This is a categorically different endeavor from searching for a universally applicable answer to the demarcation question.

My position raises another question: How are we even to know what

[2] I hope that it is clear that it is the quest for a *universal* criterion of demarcation, or worse, the application of some such alleged criterion, that I find most objectionable. I believe that there are differences between science and nonscience. But at this point in time, we are very far from understanding what these differences are. The differences are subtle, and they may not even be capable of articulation: We may know more than we are able to say. As such, our past preoccupation with the search for a criterion of demarcation seems misguided. I must add that the view that we cannot discuss criticism without first solving the demarcation problem seems to me to be a particularly short-sighted form of obstructionism.

counts as "economics" if we do not try to solve some sort of demarcation problem? Methodological pluralism looks to the practitioners of economics to see what economics is.[3] Now, of course, the practitioners disagree among themselves about this. Even a casual observer of the discipline will discern a huge and amorphous mainstream that is surrounded on all sides by the heterodoxy, a group so diverse that it sometimes seems that the only bond among them is a dissatisfaction with mainstream analysis. Which groups should be included? The pluralist answers: *all of them.* Even further, the pluralist encourages and applauds both novel approaches to theorizing in economics *and* defenses of orthodoxy against the attacks that such novelty brings. There is no perversity in this. The point is that it is only through the constant clash of a diversity of ideas that positions become sharply defined, intelligible, understandable. In short, I am contending that much is to be gained by shifting our focus of attention away from the philosophical question of demarcation and toward the more practical concern for forms of criticism.

Does pluralism lead to anarchy?

This complaint is frequently voiced. The argument usually runs as follows: Failure to solve the demarcation problem implies an absence of standards, and an absence of standards leads to anarchy. Thus Mark Blaug, in a review of my book, equates pluralism with the doctrine "let a hundred flowers bloom," and comments, "To me this seems to be the abandonment of all standards, indeed, the abandonment of methodology itself as a discipline of study" (1983, p. 3). In *The Politics and Philosophy of Economics,* T.W. Hutchison speaks darkly about the outcome of the abandonment of standards:

> it would be disastrous to collapse into the kind of obscurantism which refuses to recognize, or try to uphold, *any* common epistemological criteria or standards which should be shared by natural and social scientists alike (as Popper has always insisted). That way lies the permissive chaos in which the principle that "anything goes" will ripen into the dogmas of mob rule, and so usher in the dictatorship of some genocidal popular or "proletarian" boss, such as "the great scientist," Stalin. (1981, p. 218)

Because the charge that pluralism is anarchic is so frequently heard and so important an objection, I will answer it at length. Three counterarguments are offered. I will argue, first, that the fear of anarchism

[3] The absence of a criterion of demarcation makes the circularity of this definition unavoidable. However, the definition as stated does serve the purpose of emphasizing the important role of the scientific community in defining the domain of discourse.

misunderstands science, which is basically a traditional and conservative enterprise. Next, it misunderstands the role of the methodologist in science. And finally, it misunderstands the pluralist position, in which criticism plays a crucial role.

1. The fear of anarchy, or of a totalitarian response to anarchy, is not based on a correct perception of science as it is currently practiced in free societies. There already exist a number of powerful constraints on scientific practice today. Because these constraints are so powerful, the pluralist is less worried about *anarchism* than about *dogmatic demarcation.* These constraints are multifarious; a few will be briefly mentioned.

First is the important role played by *tradition* in science. Traditional ways of viewing the world, of approaching problems, and of solving them exist at any given point in time in the development of a particular science. As Kuhn has pointed out, the role of tradition is strongest during periods of normal science, and is weakest during revolutionary periods. But as some of Kuhn's critics (e.g., Toulmin, 1970) have pointed out, no revolution is ever absolute and complete; even after a revolution, many things remain the same. Second – and again the insight is Kuhn's – science takes place within a *scientific community;* it is a shared, communal endeavor. As in other communities, there exist *norms* of behavior within scientific communities. Such norms, though they are seldom explicitly articulated, are nonetheless potent constraints on behavior. Third, science involves the free exchange of ideas; it is an *interpersonal* enterprise. It involves *communication* among individuals, attempts at mutual *persuasion,* the use of *criticism* and of *rhetoric.* It is because pluralists recognize these sorts of constraints that they welcome studies in the history of science, sociology of science, and rhetoric, all of which illuminate the roles played by such constraints, and which lead to a deeper understanding of science.

I will grant to my critics that what I have just said presupposes that science takes place in a free society. One can surely imagine certain types of political, theological, or nationalist revolutions that would change all of that, and force science to come up with "ideologically correct" answers to its questions. I share with my critics an abhorrence of such an outcome, and it is my belief that it is the duty of every citizen to oppose such changes. But crucially, this is the duty of the *citizen,* or even the *citizen-scientist,* not of the *methodologist.* This brings me to my second line of defense.

2. Those who view the purpose of methodology as the enunciation of a universal criterion for distinguishing science from nonscience often argue that such steps are necessary to keep science free. Because advocates of this approach to methodology generally choose one criterion of

demarcation to distinguish science from nonscience, I will call this sort of approach *monism.* In my view, monist approaches to methodology misconceive the role of the methodologist and have caused much mischief in the field. I will briefly mention some of the dangers associated with monism; it should be clear that each point could be treated at greater length.

First, monist approaches have motivated the quest for a single set of immutable standards of legitimate scientific practice, a quest that most observers now recognize as chimerical.

Next, monism forces its advocates into the untenable position of clinging to *some* set of standards anyway, even when those standards cannot be applied. One is reminded of the reprise of the popular song, "Even a bad love is better than no love at all." If one's job is to protect science, even bad standards are better than no standards at all.

Third, monism fails to appreciate the richness of science, the diversity of theories that exist, and the diversity of ways of criticizing them that scientists actually employ.

Fourth, monist approaches put the methodologist in the position of telling scientists how to proceed. This breeds resentment among practitioners of science who, used to arguing in the languages of theory and econometrics, discover from their more philosophically oriented colleagues that they are not doing proper science. Thus, I place much of the blame for the second-class status of methodology within the economics profession squarely at the door of those who advocate prescriptive monist approaches to methodology. Such approaches are also responsible for the embarrassing gap between economic science as actually practiced and the writings of methodologists.

Finally, it is certainly questionable whether monist approaches to methodology provide any real defense against totalitarianism. The logical positivists were not fuzzy-minded anarchists, but they had little success against Hitler.

The role of the methodologist is not to be a guardian of science. Nor does studying methodology *teach one how to do economics;* one learns that by becoming an economist, and different economists learn different things. Studying methodology may help one to understand *what it means to be an economist.* An apt analogy is that the study of such fields as theology, or the philosophy or sociology of religion, helps one to understand religious phenomena. Clearly, such study is neither necessary nor sufficient to guarantee that one is a religious person.

3. The charge that pluralism leads to anarchy fails to recognize that criticism plays a crucial role in pluralism, because it is through criticism

that the strengths and weaknesses of various research programs are revealed.

There are many roads to criticism, and as such, I use the term "criticism" in its broadest terms. Such an approach may be contrasted with the approaches of those who believe that theories should be appraised according to some universally applicable standard of theory choice: for example, only falsifiable theories are acceptable; only fully axiomatized theories are acceptable; only theories that are fully verified are acceptable. Such approaches lead to the kinds of sterile debates in methodology that have caused many to turn their backs on the field; one group favors axiomatized theories, another prefers falsifiable ones, and the interminable debates between them lead nowhere. But even more important, such universalist approaches to criticism blind the methodologist to the richness of scientific practice.

For example, when falsificationists look at economics as currently practiced, they are forced to conclude that little in economics passes the test. This is fine if one is criticizing a particular research program that one dislikes, be it Marxism or Austrian economics or general equilibrium theory. And for programs for which one has more sympathy, one can always urge them to try harder. But neither response gets one very far in terms of understanding economic science. Matters are only a little bit better for someone like Milton Friedman, for whom predictive adequacy and simplicity are the ultimate criteria to be considered in the appraisal of theories. At least there exist some theories that pass this test in economics. Perhaps not surprisingly, these theories (perfect competition rather than monopolist competition, single-equation monetarist models rather than multiequation Keynesian models) are also the ones that Friedman prefers in terms of their policy implications.

Finally, such approaches often fail to recognize the important point that theories are attempts to solve problems. Theories are often best assessed according to how well they solve the problems they attempt to address. Given this perspective, the choice of one's tools of assessment often depends on what kinds of problems a theory purports to answer; in Larry Boland's (1982, ch. 12) apt summary, methodology is problem dependent.

What criteria of appraisal are open to pluralists? There are, of course, the traditional ones: empirical criteria of various sorts; structural criteria like logical consistency and the ability to be axiomatized; aesthetic ones like elegance, simplicity, or Machlup's "ah-ha-ness"; dynamic considerations like generalizability, fruitfulness, and the ability to encompass

previous theories; heuristic criteria like having analogies in other fields, usefulness for pedagogic purposes, realism.

Next, there is internal criticism, in which the purposes, goals, and methodology of a particular approach are taken as given, and then the research program is evaluated on its own terms. This type of criticism is especially useful when one is evaluating the claims of nontraditional groups in economics. For example, this was the direction I took in my assessment of praxeology (Caldwell, 1984).

Next, one might assess the evolution of a theory (or set of theories) through time, using Lakatosian or some other criteria for purposes of evaluation. Roy Weintraub does this, both in an earlier paper (1984) and in his contribution to this conference.

One might wish to differentiate the various types of theories that are commonly encountered in economics (e.g., axiomatic, normative, predictive, explanatory, and descriptive theories), and to evaluate a given research program to see which characteristics it possesses and which it lacks. This approach is especially useful if one wishes to emphasize the problem dependence of methodology.

One might explore other areas of inquiry – jurisprudence, literary criticism, new developments in Bayesian econometrics – to see what modes of criticism are used elsewhere, and with what effect.

There are many roads to criticism. Pluralists employ as many as they are able to find. Their purpose is not to demarcate, nor to find the "best" theory by comparing rival theories against a set of immutable standards, but to find the strengths and weaknesses of whatever program they are investigating. If they do their job well, we will all have a better understanding of what economic science is, and with luck that will lead to its improvement.

If everyone was a pluralist, what positions would be left to criticize?

This important question deserves special attention. Note first that pluralism is a *meta-methodological* position. It offers no specific methodological advice to economists. Indeed, what economists do is taken as given by the pluralist.

What economists do, whether they are part of the mainstream or part of the heterodoxy, is to work on a specific research topic. Such research takes place within a specific research tradition (e.g., neoclassical, Marxist, Austrian) and, as such, presupposes a methodology. Further refinements within traditions are also frequently encountered: A priorism has been challenged by an interpretive, hermeneutical turn in the Austrian camp; neo-Walrasians follow a methodology that is quite different from that employed by empirically oriented applied microeconomists.

The traditional methodological article is written by someone within one of these research traditions. Its purpose is to defend the methodological approach used within the tradition or to attack rivals. Given the diversity of research approaches in economics, such work will continue. Furthermore, such methodological work is beneficial, for it helps clarify the specific procedures, the hard cores and heuristics, used by researchers in the various traditions.

But this traditional approach to methodology is *not* very useful for fostering communication across paradigms; it does not lead to enhanced understanding. It is here that pluralist methodologists have a contribution to make. Pluralist methodologists do not embrace a particular tradition; their goal is the evaluation of all traditions. In a sense, pluralist methodologists attempt to practice *value-free evaluations:* Their assessments are critical, but they do not presuppose some ultimate universal grounds for criticism.

If everyone was a pluralist, there would be no positions left to criticize. But a consistent pluralist would not advocate that everyone become a pluralist! This underlies the pragmatic (as opposed to universalist) nature of my plea for pluralism. In the current environment, pluralism makes good sense. I suspect that this situation will continue. But more important, I *hope* it will continue. The emergence of a single universalist methodology is anathema to the pluralist, but so is the emergence of a single universalist nonmethodology. This is a point that Feyerabend rather remarkably failed to see.

Will pluralism lead to the discovery of true theories?

Methodological pluralism makes no epistemological claims; it is not grounded in any theory of truth. For this reason, philosophers like Dan Hausman, and philosophically astute methodologists like Wade Hands, will have difficulty in considering it a serious position. Without a theory of truth, we cannot speak meaningfully about understanding (for it may be an "understanding" of false beliefs), and we cannot talk about "improvement" or "progress" without defining what we are progressing toward.

This is probably the most serious criticism of my position. Note that it is not a problem for Popper. (Though if Dan Hausman's thesis in his paper in this volume is correct, it is a problem for Popper.) As Lakatos (1974) has pointed out, Popper discovered truth via Tarski in the early 1960s, and this saved him from skepticism. That falsificationism eliminates error is Popper's major claim in this regard. Though his fallibilism

forces him to claim that we can never know if we have reached the truth, we can know that we have eliminated error. His theory of truth makes not one whit of difference *operationally,* of course, but it does solve the problem philosophically.

I took my cues from Feyerabend, another pluralist who in his early work (e.g., 1962, 1970) tried to show how pluralism could lead to an expansion of true knowledge. He argued that because all facts are theory laden, the multiplication of theories multiplies empirical content, or knowledge. He was crucified for the initial premise of his argument, the strong theory dependence thesis, and as a result he turned to anarchism, and ultimately to Dadaism (Feyerabend, 1975). As a philosopher he had little choice.

Luckily, I am not a philosopher; I have choices. But even so, I recognize this as a very serious criticism of my position. I can think of a number of responses to this criticism, but I doubt that any will convince my critics.

I could try to use a pluralist argument to defend pluralism by arguing that a commitment to a particular theory of truth narrows one's approach to questions of methodology. But such an argument is clearly circular.

I could offer pragmatic arguments. For example, I could argue that my concern is to make methodological discourse more useful, and as such, I am not concerned with the philosophical question of truth. Or I could simply claim that my approach should be measured by its effects, and that I think the effects would be good. This seems sensible enough to me, but I fear my critics would see little sense in it.

I could point out that others have supplied very helpful analyses while still falling into the same trap as me. Kuhn, for example, is forced to use the weasel word "evolution" rather than "progress" in the final pages of *The Structure of Scientific Revolutions* for the same reasons: One cannot talk of progress without some conception of what we are progressing toward. Larry Laudan attempts to define progress without having recourse to the notion of truth in *Progress and Its Problems*. Though both Kuhn and Laudan have helped us to understand science better, both have been criticized on this point.

I could take the offensive and cite the antifoundationalist philosophers who play so prominent a role in McCloskey's "rhetoric of economics" approach. Thus I too could capitalize Truth in order to ridicule it. But I must say, this seems to me to be the least satisfactory approach. Look what it's done for McCloskey. Five years after publishing his important article in the *Journal of Economic Literature,* in which the rhetoric approach was trumpeted, he's still trying to figure out the arguments of philosophers in order to answer his critics. He has very little time left over to actually do any work using the rhetoric approach.

Perhaps the best argument I've heard was suggested by philosophers. Dan Hausman told me to argue that, given the present disarray in philosophy, I am forced to recommend pluralism as an interim position. There is a modesty to this argument that is appealing. But on the other hand, it may be a bit too modest. Uskali Mäki noted that pluralism need not be incompatible with a theory of truth and held out the possibility that some day, someone may be able to link the two together. I certainly agree with him that it would be foolish to rule out such a possibility. But that is a task for a philosopher, and one for which I have no comparative advantage. Perhaps the best position for me to take now is to invite anyone who would like to try to show how pluralism can be linked with a theory of truth.

Conclusion

The pluralist believes that the primary goal of methodological work is to reveal the strengths and weaknesses of various research programs in economics. Many tools of criticism are employed, and the focus is on the practice of economists.

In this chapter I have tried to offer a pluralist appraisal of pluralism. I have outlined the strengths and weaknesses of pluralism, I have tried to address the many arguments that have been brought against it, and I have focused on how to practice the trade of economic methodology. I hope that this will lead to a better understanding of the pluralist position.

The goals of pluralism *are* modest. Methodologists are not set up as experts offering advice to economists on how to do their science. Methodologists do not try to solve the demarcation problem, or the theory choice problem, or the problem of truth. Rather, methodologists try, together with their colleagues in the history, sociology, and rhetoric of science, to enable us to reach a better understanding of the science of economics. This is a modest goal. But it also is an achievable one. And it finally provides an answer to the question that has gone too long unanswered among methodologists: What is the purpose of our work?

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