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TWO PROPOSALS FOR THE RECOVERY OF ECONOMIC PRACTICE

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Recently I published a paper in the *Journal of Economic Literature* entitled 'Clarifying Popper' (Caldwell 1991a). The intended audience comprised everyday economists who wanted to know a little more about Karl Popper's philosophy and methodology of science and how his ideas might relate to their discipline. Much of the paper was devoted to exposition, but it also contained an argument about what I thought was valuable in Popper's work. The argument ran as follows. The part of Popper's thought that is most well known among economists, his falsificationist methodology, when strictly interpreted, is of little use to economists. Falsificationism also appears to be inconsistent with Popper's methodology of the social sciences, situational logic, a doctrine that may be of value within economics. Finally, if one emphasizes ideas found in Popper's writings on critical rationalism, it may be possible to save Popper from the inconsistency: though they apply in different domains, both falsificationism and situational logic can be used to enhance the critical environment. Because of the audience for the paper, direct references to debates in methodology were muted. One purpose of the current paper is to make them more explicit.[1] Another is to advance two proposals concerning the practice of economic methodology that, if followed, may help us to understand better the practice of economists.

MARK BLAUG'S ADVANCE (RETREAT?) FROM POPPER TO LAKATOS

There is a decent anecdote about the origins of 'Clarifying Popper'. In June 1987 I attended a conference on interpretation in economics hosted by Arjo Klamer at Wellesley College. Wellesley is one of the Seven Sisters and the men's room in the building in which we met reflected it. Since the male–female ratio among economists interested in interpretation mirrors that of the profession as a whole, there was a line in the men's bathroom at coffee break time, and it was close quarters to boot. Behind me stood

Mark Blaug, and hoping to pass the time a little less uncomfortably, I asked him an impossibly big question. I said, 'Mark, you and I have been arguing about the merits and limitations of Popper's falsificationist methodology for nearly a decade now. Where do you think our argument stands?' After about a three-second pause he said, 'We both think falsificationism is hard to put into effect in economics. You say we should abandon it, and I say we should try harder.' I was stunned. Ten years' worth of work, and he had summed it up, under duress, in two sentences. It was not the first time I was in awe in Blaug's presence, but it was doubtless the most memorable. Anyway, I resolved soon thereafter to re-read the relevant parts of the Popper corpus as well as the secondary literature in economic methodology, to see if he was right. I wrote him a couple of long letters, and he replied to them in detail. From this came a paper, and indeed, the original version of 'Clarifying Popper' looked a lot like an open letter to Mark Blaug. It took a summer of rewriting to transform it into a more standardized product.

How accurate was Mark Blaug's pithy summary? His first sentence is right: both of us agree that the problems of putting falsificationism into effect are severe, in economics and generally. But there is more to our responses to the dilemma than he gave either of us credit for, just as there is more to Popperian thought than falsificationism. It is true that at times Blaug embraces falsificationism and urges economists to alter their practices. But he also deviates from it, at least to the extent that he lauds the merits of the methodology of scientific research programmes (MSRP) and methodology of historiographical research programmes (MHRP) proposed by Popper's student, Imre Lakatos. As for me, I do not so much say 'abandon falsificationism' as I do 'embrace criticism'. Nonetheless, Blaug is right, I do not think that falsificationist principles are very helpful for criticizing economics, unless one wants to throw out virtually every economic research programme, heterodox and orthodox alike.

Let us get down to cases. There are two general instances when Blaug sounds most like a Popperian falsificationist. The first is when he is using falsificationist methodological criteria to criticize a research programme. An excellent example is the second part of his brilliant brief against Marxism (Blaug 1990: 36–56). Blaug first shows that many of Marx's predictions were hedged all around with qualifications: precious few 'bold conjectures' came from the pen of 'the socialist Böhm-Bawerk'. Of the handful of predictions that were not ambiguous, few occurred. As for later Marxists who try to revise his analysis in the light of subsequent history, all such efforts are viewed as 'immunizing stratagems' designed to protect Marxism from refutation. The second instance is when Blaug argues that falsificationism is a prescriptive ideal that economists should try to follow, hard though it may be to do. Indeed, he advocates it precisely *because* it is so easy to find confirming instances of theories in economics: economists

should try to find tests that would allow us to discriminate among competing hypotheses. Blaug contrasts this aggressive and prescriptivist role of methodology with a defensive (defensive of the economics profession), descriptivist one, and he attributes advocacy of the latter to me, among others (ibid.: 3–7).

Blaug is wrong about my wanting to defend and describe the profession (I *do* want a *better* description, but I also insist on the importance of *criticizing* our practice), but let us set his caricature of my position aside for now. In the section of 'Clarifying Popper' on falsificationism, three objections to it (labelled the Philosopher's, the Historian's and the Economic Methodologist's Objections) were offered. How might they be used against Blaug the falsificationist?

The Historian's Objection is the weakest one, because it is so easy to answer. Historians of economic thought reconstruct the evolution of economic theory. If it could be shown that through time economic theory 'progressed', and did so because economists (consciously or unconsciously) had followed falsificationist principles, then Popper's prescriptions would be very useful to historians. The Historian's Objection asserts that the intellectual history of economics cannot be so reconstructed, that falsificationism is not helpful if one wishes to describe the behaviour of economists. This is a weak objection because the committed Popperian can always respond that falsificationism is a *prescriptive* doctrine. If economists have not followed it, so much the worse for their discipline. They should try harder.

Even though the Historian's Objection is easily answerable, advocates of falsificationism in economics have not felt comfortable with the standard response, probably because the most prominent ones (Blaug and T. W. Hutchison) are themselves historians of thought. In any event, something like the Historian's Objection seems to have been behind Blaug's decision, which dates back to the early 1970s, to begin moving towards Imre Lakatos's MSRP and MHRP for assessing and historically reconstructing economics (Blaug 1976; cf. Caldwell 1991a: 10–13). Blaug has recently made the link between Lakatos and the prescriptive–descriptive dilemma explicit. In his Afterword to a conference volume on Lakatos and economics, he argues that the MSRP offers reasonable prescriptive guidelines for the practice of economics, and that they should be preserved whether or not economists ever follow them. The MHRP provides an historical framework for reconstructing the history of disciplines, and he admits that for economics it may well be 'largely false' as a descriptive vehicle (de Marchi and Blaug 1991: 501–5). The Lakatosian framework, then, allows Blaug to parry the Historian's Objection: the MSRP allows him to retain his insistence on prescriptive methodology, while the MHRP permits Blaug the historian of thought to admit that economic practice often deviates from the ideal.[2]

The Economic Methodologist's Objection is a bit more formidable. The target of the objection is Popper's prohibition against immunizing stratagems, a prohibition that Blaug used so effectively against Marxist revisionists. Popper stated that the social sciences employ the method of 'situational analysis' or 'situational logic'. In using this method, the social scientist describes the 'situation' (both goals and constraints) an actor faces, assumes that the actor chooses rationally (the 'rationality hypothesis'), then makes a prediction about the actor's behaviour. If the actor fails to behave as predicted, the social scientist re-examines the description of the situation. Crucially, the rationality hypothesis is never questioned. This is what led me to argue that the method of situational logic has an immunizing stratagem as its prime directive (Caldwell 1991a: 13). It is as if the social scientist were instructed: when an actor does not behave as predicted, fiddle with the description of the situation until you get the observed behaviour to emerge, always as a rational agent's response to some set of goals and constraints.

The method of situational analysis clearly conflicts with the prohibition against immunizing stratagems. How does this relate to economics? It should be evident that much of standard microeconomics involves modelling the rational behaviour of individual agents under variously specified constraints, some examples of which were given in my paper (ibid.: 17). If this is the case, then the consequences for a falsificationist assessment of microeconomics are profound. If one takes Popper's prohibition against immunizing stratagems seriously, then not just Marxism but many parts of neoclassical microeconomics must be rejected as hopelessly '*ad hoc*'. What is Mark Blaug's reply to that?

At times his response has been extraordinarily weak, as when he simply ignored situational logic (e.g. of Popper's positive contributions to the methodology of the social sciences, only his endorsement of 'methodological individualism' is discussed in Blaug 1980: 46–52), or cast doubt on Popper's grasp of the social sciences (e.g. Blaug 1985: 287). His ultimate response is more coherent, and again, Lakatos comes to the rescue. The MSRP states that all research programmes include 'hard core' propositions, initial 'givens' that are not to be questioned within the analysis. If one interprets the rationality hypothesis as a component of the hard core (as it certainly makes sense to do), then neoclassical microeconomics counts as a legitimate scientific research programme. Whether or not it counts as a *progressive* programme, either theoretically or empirically, is another matter. One gets some sense of Blaug's opinion of the issue in the conclusion of his assessment of radical economics: 'Radical economics has, I think, great failings, and I personally end as I began – an unconverted neoclassical economist. Nevertheless, a study of radical economics leaves one ultimately almost as unhappy with orthodox economics as with radical economics' (Blaug 1990: 81). Blaug's assessment of neoclassical economics

brings to mind Winston Churchill's (1947) description of democracy: '. . . it has been said that democracy is the worst form of government except all those other forms that have been tried from time to time'.

A few comments are in order before moving to our next topic. Mark Blaug's position was strengthened by his embracing of Lakatos. Because the MSRP has a prescriptive component, Blaug was able to retain a prescriptivist role for economic methodology, an outcome he prefers. Even better (given his preference for neoclassicism), if one uses the MSRP to assess mainstream neoclassical economics, one finds that the programme is a legitimate science (though not necessarily a progressive one). Finally, by endorsing Lakatos's MHRP, Blaug is able to avoid severing the history of economics from discussions of methodology. These are no small victories. It must be added that, to his credit, Blaug developed (and when necessary, revised) his ideas while engaging in an extended and open dialogue with his critics. The transformation of his views has not been without costs, however, two of which must be mentioned. First, Blaug's current position is a retreat from Popper's prohibition against immunizing stratagems, and as such constitutes an abandonment of strict falsificationism. Second, the movement away from falsificationism undermines those of Blaug's past critiques of research programmes (like his second paper on Marxism) that rely on the prohibition against immunizing stratagems for their force.[3] These are not trivial costs.

PROPOSAL 1: SITUATIONAL ANALYSIS AND THE RECOVERY OF PRACTICE

The recent literature in methodology and related fields is filled with references to the 'recovery of practice'. The notion provides a subtitle for a recent volume edited by Neil de Marchi; paradoxically (given the argument to follow) its title is *Post-Popperian Methodology of Economics* (1992).[4] Donald McCloskey (1983: 493, 499) juxtaposes the 'official rhetoric' of modernism with the 'honorable but unexamined' rhetoric of the workaday economist. Daniel Hausman (1992: ch. 14) characterizes his own efforts as 'empirical philosophy of science', his goals being to understand and to assess the strategies of knowledge acquisition employed by neoclassical economists. Though the approaches (be they sociological, rhetorical or philosophical) differ, all wish to understand how economists 'really' practise their trade, how they persuade one another, why they embrace certain types of arguments and reject others, and so on.

It was suggested earlier that situational logic may provide a vehicle for the recovery of economic practice. It is now time to put this hypothesis into the form of a (rather carefully hedged!) proposal: *Situational analysis provides a good starting point for the reconstruction of certain fundamental aspects of economic practice, and should so be used. It can also serve* *effectively as a template that will allow methodologists to distinguish among the varieties of practices engaged in by economists.*

Let us unpack the hedges and qualifications. First, what exactly *is* a 'situational analysis'? As noted in 'Clarifying Popper' (p. 15), the details of how one could be done were never laid out by Popper. Luckily, his student Noretta Koertge (1975: 440) provided a more systematic restatement, a short version of which reads:

1 *Description of the Situation:* Agent A was in a situation of type C.
2 *Analysis of the Situation:* In a situation of type C, the appropriate thing to do is X.
3 *Rationality Principle:* Agents always act appropriately to their situations.
4 *Explanandum:* (Therefore) A did X.

A more extensive version (ibid.: 445) reads:

1 *Description of Problem-Situation:* A thought he was in a problem-situation of type C.
2 *Dispositional Law:* For all such problem-situations A would use appraisal-rule R.
3 *Analysis of Situation:* The result of appraising C using R is X.
4 *Description of the Agent's Competence:* A did not make a mistake in applying R to C.
5 *Rationality Appraisal Principle:* All agents appraise their situations in a rational manner.
6 *Explanandum–1:* (Therefore) A concluded that X was the rational thing to do.
7 *Rationality Principle:* People always act on the outcome of their rational appraisals.
8 *Explanandum–2:* (Therefore) A did X.

Either could be used to reconstruct the practice of economics.

Situational logic is only a 'starting point' for three reasons. First of all, the framework is clearly articulated at a very high level of generality. A lot of work would need to be done to specify the description of various 'typical' situations that are of interest to economists: is the agent a consumer, a manager of a firm, a bureaucrat? Is the agent in a game against nature or against other similarly endowed agents? Are there informational asymmetries among agents?

Second, though parts of standard microeconomics clearly fit the situational analysis mould (especially those models that describe the response of individual rational agents, like consumers or firms, to a specific change in a constraint), others do not. For example, it is not evident within the situational logic framework how one gets from the agent to the market (or other more aggregated) level, nor how to handle what the Austrians

call 'the unintended consequences of human action'. To her credit Koertge recognizes that her model would have to be augmented to account for social phenomena like 'unintended consequences' (ibid.: 441). Other alterations unique to economics would doubtless be discovered as the reconstruction proceeded.

Finally, it is undeniable that certain aspects of economics are not good candidates for a situational logic reconstruction. Certain econometric investigations of social phenomena have only a tenuous relationship to microeconomic theory, and the same may be said of a number of research programmes in both theoretical and empirical macroeconomics. This is why situational analysis was described as a 'template' against which to measure different forms of economic analysis. It is *not* intended as a criterion of demarcation between 'good' and 'bad' practice. What is wanted, rather, is a device that is good at revealing the commonalities and differences that are present in our practice.

Situational analysis is not the only vehicle that could be used to reconstruct the practice of economists, of course. (We will see below that another has already been offered by Daniel Hausman, and part of my goal in making a proposal is to provoke others into offering alternatives to it). To the extent that it has merits, the advantages of using situational logic as a framework appear to be two. First, because it is based on 'folk psychological' categories, it is both a simple and an intuitively plausible device for reconstructing economic theories. Simplicity may be an especially important virtue: the mathematical formalism of much current economic theory is often intimidating, even to economists. Anything that helps to demystify our practice should be welcome. Second, by using situational analysis to recover practice, comparison with other social sciences that use similar folk psychological constructs becomes easier. Situational analysis, though modelled on the practice of economists, was intended to be a method applicable to *all* the social sciences. Different social sciences will emphasize different parts of the framework. Cross-disciplinary understanding may well be enhanced if we are able to discuss our differences in terms of a common reference point.

A final point: Noretta Koertge held out the hope that augmentation of the situational logic framework (e.g. by building-in 'supplementary theories of error, decision-making and belief formation') could increase its empirical content (1975: 447). This might occur in economics, but I think that it is equally likely that we will discover that some of the constraints of relevance for economics involve unmeasurable, subjective elements. By specifying them more precisely we may come to understand economic phenomena better, but we may also come to realize that our ability to predict is limited where such phenomena are concerned. Hayek made a similar point in his discussion of the problems of applying Popper's falsificationist principles to the 'complex phenomena' of the social sciences:

> The advance of science will thus have to proceed in two different directions: while it is certainly desirable to make our theories as falsifiable as possible, we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena.
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> (Hayek 1967: 29)

THE PHILOSOPHER'S OBJECTION

Thus far we have neglected the third objection to falsificationism. The Philosopher's Objection states that falsificationism is inadequate both as a methodology and as a philosophy of science. It is inadequate as a methodology because it can lead scientists to make bad decisions, and it is inadequate as an epistemology because it rules out any discussion of how evidence supports theories. These arguments have been pushed most forcefully by the philosopher of science Daniel Hausman (e.g. 1988, 1992), hence their name. It is important for economists to realize just how unpopular Popper is among philosophers of science. Popper's anti-inductivism and non-justificationism imply that all attempts to discover criteria for determining the warrant of arguments are chimerical and that consequently neither policy nor theory can be grounded on any appraisal of how well supported statements are. As one philosopher of science put it to me: 'Popper's error is to take Hume too far.' He meant by this that Popper's position comes very close to scepticism and, for most philosophers, too close.

The methodological part of the Philosopher's Objection carried over into the Economic Methodologist's Objection (where 'bad decisions' = throwing out situational analysis). The epistemological argument, though it touches on some fascinating issues, is sufficiently complex that we will forgo any discussion of it here. This is a perfect point, however, briefly to review the proposals for the recovery of practice made by Hausman in his recent book, *The Inexact and Separate Science of Economics* (1992).

In the first part of the book, Hausman provides an account of the structure of neoclassical microeconomics, including partial equilibrium theory (utility theory and theories of consumer choice and of the firm), general equilibrium theory and welfare economics. He argues persuasively that 'equilibrium theory', the components of which emerge in the course of his reconstruction (they are summarized in a diagram on p. 271), provides the framework within which neoclassical economists permit themselves to theorize. The boundaries of the structure are well defined; this is why he calls economics a 'separate' science. But the data that economists have available with which to confront their theories are not good enough to produce telling tests. Economists respond by severely limiting the

domain of their discipline. Since there is more to the world than is contained in that domain, their science is also an 'inexact' one.

Hausman characterizes economists as following a variant of Mill's method *a priori*. Such approaches have been out of vogue, of course, since positivist ideas came to dominate the methodological rhetoric of economists in the 1940s. Hausman likes a qualified version of Mill's method, so he spends a chapter showing that a host of writers on economic methodology from the positivist era (Hutchison, Machlup, Friedman and Samuelson) botched their philosophy, and two more dispatching the writings of Popper and Lakatos. From Hausman's perspective, it is fortunate that these methodological writings by economists and philosophers have had little effect on economic practice. Indeed, he finds the actual methodology followed by neoclassical economists both uncontroversial and rational, with one crucial caveat that is mentioned in the last sentence of the quotation below.

> . . . economists employ an uncontroversial method of theory assessment. Unfortunately, owing to poor data (relative to the state of economic knowledge), little can be learned about which theories are better confirmed. Given the initial credibility of the basic behavioural postulates of economics, it is rational to remain committed to them in the face of apparent disconfirmations. The consequence of such a defence is to leave economists unable to learn very much from typical economic data.
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> (Ibid.: 253).

To summarize: given the problems with testing in economics, economists are rational to cling to 'equilibrium theory', even in the face of disconfirmations. But by doing so, they risk being dogmatic. Hausman offers two recommendations for changes in practice to avoid that outcome. First, economists should engage in more, and more varied forms of, empirical work: they should try to learn more from 'typical economic data'. Second, the discipline should be more open to alternative theoretical frameworks, less insistent on its status as a 'separate science' (ibid.: 253–5).

This is an excellent book, especially the first section in which the theoretical structure and strategies of neoclassical economics are revealed. In addition, Hausman's analysis of the writings of economic methodologists is more philosophically sophisticated than those of economists (like Blaug and me) who have surveyed and assessed the same literature. On methodological issues, Hausman sometimes reaches the same conclusions as Blaug and Caldwell, and sometimes he differs from them. For example, both Hausman and Caldwell share (*contra* Blaug) the same negative opinion of the prescriptions offered by the falsificationist Popper. Less obviously, Hausman's preferred method *a priori* has much in common with the method of situational logic: both direct economists to cling to their theoretical framework (whether one calls it 'equilibrium theory' or 'situational analysis', both of us are talking about theories in the same domain, namely, standard microeconomics), even in the face of disconfirmations; both embrace an immunizing stratagem. As such, it is both surprising and disappointing that, in his chapter on Popper, Hausman does not mention situational analysis even once. It would be interesting to know what this vehemently anti-Popperian philosopher thinks of Popper's social science methodology. On a final issue, and it is a critical one, Blaug the Lakatosian, Hausman the Millian, and Caldwell the Situational Logician all agree: testing is difficult in economics.

PROPOSAL 2: EPISTEMIC PESSIMISM AND THE RECOVERY OF PRACTICE

I differ from my Lakatosian and Millian comrades in my *response* to our common perception of the empirical difficulties besetting economics. Though labels are always inadequate, our differences might be characterized as follows: Mark Blaug and Daniel Hausman tend towards epistemic optimism of the empirical variety, whereas I am an epistemic pessimist.[5]

In calling them empirical epistemic optimists, I do not mean that either Hausman or Blaug are naive about the difficulties facing economists who try to do empirical work. As noted above, in his description of our practice Hausman underlines the 'poor data' economists must perforce utilize. The last third of Blaug's (1992) masterful book on methodology consists of a series of case studies of fields within economics. For all but one he is forced to conclude (and lament) that there has been precious little empirical progress in evidence.[6] Both, then, recognize the difficulties. It is their *response* to this situation that leads me to characterize them as epistemic optimists. Even in the face of the problems that they identify, Blaug and Hausman nonetheless urge economists to do more, and more kinds of, and better empirical work. They do so because they think (or hope) that this is the path to progress. And for them progress means more testing and better confirmed empirical relationships, because this leads to a better understanding of the world.

It can be mentioned in passing that the call for more and better empirical studies is a common one among methodologists. Even so, it may well be that this is something of a default position. I conjecture that the prior motivating belief is that mathematical theorizing has been taken *too far*, that the balance between theoretical and empirical work needs to be restored. In any event there are many examples of this latter type of argument. T. W. Hutchison (1977: ch. 4) wrote nearly two decades ago about 'the crisis of abstraction' in economics. More recently, Thomas Mayer (1993) pushes for a clarification of the separate domains, arguing that the 'precision' that theory provides should not be counted as more important than the 'truth' about economic phenomena that empirical studies aim to discover. As a

final example, when McCloskey (e.g. 1991) demystifies the models of theorists (models are just another form of metaphor, and sometimes a form not well suited to our tasks), one of his goals is to convince the profession that formalization has gone too far. Though their approaches are very different, all of these scholars' positions lead them to arguments for more, and sometimes more kinds of, empirical work.

It is now time to lay out what I mean by epistemic pessimism. It is important to note that, at least in the variant that I endorse, epistemic pessimists (EPs) share Hausman and Blaug's view that the most important type of progress is a better understanding of economic phenomena. EPs also agree that high theory has not done much to improve our understanding. But an EP *also* has doubts about prospects for empirical work. After all, there already is plenty of empirical work going on in economics: the majority of economists do applied rather than theoretical research. To be sure, some of this is hack science. But there is also empirical work that is both sophisticated in terms of technique and significant in focus, as a perusal of the monthly list of working papers from the NBER will establish. In addition, huge data sets tracing large numbers of individuals and families through time, with data on hundreds of socio-economic variables, have been constructed. The problem is not a dearth of either data or data-massagers.

From the EP's perspective, the problem is that *none of this has led to the type of progress that optimists like Blaug and Hausman hope for.*[7] And from that (intentionally provocative and clearly unsubstantiated claim) the EP is prepared to draw an (itself admittedly premature) inference: no matter how advanced the econometric techniques and how intricate and detailed the data sets become, few robust relationships will emerge. There will always be variables that cannot be measured that will allow one rationally to question a finding, and there will always be studies that reach different conclusions when alternative plausible variables are included in a regression. As a result, differences in interpretation will always be rife.

It should be noted at the outset that this inference can never be established as true. No failure of past or present efforts will ever be enough to dissuade a convinced epistemic optimist that further efforts are unwarranted. For optimists, more (empirical or theoretical work) is always better, since it continually expands the margins of knowledge. The epistemic pessimist's inference *is* falsifiable, however. If advances in data-collection, theoretical sophistication and econometric technique have brought with them a firmer grasp of the workings of economic phenomena, the thesis is refuted. My second proposal begins with a call for the 'testing'[8] of the EP inference: *Economists should examine the recent history of their discipline to see whether and how advances in theory and measurement have improved our understanding of economic phenomena. We must answer the question:*

what do we know about the economy now that is a direct result of the manifold theoretical and empirical advances of the last half-century?

If an effort were ever made to answer this question, it would be critical to carefully distinguish among different varieties of 'progress', itself a daunting task, as the debates within the Lakatosian camp over definitions of progressivity, 'novel facts', and so on, have revealed.[9] But let us be unreasonably optimistic and assume that we *are* able to come up with a suitable categorization scheme. It seems clear that there has been a great deal of (what such a scheme might label as) various types of 'heuristic progress' within twentieth-century economics. Indeed, I suspect that the major reason that heuristic progress has been so much in evidence is precisely because we have *not* witnessed a steady improvement in our understanding and control of economic phenomena. It is only when advances in knowledge are routinely frustrated that one should expect 'progress' to become so universally measured in terms of increases in the variety or mathematical sophistication of theoretical models, or in terms of improvements in the techniques of empirical analysis, *rather than in terms of better knowledge of how the economy works.* In any event, the point is to see exactly what has taken place. This is, of course, just another recommendation for the recovery of practice.

If such a 'test' could indeed be undertaken and epistemic pessimism were to survive it, that is, if it is in fact true that there have been precious few returns (in terms of improved understanding of economic phenomena) from our sizeable investment in theoretical and empirical work, where do we go from there? Do we simply throw up our hands in despair? One could, but I think that there is a better response, namely, to try to figure out *why* economists have had so little success. The type of questions we need to ask are: what constraints do economists as scientists face? What constraints on our knowledge of the workings of the economy exist? What sorts of things do we now know, what might we someday know, and what sorts of things will we never know, due to the constraints that we have identified? The second part of my proposal is to urge us to take seriously the question: *what are the limits of our knowledge in economics?*

Again, this is not an easy question to answer. How can one 'know' what things one will never be able to know? Identifying one's own constraints is never easy, and the assessments one reaches are always provisional. Even so, I still believe that the programme is worth attempting. It is one to which methodologists and historians of economic thought, as well as applied economists and theorists, could contribute, albeit in very different ways.[10] Indeed, the programme itself could end up establishing that progress of a certain limited type *is* possible in economics, in empirical, theoretical *and* methodological work as well. But the progress would consist of discovering exactly what the *limits* of our knowledge actually

are. Perhaps this is a type of progress that only an epistemic pessimist can fully appreciate.

I close this section with a final conjecture. My manner of presentation in this section has been intentionally and deliberately provocative, with the goal of stimulating thought and discussion. But even so, it should be noted that the programme that has been proposed should not seem wholly strange to economists. After all, economics is all about rational constrained choice. A programme that encourages economists to identify their own constraints should seem natural. In addition, a systematic study of the constraints that economists and other social scientists face surely makes more sense than simply presuming that our subject matter will yield to the tools of science. Yet only a very few economists in the past three generations have been epistemic pessimists. The Austrian subjectivists in general, and Hayek in particular, are exceptions. Why has this been so? The reasons are, in my view, both multiple and complex. An initial simplified conjecture is that (the insights of the methodological community notwithstanding) the grip of positivism, with its optimism and, yes, arrogance about the methods of science, has yet to be completely loosened within economics. There was a time when such optimism was not unreasonable. But after three generations that time has passed. In addition, if we are serious about the recovery of practice, it seems to me that a better understanding of the limitations faced by economists is an admirable goal to shoot for.

CRITICAL RATIONALISM

The third section of 'Clarifying Popper' examined his writings on Critical Rationalism. Though its relationship to the concerns of economists might at first appear tenuous, there were several good reasons to include reference to this part of his work. First, critical rationalism seemed to me to provide a way around the tension between falsificationism and situational logic. If Popper's essential methodological prescription is the idea that scientists must engage in *criticism* in general (rather than that they must obey specific methodological imperatives, like 'Avoid immunizing stratagems!'), then both falsificationism and situational analysis can be employed to advance the cause of criticism. In addition, since they are applicable in different domains, they need not be in conflict. Next, the non-justificationist elements of Popper's writings on critical rationalism (these were most fully developed by his student Bill Bartley; see Caldwell 1991a: 22–4) were also appealing, since they seemed to offer a response (if not an antidote) to some of the more extreme forms of anti-foundationalism that had appeared in the methodological literature in the past decade.[11] Finally, Popper's critical rationalism had provided some of the impetus for my own rough ideas about methodology: for example, that context-dependent criticism is a prime desideratum.

My own ideas are sufficiently rough and *ad hoc* that in the end it is probably not a good idea to attach them to Popper's. And just to emphasize that my advice is aimed at practice and is not an attempt at system-building, I will close with a quotation from the pragmatist philosopher John Dewey, taken from his appropriately titled essay, 'The need for a recovery of philosophy'.

> British empiricism, with its appeal to what has been in the past, is, after all, only a kind of *a priorism*. For it lays down a fixed rule for future intelligence to follow; and only the immersion of philosophy in technical learning prevents our seeing that this is the essence of *a priorism*.
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> (1970: 68)

Could the author of the Preface to *The Poverty of Historicism* have put it any better?

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NOTES

1 This goal is somewhat unusual: authors of academic papers often do not specifically identify the targets of their arguments. But especially in a volume to be used by students, the exercise can be a useful one. I see no way of parrying another possible objection (other than to acknowledge my guilt and carry on), namely, that what an *author* may have meant is quite dependent of what a *text* means, since a text's meaning is created by the interaction between the *reader* and the text. I like the Popperian philosopher Bill Bartley's formulation of the problem: 'I learnt from Popper that we never know what we are talking about, and I learnt from Hayek that we never know what we are doing' (Bartley 1984: 19). The aphorism might be decoded as follows: Hayek emphasized the 'unintended consequences' of our actions, which implies that we do not know in advance all the implications of what we do. In his discussion of 'objective knowledge' Popper argued that once a text becomes a part of 'World 3' it takes on meanings independent of, and possibly quite different from, its author's intentions, so we do not know all the consequences of what we are saying, either.

2 In my criticism of the Lakatosian framework (Caldwell 1991b), I mistakenly attacked the MSRP rather than the MHRP as being descriptively inadequate for economics. My argument should have been against the MHRP, and given Blaug's statement that the MHRP may well be 'largely false' for much of economics, my paper should be taken as an argument in support of Blaug's conjecture. Blaug argues elsewhere that macroeconomics is an exception, more of which anon.

3 This does not mean that Classical Marxism is free from problems. Indeed, there remains a Popperian objection to it, one that can be found in the three-page Preface to his *The Poverty of Historicism* (1991). See Moseley (forthcoming) and Caldwell (forthcoming) for contrasting (though both critical) views of Blaug's critique of Marxism.

4 The title implies that Popper's work, because of its stringent normative content, has little to offer those interested in recovering practice. In a thoughtful introduction, however, editor de Marchi emphasizes that the falsificationist Popper found in some of the methodological literature in economics is a bit of a caricature. I would go further and argue that other parts of the Popperian corpus may well be useful for the task of recovery.

5 There is a third category: epistemic optimists of a theoretical variety. Examples include Hahn (1973), Gibbard and Varian (1978) and Green (1981). This is a decidedly minority view within the methodological community. Revealed preference theory suggests, however, that it is the dominant predilection among economists in general.

6 In the second edition of his methodology book Blaug includes a new chapter on macroeconomics, about which he writes, 'I hope that a reading of Chapter 12 below will convince any "reasonable economist" that the entire history of post-war macroeconomics furnishes a whole series of paradigmatic episodes of falsificationist practice . . .' (1992: xvi). But this misses the point. Clearly, economic theories *do* get falsified. The real question is whether any economic theory could *survive* the strictures of falsificationism, absent the sort of immunizing stratagems employed in microeconomics. (It is significant that the field in which economists are most likely to take data seriously is the branch that is most distant from standard microeconomic theory.) Blaug's examples demonstrate that each successive macroeconomic theory (from Keynesian economics through monetarism to rational expectations) has eventually faltered empirically. The lessons we might draw from this are exactly of the sort that I argue for later in my paper, namely, a better understanding of the *limits* of our knowledge in economics. In this, I think that my position comes closest to T. W. Hutchison's in his aptly titled paper, 'The limitations of general theories in macroeconomics', in Hutchison (1981: 233–65).

7 See the previous footnote for a brief discussion of Blaug's sole exception, macroeconomics. It is significant that the most 'optimistic' paper regarding the applicability of the Lakatosian framework in economics in de Marchi and Blaug (1991) was one by Roger Backhouse on macroeconomics.

8 The whole notion of 'testing' for the presence or absence of 'progress' by the examination of 'history' has been shown to be problematical by historians and philosophers of science. A good summary of the issues is Losee (1987).

9 The difficulties in coming up with agreed-upon definitions of 'progress' and other crucial terms are discussed in the introduction to de Marchi and Blaug (1991).

10 The line of inquiry that I am proposing is this: what can we say, in general and specifically, about the abilities and limitations of economists for understanding social phenomena? One need not be an EP to undertake such an investigation; clearly many economists view the presumption of steady progress as a reasonable one, and could ask the question in the context of 'What sorts of limitations have been overcome thus far?' One probably *does* have to have some leanings

towards pessimism to ask the question in the particular way that it is asked here.

In any event, certain current research programmes in economics can be characterized as trying to answer this sort of question. The case studies in Blaug's (1992) methodology book are actually a fine place to start. Studies of changes in our ability to measure economic phenomena (e.g. Berndt and Triplett 1990, which is the jubilee volume in the NBER's Studies in Income and Wealth series, which now has over fifty volumes in print), Edward Leamer's (e.g. 1983) work on the fragility of econometric inferences, and the work by Fisher and Summers mentioned at the end of Backhouse's introduction to this volume, are examples of very different approaches that nonetheless take the question seriously.

11 In the final chapter of his collection of essays on Popper, Wade Hands (1992) develops this line of argument more fully.

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