CRITICAL REALISM AND ECONOMETRICS: CONSTRUCTIVE DIALOGUE WITH POST KEYNESIAN ECONOMICS[1]

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> "…a difference of opinion between practicing Post Keynesian economists and critical realist methodologists should initiate a process of discussion that might well bring about modifications to critical realism as well as to practice" (Dow, 1999, p31).

1. INTRODUCTION

There is a growing literature in political economy that a philosophical commitment to 'realism' is appropriate for economic methodology. It is argued that critical realism can underpin an alternative perspective to mainstream economics. This is particularly the case in Post Keynesian economics (Dow,1985, 1990, 1992, 1998a; Arestis 1992; Arestis and Sawyer 1993; Lawson, 1994; King, 1995; Dow and Hillard ,1995; Pratten, 1996; Dow, 1999)

Despite this literature the epistemological consequences of such a commitment have not been developed in detail. There have been papers that have discussed these matters in broad terms. This is, for example, revisiting the debate about the coherence of post Keynesian economics. Arestis *et al*, 1999, Dunn, 1999 and the symposium in the Journal of Post Keynesian Economics, Fall 1999 are recent manifestations of this. Consequently, this paper argues that a useful research agenda is to carefully examine the implications of critical realism for *particular* aspects of post Keynesian economics. Accordingly the role that econometrics can play in a critical-realist epistemology is addressed. This is an important issue because many of the epistemological pronouncements of critical realism imply suspicion of econometric methods. However, post Keynesians employ these methods. It follows that there is a possibility of logical inconsistency between the methodological aspirations and practices of post Keynesians.

The paper is organised as follows. Section 2 provides some context for this paper by briefly reviewing the critical-realist ontology and epistemology provided by Tony Lawson.[2] The challenge for econometric work raised by the discussion of critical realism and post Keynesian economics is stated. Section 3 attempts to characterise econometrics in some general terms by outlining the emphases of important schools of thought. Section 4 then investigates critical-realist concerns with econometrics in some detail. Section 5 argues that one can justify its use in economic inference from a critical realist perspective by drawing upon insights from both early econometric work and discussion of econometric methods. Significantly there is resonance with the Post Keynesian literature on these themes. Conclusions then follow.

2. CRITICAL REALISM: ONTOLOGICAL AND EPISTEMOLOGICAL SUPPOSITIONS

Critical realism in economics is primarily associated with the work of Tony Lawson. Lawson (1997), for example, is the outcome of a lengthy and sustained research project designed to expand and employ the philosophy of Roy Bhaskar. In this text the implications of alternative ontological positions in economics are discussed and, inparticular, a contrast is drawn between closed and open system ontologies. In the former case two conditions of closure are assumed. The intrinsic condition of closure (ICC) - which can be loosely characterised as implying that a cause always produces the same effect - suggests that the structures of the phenomena under study are constant, unchanging and for any intrinsic state only one outcome is possible. The extrinsic condition of closure (ECC) - which can be loosely understood as implying that an effect always has the same cause - suggests that the phenomena under study are isolated from other potential influences. The extrinsic condition of closure suggests that the phenomena under study are isolated from other potential influences.

When combined in an analytical narrative, these closure conditions facilitate an account of the social world as comprising the constant conjunction of events. The relevance of this approach presupposes that closed systems, and their associated event-regularities, dominate the social world. It is argued that the prevalence of mathematical reasoning and econometric inference are evidence of the closed-system perspective of mainstream economics. Mathematical deductions articulate the constant conjunction of events. In turn econometric predictions test their relevance.

In contrast, in Lawson's (1997) terms, a critical realist maintains an open-system ontology in which certain conditions for closure do not apply. This approach implies that, for example, human agency is embedded in a social context. Behaviour is thus irreducible to individual action *per se* but, on the contrary, is both conditional on, and results in, multiple modes of the determination of events. In recognising the structured but organic nature of reality, the contrasting epistemological goal of theorists should be to identify causal laws by discovering the mechanisms underlying but not empirically equivalent to actual events. It remains that critical realism does not rule out closed systems *a priori* but would expect to find them as the exception rather than the rule and in need of explanation.

How this goal is to be achieved, however, is not straightforward and indeed has prompted this paper. Consequently, a more detailed elaboration of critical-realist epistemology is required before critical discussion can begin.

The central concepts associated with Lawson’s (1997) epistemology appear to be associated with the concepts of ‘retroduction’, ‘abstraction’, ‘tendencies’ and ‘explanatory power’. Unlike deduction, which is associated with the movement from a general to specific claim, or induction, which is associated with the movement from a specific to a general claim, the mode of inference espoused by Lawson is retroduction.

This,

> "...consists in the the movement, on the basis of analogy and metaphor, amongst other things, from a conception of some phenomenon of interest to a conception of some totally different type of thing, mechanism, structure that, at least in part, is responsible for the given phenomenon" (Lawson, 1997, p24).

This suggests that rather than focussing on events *per se*, realists should focus on the enduring causal mechanisms underlying them. For Lawson abstraction should form the basis of conceptual enquiry.

Abstraction should be concerned with what is real. This is in the sense of identifying objects that exist not only in thought but also in the real world. At the same time abstraction should be made from the concrete, which is a combination of diverse features or determinations. Abstractions should then be synthesised or assimilated into a unity of the diverse to reconstitute the concrete, which can now be understood in essence more. Crucially this suggests that abstraction is to momentarily neglect features of reality rather than to isolate them. Abstractions can thus be partial but need not be false or idealising.

In the absence of event regularities, Lawson (1997) argues that tendencies are the key subject of scientific endeavour. Tendencies are powers that are acting whatever particular events may ensue. They are not simply long run, normal, usual or average outcomes but a non-empirical activity of a structured thing or agent. Tendencies may reveal themselves in outcomes but, because they may act in conjunction with other tendencies, one cannot infer their action directly from events *per se*. However,

> "Over restricted regions of time-space certain mechanisms may...be reproduced continuously and come to be (occasionally) apparent in their effects at the level of actual phenomena, giving rise to rough and ready generalities or partial regularities, holding to such a degree that *prima facie* an explanation is called for" (Lawson, 1997, p204, italics in original).

Such 'demi-regularities'

> "... can serve to *direct* social scientific investigations, through providing evidence that, and where, certain relatively enduring and potentially identifiable mechanisms have been in play" (Lawson, 1997, p207, italics in original)

As far as exploring and offering an inference about causal mechanisms is concerned, no specific guidance is forthcoming from Lawson. Lawson notes that as far as the process of retroduction is concerned,

> "Not much can be said about this process of retroduction independent of context other than it is likely to operate under a logic of analogy or metaphor and to draw heavily on the investigator's perspective, beliefs and experience" (Lawson, 1997, p212)

Likewise, with demi-regularities,

> "... any patterning...of phenomena which turns upon differences or unanticipated or surprising or implausible relationships of some kind, whether primarily social, historical or geographical, can serve to alert us to the existence *or way of acting* of some item previously unknown, unrecognised or perhaps known only implicitly, in some taken for granted way" (Lawson, 1997, p209, italics added)

This highlights phenomena called *contrastive demi-regularities* or "anomalies" (Sayer, 1992, p. 61 and akin to Kaldor's,1985 'stylised facts'[3]), which refer to a systematic difference, a deviation from an expected pattern, an unexpected empirical phenomenon[4]. This might be between social groups, industries or time periods.

Moreover, in an open system,

> "*...for the process of theory assessment ...* event-predictive accuracy cannot be the criterion of theory selection. rather the appropriate criterion outside of the controlled-experimental ... situation must be *explanatory power*. Theories can be assessed according to their abilities to illuminate a *wide range* of empirical phenomena. And typically this will entail accommodating precisely such contrastive demi-regs as are recorded or can be found" (Lawson, 1997, p213, itlaics in orginal).

In short, other than arguing that theories must, in some sense, be consistent with a wide range of empirical phenomena, explanatory power, as Lawson's criteria of theory choice, is left relatively undeveloped. Three general aspects to identifying explanatory power are discussed by Lawson (1997). The first comprises the deduction of the consequences and effects of an operative mechanism identified in a retroduction. The second involves empirically checking these consequences which, while,

> "...there can be no guarantee that any effects will be straightforwardly manifest ... the aim must be to try and identify conditions where... the effects ought in some way to be in evidence" (Lawson, 1997, p213).

Finally the explanation needs to be explained itself. This involves checking whether a mechanism was in fact operative. As well as these general guidelines Lawson (1995, 1997) is generally critical of the role of econometrics and the use of assumptions in economics. For example, he argues that econometric inference involves making closure assumptions. Moreover, assumptions in general are used as devices for artificially

asserting closure. Ultimately, Lawson argues that,

> "... it is not possible to state categorically at this abstract level the precise conditions under which substantive theories can be selected amongst...without knowing the contents of the theories themselves or the nature or context of the conditions upon which they bear..." (Lawson, 1997, p214).

This is perhaps not surprising. Echoing Bhaskar (1978), Lawson argues that critical realism is 'ontologically bold but epistemologically cautious'. In rejecting covering law notions of scientific enquiry, realists must reject formulaic notions of scientific endeavour. Nonetheless, it does not follow, for example, that,

> "Post Keynesians ought thereby not to engage at all in formalistic methods such as econometrics. The possibility of successes with the latter requires local closures…a special configuration of an open and structured system…So the opponent of critical realism is not the post Keynesian or whoever, seriously attempting to find out if (or demonstrate that) in certain conditions some closed-systems methods or whatever could contribute to enlightenment. Rather, the opponent is the advocate of any form of *a priori* dogma" (Lawson 1999, pp7-8).

Consequently, the remainder of this paper attempts to explore the conditions under which the possibility of success with econometrics could apply.

3.DEFINING ECONOMETRICS

To begin with, econometrics requires careful definition. Not only do a burgeoning set of techniques exist for the researcher to calculate statistics, but they also form part of various inferential frameworks. It follows that some discussion of econometrics is required in order to provide a critical-realist analysis of any potential application. In this paper four distinct schools of thought in econometrics are discussed. These are the 'average economic regression' or textbook approach, the Hendry/L.S.E. approach, the Leamer/Bayesian approach and the Sims/atheoretical approach.

The average economic regression approach to econometrics ultimately has its roots in Haavelmo (1944) and has been popularised by, for example, intermediate textbooks on econometrics. This approach entails specifying a linear equation that 'explains' a particular dependent variable with reference to a set of 'independent' variables. In principle, theory defines the variables entering the equation and the appropriate equilibrium relationship to be estimated. Consequently, partial slope coefficients are estimated that act as weights in calculating the conditional average, or predicted, value of the dependent variable (for given values of the independent variables). In turn, the coefficients identify both the sign and magnitude of the influence of a particular independent variable upon the dependent variable.

As part of this procedure, the independent variables are presumed to be non-stochastic. Moreover, it is assumed that the mean of the random disturbance is zero and that the variance and covariance of the disturbances are constant and zero across observations respectively. Consequently, a great deal of effort is given over to checking these assumptions and transforming the estimated model to produce these results when necessary.

It is important to note that these procedures imply nothing about the probability distribution of the random variable. As stated, the regression model can be viewed as a purely sample-specific descriptive phenomenon. Consequently, controversy about the use of such an approach usually arises in two cases. The first of these concerns the implied direction of causality in the model and the second concerns the use of the model to 'make inductive claims with reference to the future as well as to the past' (Keynes, 1939 p566). Collectively, they arise when using the estimations to make statistical inferences. Thus in the average economic regression approach there is a sharp distinction implied between estimation and inference. This is often referred to as the 'classical form of inference'.

The issue of causality is properly discussed as an issue of identification in econometrics. Narrowly defined, identification is concerned with recovering the structural parameters of a theory from a reduced-form equation. The technical opportunity for doing this relies upon appeal to the influence of particular 'exogenous' variables. The problem of identificaton thus reduces to identifying predetermined variables. This is difficult to achieve. De Marchi and Gilbert (1989) discuss the historical debates associated with such issues. As long ago as 1938 Ragnar Frisch described the existence of these problems, which he termed 'multicollinearity'. Among other things they arose because in principle all variables, not simply some presumed 'dependent variable' modelled with causality assumed, were subject to measurement with error, that is partially determined by random influences. Consequently reordering the variables in the regression would produce entirely different estimated coefficients. In contrast, the textbook approach to multicollinearity places great stress upon the source of the problems being associated with the data rather than with the theory, or intrinsic to the regression approach (Gujarati 1999).

As far as statistical inference is concerned, it is assumed that the random influences in the model are normally distributed. Thus by appeal to the Central Limit Theorem, or specific tests of normality, significance tests of individual partial slope coefficients, the regression overall as well as various diagnostic tests proceed. It is here that Haavelmo's (1944) influence is most pronounced. Econometrics has thus become allied to both instrumental reasoning, and the ontology of event regularities. Haavelmo does not necessarily argue that the probabilistic assumptions have any real counterpart. Nonetheless, the aim of the approach is to uncover event-regularities described by the estimated equation.

The success of the average economic regression approach to discriminate between theories has been called into question by econometricians. As, for example, Pagan (1987)

and Downward (1999) note, three broad strategies have emerged in econometric discussion to address the problems associated with the approach. In essence, they are concerned with the duality in classically defined econometric inference. Thus, issues of theory estimation, discrimination and adequate statistical representation become much more fluidly defined than in the classical approach. These approaches share the agenda of starting econometric analysis from some general representation of the data and then following some criteria for model simplification. To greater or lesser degrees each approach draws upon the interplay of theory and data to achieve this objective.

The Hendry approach is presented as involving a continual interaction between theory (or subjective priors) and data (or objective facts). However it is clear that no hard and fast governing lines of demarcation exist in model appraisal (Hendry, 1995, pp1633-34). Knowledge appears to arrive from a complex interaction of deduction and induction. The basic strategy appears to follow these stages. A general model should be formulated. This is then reparameterised to obtain nearly orthogonal explanatory variables. The next stage is to simplify the model to the smallest version compatible with the data and the final stage is to evaluate the model by extensive testing. The procedure appears to be that theory decides which variables should go into the model and the data decides how to characterise the relationship.

In this latter context there are a number of crucial concepts prescribed in the employment of Hendry's method. The first is data coherency. This implies that a model should adequately characterise the evidence by not, for example, exhibiting serially correlated residuals. Parameter constancy implies that that the model's parameters are robust over sub samples. Data admissibility implies that the model satisfies definitional and data constraints. Finally valid conditioning implies that weakly exogenous variables are employed when possible. These variables are defined as those whose own estimation would have no influence on the estimation of the variable of interest (that is the variable conditional on the weakly exogenous variable).[5]

Unlike Hendry, Leamer adopts a Bayesian approach to statistical analysis. This asserts the relative primacy of subjective factors in all reasoning. It argues that traditional econometrics has degenerated into an approach that effectively employs misspecification errors as a 'protective belt' preventing the testing of the 'hard-core' propositions of economic theory. The reason for this lies in the official rhetoric of econometrics that appeals to the 'false idol of objectivity' "…unencumbered by the subjective opinions of the researcher" (Leamer, 1983, p36).
Leamer argues that it is the job of the econometrician to be 'up front' about the role of priors when engaging in econometrics. In particular econometricians must recognise that not only are economic data non- experimental but that,

> "The misspecification matrix M is therefore a pure prior concept. One must decide independent of the data how good the non-experiment is" (Leamer, 1983, p33).[6]

This is because econometrics employs priors not only in establishing the (conditional) sampling function, but also in establishing the marginal or prior probability density function. Leamer's *modus operandi* is thus to formulate a general family of models, decide what inferences are of importance, which need not be structural relations, express these in terms of parameters and form prior distributions summarising any information not in the data set. The sensitivity of inferences to a particular choice of distributions should be analysed to explore their 'fragility'.

In this respect, subsets of variables, reflecting particular interests, may be focused upon while manipulating - exploring all possible linear combinations of - what are considered to be prior 'doubtful' variables. Extreme parameter values, of a most favourable and least favourable character, can then be found for the parameters of interest. If the restrictions required to obtain a narrower range of values are considered dubious then the results should not be treated as robust.

In fact, as Pagan (1987) points out, the sensitivity analysis recommended by Leamer is implied in the Hendry approach in testing from a general to a specific model. Where the two approaches differ is in Hendry's recommendation that inference concerning point estimates should proceed from the general model if the data indicate that the complete set of variables is significant. Leamer appears to stress that this is problematic because there are conflicting (prior) grounds for inference.

Sim's (1980) approach to econometrics essentially rejects the possibility of exogeneity and identification and is confined to the analysis of reduced forms in a simultaneous or 'vector autoregressive' (VAR) context. Vector autoregressions consist of regressions of each variable of interest on lagged values of itself and the other variables under scrutiny. The largest lag structure possible is started with and then subsequently simplified in much the same manner as Hendry's approach. The objective of the Sim's methodology is to provide structure free, that is entirely reduced form, conclusions.

It is this claim that leads to one strand of criticism of the approach. VAR analysis has been presented as purely atheoretical (Darnell and Evans, 1990, p126). To the extent that Sims' approach makes claims for a theory-free econometrics this position would be clearly untenable. Sim's concern with reduced forms, moreover, does not of necessity make the approach theory free. Its intention is to avoid problems of identification and theory *choice* by emphasising data-description. The approach could thus be reasonably defended as providing potential support for a *set* of possible economic relationships.

It is clear, therefore, that the current state of applied econometrics is one of flux and change in emphasis. A number of common themes are, however, revealed in the above analysis. The first is that there is an increased recognition of the inability of econometrics to discriminate between theories though it remains an aspiration. Second, it is clear that prior data analysis is much more important in the approaches other than the average economic regression approach.[7] Thirdly, it follows that the duality between estimation and inference is broken with these approaches. Nonetheless, the emphasis on event-

regularities is reinforced. This is because probabilistic factors – or the data generating process in Hendry's language – naturally become much more prevalent in the presentation of econometric results (see also Pratten 1999). Moreover, predictions are still treated as the mirror image of explanations, and visa-versa through a 'symmetry thesis' implied by appeal to one type of evidence.(Sayer,1992, p. 133).

These points are now further discussed with a view to establishing legitimate reasons for using econometrics as a critical realist form of argument. Significantly, these reasons, it is shown also reflect the breaking down of the duality of 'estimation' and 'inference'. However, an alternative ontological perspective implies a different logic of inference. It is worth noting in this regard that Lawson's general concern with econometric methods have led him to argue that an appropriate empirical approach contributing to explanations might involve

> "The measuring and recording of states of affairs, the collection, tabulation, transformation and graphing of statistics about the economy, all have an essential (if usually non-straightforward) role to play. So do detailed case studies, oral reporting, including interviews, biographies, and so on. Indeed, I suggest it is precisely to such indispensable activities that the heading of econometrics is properly attributed." (Lawson, 1997, p. 221)

In a sense, what follows provides both a justification for this claim as well as an extension of its coverage to include regression analysis.

4. CRITICAL REALIST CONCERN WITH ECONOMETRICS: A DISCUSSION

4.1 THE LEGITIMACY OF EMPIRICAL MEASUREMENT

We begin with a discussion of the legitimacy of empirical measurement which is a precondition for statistical estimation. Critical realist concern with econometric work has often been associated with the ontological implications of empirical measurement *per se*. There are two related concerns. The first is the necessary conditions of quantification. The second concerns the non-additive nature of social scientific material.

The conditions required for performing any mathematical operation are quite clearly expressed in Allen (1962). Objects must belong to the same set. To do so requires that they exhibit formal 'equivalence', that is reflexivity, symmetry and transitivity.[8] Reflexivity essentially implies that objects are unambiguously defined. Symmetry implies that the order in which objects are compared does not affect the comparison. Finally, transitivity ensures that consistent comparisons are implied for objects not currently under consideration, but which are part of the set. As far as assigning numbers to these objects is concerned, additional assumptions need to be made. Thus 'arbitrary' numbers can be allocated to situations when cases need to be classified. In contrast ordinal 'ranks' can be established with monotonic relationships between numbers and objects. Finally, interval and cardinal measures can be established with monotonic linear transformations. In the former case various transformations need not share the same origin. This is,

however, the case in cardinal number systems.

In the econometric methods discussed above it is usually the case that interval or cardinal measures of data are employed in defining the dependent variable. This is because it is required that changes in the intensity of variables are measured.[9] In short, as Georgescu-Roegen (1966, 1971) notes, the necessary conditions for quantification imply an appeal to qualitative invariance "[w]e can measure [things] at different times or places in different conditions and know that we are not measuring different things" (Sayer, 1992, p. 177).

A basis of critical realist concern with econometrics thus must lie in the presumption that social objects are not like this; they change irreversibly and are thus not qualitatively invariant. As is well known in the post Keynesian literature, Keynes (1939) warns that there are problems of inappropriately defining econometric proxies for specific economic concepts. In this paper Keynes criticises Tinbergen for not being concerned with the units in which profits are measured. However, the point is more fundamental than this. Sayer (1992) argues that one should recognise the concept-dependence of social objects, i.e., the notion that they depend upon their meaning to society (p. 30). Thus, as observations are theory-laden, they are already pre-conceptualised, they cannot be considered 'equivalent' in the sense of necessarily belonging to the same 'mathematical set'. It is true that conventions, for example, on the reporting of profits may exist, but even so in sampling a set of firms this does not imply that profit means the same thing to the social actors concerned. The point is more easily made in considering research that seeks to tabulate and cross-classify opinions, attitudes and presume to capture notions such as the respondents socio-economic background. The notion of a random sample of such categories thus implies compromise with the principle of equivalence unless this is assumed.

A second concern of critical realists that logically follows on from this discussion is that quantitative analysis ultimately lends itself to an atomistic (Humean) ontology. Sayer (1992), for example, has argued that a requirement for quantitative analysis is that the properties referred to by the variables, which describe the system under scrutiny, must be 'parametric'. More literally, Manicas (1998) describes this as an 'additive' ontology. Neither of these characterisations is strictly speaking correct.

In the former case while each of the types of econometrics discussed above appeal to parametric forms, non-parametric methods of econometrics, for example, do not impose a prior functional form on estimates. Moreover, the results are not interpreted through estimated parameters as such. In contrast, for example, in a continuous regression, estimates are reported for close values of particular conditioning variables. Exploring the relationships sequentially can then identify the shape of the regression. In the latter case, non-linear functional forms can be identified. Either, non-linear regression techniques or log-linear transformations can be used to estimate the relationships. There remains however, a presupposition that variables can "be varied separately while retaining their identity" (Harre, 1979, p. 129). From the point of view of post Keynesian economics, students of Keynes will again recognise many of the arguments above. For instance, it

has been noted that Keynes appears to adopt an atomistic approach in the Treatise on Probability, although he might well have altered this view (Bateman, 1987; Winslow, 1989). He certainly discusses the conditions necessary for quantification and statistical inference in the Treatise and after (CW, XIV). He is clear, however, that quantification requires atomic entities (XIV, p. 286).

4.2 STATISTICAL INFERENCE

The above discussion is essentially aimed at the estimation of regression coefficients in as much that it is concerned with the process of measurement and calculation *per se*. It naturally leads onto discussions concerning the ontological nature of statistical inference implied in the econometric approaches noted above. While it is clear that statistical inference is linked to adequate empirical measurement of economic phenomena, the important issue discussed here concerns the logic that underlies statistical inference.

The econometric methods discussed above appeal to a measurable probability distribution to quantify the 'measurement errors' associated with the dependent variable. This distribution drives all of the inferential claims made for the econometric calculations. The nature of this distribution is, however, presented differently in the various approaches. For example in the average economic regression approach, drawing upon Haavelmo, there is no presumption that the distribution necessarily captures real properties of the economic system. This partially explains why econometrics is often described as instrumentalist reasoning (Lawson 1989b). In contrast for Hendry, the notion of a data-generating process appears to be a literal possibility (Hendry *et al*, 1990). Moreover, for Leamer, the probabilistic mechanism must, ultimately exist as a subjective entity. There is thus scope for debate over the ontological 'character' of the probability function.

In contrast, appeal is often made to the 'law of large numbers' to argue that, in the aggregate, subjective opinions and individual errors, theories and knowledge of the world collapse onto the actual world with a much smaller margin of error than in smaller sets of observations. At best, however, such statistical 'laws' are abstract mechanical forms of aggregation that are imposed without any ontological justification. In this respect the basis of the emergence of the 'higher order' regularities is not given context. How the subjectivity of beliefs becomes reflected in objective material is simply not addressed. At worst thus they remain an instrumentalist assumption. Moreover, Hendry ultimately argues that the 'proof of the pudding' of the existence of underlying invariance upon which to base inferences 'is in the eating' (Hendry, 1997, p18). Thus, despite these differences it could be argued that they all share a latent or explicit instrumentalism. This is important. Instrumentalism, implies that ontological and epistemological claims are formally synonymous. As Lawson (1997) argues, an epistemic fallacy is committed through the conflation of the object and subject of analysis. The variety of econometric approaches noted above thus redefine but do not address this fallacy.

The generally problematic logic of econometric inference can now be more forcefully restated in critical-realist terms. It is clear that both the estimation of regression

coefficients and the drawing of statistical inferences require the invocation of the closure conditions noted earlier. In the explicit context of the econometric approaches discussed above, the intrinsic condition of closure is equivalent to assuming the underlying homogeneity of nature (Lawson, 1989a, p. 240-1) and the atomistic combination of objects. This is required to ensure that the coefficients of a satisfactory regression are constant over time (or space). This is regardless of how they are described as satisfactory in each method. The extrinsic condition of closure (ECC) implies that all of the causal factors have been included in an econometric study, or that the effect of external factors on internal factors is constant. This last point is equivalent to assuming that countervailing factors are constant. In terms of econometrics, this assumes that all other absent factors can be captured in the stochastic error term. This is why econometrics is characterised as "post-Humean regularity stochasticism" (Lawson, 1997, p. 76). Despite the apparent differences of emphasis with the econometric approaches discussed above, therefore, measurable probabilities perform the same nomological function in each approach to econometric inference. Econometrics, as so defined, thus remains tied to the analysis of, and search for, event-regularities and is inextricably linked to instrumentalism.

4.3 EMPIRICAL WORK AND CRITICAL REALISM

The discussion above suggests that logically speaking, empirical work will imply making closure assumptions. Descriptive empirical references will assume that material is qualitatively invariant – the invocation of the ICC. Commentary on any co-variation of empirical categories that follows, moreover, implies that they are related in an atomistic way. Statistical inference finally will entail the ECC. Presented this way, econometric analysis confirms Bhaskar's argument that,

> "[t]he pure scientist…deliberately excludes, whereas the applied scientist always seeks to accommodate, the effects of intervening levels of reality…The applied scientist is an instrumentalist and a conservative, the pure scientist is a [CR] and (at the highest level) a revolutionary." (Bhaskar, 1978, p. 120)

This said, the upshot of this discussion implies that even Lawson's (1997) redefinition of econometrics remains tied to instrumental reasoning. Herein lies an important dilemma for critical realists. On the one hand pure philosophical discourse about the open-system nature of reality rejects instrumental reasoning. On the other hand, references to reality through empirical claims require that this reasoning be employed.

It follows that if empirical reasoning is to be at all employed by critical realists, the practice must, in some sense, compromise the philosophical underpinnings of critical realism. The only solution thus appears to be to minimise the potential problems of engaging in such discussion. Practice must inform philosophy. If this is not the case then the critical realist programme may find it difficult to make headway in a world impatient for practical solutions.

5. THE PRACTICE OF CRITICAL REALIST EMPIRICAL WORK

Importantly, the above arguments do not reflect particularly unique themes. Keynes was well aware of the conflict between philosophical and practical needs. It is also worth noting that many early econometricians or applied economists were aware of the problems of applied econometrics. It follows that lessons can be learned from deliberating upon how these issues have been approached in the past. Significantly, many of these ideas are strongly associated with the post Keynesian Literature. Nonetheless, it could be argued that critical realism provides the framework within which one can bring together these rather disparate discussions. Consequently, the arguments that follow reflect the interaction of philosophy and practice. They suggest a very potent way in which critical realism can make a difference to economic analysis.

5.1 ADMINISTERED PRICES AS RETRODUCTION

An interesting example from which discussion can begin is Gardiner Means' work on Administered Prices. Along with other post Keynesian pricing theories, one can argue that a sensible interpretation of Means' work can be offered from a critical-realist perspective (Downward, 2000). In more detail, Lee and Downward (2000), argue that Means used graphical and descriptive empirical analysis, mainly of Bureau of Labour Statistics data, to identify differential patterns of pricing behaviour over the business cycle. Central to explaining these patterns, Means provided a detailed discussion of the business enterprise and in particular full-cost pricing and the flow principle of production. Means argued that these concepts explained why, for example in a recession, manufacturing prices changed less frequently and by a smaller magnitude that market prices in, for example, the agricultural sector. Means also conducted simple regression analysis to calculate a relationship between the magnitude of price changes and industrial concentration as a proxy for administered pricing processes. [10] The slope coefficient indicated that in a period of declining prices generally, that is during the Great Depression, the magnitude of price changes was positively associated with the degree of concentration in the industry. Typically, thus, manufacturing was associated with less responsive changes in prices to the business cycle. Such data descriptive results were thus explained with reference to qualitative insights.

This approach, whose results have been embraced by post Keynesians, clearly captures elements of Lawson's (1997) reinterpretation of econometrics discussed above and, moreover, could be argued to be a clear example of retroductive activity. Results described in terms of empirical events are explained in terms of historical and more qualitatively described causes. Significantly, it seems clear that the regression calculation, as a sample specific descriptive analysis, is entirely consistent with this approach.

One reason for this is that the qualitative and quantitative work both invokes closure. As Downward (1999, 2000) argues appeal to qualitative evidence implies assuming intrinsic closure. There are two reasons for this. On the one hand, in the limit all empirical discourse relies on some means of collecting, collating and ordering data. 'Collecting' together insights to produce a stylised interpretation likewise. On the other hand, appeal to such conceptual descriptions such as 'administered pricing' in a generalised manner

implies the invariance of the essential qualities of the pricing process.

This suggests that a conventional distinction between quantitative and qualitative work is not particularly helpful and, indeed, misleading. Thus while Sayer (1992) argues that

"Sometimes the discovery of empirical regularities may draw attention to objects whose causal powers might be responsible for the pattern and to conditions which are necessary for their existence and activation. But in order to confirm these, *qualitative information* is needed on the nature of the objects involved and not merely more *quantitative data* on empirical associations." (Sayer, 1992, p. 114 emphasis added)

Sayer simply restates an unhelpful dualism. In contrast, what is presented is a difference in *emphasis.*The overall shift of emphasis between 'quantitative' and 'qualitative' work hinges on the fact that qualitative research techniques are concerned with interpretation rather than simply describing patterns of numbers *per se.* Nonetheless, 'empirical regularities' are sought after albeit in the form of interpretations of original data and observation. Indeed regularities at the level of interpretation will form the basis of notions such as 'conventional and accepted knowledge', as to what is accepted as 'scientific discovery'. It remains, that even if one is offering a purely context-specific account of some phenomenon that some assumptions about the constancy of structure and composition of observations are maintained in combining various insights.

This is not to say that the approaches are formally equivalent. In an interpretative context, as opposed to a data-descriptive quantitative context, one can argue that the real would be revealed by a conceptual refocus on process and action, rather than events and outcomes. This may, for example, involve a literary shift from nouns referring to states, to verbs describing processes. As a result 'estimation' and 'inference' would intertwine in a discourse that employs an appropriate ontological vernacular. This apparently subtle change hints that while rejecting the proposition that econometrics can get to the 'Truth' (which as fallibilists, realists would reject anyway), it can be argued that econometrics can be useful as part of a broad set of information on which (partial) inferences are based.

Faced with such an argument, this suggests that differences in econometric emphasis can be understood. It is clear, for example, that Means' use of regression analysis as a sample-specific description differs from that of, say, a cointegration analysis motivated by Hendry's approach to econometrics. The latter requires much probabilistic pre-testing of data series prior to engaging in the 'testing' of theory. While both approaches make the same assumptions about the additivity, constancy of data points, events *etc* the inferential emphasis is different. One nomologically appeals to a probability distribution. The other approach appeals to 'real' aspects of business behaviour and a particular causal mechanism. Consequently, both approaches appeal to an ontologically distinct invariant structure upon which to raise the inductive claims made from regression analysis. There is thus a logical difference between the application of the methods even though similar epistemological assumptions are made in the process of exploring the data.

This is an important distinction. It illustrates the claim made by critical realists that empirical work cannot be used to discriminate between hypotheses per se. Nonetheless, it also illustrates that theory evaluation will require more than the rhetoric of falsification, instrumentalism and, in general, appeal to one type of data. Ontological considerations *must* play a role in theory choice and inference. It follows that researchers interested in applied work and eager to participate in policy discussion should not ignore regression analysis. It does suggest however, that such researchers should design a research approach that exposes the sensitivity of their insights other than by appeal to probabilistic criteria. As noted above, therefore, it becomes apparent that like the econometric approaches discussed above, 'estimation' and 'inferential' claims are integrally related and yet there is a distinct difference in approach.

5.2 ONTOLOGICAL TRIANGULATION

In terms of articulating these ideas, Keynes is instructive. Keynes emphasised the importance of rational belief rather than knowledge as a basis of argument (Keynes, 1973, p10). One can suggest that the basis of critical-realist arguments can be assessed from this perspective. This is because, as implied at length above, the nature of an open-system is that we simply cannot identify underlying truth, that is causal mechanisms, with any degree of certainty or measurable precision. As reality comprises a set of related sub-systems or domains that are not mechanically combined, these need to be understood, or at least conceptually interpreted, as empirical entities or through invoking, for example, the ICC. Thus, to the extent that the world is fundamentally an open system the cause of the events may only be partially revealed empirically because of their complex codetermination. Nonetheless, for Keynes, rational belief resides in *logical* justification. There is a relative/absolute dimension to this. Probabilities, which are not necessarily numerically defined, are, on the one hand *relative* to given evidence. However once given a body of evidence or initial proposition, probabilities concerning subsequent propositions are *absolute* or *objective*.

Crucially for Keynes, relevant evidence is ascertained through a process of negative analogy. To avoid the problem of induction, Keynes argued that one should examine a particular phenomenon in different contexts. If a phenomenon appears to be a common element between various contexts then it is this commonality that indicates the relevance of a particular phenomenon. In turn this relevance adds weight to a particular account of that phenomenon. If the different contexts reveal non-common elements, then the weight of an argument will decrease revealing our ignorance.

From an operational perspective this suggests that various empirical insights should be triangulated. However, the tenets of critical-realism suggest strongly that this should have an ontological aspiration. Consequently, while descriptive and historical analysis might be suggestive of the causal mechanisms themselves, the effects of their action can be assessed, and hence the purported causal mechanism supported, with reference to more quantitative analysis. It follows, however, that there can be no presumption that only the regression methods discussed above should be applied in this endeavour. Thus, while Frisch (1948), becoming increasingly pessimistic of the ability to discriminate between

theories with regression methods argued that because,

> 'It is very seldom indeed that we have a clear case where the statistical data can actually determine numerically an autonomous structural equation. In most cases we only get a covariational equation with a low degree of autonomy...We must look for some other means of getting information about the numerical character of our structural equations.
>
> [and]
>
> The only way possible seems to utilise to a much larger extent than we do so far the interview method i.e., we must ask persons or groups what they would do under such and such circumstances' [Frisch (1948) p370].

Frisch also proposed that the set of possible econometric relationships should be explored. Termed 'confluence analysis', Frisch argued that a data-descriptive approach be conducted by mapping out, in the form of 'Bunch Maps', the behaviour of parameters when minimising the errors in each variable sequentially. Moreover, Frisch argued that other, less parametric, methods to analysing economic data, as alternatives to the bunch map approach, could be employed. Examples of these were characteristic root analysis, principle components analysis and cluster analysis. All of these eschew the use of probability theory. Their shared idea is to explore the data 'experimentally' and more subjectively. While it is clear that aspects of this outlook are described in the econometric approaches discussed above, particularly perhaps Sims' approach, nonetheless, it follows that the critical realist could be legitimately pragmatic in looking to assess the relevance of various empirical insights. Intuitively, the procedures indicated by Frisch would be more likely to reveal contrastive demi-regularities, for example, as clusters of co-variations, than single equation estimates. It remains, however, that the latter might be legitimately sought in the analysis of particular purported causal mechanisms. Moreover, while statistical tests clearly cannot drive critical-realist empirical analysis, they need not be ignored. Their legitimacy, as with other insights, resides in the triangulation.

5.3 ECONOMETRIC PREDICTIONS?

It is clear from the above discussion that econometrics can potentially perform many tasks. Most obviously it could be used to codify the empirical level and to both facilitate and help to legitimise retroduction. A central question remains, however, concerning the role of econometric predictions. This is, of course, the traditional instrumentalist role ascribed to econometric methods. In the critical-realist literature two commentaries exist. Sayer argues that,

> "..open system predictive methods are neither completely non-explanatory nor fully explanatory but a compromise usually taking the form of a model in which some of the main processes are summarily represented by 'variables.' These 'empirical models' are fitted to existing data and extrapolated forward. They involve curve fitting but the curves are fitted

> to relationships which might be interpreted as causal: they do not attempt to model actual processes closely" (Sayer, 1992, p133)

In contrast, Dow argues that:

> "..the nature of prediction will be quite different…a process truth realist would discuss the outcome of attempts to control the money supply with reference to the likely nature and extent of resulting financial innovation (both in behaviour and institutional structure), and possibly the interplay with price-setting patterns. An event truth realist would focus rather on the predicted outcome in terms of the rate of growth of monetary aggregates and the rate of inflation." (Dow, 1990, p. 351)

These two positions appear to stress opposite intentions. Sayer appears more concerned with the prediction of events *per se*. In contrast Dow appears to be primarily concerned with qualitative change. The discussion in this paper argues that such 'duals' need not be a cause of debate. They can be transcended in the approach to econometrics discussed above. Quite clearly any quantitative prediction becomes merely a scenario whose legitimacy will rest upon the robustness of the qualitative invariance invoked as causal mechanisms and, of course, the lack of influence of countervailing causes. The discovery and robustness of such causal claims will, along with their implications, of necessity, be always open to revision. In this sense critical realism removes the 'explanation' and 'prediction' dual that has characterised econometric discussion. More importantly it clearly demonstrates the important difference that critical realism makes to economic discourse. What is encouraging is that both critical realism and post Keynesian economics, both philosophy and applied economics, appear able to benefit from such discourse. This is in terms of articulating the problems of adequately capturing real elements of economic processes, and working towards concrete analysis and policy prescription.

CONCLUSION

Mathematical and statistical methods remain a potent source of controversy in economics. Indeed many related arguments to those in this paper are present in a recent *Economic Journal* debate over formalism. Here concerns were expressed about definition: what do we mean by 'formalism'? In this paper we mirror this by asking what exactly is meant by 'econometrics'. Does this term mean merely a set of tools, or is it a programme, inevitably wrapped up with prediction, falsification and unwarranted claims of causality? This paper argues that despite concerns with econometrics in the critical-realist literature, ideas that have been cited in the Post Keynesian literature can help to establish an epistemological role for econometrics. Most notably this is because of the inevitably empirical character of realist research and the non-duality of quantitative, i.e. econometric, and qualitative, i.e. interpretive, methods. In echoing Chick (1998) comments over the applicability of formalism to open systems this paper argues that " [econometrics] is fine, but it must know its place. Economists need to debate further the

boundaries of that place" (p. 1868). In particular, careful discussion of the assumptions of empirical insights must be a central part of empirical claims.

References
TO FOLLOW

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[1] This paper has benefitted from comments from John Finch, Fred Lee and Andrew Brown.

[2] A more general discussion of the relationship between the 'realism' of Tony Lawson, Sheila Dow and Uskali Maki is provided in Downward (1999)

[3]Lawson (1997, p. 207) uses the term contrastive demi-regularities because it allows him to express the idea of partial rather than strict event regularities and also because he feels that Kaldor's term has been distorted by neo-classical economics into the concept of an idealisation.

[4]This represents a significant extension of Critical Realist epistemology. It is designed to answer the long-standing question in Critical Realism of the lack of a compensator for experimentation in social science.

[5] Thus Hendry could be said to have answered some of Keynes' (1939) concern with econometrics. Keynes (1939) is often cited as implying that regression analysis is rather pointless because of the lack of homogeneity of social material over time. It is important to note that Keynes makes a *prima facie* case here and, moreover, is concerned that Tinbergen did not account for the possibility of analysing sub-periods in the data to explore its uniformity. Keynes (1938), for example stresses the need for 'messy acquaintance with the facts'.

[6] The distinction between experimental and non-experimental data is not crucial to Leamer's arguments. Experiments can also, through design or failure to produce closure, involve bias.

[7] This is perhaps most clear in cointegration analysis which has become wedded to Hendry's approach. Here much pre-analysis of the statistical characterisitics of the data precedes any attempt to test economic relationships.

[8] Naturally, these assumptions are invoked in neoclassical economics, for example, to ensure the conditions required of preferences in consumer theory are logically robust.

[9] Of course many econometric techniques exist for dealing with data on variables that are dichotomous, ordered but not interval or cardinal, or even integer values. Moreover, non-parametric methods of analysis allow for unknown functional form (see Manski, 1991).

[10] It is worth noting that Means was one of the first economists to construct concentration indices. In this sense he is a forerunner of 'Industrial Economics'. For more on this see Lee (1998).