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Some Practical Aspects of Pluralism in Economics

by

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Truth is so important, however, that it behooves us not to jump to conclusions about it (Samuels, 1997).

Thomas Mayer

Warren Samuels' contribution to our thinking about pluralism has been recognized by his selection as the author of the article on methodological pluralism in the Handbook of Economic Methodology (Samuels, 1998). This essay supplements his treatment by presenting a version of epistemic pluralism that is not grounded in post-modernism, and is not subject to the objection that in its strong version it amounts to an "anything goes" relativism, while in its weak version it amounts to no more than the platitudinous mandate: "be open to ideas that differ from yours."

I try to counter the above objection to the weak version of pluralism by presenting a version of epistemic pluralism that focuses on our limited knowledge and our uncertainty about many important aspects of the economy, while accepting the tradition in economics of offering answers to most practical questions that come up, even if such answers have to be based on evidence that is far from compelling. As recent discussions of global warming illustrate, natural scientists sometimes do the same.

I do not presume to contribute to the large philosophical literature on methodological pluralism. The argument is localized to economics - and to the current situation in economics - since it does not discuss whether ultimately we may possess sufficient knowledge to pick the one correct theory. Most of it is therefore consistent with an anti-pluralist position as this is understood in philosophy. Hence, it bypasses some of the problems faced by pluralism that Caldwell (1988) discusses, such as its relation to a theory of truth. And unlike Caldwell it looks at pluralism in economics as a whole, instead of focusing on pluralism in methodology.

On many issues in economics only zealots hold their conclusions with certainty. Most, while they may think that the evidence for their conclusions is strong, admit at least the possibility that they may be wrong. Yet, in practice they usually ignore this possibility. Thus, some methodologists - following the lead of many

philosophers - seem to argue that some particular method, such as methodological individualism, is the only correct way to do economics, so that any results reached by some other method are worthless. Similarly, in welfare economics economists start (implicitly or explicitly) with a particular value judgment, e.g. the importance of egalitarianism, and usually proceed from there without, at least explicitly, allowing alternative value judgments any weight at all. Likewise, only rarely do those who offer policy recommendations discuss whether these recommendations are robust with respect to errors in their model, despite the familiar saying that "all models are wrong - but some are useful".

Such a procedure differs sharply from the way economists describe the behavior of agents. In simple models agents are assumed to calculate certainty equivalents rather than to operate just on the basis of the most likely forecast. And even half-way sophisticated models go beyond certainty equivalents and allow for risk aversion. I will therefore explore the implications of assuming that economists should operate with as much sophistication as they ascribe to agents, so that they realize that they, as well as the agents whose behavior they model have to live with uncertainty.

My target is therefore argues against segmented decision-making, that is the standard practice of first selecting on one's reading
of the best - but perhaps far from conclusive - evidence, a certain proposition (that is a value judgment, a methodological rule, a theorem or an empirical finding), and then to use this proposition in further reasoning as though it were definitively established. This procedure and its questionable nature are both most evident in empirical work. Typically, an econometrician
presents not only her point estimate, but also it standard error. But in the next step of the argument she ignores the standard error, and plugs only the point estimate of the coefficient into her subsequent equation. One should therefore not be surprised if the resulting conclusions are not robust.

A more reasonable alternative is often, though not always, not to accept as the truth a particular proposition because the
evidence for it seems strongest, but to carry in one's

mind two or more conflicting propositions, while attaching unequal weights to them. (In deciding on these weights one should usually take account of the relative losses from accepting the wrong proposition.) As Mäki (1997, p. 43) has remarked: "it may be that at some point in time, such as now, the epistemic standing of economic theories is such that we had better tolerate a number of strong ... [substitute theories] at the same time." More generally, Richard Foley (1983) has shown that it is not always irrational to hold at the same time two logically inconsistent theories.1

Such a procedure might be called "probabilistic pluralism", since it attaches different weights to various alternatives. It therefore differs sharply from relativism with its notion that all theories are equal. On the contrary, it assumes that we can - and should distinguish between theories that are more likely to be true and those that are not. Such probabilistic pluralism is hardly new. Thus it seems to describe how the Federal Reserve, and presumably other policy-makers often behave. Thus as monetarism obtained more academic respectability in the 1960s and 1970s the Fed shifted towards monetarism - but only part of the way.

I first discusses probabilistic pluralism with respect to economic policy, and then discusses theory, methodology and value judgments. It then responds to the argument that pluralism on the level of the individual economist is not needed, since truth will emerge from the debate of various economists each of whom presents just one point of view and disregards the contribution of other views.

I. Policy

Choosing a policy is analogous to choosing a portfolio of securities. Economic theory tells us that rational investors do not allocate all of their net worth to the asset that they expect to have the highest yield, but diversify in a way that takes into account various expected states of the world and their relative probabilities, as well as risk aversion. Compare that to the typical paper on economic policy. Here we are told that a certain policy would improve the functioning of the economy, and the risk inherent in the adoption of this policy is either ignored, or the reader is given reasons for thinking that it is minor. But the probability that these reasons are mistaken

and that the risk is substantial, is usually ignored. Moreover, while some specifics of the analysis are often spelled out in excruciatingly detailed mathematical form, we are told little if anything about the loss function implicit in making the policy recommendations, or about the assumed degree of risk aversion.

To be sure, not all policy literature proceeds in this way. Thus there exist a series of models showing that the optimal size of a stabilization policy depends, in part on the standard error of the GDP forecast (see for instance, Friedman, 1953, Brainard, 1967, Mishkin 1998). Moreover, in many cases - though far from all - it would make little sense to diversify the policy "portfolio" by adopting the policy only in part. But even in all-or-nothing cases, it would help to indicate the riskiness of the policy. To be sure, saying that the author should point out all the weaknesses of the analysis he or she knows about may be a counsel of perfection, because it would require pointing out these weaknesses not just to the reader, but also to the editor and referees. But even leaving aside the ethical problem in hiding weaknesses, there are sources of risk that could safely be pointed out.

For an example of how risk is disregarded consider the debate on whether banks should be allowed to merge with nonfinancial firms, or to hold stock in them along the lines of the German and Japanese "universal banking" systems. One might agree with the proponents of such a change that it would probably improve the American financial system. But "probable" is not "certain". Hence, one has to look not just at the size of the gain if they are right, but also at the size of the loss if they are wrong, and at the relative probabilities, as well as the appropriate coefficient of risk aversion. Another example is the argument (Cf. Calomaris, 1990) that the FDIC should be abolished, and that instead banks should insure each other. One can make a plausible case that this would probably make the financial system more efficient by reducing moral hazard, but there are also risks. The obvious one is that without the FDIC massive bank failures could set off a major recession.2 It is not irrational for someone to agree with Calomaris that such a contingency is unlikely, and that the reduction in moral hazard that would result from the abolition of the FDIC is highly desirable, and yet

to oppose this change because massive bank failures would be so damaging that, when multiplied by even a small probability, the expected loss outweighs the gain from abolishing the FDIC. And even someone who believes that the mathematical value of the benefit from abolishing the FDIC exceeds the mathematical value of the cost, might reasonably oppose it due to risk aversion.

Disregarding the risk of error could be defended by arguing that the researcher's task is to develop a particular line of reasoning, and it is up to the reader to keep in mind that this reasoning may be wrong, and to evaluate the resulting risk. There is certainly something to this response, but the reader may not be in as good a position as the researcher to evaluate the probability of error. I return to this topic in the penultimate section.

II. Theory

The choice of a theory presents a similar situation. Accepting a particular theory is often (like adopting a policy) an action that has consequences, such as influencing the views of other people, or changing one's research procedures or research agenda. To be sure, the loss function is often hard to specify because we do not know all the implications that acceptance of the theory will have, but that does not justify ignoring the loss function altogether. One reasonable response is to say, for example, that although I accept theory T, I am aware that there is a significant probability that it is wrong. Consequently, when undertaking some action, such as choosing a research project, I am reluctant to select one that is worth doing only if T is correct. And my reluctance will depend not only on this probability, but also on the costs and potential pay-offs of the research project.

It is, of course, not possible to calculate these probabilities precisely, but one can obtain some rough idea, particularly if one is willing to take other economist's opinions seriously. Thus, if I know that someone who is just as well informed as I am, disagrees with me and thinks that T is false, should I not tell my students that, though I accept it myself, there is considerable doubt that T is true? And if I write an op-ed article predicated on the truth of T, if there is much dispute about it, shouldn't I warn readers who are unaware of

this?

Moreover, theories have specific domains. Not only is this so in the formal use of theories as logical systems whose conclusions depend upon the validity of certain assumptions, but also in their informal use relevant to answering empirical questions, where the assumptions are not intended to hold strictly, but only approximately. If the domains of theories are properly specified, then there need be no conflict between different theories. But if, as is usual in economics, the domains of theories are not clearly specified, then there is likely to be an area in which their claimed domains overlap, or at least an area where it is unclear which theory applies. Hence there is often room for an eclectic theory that combines elements of two or more conflicting theories, or for maintaining both theories. We know that theory A is better for one problem, and theory B for another, but are uncertain for a third problem where we may want to look at the solutions given by both theories, and if they differ admit our uncertainty.

To a formalist who would like to model economics on geometry such vagueness may seem "unscientific". But not to those who prefer to model economics on the natural sciences. Ian Hacking (1983, p. 264) reports that:

> Even people in a team, who work on different parts of the same large experiment, may hold different and mutually incompatible accounts of electrons. That is because different parts of the experiment will make different uses of electrons. Models good for calculations on one aspect of the electrons will be poor for others. Occasionally a team actually has to select a member with a quite different theoretical perspective simply ... to get someone who can solve those experimental problems.

Such conduct is not inconsistent with the belief that there exists only one given reality (that is, ontological realism) which we may be able to grasp eventually, but for now we have to make do with what Mäki (1997) calls "temporary pluralism".

III. Methodology

In the 1950s an anti-pluralist view of methodology was much easier to justify than it is now. One could then

argue that there is a delineable set of disciplines known as "science", that we know this set's defining characteristics, and that this set is the sole reliable path to genuine knowledge. To be sure, there were disputes even within the mainstream philosophy of science, but by hindsight they were mere family squabbles. The main task of the economic methodologist was therefore to decide which of the prevailing economic methodologies best captures the defining characteristics of science as set out by philosophers, or if none do so, to develop one that does.

But the "received view" is no more; the old certainties are gone. Even if one rejects (as I do) most of the post-modernist turn, the search for the one true methodology seems quixotic. Now we need to look not for a demarcation criterion, but for heuristics that advance the growth of knowledge. And these heuristics may have features that seem strange. For many years biology made substantial advances even though it used the crutch of teleological argument. Inconsistency between quantum theory and relativity theory has not prevented physicists from working with both and making progress

One should therefore be skeptical about statements that economists must do one thing or another, for example that they must not make statements about aggregates unless these can be shown to be necessary implications of rational utility maximization. That claim seems particularly hard to accept when it comes from those who, in defending the unrealism of assumptions take refuge in Friedmanian instrumentalism and is worth considering in some detail, because it provides a good example of the flaws of such a monistic methodology.3

If physicists could get by for so long by taking gravitation as an unexplained "given", why should macroeconomists, too, not be allowed to take observed regularities as given?4 Even in the case where macrotheory is actually inconsistent with microtheory, instead of just being not derivable from it, we need not, as the example of relativity theory and quantum mechanics illustrates, drop either. If we want to claim complete knowledge something will have to give and consistency between macro and micro economics may

have to be established. But until we reach the point where such a claim is realistic we can use both theories. It could be that rational choice theory is a sufficiently close approximation to be useful when dealing with most microeconomic problems, but not when dealing with certain macroeconomic problems.

The insistence of new classical economists that we have to reduce all macroeconomics to microeconomics is therefore hard to justify at this stage of our knowledge, as long as one views economics as an empirical science, that is as an attempt to explain and predict regularities, rather than as a branch of logic. It would be correct only if three conditions hold. The first is the claim that we may not tolerate - even for now contradictions that we hope to resolved in the future. That claim is hard to justify, though many new classical theorists seem to treat it as obviously correct. The second is that we have sufficient grounds to be more confident in rational choice theory ..
than in the relevant macro theory - and that is by no means obvious. Third, even if these two conditions hold we would be justified in dropping the macrotheory only if it is inconsistent with the microtheory, not merely if it cannot be derived from the rational choice theory. Biology is consistent with quantum theory, but nobody would try to reduce biological statements to quantum theory statements.

How then can we understand the new classical insistence on rigorously deriving all macrotheory from microtheory? One possibility is to view new classical theory, not primarily as an attempt to explain observed economic phenomena, but as an attempt to derive the logical implications of rational choice theory, thus in effect defining economics in a Robbinsian rather than a Marshallian way.

Maartan Janssen (1998, p. 308) has made the interesting suggestion that it is a unity-of-science argument that provides the valid justification for the reductionism that the new classicals insist on, writing:

> Two distinctively separate disciplines such as microeconomics and macroeconomics can only coexist
> in a fruitful way if they have different domains of application. However, as ... both study aggregate phenomena, it is not clear when to apply one (and

not the other). It is thus natural to study the compatibility of the two disciplines, and as microeconomics has a better developed analytical structure, the reason for investigating the possibilities for microfoundations becomes clear.

Janssen is right in saying that the interrelation of micro and macro economics is worth studying, and that it would be desirable to relate them more closely. But that is very different from saying that we must avoid macroeconomic statements that cannot be derived from microeconomic foundations, even if it means having no answers to certain questions, or giving answers that have a much less secure empirical foundation. Moreover, that microeconomics has "a better developed analytic structure" does not necessarily mean that it is superior to macroeconomics. Other criteria, such as empirical confirmation and applicability matter too, particularly if one thinks of theories as instruments for either prediction or explanation.

Yet, none of this means that being rigorously derivable from microtheory is not a desirable attribute of a macrotheory. It does add an additional way of testing a macrotheory, and furthermore, if the macrotheory is well confirmed, it provides a way of testing the microtheory. But there is a distinction between being a desirable attribute and being a necessary attribute. Hence, it may well be useful to have two or more macrotheories (or versions of macrotheory). One version is at least loosely derivable from microtheory (even if it does not meet the strict conditions of reducability.5) The other version, though not inconsistent with microtheory, is not derivable from it, but can solve some problems that the derivable theory does not, or has better empirical support. Moreover, the consistency of micro and macro theory should be treated as a two-way street.

Another instance where a pluralistic attitude towards methodology is appropriate is the debate about Verstehen. Obviously, Verstehen does not provide evidence that meets the criterion of interpersonal knowledge; what makes you understand why 12th. century peasants did not revolt may not necessarily make me understand it. But if good empirical evidence that is interpersonal is not available, evidence derived from Verstehen is preferable to mere guesses. Moreover, Verstehen can add to the usefulness of a theory in

satisfying our intellectual curiosity, and by making the theory easier to work with on an intuitive level. So, instead of treating Verstehen as either totally useless or as a necessary attribute of any adequate theory, why not treat it as a desirable but not a necessary feature?

IV. Value Judgments

In making policy recommendations economists must make value judgments. Even the application of the Pareto criterion is a value judgment, though an unusually vacuous one that might even be called hypocritical.6 Usually much less restrictive value judgments are needed. Ideally policymakers or philosophers could provide them. But policy-makers are often reluctant to be that explicit, and philosophers do not speak with one voice.7 Thus, some philosophers tell us that individuals have an inherent right to their property, so that a just state will not redistribute income. Others say that the state should redistribute income because equality is the moral default setting and inequality of income can be justified only insofar as it benefits the poor as well as the rich. Both sides present coherent arguments.

So what should the economist do? One alternative is to confine himself to purely positive economics. But that is hard to do, both because value judgments tend to seep unnoticed into positive analysis (the assumption of a quadratic loss function is one example), and also because the economist's clients, that is the general public and policy-makers, want definite answers to questions that combine positive and normative elements. Another alternative is for the economist, despite his lack of training in philosophy, to take the plunge and make the required value judgments herself. But that adds an arbitrary element to his conclusions. A third alternative is to study ethics and political philosophy in depth, However, that suggestion not only comes up against a time constraint, but also seems insufficient. To those who have at least some positivistic tendencies the various resolutions provided by political philosophy, despite their great sophistication and substantial value-added over untutored common sense, often seem tenuous.

Hence, in making value judgments - regardless of whether economists take an untutored plunge, or devote much study to it they are on insecure grounds.

A related and serious problem is that one's value judgments may be inconsistent. I may believe in the importance of property rights, but also in the need to provide more help to the poor than private can makes available. Should I favor a progressive income tax to finance a welfare program?

Such problems suggest a fourth possibility, straddling. Given uncertainty, straddling, that is going part of the way with each of two or more contradictory principles, is appropriate if there are increasing costs to errors.

Suppose the issue is whether to tax Peter $10 to pay to Paul, and that we believe that this redistribution is appropriate, but are not sure. Hence, we may tax Peter only, say $5. If we are right in believing that a $10 tax is appropriate, then by imposing only a $5 tax we commit two injustices, depriving Paul of $5 that are due to him, and permitting Peter to retain $5 that should not be his. And if our belief is not justified, then we also commit two injustices. Alternatively, if we impose the $10 tax, then if our belief is correct we commit no injustice, but if it is incorrect we commit an injustice that in dollar terms is twice as large as in the first case.

Obviously, our decision should depend, in part, on how confident we are in our belief, as well as on our degree of risk aversion.8 But it should also depend on our evaluation of the relation between the magnitude of this injustice and the loss that this injustice creates, that is on whether there are increasing, constant or decreasing costs to injustice (Cf. Johnson and Mayer 1962). And that is not clear. Neither increasing, constant nor decreasing costs can be dismissed out of hand. Some might argue that there are increasing costs to injustice: that imposing a $10 unjustified tax on any one person has more than twice the moral cost of imposing a $5 unjustified tax on two persons, because unjustified suffering should be spread as thinly as possible rather than concentrated on one person. At the other extreme some might argue that the very existence of an injustice represents an evil whose undesirability has nothing to do with its size - that it is meaningless to quantify injustice. This is the extreme case of decreasing cost to injustice. A more moderate position

is
that there are both fixed costs and rising variable costs to injustice, in the sense that the very existence of any injustice, however small, is offensive, and hence imposes a fixed cost, but that there is also an additional cost to injustice that varies (probably more than proportionately) with its magnitude.

In the first case (increasing cost) there is a role for straddling, while in the second case (decreasing costs) there is not. In the third case the answer depends on whether the variable costs are increasing at a fast enough rate to outweigh the fixed cost component. Thus in at least one, and perhaps two of the three cases, the pluralist's tendency to straddle may be justified.

Probabilistic pluralism in value judgments can also be
justified in another way. Theories of ethics are grounded in the basic principles that the public perceive to be ethical. In turn, much of the public's perception is based on its evaluation of particular situations (which may be either actual or hypothetical). And since these evaluations do not necessarily form a coherent, logical system, but have a strong emotive element, it would not be surprising if sometimes there are contradictions. When confronted with a specific situation I may decide that X's rights have precedence over Y's, and derive a general rule from this. But when confronted with a very different case I may decide that Y's rights have precedence and derive a rule that conflicts with the previous one.

A pragmatist might well argue that we simply have to make our peace with a world in which certain moral imperatives have domains that are not adequately demarcated. A decent society needs to
think of human life as in one sense sacred, not as something whose value is measured in dollar terms. We do not permit someone to sell to another person the right to kill him. At the same time, in making practical decisions, society sometimes has to weigh human life in dollar terms. Few would advocate spending, say $100 million on a medical procedure that would prolong one person's life by only one day.9 And we permit risky activities like coal mining.

V. Individual or Collective Pluralism

A speech community may act in accordance with

pluralism, even if no individual member does so, by allowing an adequate hearing to diverse points of view. Is that all that one should ask for, or is it desirable that on many issues individual scientists keep several divergent views and their probabilities in mind? In other words, should economists follow lawyers and act as advocates, or should they try to resemble as best they can the picture of the dispassionate scientist found in idealistic discussions of science. There is something to be said on both sides.

On the one side, acting as an advocate rather than dispassionately weighing the evidence and admitting weaknesses in one's arguments comes naturally because it avoids the discomforts of cognitive dissonance. Hence, regardless of whether an economist (or other scientist) should behave this way or not, in practice she is likely to do so, and, one might argue, it is better to
do so openly, and put the audience on notice. Moreover, economists, like other flesh and blood scientists, need the stimulus that comes from motives like status enhancement; perhaps there is some truth to the frequent confusion of disinterested and uninterested. An attitude of "my theory, right or wrong" is
likely to motivate them to work harder than is an objective search for the truth. One might therefore argue that the advocacy model of economics represent a useful and almost necessary division of labor.

But there is a stronger case to be made on the other side. Even if full attainment of the ideal of the dispassionate scientist is out of reach, maintaining this ideal points economists in the right direction. Moreover, the advocacy model has its comparative advantage in situations where the public or policy-makers can exercise reasoned judgment about who is right, or putting it less positively, where experts can do little better than the lay public. Thus we let juries decide the facts of the case, while judges decide the law. In economics the public and policy-makers find it difficult to decide who is right. And not only the public, but also other economists face this difficulty. Authors of empirical papers often have many opportunities to skew their results by, for example, leaving out observations. If readers are to take such papers seriously they must

have some confidence that the author is motivated, or at least more or less constrained, by the dispassionate-scientist model.10 Furthermore, the public is less likely to take the views of economists seriously, even on issues on which most economists agree, if on other issues it sees economists occupying immovable positions that concede nothing to the other side. In addition, if economists, because they see themselves as advocates, are in the habit of ignoring the evidence on the other side when they present their case, they are likely to slip into the same habit when deciding which side to choose, and then to stay with it even if strong contrary evidence comes along. Hardly a formula for swift progress.

VI. Conclusion

Theorems are subject to demonstrative reasoning. Theories and the evidence for or against them generally are not. Insofar as economics models itself on empirical science with its focus on theories rather than theorems the principles about behavior under uncertainty that economists proclaim with regard to agents should apply to economists themselves. This provides an opening for pluralism. Hence, in giving policy advice economists should seek diversification and consider the probability of error, and also the loss function and risk aversion. And since advocacy of a theory is in a relevant way like a policy decision, the same applies to theories. If this means employing several contradictory theories, then that is consistent with rational behavior. In choosing a methodology, too, a sharp dichotomy of right and wrong is not useful, as the example of new classical reductionism illustrates. Similarly, there is a role for probabilistic pluralism in making value judgments.

Endnotes

1. Foley (1983) gives an example of rational belief in two conflicting propositions. Suppose that a persuasive experiment shows that there is a 99 percent probability that each one of a set of a hundred hypotheses is true, while another, conclusive experiment shows that not all of these hundred hypotheses can be
true at the same time. It is then reasonable to accept each of these hypotheses, while also accepting that at least one of them must be false.
2. There is also the problem that banks might use membership

in their insurance organization as a tool for collusion.

3. For a powerful and general critique of reductionist claims see Kincaid (1997)

4. To be sure, macroeconomic regularities are not as reliable as the regularities that physicists deal with. But then economic theory is also not as solidly established as physical theory. It is therefore far from obvious that in economics observed regularities should play a relatively lesser role relative to theory than in physics. And while, in principle, the Lucas critique makes macroeconomic regularities suspect, there is not much empirical evidence to suggest that in most situations it is empirically important.

5. I am using the term "derivable" in a loose, common-sense fashion that does not imply that the strict conditions for reduction (see Kincaid, 1997) are fully met.

6. Sheltering behind the Pareto criterion is hypocritical because the probability that any change would make nobody worse off is so small that if economists take the Pareto criterion seriously they could make virtually no policy recommendations.

7. Presumably policy-makers are reluctant to make value judgments in part because they do not want to admit to themselves that they are making some people worse off. Moreover, in democratic countries policy makers are selected in part on the basis of their skill in papering over differences.

8. This assumes that committing an injustice has no further consequences.

9. Along these lines see Hausman and McPherson's (1996, Chs. 1 and 14) discussion of rich countries exporting pollution to poor countries.

10. For a survey of the extent to which economists do take econometric evidence seriously see Mayer (1995, Ch. 9)

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