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Econometrics, Realism and Policy in Post Keynesian Economics

William Mitchell[1]

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Centre of Full Employment and Equity
The University of Newcastle, Callaghan NSW 2308, Australia
Home Page: http://e1.newcastle.edu.au/coffee
Email: coffee@newcastle.edu.au

1.0 Introduction

While not disagreeing with the importance of fundamental uncertainty as a means of distinguishing orthodox economics from a progressive heterodox paradigm, this paper argues that econometric research and practice is a necessary part of a paradigm which ultimately emphasises policy intervention. Some Post Keynesian economists have extended the fundamental uncertainty concept to develop a realist perspective of economic thought (see Lawson, 1989a, 1989b). This form of realism is antagonistic to econometric modelling, and echoes Keynes's criticisms of Tinbergen in the late 1930s. Keynes used his idea of the future being unstable and uncalculable to refute the validity of Tinbergen's early modelling. Chapter 12 of the General Theory and the 1937 Quarterly Journal of Economics articles are offered as evidence of Keynes position on the role of uncertainty. He regarded the future as being unable to be analyses in probabilistic terms because of the infinite nature of the options. Yet, as Coddington (1982: 485) pointed out

> In doing so, however, these commentators may be taken to task for omitting to read the first half of the article in its proper context: namely, the second half. For having completed his brief essay on the incalculability of things in general, Keynes goes on to perform some comparative statics exercises on the assumption that consumers' aggregate expenditure is a stable function of disposable income ... (In the context of Keynes' policy proposals, it is also required that this stability be maintained in the face of arbitrary time paths for fiscal and monetary variables). All this requires that consumers' expenditure is calculable on the basis of their current disposable incomes and that, therefore, their saving behaviour, by subtraction, is also calculable on the same basis. Accordingly, those who have pointed to this article as an interpretative key, owe us, at the very least, an explanation of why, immediately after having provided his key proposition, Keynes engages in analysis in flagrant contradiction with it.

In this paper, three themes are developed. First, we critically examine the applicability of the emergence of realist thought in Post Keynesian economics which attempts to detail how explanation can proceed despite the existence of fundamental uncertainty. A tension exists between the realists who advocate explanation and econometricians who add prediction to the goals of the Post Keynesian theory and praxis. This is in the context of a perceived need to treat the future in deterministic terms to allow for aggregate policy interventions. This tension constitutes our second theme. Finally, without negating the existence of fundamental uncertainty the paper argues that the demands of policy can be accommodated and the utility of econometrics justified within a satisficing environment where rules of thumb and habits create a stable context for decision making.

The literal application of uncertainty introduces a nihilism, which would appear to be at odds with an interventionist policy emphasis. If we 'simply do not know' (Shackle, 1973: 516) what influences decisions about the future, then we can do neither econometrics nor economics. This paper discusses why we can and should do both. There must be at least a form of determinism for interventionist policy to be a viable option. The determinism is to be found in the habits and social uniformities which exist in human society. It is argued that coherent econometric analysis is essential for sound policy development, design, and implementation. Once it is established that

decision makers limit the set of events that they are prepared to consider in the future, much of the operational significance of fundamental uncertainty is lost. Further, it is argued that decision makers have broad goals which are not confined to profit maximisation. In that sense, the point estimates of the future that are generated by econometric models are less relevant than the confidence intervals that accompany them.

The outline of this paper is as follows. Section 2 considers the dilemma facing Post Keynesians who wish to advocate an activist policy agenda but who are trapped in rigid notions of uncertainty. Section 3 considers the concept of realism and explores the limitations in the way that it is represented by some progressive economists. Section 4 examines the flaws in the realist argument against the practice of econometrics. Section 5 considers the role of prediction and its limitations. Concluding remarks follow.

2.0 Uncertainty and the policy dilemma for Keynesians

Davidson (1991) uses the distinction between ergodic and non-ergodic processes to demarcate measurable and unmeasurable uncertainty. Later Davidson (1994: 90) says 'if a stochastic process is ergodic, then for an infinite realisation, the time and space averages will coincide … In an ergodic environment … *The future is merely the statistical reflection of the past*' (emphasis in original). However, in the real world, economic processes are not ergodic and according to Davidson (1994: 91) this means that "if people believe that the economic environment is uncertain (nonergodic) then it is perfectly sensible for them to disregard past and present market signals. The future is not statistically calculable from past data and therefore is uncertain.'

In the *General Theory,* Keynes (1936: 148-160) emphasises the difference between probability and uncertainty, which marked a significant departure from the accepted view of risk. Uncertainty is deemed important because economies operate in historical rather than logical time and resource commitments must be made without full knowledge of the realisations that the decisions were made in relation to.

Davidson (1994: 89) says an uncertain environment occurs when

> The decision maker believes that during the lapse of calendar time between the moment of choice and the date(s) of payoff, unforeseeable changes can occur. In other words, the decision maker believes the reliable information regarding future prospects does not exist today. The future is not calculable, even if the decision maker competent to perform the mathematical operations necessary to calculate probabilities of conditional events given the necessary information. This is uncertainty (or ignorance about future consequences) in the sense of Keynes and the Post Keynesians.

Where does this lead us? Coddington (1982: 485-487) outlines two ways in which those who have been influenced by the notion of fundamental uncertain attributed to Keynes have developed the theme.

> The first group, the post-Keynesians, are interested in uncertainty insofar as it helps to show that, under capitalist institutions the decentralisation of production and investment decisions leads inevitably to chaos and waste ... uncertainty and subjectivism in themselves have no bearing on the institution of private property ... these ideas are as applicable just as readily to members of parliament, Ministers and their officials within the civil service as to

> entrepreneurs within the private sector of the economy ... The post-Keynesians are quite happy to make appeals to uncertainty insofar as this enables them to drive a wedge between behaviour and circumstances in some cases; but if the wedge were to become comprehensive, they would be left with no theory at all, all behaviour would appear equally capricious and unintelligible.

As to the second group, Coddington (1982: 486) asserts that they have been 'consistent but analytically nihilistic.'[2] He includes Shackle (1967, 1972, 1973, and 1974) and Loasby (1976) among this group. Shackle's nihilism sees human decision-making and action as a product of individual imagination and expectations. Thus, we can do little to formalise the basis of future action, for these individual propensities, according to Shackle (1973: 516), 'do not rest on anything solid, determinable, demonstrable. "We simply do not know."' Hodgson (1989: 103) concludes that while the Austrians (presumable minus Von Mises) believed we might be able to employ psychology to explain the formation of expectations and decisions, '... both Shacklean and mainstream Austrian theory is incapable of building a model of the economy with a sufficient degree of order and regularity, and as a result cannot generate predictions concerning the future.'[3]

What implications does this have for practising economics and econometrics? It is clear that the future is an infinite set. We cannot know all of the possible outcomes to order them in any meaningful way and therefore we cannot construct probability density functions. In that sense, if econometric analysis is invalid because of endemic uncertainty, so is theoretical explanation, and, *pari passu*, so is the economics profession. However, it is not clear that the constraint of uncertainty operates in a way that either of these groups suggests. While we are unable to do what we would ideally want to do (find truth), this is not equivalent to being hog-tied into inactivity. In terms of theoretical endeavour, we must initially recognise that people and institutions do establish order in their activities. They do make decisions about the unknown future, and they do use information to formulate future actions and anticipations of the actions of others.

It is futile to pursue the true algorithms that these agents employ to transcend time. Even if we chanced upon the true process we would have no way of knowing that it was the truth. But while we are fallible, we can still aim to increase our understanding of these processes by continued searching, devising and formulation of propositions.

Palley (1993: 6) raises some interesting propositions in this regard. He traces Davidson's (1991) argument that under uncertainty (within a non-ergodic environment), individuals are denied 'the possible relevance of any form of probabilistic based approach to decision making.' But Palley (1993: 7) argues that the subjective development of probability estimates (emphasised in the Friedman-Savage approach) is 'itself a product of social construction.' So individuals become conditioned by educators who use decision making techniques which assume an ergodic world and thus put bounds on their set of possible outcomes and generate finite futures upon which they make decisions. Palley (1993: 7) says that

> It doesn't matter that there may be no objective grounds for forming estimates of probabilities, and that these estimates are pure constructions ... In this case, despite the fact that the world may actually be characterised by non-ergodic processes, the social construction of decision making may be such that the reasoning associated with actual decision-making processes may be of the type

> one would associate with an ergodic world. The world may be Davidsonian, yet its external appearance can take on a Friedman-Savage character.

While the set of possible outcomes in the future may be infinite, individuals and institutions modify this to make their decisions tractable. Through social conditioning they deliberately limit the available outcomes to a smaller set which are finite, tractable and in the minds of the individuals and/or institutions, probabilistic. Part of this response to uncertainty also occurs in the type of decision making criteria that individuals employ.

Lavoie (1992: 55) says that individuals facing uncertainty have 'limited computational abilities' will adopt satisficing strategies which 'consist of means to avoid complex calculations and considerations, and of procedures enabling decisions to be taken despite inaccurate information. Some of these procedures are conscious – we may then speak of rules – while others are unconscious – we may refer to them as habits.' Later Lavoie (1992: 56) says that 'rules allow individuals or institutions to take decisions without having to consider or reconsider all of the available information.'

While Post Keynesians tend to analyse the individual response to uncertainty (procedural rationality), important structures and institutions exist at the more aggregates levels which allow the economy to adjust to the flux and uncertainty of economic behaviour. Keynes, like the neoclassical paradigm, focused essentially on the individual. Segmented labour markets are an example of the way capitalist institutions have developed to cope with the existence of fluctuations that arise due to uncertainty. It used to be thought that the solution to the poverty and underemployment embodied in the secondary labour markets was to dismantle them through public sector employment schemes and increased educational participation by disadvantaged groups (Doeringer and Piore, 1971). However, as economists achieved greater understandings of the functional relationship between primary and secondary labour markets it became clear that the security enjoyed by the workers in the primary labour market was "paid for" by the insecurity of the secondary labour market workers. The secondary market was unstable and of less rigid structure because it was the mechanism through which capitalists adjusted their activities to meet the uncertain variations in aggregate demand.

Fundamental uncertainty only prevents us from ever discovering, knowingly, the truth and therefore the best solution. People and institutions thus search for adequate solutions to their problems. In the institutionalist literature this has been called satisficing. When we discuss satisficing the role of rules of thumb and habits emerges (see Hey, 1982; Hodgson, 1988).

Coddington (1982: 482) argues that:

> For what is required within the Keynes' scheme is not the uncertainty, as such, surrounding private sector investment decisions; it is the wayward and unruly behaviour of the aggregates resulting from the decisions taken in the face of this uncertainty. Indeed, Keynes' system requires private sector investment to display this unruliness in two quite distinct senses: first, when compared with private sector *consumer* expenditure (this is required for Keynes' model to work); and, second, when compared with sector investment expenditure (this is required in order for Keynes' policies to work (emphasis in original).

The question that must be answered by those who wish to advance uncertainty as a guiding concept for Post Keynesian thought and use it to negate the validity of econometric analysis is in what way does uncertainty impact differently on private sector investors, private sector consumers and public sector investment and consumption decisions.

It is not the existence of uncertainty which is important, but rather the ways in which people in the economy deal with its existence. Coddington (1982: 482) concludes that 'the *fact* of uncertainty does not, of itself, establish the conclusion concerning the wayward and unruly behaviour of particular macroeconomic variables' (emphasis in original). He gives an example of how basic decision rules in an environment of uncertainty 'might result in greater stability than would result from sophisticated calculations based on epistemologically privileged beliefs or an uncanny degree of foresight' (p.482).

3.0 Realism

3.1 Realism and instrumentalism

Many Post Keynesian writings on methodology (for example, Caldwell, 1989) challenge neoclassical economics on the basis that theories should represent reality.[4] At the same time, many Post Keynesian economists consider that the existence of fundamental uncertainty constrains what we can say and do about the future. One possible solution to this dilemma is to propose a realist approach to economic analysis. What is realism? Lawson (1989a: 237-238) says that realism, in contradistinction to instrumentalism, is an assertion of 'the existence of the objects of analysis independent of the enquiry in which they are objects.' Lawson (1989b: 60) proposes realism as 'a serious alternative to the current orthodox approaches to economic analysis' because it does 'not suffer from certain of the latter's more obvious limitations.'

While the realist approach is plausible, as long as it is accompanied by some fallibilistic view, the appropriateness to economic theorising is problematic. In this section, realism (as enunciated by Lawson following Bhaskar, 1978) is rejected as a useful basis for Post Keynesian research. Specifically, Bhaskar's conception is developed within the context of natural science where experimental production and control is central to locating causal laws of natural generating mechanisms.

At the outset, a distinction between philosophic and scientific activity must be made. Philosophically, a realist says that real things and generative mechanisms must exist and act. Bhaskar (1978: 52) considers that science assumes the task, through actual experimentation, of discovering 'what the real mechanisms are.' Philosophical endeavour cannot accomplish the identification of specific real mechanisms.

In philosophical terms, realism aims to learn about mechanisms which produce phenomena. A realist ontology makes a distinction between reality and sequences of events. Bhaskar (1978: 17) says that these mechanisms

> ... endure and act quite independently of men. The statements that describe their operations, which may be termed "laws", are not statements about experiences ... or statements about events. Rather they are statements about the ways things act in the world ... and would act in a world without men, where there would be no experiences and few, if any, constant conjunctions of events.

In other words, these mechanisms would exist independent of human existence and these 'things would still act, be subject to laws and preserve their identity through certain changes' (Bhaskar, 1978: 48).

Realists criticise empiricism for, allegedly, seeing causal laws in terms of a constant conjunction of events. According to Bhaskar (1978: 70) the latter term means that 'an event of type *a* is invariably accompanied by an event of type *b*.' But, a realist claims that causal laws continue to operate under normal conditions 'which may be characterised as open, where no constant conjunction or regular sequence of events is forthcoming' (Bhaskar, 1978: 48).

Lawson (1989a, 1989b) argues that it is this distinction (laws and events, closed and open) which distinguishes realism from instrumentalism, and hence valid economic endeavour from, say, econometric modelling. According to Lawson (1989a: 239) instrumentalists (taken to be econometricians) deem a model to be adequate if the 'empirical "data" are merely "as if" they had been generated in accordance with it.' The difference between realists and instrumentalists according to Lawson lies not in their assumptions about 'some object of knowledge to be ... fixed or enduring' (p.239), but rather, in what each assumes to be constant.

Instrumentalists, allegedly, assume the relation between events are stable, whereas a realist believes, according to Lawson (1989a: 239) that 'it is real things and their powers or ways of acting' that are assumed enduring. In other words, while events may not endure in any stable fashion, causal laws underlying the generative mechanism endures and act in its characteristic way. For a realist, the constancy of events and the enduring action of causal laws is not equivalent 'because the action of any causal mechanism will, in general be offset by the counterveiling actions of juxtaposed mechanisms' (Lawson, 1989a: 240).

For Bhaskar, the conceptualisation of causal laws is clearly non-empirical, in that things exist and act outside of the empirical conditions through which we identify them.[5] Laws, however, cannot be the regularities that constitute their empirical grounds. In general, when a closed (experimental) system is absent, events represent a juxtaposition of causal laws. Yet, as the causal laws are independent of the events, a realist asserts that they continue to operate in open systems, and that a 'rational explanation of phenomena occurring in such systems becomes possible' (Bhaskar, 1978: 34).

3.2 Notion of a tendency

It is here that the notion of a tendency arises. Causal powers behave through generative mechanisms. There is a subtle distinction between powers and tendencies. Bhaskar (1978: 50) says that 'whereas powers are potentialities which may or may not be exercised, tendencies are potentialities which may be exercised or as it were "in play" without being realised or manifest in any particular outcome.' By focusing on these enduring powers which may be unrealised, a realist believes we can explain world phenomena. Indeed, Lawson (1989a: 240-241) argues that in an open system 'a realist can still explain an even predict the operation of identified tendencies (although the juxtaposition of different causal mechanisms may prevent the prediction of actual events).' Lawson uses a "leaf" analogy to explicate his conception of realism. The constant intrinsic nature of a leaf leads to the tendency that it falls to the ground upon dislodgement from a branch.[6]

A major problem with this analogy is that Lawson fails to address how human society is alike or unlike the leaf from the natural world. This lack of mapping in realism from an experimental physical world to non-experimental human society is its major drawback in terms of applicability for Post Keynesian research. Lawson sees no problem here and recognised that economics is a 'non-experimental science' (1989a: 243). He concludes that 'the realist may just rest content with the ability to explain and predict the tendencies of identified causal structures' (Lawson, 1989a: 243).

Can we therefore be content with saying a leaf has a tendency to fall to the ground?[7] Is policy advice going to be limited to us saying that income has a tendency to rise if government increases spending but may not if juxtaposed causal mechanisms which we cannot predict are at work? Bhaskar clearly identifies the difficulty of this position for a realist. Bhaskar (1978: 4) asks whether the statement 'that a generative mechanism endures and acts in its characteristic way mean anything more than to say that a thing goes on acting in a certain way?'

Bhaskar clearly considers that causal laws can only be identified in an experimental context. Experimental activity has two dimensions; experimental production (where a scientist gets the mechanism to work), and experimental control (where the influence of juxtaposed mechanisms are nullified). Bhaskar (1978: 53) argues that only

> if the mechanism is active and the system in which it operates is closed can scientists in general record a unique relationship between the antecedent and consequent of a law-like statement. The aim of an experiment is to get a single mechanism going in isolation and record its effects.

So experimental production and control ensures a closed system where a constant conjunction of events is possible. From this we can derive law-like statements about the tendency of a generative mechanism, which may not be fulfilled in an open system if offsetting causes are also operating.

3.3 Expermentation and social science

Experimentation is the third (and crucial) stage in realist science. Empirical testing (experimentation) allows a realist to move from imagined (conjectured) understanding of a generative mechanism to a real comprehension.[8] The philosophical argument establishes the ontology - that some real things and generative mechanisms must exist - scientific endeavour attempts to discover which ones actually do.[9]

The distinction in realism between causal laws and patterns of events (the independence of real and actual), requires that experimental production and control is possible. Bhaskar (1978: 244) concludes that '... it is clear that experimental activity is impossible in the social sciences ...' If experimental activity in natural science provides access to enduring and active structures of generative mechanisms and it is only under closed conditions that confirmation and falsification of theory is possible, then for social sciences we have to devise 'an analogous procedure of inquiry' (Bhaskar, 1978: 245). What analogous procedures in the social sciences would allow controlled investigation and provide a basis for identifying the causal laws and their tendencies - the realist understandings? There is clearly a basic problem of confirmation and falsification in non-experimental sciences.

But Bhaskar (1978: 245) effectively reduces realist social science to the vacuous when he concludes that

> social structures, unlike natural structures, cannot exist independent of their effects. Thus real definitions of concepts such as capitalism, democracy, power, love can only be justified by their capacity to render intelligible a certain domain of phenomena. I suggest that they are falsified by their incapacity to explain in a non-ad-hoc way a range of phenomena that takes on a special significance for the agents that participate in the forms of social life they define.

So we have to test our theories at the phenomena level - the level of events. We have to appraise theory in terms of open systems. The idea of real and actual domains is now blurred. What are the enduring causal mechanisms in human society? We simply cannot operate in realist terms if we can never produce and control the actions of the causal mechanisms?

Lawson provides little insight into how a realist might conduct scientific enquiry into determining constant intrinsic structures (causal laws) in a social science setting. He says (1989a: 239) that while an instrumentalist assumes the 'conjunction of events' to be constant, the realist assumes it 'is a relation between the conditions which activate a causal structure and its way of acting' which is assumed constant. The distinction is simply untenable in a non-experimental setting. Lawson (1989a: 239) says that 'once we understand the nature of private enterprises, trade unions, multinationals and copper, for example, then we can deduce their respective powers or dispositions to seek profits, defend conditions of workers, operate in different national markets and conduct electricity well.'

To make this point, Lawson (1989b: 61) identifies three conceptual stages of a research process. First, we identify an interesting empirical phenomena. Second, we construct an explanation (Bhaskar's imagined model). Third, we start 'subjecting the entities postulated at the "modelling" or "explanatory" stage to further/continuous scrutiny.' Bhaskar considers the third stage in Popperian terms - the empirical testing stage. The aim is to understand the constant intrinsic structure of some real thing in isolation from the 'juxtaposed causal mechanisms' which modify its behaviour. But Lawson (1989a: 241) argues that this does not carry through to an understanding (and then prediction) of actual events 'because the action of any causal mechanism will, in general, be offset by the counterveiling actions of juxtaposed mechanisms.' In other words, the data of the real world, the basis of Lawson's first analytical (scientific) step, is an amalgam of interacting causal mechanisms. How do we decompose the intrinsic causality from the juxtaposed effects? Yet despite this, Lawson believes we can construct explanations of the causal mechanisms in isolation (1989b: 62).

To demonstrate the problem, consider inflation as an event. Lawson (1989a) says that a realist can formulate a true theory of a trade union and a private firm, considered as intrinsic causal mechanisms.[10] A simple conflict theory of inflation asserts that it is the outcome of the interaction between these two causal mechanisms. What we observe is not a trade union or a private firm - a thing - (for example) as a causal mechanism, but two institutions anticipating and attempting to control each other, with inflation being one outcome of the event. What can we deduce about the causal mechanisms from this? We might only be able to say that trade unions have a tendency to protect workers!

Bhaskar (1978: 49) believes that generative mechanisms endure while inactive. But when is a dynamic system like a trade union, for example, inactive. What is the tendency of a trade union? Does this tendency come into play without there being an

empirical disclosure? What experimental (closed) system is possible so that we can empirically identify this tendency? Although the tendency is not a regular sequence of events, Bhaskar (1978: 49) argues that 'the occurrence of such a sequence may ... provide grounds for the hypotheses of the existence of the mechanism.' Does this amount to more than observing that a trade union is usually involved in events associated with working pay and conditions which might lead one to infer that trade unions tend to defend conditions of workers? That is, a leaf tends to fall to the ground!

4.0 Econometrics and realism

4.1 Theories and econometric models

Realism as discussed above eschews the use of econometrics in the development of the knowledge base of a paradigm. However, much of the criticism of econometric practice is based on a misconception of what such activity aims to achieve, and indeed what it can achieve. Crucial to this is the distinction between economic theory and the econometric model. Lawson (1985a: 123) says that 'Econometricians seem universally to report their results as if they interpret themselves as working within the falsificationist bold predictions framework.' In an obvious sense, all econometric practice does operate within a falsificationist context. For example, general-to-specific modelling is characterised by sequential hypothesis formation, testing, and rejection or acceptance.

But there is a crucial distinction between falsification as the epistemic rule, and the demarcation criterion, and hypothesis testing in the context of an econometric modelling exercise. Even a realist relies on falsification to make the move from an imagined world to reality (the world of laws). For a realist, a non-falsified hypothesis is highly useful, and, this outcome would not be at odds with a rational person holding a high degree of belief in the proposition.

Applied econometrics does not test economic theories. Economic theories are untestable. Intriligator (1978: 14) says an econometric model 'is any representation of an actual phenomenon such as an actual system or process ... Any model represents a balance between reality and manageability.' While the data generating process (DGP) is held to be a true process, a model is considered fallible, it cannot be true because to make the process tractable a marginalisation of the DGP has to be made. A theory of the DGP might be true, but such speculation is futile because there is no way of telling. A model of the theory is false by definition. Hendry (1983: 70) distinguishes the DGP (the mechanism) which is true and unique, from the simplified representation of the DGP (the model) which is non-unique.

An econometric model is specified in terms of theoretically-motivated variables and applied to some data. These specific representations contain hypotheses which can be tested. Based on visible criteria, a particular representation can claim to be the most adequate current picture of the DGP. There can be an ordering of representations, some more adequate than others. All representations are tentative and time-dependent.[11] A Post Keynesian econometrician would only aspire to empirically adequate and hence tentative representations of theoretical posits which have satisfied a range of currently accepted diagnostic criteria.

Clearly if we reject a particular econometric model, we are not able to say anything convincing about the economic theories upon which the model is based. A person who wants to hold to a theory which appears to be problematic when the associated

model fails can always appeal to a deficiency in the mapping of theory into econometric form. The problem of theorising about unobservable concepts (for example, expectations behaviour) is particularly relevant here. But, nonetheless, econometric evidence accompanies and can reinforce the beliefs (based on plausibility, prejudice, etc) that we have about one theory or another.

Lawson (1981: 317) distinguishes two 'alternative philosophical approaches to model estimation.' The 'standard approach - accepts the existence of "correctly specified" equations and "true models" and assumes the role of the econometrician is to utilise existing data in order to identify such structures.' The alternative 'Keynesian' approach 'essentially rejects the existence of "true" or "correct" models and concentrates on obtaining tolerable, data-consistent representations of, or approximations to, *a priori* theory' (p.317). Hendry (1983) has already convincingly criticised Lawson's claims that associated Hendry with the standard approach.

At the outset, the "Keynesian" - "standard" dichotomy is unhelpful. It is simply incorrect to label a testing strategy which develops tentatively adequate (but obviously false) models of the true DGP as "Keynesian" - which in another context Lawson (1981: 312) equates with the Cambridge (U.K.) tradition (see Hendry and Richard, 1982; Hendry and Mizon, 1978). This testing strategy is applicable to any theoretical hard-core, the procedure being separate from the axiomatic basis of research. Further, the manner in which so-called Keynesian econometrics is practised (as described by Lawson) is not in the Hendry mould and, as we will argue is unsuitable for a rigorous Post Keynesian agenda.

The more meaningful taxonomy of econometric practice is provided in Gilbert (1986), although this in turn is not complete. He compares Hendry's approach with a 'straw man (sic)' approach to doing econometrics. The latter is called (with humour) the Average Economic Regression (AER) approach which Gilbert locates largely in North American institutions. The AER approach seeks 'to illustrate the theories which we believe independently' (Gilbert, 1986: 284). Poor diagnostic performance is followed by a series of ad hoc changes to the specification (new variables and lagged variables are introduced) or estimation method (AR1 corrections). Two models can emerge to 'explain' the same data. This approach builds more general models from very specific (often static) representations. We are never able to say that a more general model could reject the current model.

A suitable modelling strategy must involve a prior theoretical specification. Post Keynesian modelling is not unique here. Yet it remains true that economic theory cannot provide guidance, for example, in dynamics. Determining the timing and strength, multipliers, and such, cannot be done without recourse to econometric modelling. Economics is a quantitative discipline. We need to generate numbers. I do not share Keynes' (audacious) belief that he could get the numbers right without recourse to formal methods (see Patinkin, 1976).

4.2 Theory and observation

Can we make a valid distinction between theory and observation? There is now an acceptance that there is no absolute independence between observations and theory. All concepts are theory-laden. We simply accept certain observation statements for the purposes of science and everyday life without any justification. Does this mean that empirical evidence is incapable of being an independent basis for testing and comparing hypotheses?

O'Hear (1989: 82) outlines two theses here. The weak thesis contends that all observation is conditioned by presuppositions (theory). But this 'does not amount to an elevated sense of theory.' (p.83) Agazzi (1985: 65) argues that 'empirical evidence in science ... is already subject of a rich variability in itself and quite independently of the theoretical framework ... [and] ... gives us the possibility of restricting the impact of the theory-ladenness thesis, and may grant the possibility of theory comparison.'

It is not empirical evidence "in general" that we use in hypothesis testing, but only restricted and specific evidence, which result from standardised procedures and measurement (for example., the Labour Force survey). While these procedures reflect some theoretical context, this is not necessarily the context of the theory(ies) under scrutiny. Thus, the Labour Force framework reflects theories, for example, about what constitutes work ("gainful employment for pay"), but becomes an instrument for generating data (like unemployment rates) which may be relevant to other theories.

The strong thesis (not necessarily implied by the weak) is that observing data critically depends on the paradigm one holds and that one's observations are always biased in favour of that paradigm.[12] Thus, O'Hear (1989: 84) concludes that 'the hope for a neutral, more fundamental, and less theoretically conditioned level of observation is apparently doomed, and our paradigms remain incommensurable ...'

The point is that we may be able to usefully separate differing theoretical levels of observation. The rigid position is that everything is theoretical. The well-known statement "here is a glass of water" is a theory-laden observation which is not verifiable. However, in practice, we usually decide as a matter of convention to accept certain observational statements. So we might be able to examine the liquid and conclude that it is currently water (by convention) without any having theory about its future behaviour.

4.3 Realism and econometric modelling

With this background established, we can now specifically consider the role of econometrics in Post Keynesian economic analysis. Lawson equates applied econometric research with instrumentalism, on the grounds that both allegedly presuppose a closed system where event regularity occurs.[13] As Bhaskar only establishes a closed system in an experimental natural context, it is hard to imagine what Lawson's concept of a closed system in economic research might be. However, his usage is clearly oriented to the idea of within-sample and post-sample stability used by econometricians in their testing activities.

4.3.1 Keynes and Tinbergen

Realists have attacked the validity of modern econometric research and focused particularly on alleged deficiencies in its predictive element (see Lawson, 1981, 1985a, 1989a, 1989b). Much of this criticism has its beginnings with Keynes notions of probability and his attacks on Tinbergen's first League of Nations Report in 1939.[14]

Keynes' review of Tinbergen, which appeared in the *Economic Journal* in 1939, expressed major disappointment with Tinbergen's work. The latter had been motivated by the macroeconomic developments following the publication of the *General Theory*. Keynes' review has been harshly rejected by Klein (1951: 450)

> In econometrics Keynes was even less well versed … [than he was in mathematical economics] …, his presidency of the Econometric Society notwithstanding. His review of Tinbergen's celebrated study for the League of

> Nations was one of his sorriest professional performances. Many econometricians have remarked on Keynes's review with the comment that he simply did not understand the method he was criticizing and failed to see at what Tinbergen was aiming, a type of comment Keynes frequently use to characterize his own critics. Ironically enough, much of Keynes's greatest support has come from econometric testing and application of his theory.

Important letters written by Keynes and Tinbergen, and Harrod have since appeared in Keynes (1973a, 1973b, 1973c). Rowley (1988: 23) suggests that these letters mean that 'some of the ambiguities and poorly expressed portions of Keynes's review can be put aside and its primary arguments reassessed in terms of a more recent econometric perspective.' In fact, the letters only clarify how little Keynes knew about the work that Tinbergen was doing.

Keynes claimed Tinbergen was an inductivist. In his review of Tinbergen (1939), Keynes (1939: 560) said he thought econometrics as a method 'is one neither of discovery or criticism. It is a means of giving quantitative precision to what, in qualitative terms, we know already as a result of a complete theoretical analysis - provided always that it is a case where the other considerations ... are satisfied.'

Keynes's (letter to Tyler sent on August 23, 1938) principal criticism of the use of econometrics was that the 'logic of applying the method of multiple correlation to unanalysed economic material, which we know to be non-homogenous through time' could not be used to test the incorrectness of economic theory. This point is now well known. His main concern was with the stability of the regression coefficients. Klant says (1985: 920) that 'Keynes believed that statistical analysis as a means of induction must fail in economics because the condition of a constant environment is not met.'

Keynes (1973b: 296) explicitly believed that economics was 'a branch of logic, a way of thinking.' He thought that the conversion of 'a model into a quantitative formula' destroyed its usefulness as an instrument of thought' (Keynes, 1973b: 299). Klant (1985: 81) says on this that 'if we take these words literally, they convey that economics is merely applied logic, some kind of formal system such as geometry or decision theory, thus without empirical content.'

Unfortunately, Keynes did not understand Tinbergen's work. Morgan (1990: 121-122) says that:

> It was unfortunate that, while Keynes attacked the subject with his usual rhetorical flourish, he had clearly not read the volume with any great care. Some of his criticisms also revealed his ignorance about both the dynamic economic models of the business cycle developed in the previous decade and the technical aspects of econometrics ... In another example, Keynes supposed wrongly that trends were measured by joining up the first and last observations of the series, whereas Tinbergen has used a moving average trend or a linear trend term in the multiple correlation ... In fact, most of Keynes' specific points of criticism proved invalid since, in his model building and applied work, Tinbergen had dealt with the particular problems raised and carried out various tests or procedures which Keynes criticised him for omitting.[15]

4.3.2 Closed and open systems and instability

Pheby (1988: 92) argues that '... we are likely to experience a growing tendency to construct models that are statistically tractable, even if they ignore important conceptual matters such as uncertainty, expectations and economic dynamics.' Realist

theory (searching for ultimate laws) would flounder here for exactly the same reason as the realists indict 'probabilistic economics' - even if a law existed today (and could be discovered to exist), how do we know that it will operate tomorrow? Moreover, if uncertainty is endemic, what is a satisfactory theoretical explanation of behaviour look like? What is a satisfactory theory of expectations formation? What theoretical structure offers any insight into the dynamic processes underpinning the DGP's?

A realist distances herself/himself from a regularity determinist who considers that for 'every event *y* there is an event *x* or set of events x_1 ... x_n such that *x* or x_1 ... x_n and *y* are regularly conjoined under some set of descriptions ... [thus] whenever this, then that' (Bhaskar, 1978: 69) applies. If at time *t*+1, a type *a* event was not followed by a type *b* event, Bhaskar concludes (1978: 73) that 'instability, in space or over time, of actually recorded empirical relationships' occurs. For Bhasker, this coincides with an open system which is defined in terms of type *a* events not invariably being followed by type *b* events. Lawson uses this experimental context to describe instability in an economic structure. He equates stability with the regularity of empirical phenomenon (1989b: 61) So the leaf 'does not fall to the ground in strict conformity with an empirical regularity, for its actual path is influenced by aerodynamic, thermal and other tendencies. Yet its path is still recognised as being subject to the law understood as a tendency.' (p.63) Of course, the 'empirical regularity' that a leaf falls is uninteresting once we have comprehended gravity. The interesting question is why and how far it deviates from orthogonality. Suppes (1985: 192) says that stability occurs when a 'process is ... not disturbed by causes of small magnitude.'[16]

Clearly if empirical relations are not stable then prediction of future relationship magnitudes is futile. It is generally claimed that a predictive social science is impossible because of the 'complexity and subtlety of human affairs' (Suppes, 1985: 189). As an aside, Suppes (1985) demonstrates that problems of prediction due to instability and randomness also plague the natural and physical world.[17]

If we can isolate causal mechanisms we could 'categorically predict the future' but 'it always remains on the cards that an unpredicted change in the external circumstances of the system or the internal states of its individuals will occur so as to upset the established regularity and so render inapplicable any hypothetical predictions, formulated subject to ... *ceteris paribus* clauses' (Bhasker, 1978: 76). So if the relations between external events and changes in the internal structure of the mechanism are not stable then prediction is impossible.[18] One response to unstable empirical relations is to appeal to omitted variables. Thus, the relations between event *a* and event(s) *b*(s) are not stable because we have failed to fully specify our state-description.[19]

The problem of omitted variables is well understood by econometricians. First, stable empirical relationships can still exist if the omitted variables are themselves constant or unimportant. Second, in terms of procedure, specification and misspecification testing becomes paramount, because it limits the number of significant factors that we have to assume constant.

Part of the preliminary modelling effort is to try to identify the 'juxtaposed' causal forces and develop hypotheses about them individually and in relation to other mechanisms. Lawson says (1989a: 243) that 'the econometrician will usually have positive knowledge of numerous - perhaps of an almost unlimited number of - potentially relevant *causal* factors that it is not possible to explicitly consider.' (emphasis not in original).

Importantly, good econometric practice involves the testing for these problems. Lawson (1989a: 283) believes that econometricians have relied on *ad hoc*, fictional and creative practices to render the influences of the omitted variables benign so that the estimated model is data consistent. While clearly true in specific cases, such 'conventionalism' is not inevitable. There are now a number of diagnostic tests available to guide our model-building procedures. If the battery of tests are not computed and published then we are alerted to the potential that the practices employed by the econometrician are aberrant (see Mayer, 1980).

So while omitted variables can be a destructive and terminal problem, we do not proceed as if the problem is present. It becomes one of many plausible hypotheses we might pose in relation to our model and then test. If our model fails - in part because excluded factors are significant - then we do not have a valid and adequate model on which practical statements can be made. Just because methods can be abused does not mean that in appropriate hands useful analysis cannot be forthcoming.

Should stability be a requirement of an acceptable specification? There is no definite statement about stability that can be made in advance. While it is sensible to assume that a DGP could behave in a discontinuous fashion, it is also possible 'that the DGP manifest enough 'continuity' over time' (Hendry, 1983: 71). Lawson provides a negative, assertive answer to the above question. His position, in summary, is that if we are modelling inherently unstable processes, then why would we consider stability to be a desired property. Apart from the obvious retort that modelling and instability are contradictory concepts, although Lawson would argue we focus on tendencies, we cannot presume instability.

The developments over the last decade or so in the modelling of cointegrated sets is based on a long-term (nothing to do with Marshall) concept of stability (proportionality) among variables. In many areas, economic theory posits a cointegrating relation between two or more variables, in which case a cointegration test provides some guidance as to whether the variables in question have displayed proportionality over a long period. Establishing cointegration is an interesting result and certainly suggests that economic variables are conjectively stable.

When an econometrician thinks of an estimated model, he/she does not think in terms of a natural or physical model established in an experimental context. Hendry (1983: 72) says that we rather attempt to establish a 'conjectural' degree of stability for the sample of available data. Clearly, if we find evidence of instability, then the conjecture is problematic. However, we tentatively accept a concept of time-dependent stability if our model displays within-sample stability (defined by conventional likelihood tests).

Without exception, a model, which is within-sample unstable, cannot be used for the same type of analysis as a 'stable' model. Lawson (1985a: 119) criticises the testing procedure used by econometricians to determine within-sample stability. The practice of saving some observations for a predictive failure test, or comparing residual squared sums over two large sub-samples is considered unacceptable by Lawson. 'The intuition ... is that a model that predicts part of the data is, in some sense, better than one that merely accommodates the data' (1985a: 119).

While we certainly desire a model 'to accommodate' the sample available, a model which also passes a predictive failure test provides additional information to us about the relations between the modelled variables. Moreover, Lawson (1981: 319) claims

that ‘the preferred Keynesian’ approach to econometrics, ‘rationalises the predictive failure of an econometric system on the grounds that some structural break has occurred’. First, formal testing of a model is required to establish whether a single or number of large residual observations represent significant changes in the regression surface. Without performing split-sample comparisons, we could not differentiate between an unusual observation and a structural change. Second, econometricians do not either (a) estimate a model which attempts to closely correspond with the sample observations (accommodate the data) or (b) estimate a split-sample and forecast over some later part of the data. They do both! Third, when, ultimately, structural breaks do occur in our models, we must do more than merely recognise that the break has occurred. The on-going research programme necessitates that we form explanations and test the same. Finally, a model which passes the various ‘stability’ tests currently in vogue is the only model which could be used to forecast into the unknown future. The model which failed to capture the dynamics of the past, would not, reasonably, be the basis for any future statements.

So given that many groups in the society desire numerical forecasts to be generated by economists there is clear virtue in basing these predictions on a model which has not yet broken down. A model is not intended to be universal or ahistorical. Post Keynesians criticise neoclassical economics exactly on this point. A DGP may encounter a violent shock and behaviour changes irrevocably from then on. All existing models will then fail to predict. The task of the modeller is to capture the shock, after which, stability becomes, again, a conjecture based on the best evidence available.

4.4 Kalecki on structural change and econometrics

It is interesting to compare Kalecki’s approach to econometrics and structural change. While Keynes was overtly hostile to econometric modelling (see Keynes, 1939; Tinbergen, 1940; Patinkin, 1976), Kalecki embraced it. It is interesting that Kalecki recognised the issue of structural change while the same is often used to defend Keynes’s own hostility and rejection of econometrics. Interestingly, just as Keynes was also hostile to Marx’s approach, Kalecki was sympathetic to it and approached the issue of structural change within the framework of historical materialism.

As an economic interpretation of history, historical materialism focuses on the development of the productive forces and relations, allowing them to interact with other social phenomena like government, science, culture etc.). Structural changes occur as a result of the development of the productive forces. So under what circumstances could one practice econometric modelling?

Focusing on what we now call time-series analysis, Kalecki (1964: 233) said that econometrics

> is based on functional relations between the econometric variables in the period considered as well as between these variables and the same variables in the past periods. The relations are assumed to be given and not subject to change. In this way a definite dynamic process is established which, however, corresponds to the actual development only in the case where the basic assumption of the invariability of functional relationships referred to above is fulfilled.

Kalecki was easily able to combine the economic development ideas with the invariability requirements, a task that Keynes was unable to even broach. Kalecki (1964: 233) argues that the Marxian reproduction schemes were

> nothing else but simple econometric models. In fact in a special case where no changes in natural resources, productive relations and the superstructure affect the development of productive forces the system will follow the path determined by an econometric model because the condition of relationships between the economic variables not being subject to change is then fulfilled. In a more general case these functional relationships alter under the impact of event in three other spheres of the system and the economic development is then a much more complicated process than that presented by an econometric model as it reflects the evolution of the society in all the aspects.

The obvious question, which did not escape Kalecki, was under what circumstances could one know that they were in a period where the relationships between the economic variables were not subject to change. Of importance, Kalecki (1964: 234) said that

> It should be noted that even in an econometric model the relationships represented by the function *f* cannot be considered strictly invariable. For the economic relations are by their very nature rather loose: the parameters involved are not strictly constants but constants plus some small random element. Thus the relationships between economic variables represented by *f* are quasi-invariable in the sense that they are subject to small random disturbances.

Kalecki (1964: 236) proceeded to present a linking between the econometric models and the process of societal development. He outlines two conditions which generate invariability in *f*. '(a) there are no autonomous changes in the spheres other than strictly economic conditions or if any they do not affect significantly the pattern of economic development; (b) there is no significant feedback effect involved in the impact of economic development upon the other spheres of the system.' Kalecki saw that economic development would superimpose itself on the relations represented by *f* and while small changes would not render it too variable, occasional abrupt parth changes would lead to periods of 'extreme instability of economic conditions' (Kalecki, 1964: 237).

Kalecki (1964: 238) finished by saying that

> there emerges ... a new way of presenting the evolution of society. The focal point of it is in a sense the economic development whose course is determined by a 'generalised econometric model' which involves changing relationships between the economic variables present and past ... .These changes result from the impact of the evolution in the spheres of natural resources, productive relations and the superstructure which in turn is profoundly affected by the course of economic development.

5.0 Prediction – the partner to explanation

Caldwell (1989: 57) discusses a possible methodological 'plan of action for post-Keynesians to consider.' He says that a useful (and long-standing) idea is that science seeks to explain and predict. For Post Keynesians theory is about reality. Caldwell (1989: 58) notes that Post Keynesians place a premium on portraying reality. He also

suggests that among Post Keynesians there is a view that realism is less important to economists who are concerned with prediction. Caldwell (1989: 58) asserts that 'it may therefore seem that the natural division between post-Keynesians and neoclassicals is along the lines of explanation versus prediction.' To go from Friedman's untenable position to one where all predictive endeavours exclude explanation is to present a false mutual exclusivity. Not all prediction is based on the as if principle. Caldwell (1989: 59) recognises this and hints that some Post Keynesians have already fallen prey to this false dichotomy.

A Post Keynesian econometrician will seek to develop models which reflect and tentatively explain reality, as a complementary and prior task to using such a model to for prediction. It is this duality of purpose that distinguishes a Post Keynesian econometrician from a Friedmanite who works in the *as if* framework.

Econometrics does not necessarily imply instrumentalism. Pheby (1988: 82) quotes Hesse (1967) where instrumentalism assumes 'that theories can be used to relate and systematise observation statements and to derive some sets of observation statements (predictions) from other sets (data), but no question of the truth of the reference to the theories themselves occurs.' Thus, Pheby (1968: 82) concludes that econometricians follow 'an essentially instrumentalist methodology.'[20] Yet Caldwell (1982) advocates a *critical pluralism* in preference to the arbitrary adoption of a particular set of criteria, the abandonment of any scientific pretence or justification, or a retreat into descriptivism. Caldwell (1989: 44-45) summarises critical pluralism as follows. First, one cannot claim that one's work is scientific whereas another's is not. While a scientific mainstream exists, there is no way of knowing that this is science. Second, a number of methodologies should be considered. Given that this raises the issue of comparison, a number of ways should be used to evaluate methodologies (see also Feyeraband, 1978).

Critical pluralism implies that both explanation and instrumental prediction have a place in a Post Keynesian research program. Practical issues are important for policy design and implementation. So while prediction is not the only legitimate goal, at times, particularly for an interventionist policy design, it becomes a crucial aspect of our activity.[21]

It is clear that human affairs cannot be produced and controlled in an experimental context. The non-repeatability of social actions makes falsification a dubious criterion for demarcation in economics. Even in the natural sciences there are problems in assessing the truth of the observation statement, of repeating the results, of controlling for extraneous factors, and more. These problems are compounded in the social sciences.

Moreover, this order in human affairs also provides a tentative basis for econometric modelling. Hodgson (1989: 104) says that a

> … more plausible view is that there are external influences moulding the purposes and actions of individuals, but that action is not entirely determined by them. The environment is influential but it does not completely determine either what the individual aims to do or what he or she may achieve ... Action, in short, is partially determined, and partially indeterminate: partly predictable but partly unforeseeable.

To justify prediction, we might seek refuge in Hodgson's statement of institutional determinism. Indeed, contrary to Shackle's anarchy of the human predicament, human

affairs in aggregate display remarkable stability. Hodgson (1988) outlines the importance of habitual behaviour which he traces back to Veblen. Habits embrace customs, traditions, and institutions. The importance of habits lie in their ability to reduce uncertainty and to provide other agents with a fairly reliable information source as to how one might act in the future.[22]

Habitual behaviour has manifest implications for a neoclassical paradigm. But for our purposes it provides the rationale for holding the belief that we might expect stability in macro human affairs rather than anarchy. It obviously opens up a separate research agenda for Post Keynesians as to why habitual behaviour is preferred to a more complicated, though perhaps, more interesting behavioural patterns. But as Hodgson (1988: 109) says, 'One consequence of this function of institutions is that in a highly complex world, and despite uncertainty, *regular and predictable behaviour is possible*' (emphasis added).

Lawson (1983: 81, footnote 2), in quoting Cripps (1981: 43), acknowledges that 'uncertainty and ignorance are assumed to be dealt with by co-operative institutions and traditional decision rules which persist or change slowly unless they come into crisis.' So if human society is characterised by remarkable stability, interrupted by bursts of discontinuity, arising from natural forces or human flux, then sometimes 'good' predictions will be ridiculous, but more often they will approximate the events that occur. Even when crisis occur which promote irrevocable displacements in the DGP, on-going modelling can absorb these changes and proceed into the next phase of stability.

The objective of problem solving in economics is to advance the welfare of people (presumably in harmony with nature) through policy. Policy analysis is a separate activity to explanation. Prediction is an essential aspect at both the explanatory level and empirical level of our problem solving activity. Our theoretical models have to be able to generate qualitative predictions of the likely outcomes of changes in key variables. A realist model builder would clearly agree with this. However, the qualitative predictions have to be translated into quantitative predictions. Econometrics helps to translate the qualitative impacts into quantitative impacts which allow us to conduct economic policy.

The purpose of theoretical activity, then, is to provide proposals for policy which will improve the outcomes of human society. It only makes sense to base policies on qualitative predictions derived in advance from theory. To use actual public policy programmes as the laboratory for testing theory would be dangerous and untenable. Eichner (1983: 510) calls this the praxis test. Even if there are major problems with developing these predictions, we really have no other choice.

At the theoretical level, Klant (1985: 92-93) provides an apt demonstration of this. In a simple two sector macroeconomic model, we can derive a relationship between a change in real income, given a change in exogenous investment, based on the inverse of the marginal propensity to save. If the marginal propensity to save is assumed to be stable, then the relationship is positive. Yet if the marginal propensity to save is unstable and we cannot explain its vagaries, then Klant says (1985: 93) 'the theory does not contain meaningful theorems.' It can only produce 'tautological non-prediction', that is theory of this type is always true and produces no predictions upon which a praxis test could be based. Economic theories of this type are merely systems of logic. They have no practical value - in offering guidance in changing and improving social outcomes. To move from the logical non-predictive world into a

world where theory can be used as a basis for policy via prediction, we must make some assumptions about the temporal stability of the parameters of our models.[23]

Keynes argued that policy could be accompanied by persuasion to ensure its success. But persuasion must be accompanied in turn by quantitative information. It is one thing to persuade people to act in a certain manner deemed to be desirable, but another to know what the likely outcomes of such behaviour will be. Caldwell (1989: 60) argues that '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.'

There is no claim that prediction at the policy analysis level is highly subjective despite the sophistication of the econometric structures used. Clearly a large amount of subjective judgement in a trial and error process underlies the use of econometric forecasting. Klant (1985: 96) eloquently concludes that 'The basic theory is a conjecture, the interpretation is a conjecture and the application is a conjecture. Usually the forecasts on the basis of the three-layered conjectures are not accurate but are better than those of businessmen and politicians. They are at least consistent. The predictions which they yield are neither tautological nor contradictory.' However, inasmuch as policy is conducted in terms of observable variables, econometric models have a direct utility.

6.0 Conclusion

Econometric analysis is clearly 'a tool to aid thinking and a focus for policy analysis and discussion.' (Lawson, 1983: 80) In this regard, it is an essential component of a sound epistemology. Theoretical activity alone is as limited as is a focus on mechanistic predictive modelling. However at the theoretical level and the empirical level, prediction is crucial to our role in society.

Although we have seen great advances in econometric techniques since the 1930's, there are still major (well documented) limitations which demand candid care from their users. These difficulties however do not preclude their use. A chainsaw can cut a pile of deadwood for a log fire far better than a small hand saw, but in the wrong hands the same tool can ruin a virgin forest. A descent into the nihilism of fundamental uncertainty is a road to nowhere.

7.0 References

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[1] The author is Professor of Economics and Director of Centre of Full Employment and Equity, University of Newcastle, Australia.

[2] Lavoie (1992: 50) argued that 'one must admit that some defenders of fundamental uncertainty, most notably Shackle … have left their readers with the impression that uncertainty only allows nihilistic conclusions. But this is not the impression of the majority of post-Keynesians … The impact of uncertainty on economic analysis and on economic results depends on "how individuals are supposed to respond to the fact of uncertainty" (Coddington, 1982: 482).'

[3] Dow and Dow (1985: 46) argued in relation to uncertainty in economic life that 'If long-run expectation formation by entrepreneurs cannot be explained by rational behaviour which can be modelled …, then this must be true for all expectations of agents in all sectors. But then if all decision making is subject to the exogenous influence of expectations shifts, economics must retreat into nihilism.'

[4] Caldwell (1989: 54) said that '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.'

[5] In a simple experimental situation, where control is possible, Bhaskar (1978: 34) said that '… it ties within the power of every reasonably intelligent schoolboy or moderately clumsy research worker to upset the results of even the best designed experiment, but we do not thereby suppose they have the power to overturn the laws of nature.'

[6] Lawson (1989a: 240) claimed that understanding the tendency of a leaf to fall to the ground, does not allow us to predict the 'actual motion of a leaf'. An econometrician might attempt to measure the disparate influences on the leaf's trajectory. Does Lawson believe that if we knew the characteristics of the leaf, its behaviour in a vacuum, the wind direction and strength, and other relevant observable influences, that we couldn't approximately predict where it would land?

[7] Coddington's (1983: 59) clearly saw the irony of this position when he identified the 'two contrasting ways in which Keynes's ideas on uncertainty developed by those who have seen them as central to his work'. Coddington's (1983: 61) reference to the 'post-keynesians …. [who he said were quite] … happy to drive a wedge between behaviour and circumstance, but if the wedge were to become comprehensive, they would be left with no theory at all' is juxtaposed to the second group who have 'led a position that appears to be consistent but analytically nihilistic. A consistent or all-embracing subjectivism is, analytically, a very self-denying thing. If subjectivist logic is followed to the point of becoming convinced that there is nothing for economists to do but to understand certain (praxiological) concepts, then the only problem that remains is that of subjugating one's conscience long enough to draw one's salary in exchange for imparting this wisdom. One could, of course, having got into this state of mind, spend a good deal of time and energy in trying to convince those who engage in macroeconomic, econometric model-building, mathematical economics, general equilibrium theory and so on, of the folly of their ways. But, that task accomplished, there would be nothing left but for the whole profession to shut up shop.'

[8] Bhaskar (1978: 145) said 'if and only if the third step is taken can be an adequate rationale for the use of laws to explain phenomena in open systems (where no constant conjunctions prevail) or for the experimental establishment of that knowledge in the first place.'

[9] Some have criticised realism because the conditions under which the world exists without humans is incomprehensible. None of us can truly say that we do not exist. Bhaskar (1978: 48) said 'a thought must always contain, or at least be accompanied by, a though of the thinker of the thought thinking the thought.'

[10] Lawson (1989a: 237-238) asserts that realism accepts the existence of a 'material and social world independent of (any individual) consciousness but which is knowable by consciousness – it is accepted that true theories can be obtained.'

[11] Klant (1985: 94) says that these econometric models 'are generally not proper instances (subrelations) of basis theories which describe sets of structures. A basis theory allows empirical investigations too much choice. They can choose how to translate theoretical concepts into operationally-defined concepts, how to apply dis-aggregation, how to plug in auxiliary hypotheses justifying the use of proxies, how to produce a dynamic version of the theory by introducing time as a variable and dating the variables … The mathematical form of the relations also rest upon the choices and the model is often augmented by hypotheses which are not included in the basic theory.'

[12] O'Hear (1989: 83) says that 'Aristoleans saw constrained fall, whereas Galileo saw a pendulum; what Priestley took to be dephlogisticated air, Lavoiser sees as oxygen.'

[13] Pheby (1988: 123) makes the distinction between empirical and empiricist. 'To be empirical in approach means one endeavours to go to some lengths to substantiate, or refute, one's theories by considering factual or other data …' On the contrary, an empiricist believes that knowledge can only be attained through observation (sensory perception and facts collection).

[14] The Keynes-Tinbergen interchange remains one of the most fascinating in the interface between economics and econometrics. See Stone (1978), Hendry (1980), Pesaran and Smith (1985) and Morgan (1990) for interesting accounts of it.

[15] Keynes showed remarkable ignorance in his exchange with Tinbergen. In a letter to Tinbergen (September 20, 1939), he says 'suppose you have statistics covering a period of 20 years, what is required, it seems to me, is to divide these into convenient sections, say, of 5 years each, and calculate a proper equation for each period separately, and then consider what concordance appears between the different results. Until this has been done, a formula applying to the whole of the 20 years can have very little significance.'

[16] Suppes (1985: 192) says 'thus a chair is stable if it cannot be easily pushed over. A political system is stable if it can withstand reasonable sustained shocks. A person is stable if he is not continually

changing his views. More specifically, a person's belief in a given proposition is stable if it can only be changed by very substantial new evidence.'

[17] Suppes (1985) suggests we think of a golfer making a remarkable birdie after bouncing the ball of a tree into the hole. While prediction here is impossible, he says explanation can be made in terms of physical laws, *ex post*, which operate in stable environments. On human behaviour, he says (1985: 190) that '… when intentions are pure and simple we can expect human behaviour to be stable and predictable, but as soon as major conflicts arise, … knowledge of intentions is in and of itself of little predictive help, though possibly of great explanatory help after the fact.'

[18] This could be either constancy or an absence of an internal structure. We might plausibly assume that internal institutional behaviour is fairly stable.

[19] Bhaskar (1978: 74) recognises 'that the conjunct events referred to under the same description "a" before and after *t* were not really the same in all relevant aspects; in short that the system had been incompletely described (or enumerated).'

[20] Lawson (1989a: 237) sees realism and instrumentalism as opposing approaches. He asserts that while econometricians may adopt a realist position, in fact, 'because of certain premises that must be accepted if econometrics is to be advanced, then, whatever, their philosophical inclination might otherwise be … [they] … appear to turn, ultimately to a *form* of instrumentalist reasoning (emphasis added).' Lawson refers to a form of instrumentalism rather than instrumentalism *per se*. He defines the latter as reflecting 'an idealist position whereby theories can never be considered true or false but merely instruments of prediction' (Lawson, 1989a: 237).

[21] Caldwell (1991: 29) says that 'Friedman is much less concerned with which theory is *true*. His concern, rather, is with which theory *works best*, given some problem. Usually the problem for economists is to forecast the future, hence the importance of predictive adequacy … When are economists most likely to follow Friedman's prescriptions? Simply put, Friedman's instrumental approach is most likely to dominate when knowledge is incomplete but some form of policy decision has to be made (emphasis in original).'

[22] Hodgson (1988: 106) says that 'One of the functions of habits is to deal with the complexity of everyday life; they provide us with a means of retaining a pattern of behaviour without engaging in global rational calculations involving vast amounts of complex information.'

[23] Keynes dismissed the role of prediction in theoretical development. Klant (1985: 85) argues that 'Keynes referred … to the (supposed) validity of the inductive method, but not to the reasons for constructing theories. Theories are formulated to solve problems. New theories to predict new events. He … [Keynes] … seemed to ignore this. He wrote: "The peculiar virtue of prediction or predesignation is altogether imaginary. The number of instances examined and the analogy between them are the essential points, and the question as to whether a particular hypothesis happens to be propounded before or after their examination is quite irrelevant (Collected Works, VII, 337)"'