Post-Keynesian methodology: an assessment*

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The purpose of this article is to offer a preliminary assessment of post-Keynesian methodology. This is undertaken by initially defining and developing the methodological perspective known as critical pluralism. It is argued that post-Keynesians might well benefit from a better understanding of the methodological views of other groups within economics.

I Introduction

The purpose of this paper is to offer a preliminary assessment of post-Keynesian methodology. A roundabout method of theme production is employed. First, a particular approach to the evaluation of methodological positions, critical pluralism, is defined and defended. An application to another research programme, the Austrian School, follows. Some of the major methodological positions that have been staked out by assorted post-Keynesians are then outlined and preliminary assessments rendered. Recommendations concerning the best way forward form the conclusion of the paper.

II Critical pluralism

a Critical pluralism defined. Critical pluralism is *not* in itself a methodological position. It is a *metamethodological* position, which is to say, it is a set of normative guidelines for the assessment of alternative *methodological* positions. Another way to put this is to say that the critical pluralist is an historian of ideas whose chosen subject is the methodological writings of economists, and whose task is the reconstruction and evaluation of the positions contained in those writings.

The first task of the critical pluralist is to find out exactly what the

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economist (or economists) in question said about methodology. This is often rather difficult, for a variety of reasons. Methodological pronouncements are usually only roughly thought out; they contain seemingly random citations of sometimes incompatible philosophical positions; there is usually an implicit political agenda which is carefully hidden by a veneer of scientific objectivity; almost always the point is to direct the reader to embrace a particular theory; and there is often a specific opponent in mind as well as fellow travellers, but they are seldom explicitly identified. Once a rough idea of what has been said has been reached, the next step is to try to express the methodological position under consideration in as coherent a fashion as is possible. This step is taken because, given the usual incoherence of the source material (for all the reasons just mentioned), it is often too easy for an opponent to construct a straw man out of the position under review. Finally, one drops the effort at reconstruction and embarks on an evaluation, an assessment of the strengths and weaknesses of the position.

b Why critical 'pluralism'? For much of the first half of the twentieth century, philosophers devoted themselves to a search for assessment criteria which would enable them to distinguish science from nonscience and good theories from bad theories. Had they been successful, there would be no need for critical pluralism. There would not be a number of methodologies, there would be one that all legitimate sciences followed. Assessment would be a straightforward matter; if a methodology accorded with the criteria, accept it; if not, reject it. But as is documented in *Beyond positivism* (1982), the quest to articulate the set of criteria ended in failure. How should one respond to this failure?

There are many possible responses: ignore the problem and embrace a particular set of criteria; claim to know what science 'really is' even if one cannot articulate how one knows it; claim that there is no science; focus on the context of discovery rather than the logic of justification; embrace descriptivism; turn to the sociology or the psychology or the politics or the rhetoric of science, and so on. The critical pluralist gives the following response.

1 The failure to discover a universal set of criteria *could* mean that none exists. Or it could mean that a set exists but that, *à la* Polanyi (1958), it cannot be articulated. Or it could mean that we just haven't found it yet. One thing that we *do* know for sure, though, is that the casual claim made by certain scientists that *their* theories follow the scientific method and that their *opponent's* theories do not cannot be sustained.
2 As such, the usual practice of dismissing as unscientific all theories whose methodologies differ from that of the presumably scientific mainstream must be viewed as illegitimate.
3 Instead, the plurality of methodologies should be individually evaluated. Here is one way in which pluralism comes into play: a *plurality of methodologies will be assessed.*

4 But a question immediately presents itself: Against what criteria should the various methodologies be measured? There is no single vision of appropriate scientific practice. How can assessment take place in such an environment?

5 In answering this question, pluralism again appears. *There is a variety of criteria of assessment; there are many ways to evaluate methodologies.* Indeed, one of the missions of the critical pluralist is to show that the rational assessment of diverse methodologies is possible even in the postpositivist age. The philosophy of science may be of some assistance in this endeavour. Too often, though, philosophers (both in the past and, regrettably, even on the present) focus most of their attention on the physical sciences, and the sorts of insights they have gained have not always transfered very readily to our discipline.

The 'pluralism' in critical pluralism shows up, then, in the plurality of methodologies that are evaluated and in the plurality of criteria that are used to assess them.

Why 'critical' pluralism? Pluralism among methodologies is not intrinsically good. For the critical pluralist, pluralism is instrumentally good: given that there is at present no agreement concerning the characteristics of a universal scientific method, it is good to have a number of alternative methodologies to inspect. Thus critical pluralism is *not* the same doctrine as P.K. Feyerabend's theoretical pluralism or epistemological Dadaism (see Feyerabend, 1975; 1978).

Without constraints placed on it, pluralism can lead to some bad results. Two of these are *anarchism* and *scepticism*. The former characterizes a situation in which there are dozens of competing methodologies. The latter is the basis for the claim that there is no better or worse among methodologies, that all methodologies should be viewed as equal. The emphasis on criticism in critical pluralism is designed to combat both anarchism and scepticism. Through the process of criticism, some methodological views are shown to be better than others, and the number of competing positions is reduced.

Another reason for embracing criticism is offered by followers of 'evolutionary epistemology' (see e.g. Radnitzky and Bartley, 1987, and citations therein.) One of the goals of this group is to construct an environment of 'optimal criticism' in which good ideas, theories, policies and so forth are more likely to emerge. It is worthwhile to quote at length from a paper by a leading evolutionary epistemologist:

> The Darwinian law of evolution proceeds in three great steps or rhythms: (a) blind or unjustified variation; (b) systematic selection and elimination; and (c) retention and duplication. . . . [The] question is: How can our intellectual life and institutions, our traditions, and even our etiquette, sensibility, manners and customs and behaviour patterns, be arranged so as to expose our beliefs, conjectures, ideologies, policies, positions, programmes, sources of ideas, traditions and the like, to optimum criticism, so as at once to counteract and eliminate as much intellectual error as possible, and also so as to contribute to and insure the fertility of the intellectual econiche: to create an environment in which not only negative criticism but also the creation of ideas, and the development of rationality, are truly inspired? To answer such a question is to begin artificially to construct – in the imagination and perhaps in reality too – an environment for the advancement of science and learning. Paramount, in any such construction, will be the ecological question of *balance* – for the law of evolution puts those three steps or rhythms permanently at odds with one another. Thus variation and retention, for example, are always opposed. Methodologists, however – even nonjustificational methodologists – frequently give unbalanced advice. Thus Paul K. Feyerabend overemphasizes variation; Popper overemphasizes elimination; and the justificationists generally overemphasize retention (Bartley, 1984: 89).

d Opponents of critical pluralism. Opponents of critical pluralism fall broadly into two categories. On the one hand, there are those who claim to know what the scientific method is. There is nothing wrong *per se* with such a claim. Usually, though, the person making the claim is simply unfamiliar with the recent philosophy of science literature. Those who do know the literature usually argue that *some* standards are necessary, that their methodology is the *best we can do under adverse circumstances* and that the alternative is *anarchy*. The critical pluralist answers that his own analysis *does* employ critical standards, that it provides the *hope* that we may be able to do better, and that critical pluralism is a *foe* rather than a friend of anarchy.

The other group of opponents either denies that science exists, or denies that methodology has any important connection to science. Often it is urged that, if one *really* wants to understand how science works, one should study the politics or sociology or rhetoric of science. The critical pluralist readily admits that our knowledge of science is sadly deficient in many areas, and applauds those who would try to remedy the deficiencies. Those who attempt to *prohibit* methodological approaches, however, usually do so on the basis of a faulty inference: past attempts to find a universal method of science have failed, therefore we should stop examining methodological issues. Such arguments have little persuasive power.

e The goals of critical pluralism. The initial goal of critical pluralism was the modest one of enhancing our understanding of the practice of economics (see Caldwell, 1986; 1988d). But it now seems to me that perhaps more is possible. It is not that the prospect of a single methodology for the discipline has become more likely. But it may occur in the next few decades that a consensus emerges in which *certain* approaches are viewed as *broadly* acceptable across a variety of *different* paradigms in economics. I never thought that such an outcome was possible when I first began working in this field. Even now the very idea of it sets me to shaking.

III Critical pluralism and Austrian methodology

Before proceeding to an assessment of post-Keynesian methodology, we will turn our attention briefly towards the Austrians, the nonorthodox group in economics with which I am most familiar. There are a number of reasons why such a diversion may be worthwhile. First, the Austrians and post-Keynesians share certain similarities, both sociological and methodological. Next, the methodological history of the Austrians contains some lessons. Last and most important, the Austrians are a far more suitable adversary for the post-Keynesians than are the neoclassicals.

a Similarities between Austrians and post-Keynesians. There are what might be termed common sociological traits which the Austrian and post-Keynesian traditions share, and which differentiate them from the neoclassical mainstream. Both groups take very seriously the work of certain seminal writers from the past. As a result, historical exegesis among modern members of each school is common. The old texts are interpreted and reinterpreted in light of current concerns. None of these practices are important to mainstream economists. Both schools occupy similar positions in the profession, in the sense that they define themselves in opposition to the dominant paradigm. Certain universities are known within economics for their affiliation with the traditions. Each group has its own hierarchical structure, its own students, its own journals. Both are pretty much ignored by the majority of economists. Those in the mainsteam who do know about them are either hostile or generously tolerant. The latter reaction is arguably the less flattering one, given that the expressed goal of each school is to change radically the way that economics is done.

Similarities continue at the methodological level. Both oppose the positivist pretensions of neoclassicism. Two particular approaches within mainstream economics, general equilibrium theory and rational expectations models, are frequent targets of attack. The preferred theories (of at least some members) of each group would emphasize that economic activity takes place in 'real' irreversible time; that the world is full of uncertainty, novelty and surprise; that the assumption of perfect knowledge or the reduction of uncertainty to risk obscures the most fundamental problems of economics; that choice is an active rather than a passive process.[1] A final commonality is that both groups currently exhibit a great deal of methodological diversity, though this latter characteristic is of more recent vintage among the Austrians.

b The years of Misesian dominance. For a number of years Austrian methodological thought was dominated by the a priorist approach of Ludwig von Mises. Mises claimed that the fundamental postulates of

[1] This is not to say that Austrians and post-Keynesians always agree about methodology. Compare, for example, the very different opinions of Hodgson (1988) and Boehm (1989) concerning the merits of subjectivism.

praxeology, the science of human action, are apodictically certain, they are known to be true *a priori*. In addition, the axioms have empirical content, though the postulates themselves are neither verifiable nor falsifiable. Finally, the truth of the axioms can be transmitted to the conclusions of praxeology if no mistakes are made in the 'verbal chain of logic' which links them.

Mises was as prolific as he was explicit in writing about praxeology. In the first section of his *magnum opus, Human action* (1949), seven full chapters covering more than 100 pages are devoted to defending the epistemological underpinnings of the science of human action and to excoriating alternative approaches. If this were not enough, he wrote another book in 1962 (it turned out to be his last book) entitled *The ultimate foundations of economic science: an essay on method*. The volume of his work, the explicitness with which his views were expressed, and (it must be said) the strangeness of his position made Mises's praxeology a critical pluralist's dream. An assessment was clearly in order. It is vital to note, however, that the usual criticisms of praxeology, *all of which assumed the truth of the positivist vision of what constitutes legitimate scientific practice*, could not be effectively used in a critique of Mises's position.

An alternative approach (exemplified in Caldwell, 1984; 1986) is to undertake an internal criticism of praxeology. My conclusion was that though many of the arguments traditionally voiced against praxeology had been anticipated and answered by Mises, there were still some substantial difficulties associated with the position, both with the notion of apodictically certain axioms and especially with the asserted linkages in the 'verbal chain of logic' leading from axioms to conclusions.

c Current methodological diversity in the Austrian camp. As one examines the Austrian tradition more carefully, it becomes apparent that many of his successors disagreed with Mises about methodology. Two Austrians had broken with him as long ago as the 1930s, though they were so respectful about it that it was not at all apparent to anyone except for a few insiders that a break had taken place. This reticence about methodological debate has subsided in the 15 years since the Austrian revival began. In addition, in the past five years two entirely different approaches to methodology have surfaced. Certain of these approaches raise issues that may be of interest to post-Keynesians (see Boehm, 1989 for additional citations).

Hayek's work is the least accessible. His position appears to have changed considerably through time; longevity may be the greatest scourge of methodological consistency. Hayek began as something of a champion of equilibrium theory, but soon changed into an opponent. He viewed the explanation of how agents' plans could come into coordination as the central problem facing the social sciences. By the 1940s, he argued that equilibrium theory could shed no light on the process by which agents' beliefs are coordinated. Hayek opposed scientism and seemed to endorse the

falsificationist methodology of Karl Popper. Yet many of his methodological articles can be read as arguments in favour of the proposition that falsificationism has only limited applicability in a social science like economics. Having undertaken a number of studies of Hayek (Caldwell, 1988a; 1988b; 1988c), I suspect that an adequate reconstruction of his position warrants a book-length treatment.

Ludwig Lachmann's views have changed less through time. Lachmann is a thorough-going subjectivist. He praises Mises and Hayek for applying Menger's subjectivism to the theory of value in the 1930s, but he criticizes them for failing to apply the same insights to the theory of expectations. He has made the provocative statement that men like Keynes and Shackle, rather than the Austrian leaders, were more responsible for carrying forward the mantle of subjectivism since the 1930s (see Lachmann, 1976; 1983). Lachmann's position is unique. He embraces the radical subjectivism of Shackle and blends it with the 'common-sense sociology' of Alfred Schuetz (see e.g. Lachmann, 1986). Aspects of this tradition continue in the work of the 'interpretive understanding' or hermeneutical branch of the Austrian School, which is now centred at George Mason University (see e.g. the essays by Ebeling, Lavoie and Shackle in Kirzner, 1986). It should be noted, though, that Lachmann is more eclectic in his views than are his followers.

The last two movements have published little yet. The first is the attempt, at present a single-handed one by Uskali Mäki, to reconstruct the Austrian programme along the lines of scientific realism (Mäki, 1986). The second goes by the name, 'the new institutional economics'. Proponents employ an interesting blend of traditions and techniques: game theory, the property rights literature, psychological explanations of rule of thumb behaviour, evolutionary approaches to the theory of the firm, and a reconstruction of the idea of 'invisible hand' explanations (see the essays in Langlois, 1986).

d What can the post-Keynesians learn from the Austrians? Though the Austrian methodological experience is quite different from that of the post-Keynesians, there are some insights that can be obtained from reflecting on their history.

The first lesson is a procedural one: a full-fledged assessment of the methodology of a research tradition is a time-consuming and painstaking process. There is no natural endpoint to such a process: the metaphor of evolution in 'evolutionary epistemology' is apt. This is why the second half of this paper is best viewed as a preliminary assessment of post-Keynesian methodology.

But turning to more substantive matters, it should be evident from the description above that some progress has been made in the Austrian camp in the area of methodology. For too long the majority of Austrians were under the methodological influence of Mises. Much time and energy was spent justifying an 'a priorist' approach. This was a recipe for intellectual ossification. It is difficult to make progress when members of a tradition focus their

attention on methodological justification. Worse still, the *particular* position in question was a noncritical, justificational one. It insisted on order and doctrinal purity rather than on criticism and progress.[2]

The more general point concerning methodology which the Austrian experience illustrates is an extremely important one. *Methodology matters.* All methods are *not* equal. Adherence to a particular methodological paradigm can retard the progress of scientific thought. Breaking loose from such a paradigm can promote scientific progress.

Modern day Austrians have more or less freed themselves from the methodological influence of Mises. The exciting question arises: Which of the methodologies mentioned above, if any, offers the best hope for scientific progress? Given that post-Keynesians are asking themselves the same question, it is useful to try to formulate an answer.

Hayek has made a number of contributions which have substantive economic content: his emphasis on subjectivism, on the coordination problem, on the market as a mechanism for discovery, on the importance of institutional arrangements for the question of coordination, and on the nature of constitutional governments will all play their roles in future work by Austrians. His insights extend into fields as wideranging as philosophy, law, history, ethics, and even psychology. Unfortunately, he probably has *not* succeeded in providing the Austrians with a consistent methodological approach. Quite different from Mises, he seemed content to borrow ideas from many different traditions. At present the evidence suggests that that those ideas may not always have been mutually compatible (see especially Caldwell, 1988c). It will be a fascinating task to undertake a comprehensive study of Hayek's methodological thought. His was a rich and fertile mind, and he knew most of the principals who created the methodological literature of the twentieth century. But there exist serious doubts that he was successful in formulating a coherent methodology.

Of the remaining three approaches – interpretive understanding, realism and the new institutionalism – my initial sympathies are with the new institutionalists. Hermeneutics sheds light on certain of the problems of economics, but it probably is not wide enough to incorporate all of it. Realism, on the other hand, is a doctrine that is wide enough to cover a number of different paradigms in addition to that of the Austrians. The new institutionalism draws on many traditions (some of which are quite different from the Austrian tradition), it is willing to experiment, it is iconoclastic towards the prohibitions of the past Austrian masters, and it is not self-consciously methodological. Given the past history of the Austrian movement (and that is

[2] Praxeology, which states that a social science can be built upwards from the purposeful actions of individual agents, is a position that merits attention and discussion. It was Mises's attempt to insulate the basic axioms or starting points of praxeology from criticism with the claim that they are true *a priori* which turns his methodology into a justificational one.

an important qualification!), such behaviour is healthy. But most important, the specific techniques and paradigms they have drawn upon (behavioural economics, game theory, evolutionary theory, institutional studies) may well provide a much better set of tools than has heretofore been utilized for the explication of the workings of the market process. And as recent work by various post-Keynesians suggests (e.g. Earl, 1983; 1986; Hodgson, 1988), such tools may also have applications which extend well beyond the uses to which the Austrians would put them.

A final point about the Austrians, and their importance for post-Keynesian thought: it was pointed out earlier that Austrians and post-Keynesians share a common methodological enemy, mainstream neoclassical economics. Those who are familiar with Austrian criticisms of general equilibrium theory, or IS-LM analysis, or rational expectations models, or the standard theory of the firm, or any of a number of other neoclassical constructs, will experience feelings of intense *déjà vu* upon reading the critiques of their post-Keynesian compatriots. It is possible to exchange entire sections of, say, Kaldor's 'The irrelevance of equilibrium economics' (1972) with Lachmann's *The market as a process* (1986) with no loss of meaning. But when members of either group engage in a rousing session of axiomatics-bashing, or rational expectations-smashing, or general equilibrium-trashing, it is pretty clear that they are preaching to the choir. A much more informative battle could be fought between two proponents of the Shacklian kaleidic society, one who is convinced that such anarchism demands the guiding hand of government if unacceptable outcomes are to be avoided, the other who insists that no centralized decision-maker could for long intervene wisely in so dynamic a world. A careful and systematic comparison of their similarities and differences would clarify both the Austrian and the post-Keynesian positions. And the ensuing fight would be a contest worth watching.

The most interesting question which might be answered by such an encounter concerns the nature of the fundamental differences which separate the two groups. Is it primarily a disagreement about the effects of various policies, and hence an empirical matter (albeit one that current empirical techniques cannot adequately answer)? Or is it a matter of the weights that each would attach to the various goals of policy, like equity or liberty or efficiency, in which case it is a question of values? Or, finally, are there truly methodological and epistemological differences which separate them?

I will close this section with a provocative statement: it is my hunch that methodologies will ultimately be shown to be neutral with respect to policy outcomes. A priorism can be used in defence of markets or to attack them, and the same can be said for subjectivism, realism and the rest. If true, this hunch has important implications for methodological discourse. Arguments between groups would focus on empirical and value questions rather than

methodological ones. That would be a very different world from the present one.

IV Post-Keynesian methodology

a Diversity an apparent characteristic of post-Keynesian methodology. One problem that the Austrians were forced to confront that does not plague the post-Keynesian movement was the necessity to escape from the dominance of a single methodological paradigm. There is no Mises among the post-Keynesians. But neither is it true that there are a number of well-articulated, competing methodological positions. There have been, of course, a large number of methodological critiques of neoclassical economics. But only a few examples of positive methodological construction exist.

One of the more complete methodological tracts comes from the neo-Ricardian branch of post-Keynesian thought: it is Hollis and Nell's *Rational economic man* (1975). Before beginning our analysis of their argument, it must be mentioned that it is rather difficult for an outsider to understand *why* Sraffa and his followers are included in the post-Keynesian camp. If the popular myths about him are correct, Keynes preferred Malthus to Ricardo, and had little sympathy for the ideas of Marx. He produced a short period analysis in which demand was emphasized and expectations were important. Finally, Keynes can credibly be interpreted as a subjectivist, at least in certain areas of his thought. None of this fits in very well with the ideas of the neo-Ricardians. To be sure, Sraffa was a member of the famous Circus, and he did not like to refer to Marx very much. But these hardly seem sufficient reasons to call him a post-Keynesian. If labels are to have any meaning, they should refer to sets of ideas which are mutually compatible. This point will be taken up again shortly, but in reference to methodological rather than theoretical issues.

In any case, a good part of Hollis and Nell's book consists of a philosophical attack on logical positivism, though pragmatism is also briefly taken to task. In the authors' view, this attack is of crucial importance because positivism comprises the philosophical foundation of neoclassical economic theory. Two chapters are then devoted to an explanation and defence of an a priorist approach to economic methodology. In the final two chapters, it is argued that the unrealistic assumptions of neoclassical theory eliminate it as a candidate for a reconstructed *a priori* science of economics. The preferred theory is a classical-Marxian one, and the fundamental economic concept of the theory is production. Production is viewed as fundamental because the reproduction of the system must occur in order for any other human activity (such as, for example, choice, or exchange) to exist.

The opposite position is embraced by Alfred Eichner in his paper, 'Why economics is not yet a science' (1983). Reviewing what he terms 'the epistemological rules of science', Eichner claims that there are a series of tests

which all scientific theories must pass. These include a coherence test, a correspondence test, a comprehensiveness test and a parsimony test. He then examines six theoretical constructs drawn from neoclassical theory: indifference curves, isoquants, positively-sloped supply curves, marginal physical product curves, the IS-LM apparatus and the Phillips curve. All are shown to lack 'empirical validity'. Post-Keynesian theoretical constructs are then described which meet the criterion of empirical validity.

If one compares the two views described above, it is immediately apparent that *both* cannot be right: neoclassical theory cannot be found guilty of being both positivist and insufficiently positivist. That both positions cannot simultaneously be right does not imply that one or the other must be correct, however. I will argue that neither is right, and that post-Keynesians would do well to avoid them.

Hollis and Nell are right about one thing: positivism is a dead position in philosophy. Where they go wrong is in believing that neoclassical theory is built on positivist foundations. Their mistake is understandable. Neoclassicals like Samuelson and Friedman certainly talk like positivists. But they are not really positivists, as is shown in excruciating detail in *Beyond positivism*. One of the reasons that positivism was ultimately rejected by philosophers, after all, was that *no* science had followed or could follow its tenets. Economics is certainly no exception. Thus the critical goal of the authors was not reached, they did not succeed in destroying the philosophical underpinnings of neoclassical theory.

Turning to the constructive work undertaken in *Rational economic man*, the case that Hollis and Nell build for a priorism is very similar to the one provided by Mises. The assumptions of mainstream theory are shown to be unrealistic and hence unsuitable as starting points for an *a priori* science of economics. Alternative basic axioms are proposed which are realistic. There are a number of problems with such approaches. Intuitively plausible first axioms are capable of multiplication. The meaning of the term *a priori* is often unclear. Connections between first postulates and the conclusions of a theory are not always transparent. A priorism is noncritical and justificationalist. There is not space here to undertake a sustained critique of a priorist methodologies in economics. Some of the criticisms mentioned above are developed at greater length in Caldwell (1984).

The problem with Eichner's position is a simpler one. His description of 'the epistemological rules of science' is pretty crude: he sounds rather like the verificationist sort of straw man who critics of positivism are fond of creating in order to knock down. Nor does he demonstrate how post-Keynesian theory meets all of his empirical tests. But the real point is this. The basic complaint of Eichner is that neoclassical theory is *unrealistic*. The real world does not contain perfectly competitive firms with smooth isoquants operating at constant returns to scale. Consumers do not know all of the options facing them, they do not carefully equate marginal rates

of substitution with price ratios over all goods, they do not adjust smoothly and instantaneously to small variations in price and income. In the real world, decisions are made in historical time with incomplete information. Timeless, perfect information, static equilibrium models cannot capture the richness of economic reality.

These are perfectly legitimate complaints against neoclassical theory. One does *not* need to add anything to them, and especially not a lot of mumbo-jumbo about 'epistemological rules of science'. More important, there is a danger when one adds such excess baggage. A critic of Eichner could quite correctly challenge his vision of what constitutes 'real science'. Eichner would then be forced to fight a battle that had little to do with what is actually significant in his critique.

Recently some attention has been given to the methodological work of J.M. Keynes. As is noted in Lawson and Pesaran (1985), progress is now being made towards the goal of providing a thorough and systematic exposition of Keynes's methodology. The essays in the volume provide contrasting pictures of his contribution, though some of the conflicts in interpretation may be more apparent than real. Once the reconstruction of his position is completed an assessment can be undertaken.

Other post-Keynesians have written about methodology, but few have tried to develop a comprehensive methodological vision. The emphasis has been on the criticism of neoclassicism. When Robinson writes so forcefully about the importance of historical time, or when Kaldor insists that models must account for the stylized facts of economics, or when Shackle eloquently describes a kaleidic world full of novelty and surprise, it is clear that all have a target in mind (see e.g. Robinson, 1978a; 1978b; Kaldor, 1972; 1985; Shackle, 1973). More recently, post-Keynesians who favour an interdisciplinary approach to economics (Earl, 1983; 1986; 1988, draw on psychology, while Hodgson, 1988, advocates an institutional approach) also support their claims by examining the deficiencies of orthodox theory.

What is it about neoclassical economics that so many find so aggravating? Though plainly matters are not so simple, it *could* be argued that a single methodological point underlies all of these complaints. Neoclassical economic theory contains a number of abstractions which are dictated by the mathematical form in which the theory is expressed. These simplifications distort crucial aspects of our picture of economic reality. Such distortions cause us to misunderstand the actual nature of economic reality, and perhaps more important, they lead us to commit errors in the formulation of economic policy. The implicit methodological claim in all of this is that *theories should accurately represent economic reality*. In the final section of this paper, we will return to this claim, examine under what circumstances it makes sense, and see how effective it is as a criticism of neoclassical economics.

But another topic must be taken up first. It was argued above that at

present there appears to be considerable methodological diversity in the post-Keynesian camp. But a closer look suggests that some of this diversity may be illusory (since many post-Keynesians agree that neoclassical theory should be replaced by a more realistic theory) and that some of it might be eliminated (if a priorism is rejected and the empiricist window-dressing in Eichner's article is ignored).

Some might respond that the elimination of methodological diversity is not necessarily in the interests of post-Keynesians. Since this topic has been discussed recently in the post-Keynesian literature, it is appropriate to examine briefly the arguments on each side. What are the advantages and disadvantages associated with a plurality of methods?[3]

b Advantages of methodological diversity. The absence of a central methodological paradigm helps to explain why it is so difficult to say exactly who should be considered as a member of the post-Keynesian community. This is not without its advantages. Those who might otherwise be called Marxists, or market socialists, or institutionalists, or welfare statists, or psychological economists can all be dubbed post-Keynesians. There is undeniably comfort, as well as strength, in numbers. Another advantage is that such diversity might promote a general attitude of tolerance among post-Keynesians, at least towards one another. Of course, tolerance is not always a virtue. But often enough it is, so that the attempt to cultivate tolerance is usually a worthwhile exercise. Finally, methodological diversity makes very good sense in the post positivist age. Why commit to a single methodology when philosophers cannot agree among themselves about what an optimal one should look like?

Such arguments provide a foundation for Sheila Dow's (1985) contention that the eclectic 'Babylonian mode of thought' which is characteristic of the post-Keynesians is superior to the 'Cartesian-Euclidean mode of thought' of neoclassical economics. Similar sentiments are to be found in a recent essay by Hamouda and Harcourt, which carries the significant title, 'Post-Keynesianism: from criticism to coherence?' Their position on the issue of diversity is stated clearly in the conclusion of the paper:

> We subtitled this reflective survey 'from criticism to coherence', deliberately ending with a question mark. What we have tried to show is that within the various strands that we have discerned and described, there *are* coherent frameworks and approaches to be found, though obviously there remain within each unfinished business and unresolved puzzles. The real difficulty arises when attempts are made to synthesize the strands in order to see whether a coherent whole emerges. Our own view is that this is a misplaced exercise, that to attempt to do so is mainly to search for what Joan Robinson called 'only another box of tricks' to replace the 'complete theory' of mainstream economics which all strands reject (Hamouda and Harcourt, 1988: 24–25).

[3] Some might think that a critical pluralist would automatically endorse a plurality of methods within a research tradition. But this is a mistake. Recall that critical pluralism is a *meta-methodological* position, that pluralism among methodologies is an *instrumental* good, and that *criticism* is an essential element of critical pluralism.

c Disadvantages of methodological diversity. There are at least three disadvantages associated with the methodological diversity which presently characterizes the post-Keynesian movement.

The first has already been noted: some of the methodological positions embraced by post-Keynesians are inconsistent with others. If one believes that Eichner is right about making economics a science, then Shackle's vision of a kaleidic world undercuts the hope that Eichner will be successful. If Hollis and Nell are correct in choosing rationalism over empiricism, then post-Keynesians who use empirical methods are guilty of the same crimes as those committed by the 'positivist' neoclassical economists. This is *not* to say that all of the methodological approaches identified above are mutually exclusive. Some are clearly complementary: one can easily imagine a methodological position which endorses the incorporation of insights from both psychology and sociology, for example. What cannot be accepted is the mingling of two positions (such as empiricism and a priorism) when each of them assumes from the outset that the other is false. One does not need a truth table to see that such a conjunction cannot succeed.

The second reason why post-Keynesians should consider eliminating some of their methodological heterogeneity has to do with what evolutionary epistemologists characterize as the provision of an optimally critical environment. The point can be made simply: *diversity, which is just another term for pluralism, leads to growth only if it is accompanied by criticism*. The point, though simple, is a very important one. There is nothing *wrong* with the current methodological diversity which characterizes post-Keynesian economics. Indeed, such variety is to be expected in a movement that is of comparatively recent origin and which has felt the influence of so many diverse thinkers. Furthermore, the example of the Austrians shows the hazards of too quickly embracing a single methodology. But there is a danger facing the post-Keynesian movement, one that is quite different from the one encountered by the Austrians. The danger is not that a single methodology will dominate, but that post-Keynesians will come to view any and all methodological positions as acceptable. There is a world of difference between recognizing that a new movement will exhibit methodological variety and claiming that such a state of affairs is the *preferred* one. Post-Keynesians need not agonize over the diversity in their camp, but neither should they be complacent about it. Just as Austrian thought began to grow when Mises was finally criticized, the criticism of certain positions within the post-Keynesian fold may help to facilitate growth.

There is an important third reason, one which is partly tactical and partly epistemologically substantive, for post-Keynesians to attempt to reduce their methodological heterogeneity. The glue that binds together post-Keynesians of every stripe is their opposition to neoclassical economic theory. But if one

wishes to launch an effective attack against the mainstream, methodological variety is a hindrance. The tactical aspect of this point should be obvious. If one is trying to convince an opponent that his methodology is wrong, one must have a replacement in hand. Thomas Kuhn was right when he noted that paradigm change will not occur unless there is a new paradigm to change to. The same insight applies to methodology.

The epistemological reason is even more significant. Without wishing to put too sharp a point on it, it nonetheless must be said that one often gets the impression when reading post-Keynesian critics of orthodox economics that there is *nothing* of value in neoclassical economics. All of it is wrong. Such a conclusion is more likely to be reached if there are many different theories and methodologies labelled 'post-Keynesian', and only one opponent labelled 'neoclassical theory'.

It may be psychologically comforting to think that nothing that one's opponent believes is true. But the disadvantages of holding such a view are enormous. First of all, it is wrong. Next, it causes one to caricature the views of, and thereby to underestimate, one's adversary. It also eliminates the possibility of learning from the enemy. Furthermore, it is reactive: one's position is forever defined as the opposite of the opponent's position. If, on the other hand, post-Keynesians were to decide on a particular methodological approach, both their criticisms of neoclassicism and their portrayal of a preferred alternative would be sharpened. It would be clear exactly what was to be rejected in mainstream thought; why it was being rejected; and why its replacement was better. There might even be some progress made towards that most difficult of all goals, enhanced communication across paradigmatic barriers.

V Which methodology should the post-Keynesians choose?

It is easy enough to argue in general terms about the benefits to be derived from a reduction in methodological heterogeneity. It is quite something else to choose a specific methodology. A fitting conclusion to this paper, then, is to map out a plan of action for post-Keynesians to consider.

a The goals of science. We can begin by recalling the time-worn but nonetheless useful dichotomy which states that, at the most general level, there are two goals of economic science: the goal of explanation and the goal of prediction. It is usually the case that those who emphasize the importance of the explanatory goal of economics also desire that their theories be realistic.[4] It was argued earlier that if one examines the complaints of people like Robinson, Kaldor, Shackle and (with proper modification) Eichner, as well

[4] The term realism is notoriously difficult to define. Some progress towards its clarification has been made recently in Mäki (1988). If we adopt Mäki's terminology, 'realisticness' rather than 'realism' is what is being discussed in the text.

as the arguments of more recent contributors like Earl and Hodgson, all of them agree that the chief methodological sin of neoclassical theory is that it is unrealistic in its portrayal of economic reality. The preferred theory is a more realistic one. Thus, it would seem that post-Keynesians value explanation in economics more than they do prediction.

Realism is much less important to economists who are mostly concerned with the predictive adequacy of their theories. As is well-known, Milton Friedman (1953) provided the canonical instrumentalist defence of predictively adequate but descriptively unrealistic theories. The genius of his defence, of course, was to link instrumentalism to a particular theory in economics, neoclassicism.

It may therefore seem that the natural division between post-Keynesians and neoclassicals is along the lines of explanation versus prediction. If this were the case, post-Keynesians would pursue the following methodological strategy. First, it would be claimed that explanation is the more important goal of science. To buttress the argument, it would further be claimed that prediction is difficult if not impossible in the social sciences, so that the pursuit of explanation makes even more sense. Certain post-Keynesians (see e.g. some of the essays in Eichner, 1983) have already begun making these sorts of arguments.

I do not think that this is a very useful approach. Indeed, there are a number of reasons why post-Keynesians should avoid it. To show this, we will examine the two goals of science in greater detail.

b The goal of explanation. First of all, there is little reason to expect that neoclassical economists will so easily surrender the explanatory goal to the post-Keynesians. There are some powerful arguments on their side.

As has been noted by Backhouse (1988), neoclassicals in recent models have paid more attention to specific 'real world' problems like signalling, credit rationing and the provision of insurance. More generally, a host of theoretical advances (like the incorporation of costly information, transactions costs, imperfect competition or game theoretical considerations into theories) could all be interpreted as attempts to make the assumptions of mainstream theory more realistic. Thus, recent developments in theory could be cited in the defence of an explanatory role for neoclassical economics.

There is a massive literature in the philosophy of social science concerning what constitutes a legitimate social scientific explanation.[5] One important strand of thought, the Popperian, insists that most (if not all) legitimate explanations of social phenomena must employ some form or another of the rationality principle: investigators should attribute a means-ends framework and purposefulness to the agents under study. Whatever one may think about the merits of this approach, two things are clear. Among all the competing

[5] For an excellent survey applied to the field of social history, see Lloyd (1986). Coats (1988) relates this literature to the methodological literature in economics.

research programmes in economics, neoclassicism alone has staked out the use of the rationality principle as its defining characteristic. And secondly, despite all the controversy over its logical status, the rationality principle is one of the most powerful and fruitful heuristic devices available to analysts of social phenomena. It provides the starting point for both mainstream economics and behavioural psychology. Thus neoclassicals could make the claim that, *because* of their single-minded use of the rationality principle (albeit a very restricted form of it), their programme is the only tradition in economics which fulfills the explanatory function.

Finally, even if neoclassical economists abandoned all claims to explanation, the battlefield would not necessarily belong to the post-Keynesians. Recall that Austrians, Marxists, institutionalists and behavioural economists all claim to be offering more realistic descriptions of economic phenomena than is available from the orthodoxy. Winning a victory against the mainstream would be only the first stage of a very length campaign.

Let us imagine, however, that the post-Keynesians are ultimately successful. Even if such an unlikely outcome was realized, neoclassical economics could still claim a place at the table of science. Acknowledging the strength of post-Keynesians at explanation, neoclassicals could still claim for themselves the role of prediction.

c *The goal of prediction*. Most post-Keynesian critics are familiar with Friedman's predictivist defence of mainstream economics, but few recognize how powerful it is. Friedman's little article captures perfectly the attitude of the everyday, working microeconomist. Such economists know that people don't really have indifference curves, that the theory of the firm is a caricature, that the marginal productivity theory of distribution is false. Of course, the use of such artificial constructs would be unwarranted if the goal of science was to render accurate descriptions of phenomena. But their use can be justified if a major goal of science is prediction. And neoclassical theory *does* yield some pretty accurate predictions, at least in certain cases. At the most general level, economic theory states that agents will respond to changes in perceived benefits and costs: that is just the rationality principle expressed in economic terms. But there are more specific and unobvious predictions. Price-fixing leads to gluts or shortages. Demand responses to a price change are more elastic the more time that is allowed to pass. Price dispersion is more likely in markets in which information is imperfect. Such predictions are not obvious to noneconomists; they are relevant for policy; and they work often enough to be very useful. On the other hand, most neoclassicals recognize that adequate prediction is not attainable in certain economic situations. Share prices on the stock market are not viewed as predictable, and 'financial consultants' who get paid for their claims to the contrary are considered to be charlatans. (Rich charlatans, perhaps, but charlatans nonetheless.) Those who pretend to be able to predict turning points in the business cycle are similarly viewed with disdain. Indeed,

macroeconomists are not held in high esteem: too little of what they do is based on 'real' economics.

If one accepts the position just outlined, it is very difficult to make any sense out of the sorts of criticisms offered by the post-Keynesians. The post-Keynesian demonstrates that many assumptions of mainstream theory are unrealistic. The neoclassical readily agrees, but wonders why this is important: the goal of science is to make accurate predictions and thereby to assist in the making of policy. The post-Keynesian shows that general equilibrium theory is both unrealistic *and* yields false predictions. The neoclassical vigorously nods his assent: being of a more applied bent, he has always thought that general equilibrium theory is a waste of time. He points out that Milton Friedman, who criticized attempts to turn economics into a branch of applied mathematics, thinks so, too (1953: 11–12). The post-Keynesian claims, on the basis of examples like the stock market or the business cycle, that prediction is impossible in economics. The neoclassical is surprised that the post-Keynesian would make such a mistake in inference. No neoclassical worth his salt would claim to be able to predict such phenomena (though the problem with the business cycle, he chuckles, is really an indictment of macroeconomics). In fact, there exist some good neoclassical arguments which explain why such prediction is so difficult (see e.g. McCloskey, 1987). But other predictions, ones which are extremely important for the formulation of policy, are made by neoclassicals all the time.

One does not have to be an ideologue to hold such views. Many economists who fall squarely within the neoclassical camp do not worship at the alter of *laissez-faire*. The question is not *whether* to intervene, for there exist clear cases of market failure for which intervention is necessary. The question is *how* to intervene in the least costly way. (Thus most neoclassicals agree that if one wishes to redistribute income, it is better to do so through the tax system than through the fixing of prices. It must be remembered that Milton Friedman was one of the developers of the negative income tax!) Indeed, it is hard to imagine how one could ever hope that interventionist policy could succeed unless one also believed that we can have at least some success at predicting the outcomes of alternative policies.

This last point was not lost on the Austrians, who *do* worship at the alter. As long ago as the 1930s, they associated terms like prediction, rational planning and positivist science with the left. One can trace the story in the methodological writings of Hayek. Lange had used a general equilibrium system to refute the Misesian claim that rational planning under socialism was impossible. Hayek's response was to argue that a perfect information general equilibrium system obscured the fundamental problem of the social sciences, the coordination problem. Socialists claimed that a rational science of society was within reach. Hayek identified such ideas with scientism, and wrote scathingly about the hubris of the planners of L'Ecole Polytechnique

(Hayek, 1952). The scene, it would seem, was set. The socialists were the rational, positivist, scientific planners. Much of the thought of the Austrians in this period was simply a reaction against social planning. Interestingly enough, the Austrians still managed to come up with some pretty good ideas. But their general methodological position was one which emphasized the limits of a science of society.

It was Friedman, of course, who turned the methodological tables around by claiming the mantle of science for neoclassical theory. It was a brilliant move, and it succeeded marvellously. What better evidence is there for this than the fact that Marxists, institutionalists, Austrians and post-Keynesians all end up sounding alike when they start talking about neoclassical methodology?

d A new direction for post-Keynesian methodology? If one accepts the explanation-prediction dichotomy for describing the goals of science, it appears that at present the post-Keynesians are aligned with other groups who endorse explanation, while the neoclassicals (at least those who follow the methodology of Friedman) come down on the side of prediction. It was argued that this is an unenviable position for the post-Keynesians. Neoclassical economics may well have a legitimate explanatory role to play. Even if it does not, the post-Keynesians have other competitors for the part. Finally, even were the post-Keynesians to emerge from the fray possessing the explanatory mantle, the neoclassicals have at present an uncontested claim out on the predictivist role.

What are the implications of all of this for post-Keynesian methodology? I offer the following thoughts, with the hopes that they will stimulate discussion and debate.

1 Post-Keynesians should continue to develop descriptively realistic models of economic phenomena. Continued use should be made of innovations in behavioural economics, institutional studies, game theory, and other areas of thought. But the new models should *not* be assessed according to how well they compare with the admittedly unrealistic models of neoclassicism. Rather, they should be evaluated against the alternative and equally realistic models of other economic traditions. By now it should be obvious that I consider the Austrians to be perfectly suited as adversaries.

2 In addition, post-Keynesians should consider adding the methodological weapons of their enemy, neoclassicism, to their own arsenal. Friedman's brilliant methodological turnabout is capable of duplication. The rationality hypothesis, when properly used, is one of the most powerful tools available to social theorists. Its limitations are as interesting as its applications, and finding out what each are could keep many busy for a long time. And its examination and use does *not* commit one to a belief in the superiority of the market system. The same things can be said about econometric modelling. Post-Keynesians are in an empirical tradition, as Kaldor's frequent references to stylized facts aptly illustrate. Econometrics is a tool, and tools do not have ideologies. One suspects it is a tool that could be used to good purpose by post-Keynesians.

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