CRITICAL REALISM AND ECONOMETRICS: CONSTRUCTIVE DIALOGUE WITH POST KEYNESIAN ECONOMICS

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(September 2000; revised January 2002)

ABSTRACT

The philosophy of science known as critical realism, as developed in economics, offers both a methodological critique of mainstream economics and a methodological foundation for alternative economic perspectives. Post Keynesian economics is a school of thought that has often been defined in terms of its opposition to the mainstream and has, to varying degrees, become allied to critical realism. Post Keynesians have not fully developed the detailed epistemological consequences of a commitment to critical realism. One such consequence is a possible tension over the use of econometric methods. This arises because, on the one hand, many of the epistemological pronouncements of critical realism imply the suspicion of econometric methods. On the other hand, many Post Keynesians regularly employ these methods in their desire to apply economic analysis. This paper examines these tensions and shows that drawing upon Post Keynesian economics a useful symbiosis of ideas is produced which has positive implications for the practice of critical-realist empirical work including econometrics.

... a difference of opinion between practising 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, p. 31))

1. INTRODUCTION

The philosophy of science known as critical realism, as developed in economics, offers both a methodological critique of mainstream economics and a methodological foundation for alternative economic perspectives.

* This paper has benefited from comments from John Finch, Fred Lee and Andrew Brown. Also detailed comments from referees of this journal have helped to improve the exposition greatly. Very helpful feedback was also received at the Cambridge Realist Workshop Conference 2000, 5–7 May, Sidgwick Site, University of Cambridge. The usual disclaimer applies.

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Post Keynesian economics is a school of thought that has often been defined in terms of its opposition to the mainstream; and it has been accused of incoherence. For both reasons, therefore, it is perhaps not surprising that Post Keynesianism has become allied, to varying degrees, to critical realism (Dow (1985, 1990, 1992, 1998, 1999), Hamouda and Harcourt (1988), Arestis (1992), Arestis and Sawyer (1993), Lawson (1994), Dow and Hillard (1995), King (1995), Pratten (1996)).

Arestis et al. (1999) and the Fall 1999 symposium in the Journal of Post Keynesian Economics demonstrate this, as both appeal to critical realism as a means of addressing the coherence of Post Keynesian economics. Likewise, Downward (1999, 2000) has addressed the implications of critical realism for pricing theory. However, Post Keynesians have not fully developed the detailed epistemological consequences of a commitment to critical realism. One such consequence is a possible tension over the use of econometric methods. This arises because, on the one hand, many of the epistemological pronouncements of critical realism imply the suspicion of econometric methods. On the other hand, many Post Keynesians regularly employ these methods in their desire to apply economic analysis. It follows that there is a possibility of inconsistency. The same could, of course, apply to other political economists that subscribe to critical realism and yet employ formal statistical methods. This paper aims to examine these tensions.

Specifically, it is argued that on the question of econometrics critical realism is somewhat problematic. Indeed, as we assess the empirical application of theories in detail, we argue that a degree of tension exists between the philosophical pronouncements of critical realism and the consequences of referring to empirical concepts in the practice of inference. Significantly, however, it is argued that drawing upon Post Keynesian economics a useful symbiosis of ideas is produced which has positive implications for the practice of critical-realist empirical work. Thus, while critical realism has epistemological implications for Post Keynesianism, the practice and existing doctrines of Post Keynesian economics can also impact on critical realism. There is an iterative relationship between philosophy and practice.

The paper is organized as follows. Section 2 defines critical realism by drawing upon the work of Tony Lawson—summarized in Lawson (1997). Critical realism has developed from a broad set of philosophical ideas; nonetheless Lawson's work is central to discussions of critical realism in economics and hence the Post Keynesian literature. Key aspects of critical-realist ontology and epistemology are discussed. The basis of the critical-realist opposition to mainstream economic analysis
and, similarly but not equivalently, to econometric inference is provided. In order to address critical-realist concerns with econometrics, and in contrast to the generalizing nature of the critical-realist literature, Section 3 attempts to carefully define econometrics. For the purposes of discussion, the emphasis is upon comparing a traditional and a more ‘modern’ approach to econometrics. Section 4 then addresses in some detail the critical-realist case for concern with econometrics. Allied to this discussion of critical realism, section 5 argues that by drawing upon insights in the Post Keynesian literature relevant to early econometric work, and the discussion of econometric methods, one can justify the use of econometrics in a methodology based on critical realism. The conclusions noted above then follow.

2. CRITICAL REALISM: ONTOLOGY AND EPISTEMOLOGY

Critical realism in economics is primarily associated with the work of Tony Lawson. Lawson’s volume *Economics and Reality* (1997) is the outcome of a lengthy and sustained research project drawing upon the philosophy of Roy Bhaskar to provide both a critique of mainstream economics and an alternative economic methodology. Taking these two points in turn, the core of the critique is ontological, maintaining that the ‘set of methods or ways of proceeding in economics that can be shown to be ... fundamental to the mainstream project ... [are] ... largely irrelevant to the analysis of social phenomena’ (p. 16). For Lawson, mainstream economics embraces a ‘dedustivist model of explanation’ (p. 17). Explanation emphasizes constant conjunctions of events or causal-law statements of the form ‘whenever event x then event y’. This implicitly presupposes closed-system ontology. Two main conditions are assumed to support this model of explanation. The intrinsic condition of closure (ICC), which can be loosely characterized as implying that a cause always produces the same effect, suggests that the structures of the phenomena under study are constant, unchanging, and that 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. In general, therefore, the emphasis on mathematical reasoning in economics can clearly be traced to this structure of explanation. In a related manner prediction and explanation become formally equivalent. This is known as the ‘symmetry thesis’.

From an empirical point of view, this implies a search for constant
conjunctions of events. Deductivism thus becomes tied to ‘a version of positivism that is rooted in Hume’s analysis of causality’ (Lawson (1997, p. 19, emphasis in original)). Moreover, constant conjunctions can be reinterpreted to apply stochastically. The qualifier is that event y follows from event x ‘on average’ (Lawson (1997, p.69)). It is here that critical realists argue that econometrics finds its traditional role in economics, in testing the predictions/explanations associated with covarying magnitudes.

In seeking to propose an alternative methodological approach, critical realism rejects the applicability of the closure conditions, the covering law model of explanation (see Bhaskar (1978, pp. 130–33)) and hence econometrics. Reality is conceptualized as stratified into actual events, our empirical experience of them and most significantly their real causes. Importantly, these are out of phase with, and not reducible to, one another. For example, human agency is embedded in social context. Behaviour is thus irreducible to individual action per se and on the contrary is both conditional on and results in multiple sources determining events. Collectively this implies that real causes, i.e. structures and mechanisms that generate powers, and in turn, events, are the object of knowledge.\(^1\) Specifically, Lawson (1997) argues that ‘tendencies’ are the key subject of scientific endeavour. 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. Thus, causes act transfactually: they can be operative irrespective of observed events. Therefore, experiments can be understood as a means of closing off reality so as to observe one mechanism in isolation.

In the absence of experimental conditions, this critical-realist ontological position has distinctive, if underdeveloped, epistemological implications. In recognizing the structured and open nature of reality, according to Lawson (1997), the contrasting epistemological goal of theorists should be to identify causal laws by discovering the mechanisms underlying, but not equivalent to, actual events. It follows that helping to identify causes from empirical observations of events becomes a non-trivial challenge. How this goal is to be acheived, however, is not straightforward and indeed has prompted this paper.

The central concepts associated with Lawson’s (1997) epistemology appear to be the concepts of ‘reduction’, ‘demi-regularities’, ‘abstraction’ and ‘explanatory power’. Unlike deduction, which is the movement from general premises to a specific conclusion, or induction, which is

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\(^1\) See Collier (1994, pp. 61–66) for a definition and comparison of these concepts.
the movement from specific observations to a general conclusion, the mode of inferential logic espoused by Lawson is retroduction. This,

consists in 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, p.24))

Unfortunately, as far as exploring and offering an inference about causal mechanisms is concerned, no specific guidance is forthcoming from Lawson, who argues that this will depend on the context (Lawson (1997, p. 212)).

It is argued, however, that while strict event-regularities might not be observed, rough and ready patterns called ‘demi-regularities’ can ‘direct social scientific investigations’ (Lawson (1997, p. 207, emphasis in original)) towards relatively enduring mechanisms that the researcher might identify. Of particular interest are phenomena called contrastive demi-regularities or ‘anomalies’ (Sayer (1992, p. 61), Pinkstone (2000)), which refer to a systematic difference, a deviation from an expected pattern, an unexpected empirical phenomenon that highlights the need for retroduction.

Once demi-regularities have been identified, for Lawson (1997), abstraction should form the basis of conceptual enquiry. Abstraction should be concerned with what is real but involves the momentary neglect of features of reality, rather than their actual isolation. Abstractions can thus be partial but need not be false or idealizing. In this way, Lawson claims to offer a powerful critique of orthodox modelling. Finally, it is argued that ‘Theories can be assessed according to their abilities to illuminate a wide range of empirical phenomena’ (p. 212). However, explanatory power, as the criterion of theory choice, is left relatively undeveloped. Crucially, also, Lawson (1989, 1995, 1997) is critical of the role of econometrics. This essentially follows from the ontological analysis outlined above and is despite the clear empirical nature of study according to critical-realist principles. In the remainder of this paper we thus attempt to explore the conditions under which econometrics could be applied successfully in a research process consistent with critical realism. The first task in building this argument is to define econometrics carefully and to establish its properties in some detail. These can then be juxtaposed to a specific outline of critical-realist concerns with econometrics from which constructive dialogue can begin.
3. DEFINING ECONOMETRICS

Providing a simple definition of econometrics is not easy because a burgeoning set of techniques exists for the researcher who wishes to calculate statistics. Moreover, these techniques form part of various inferential frameworks. Nonetheless a number of key properties of econometric methods can be elicited by comparing various approaches to econometrics. Obviously space prevents a full discussion of the range of methods in contemporary econometrics. A broader comparison of various schools of thought in econometrics is available in Downward (1999) and Downward and Mearman (2000). For expositional ease, in this paper more attention is paid to the LSE or British approach associated particularly with Hendry, as a comparison with more traditional econometric analysis.2

Discussion begins with a consideration of the ‘average economic regression’ (AER) approach. This approach is ultimately rooted in Haavelmo (1944) and is the predecessor of all contemporary approaches. It remains central to, for example, intermediate textbooks on econometrics. The basic approach entails specifying a linear equation, such as equation (1), that ‘explains’ a particular dependent variable \( y \) with reference to a set of ‘independent’ variables \( x_k (k = 1, 2, 3) \), for a particular set of observations \( i \). \( u \) represents a random error term and \( \beta_n (n = 1, 2, 3) \) are partial slope coefficients to be estimated from the data.

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y_i = \beta_1 + \beta_2 x_{2i} + \beta_3 x_{3i} + u_i \quad (1)
\]

In principle, theory defines the variables entering the equation and hence the appropriate ‘causal’ relationship to be estimated. Regardless of the specific context, there is an implicit but strong assumption (the maintained hypothesis) that the econometric model is correctly specified (see also Gerrard (1995)). Consequently, the partial slope coefficients that are estimated 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 particular independent variables upon the dependent variable. The independent variables are presumed to be

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2 Hendry (1995a, p. xxxi) acknowledges that a significant part of his 20-year research programme has been to combine the LSE approach with the CORE (Contributions to Operations Research and Econometrics) approach of, say, J.-F. Richard. By Hendry (1995a) he seems satisfied that this has occurred.
non-stochastic. It is important to note, however, that this procedure implies nothing about the probability distribution of the random variable. As stated the regression model can be viewed as producing purely sample-specific descriptive insights.

This implies that in the AER approach a sharp distinction is drawn between estimation and inference. This is often referred to as the ‘classical form of inference’. Interestingly, it is when regression models are employed to make inferences that controversy about the use of regression analysis has arisen in the past. Concerns have arisen in applied econometrics in two interrelated areas: the implied direction of causality in the model and the use of the model to ‘make inductive claims with reference to the future as well as to the past’ (Keynes (1973, XIV, p. 315)).

The issue of causality is perhaps 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. This implies that if equation (1) is simply one of a number of structural equations in a system of equations, its parameters cannot be directly estimated. Consequently, the model needs to be solved to produce a single equation in which the independent variables are not endogenous and hence related to the random error terms in the model. The problem is then to recover the parameters of the structural equation. The technical opportunity for doing this relies upon appeal to the influence of particular ‘exogenous’ or predetermined variables.

The problem of identification thus reduces to the identification of such variables. This is difficult to achieve. DeMarchi and Gilbert (1989) discuss the historical debates associated with such issues. As long ago as 1948, 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’, 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 this respect, multicollinearity was associated with model selection. In contrast, the AER approach stresses that the source of the multicollinearity is the data rather than the theory, i.e. multicollinearity is not intrinsic to the regression approach (see for example Gujarati (1999, ch. 10)). The collection of more data, or its manipulation, is offered as a remedy.

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3 Hereafter, Keynes’s works will be referenced only by their volume number in his Collected Works (1973).
As implied above, this prescriptive emphasis on data manipulation carries over to statistical inference. In general it is assumed that the model is correctly specified. Moreover, it is assumed that the random influences in the model, $u_i$, are normally distributed, that the mean of the random disturbances is zero and that the variance and covariance of the disturbances are, respectively, constant and zero across observations. It is the latter assumptions that become the focus of the econometrician. The first is either justified by appeal to the central limit theorem or assessed by tests of normality. Problems with normality are then dealt with by, say, controlling for ‘outlier’ observations. The last two assumptions are addressed by various other diagnostic statistics for heteroscedasticity and autocorrelation. Transformations of data—captured in generalized least squares models—provide typical strategies for rectifying problems identified with the inapplicability of these assumptions about the random error terms. Once corrected, significance tests of individual partial slope coefficients and the regression overall proceed.

It is not surprising that the influence of the AER has persisted and become embedded in econometrics textbooks. Its progenitor, Haavelmo (1944), argues that the essence of econometrics is to build, identify, estimate and assess various models conforming to the optimizing behaviour given by neoclassical precepts—the maintained hypotheses. A further consequence of this is that AER econometrics has helped to promote the idea—arguably held by critical realism—that (mainstream) economics and econometric techniques are inseparably related. Further impetus has naturally followed from the act of linking econometrics to a form of instrumentalist reasoning. While this issue is discussed further below, it should be noted that in adding a stochastic term to deterministic theoretical concepts Haavelmo (1944, p. 43) insists that the approach does not necessarily require that probabilities have any real counterpart. Nonetheless, the aim of the approach is to uncover empirical regularities described by the estimated equation. In critical-realist jargon, the aim is to focus upon event-regularities.

The success of the AER approach to discriminate between theories has been called into question, however, by econometricians, prompting the broad developments in econometrics noted earlier. Indeed Hendry (1995a, p. 18) describes this situation as reflecting a ‘methodological crisis’. As, for example, Pagan (1987), Downward (1999) and Downward and Mearman (2000) note, the strategies to cope with this crisis have been directly concerned with the duality in classically defined econometric inference. Thus, issues of theory estimation, discrimination and adequate statistical representation become defined much more fluidly than
in the classical approach. These new approaches share the agenda of starting econometric analysis from some general representation of the data and then following some criteria for model simplification.

One such approach, derived from David Hendry, has become known as the Hendry/LSE method. It stems from developments in time series econometrics. Central to Hendry’s research programme is a continual interaction between theory and data; thus, knowledge appears to arrive from a complex interaction of deduction and induction. In principle no simple rules for choosing between models exists (Hendry (1995b, pp. 1633–34)). The basic strategy is of moving iteratively from the general to the simple (Hendry (1979, p. 218)).

The approach makes use of three crucial concepts. The first is the data generation process (DGP). The DGP is ‘the actual process which generated data’ (1995a, p. 55). It is the combination of an economic mechanism, or set of mechanisms, and the measurement system that links events and observations. The DGP need not be probabilistic but it is assumed that it can be *presented* as such. Consequently, the second aspect of Hendry’s approach is to recognize that a statistical generating mechanism represents the assumed stochastic behaviour. The final concept is that of a general unrestricted model (GUM). Such theoretical models are conjectured (tentative) representations of the DGP (1995a, p. 55) and are the starting point of analysis. Consequently they should be ‘formulated to contain the parsimonious, interpretable, and invariant econometric model at which it is hoped the modelling exercise will end’ (1995a, p. 361). The object of econometric modelling then becomes an attempt to obtain ‘structural knowledge’ of the DGP via a series of ‘reductions; to the GUM. The GUM usually involves current and lagged values of independent variables. This is then reparameterized to obtain nearly orthogonal explanatory variables. The next stage is to simplify the model to the most parsimonious version compatible with the data. The final stage is to evaluate the model by extensive testing. To assess whether or not the final model is an adequate representation of the DGP, it should satisfy certain key criteria associated with, for example, serial correlation, parameter constancy and so on (see Hendry (1995a, p. 366). Underpinning the approach, however, is predictive success. Indeed this is implicit in the definition of invariance above (Hendry (1995a, p. 361)). Significantly, Clements and Hendry (1995, p. 1033) and Hendry (1997, p. 1331) both claim that successful econometric forecasts depend on the

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4 Hendry and Richard (1982, p. 377) define the goal as to find a tentatively adequate conditional data characterization with essentially the same criteria as discussed below.
existence of event-regularities that can be captured by the econometrician. In practice, therefore, the procedure appears to be that theory decides which variables should go into the model and the data decide how to characterize the relationship. This is demonstrated, for example, in the focus upon error-correction models in the cointegration analysis of time series. Theory describes the long-run relationship. The data reveal the short-run dynamics of adjustment.

As the above makes clear, the current state of applied econometrics is one of flux and change in emphasis. However, a number of general themes can be identified in discussing Hendry’s 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 modern approaches than in the AER approach. Third, it follows that the duality between estimation and inference is less apparent with these approaches. Despite these differences of emphasis with the AER approach, however, the underlying focus remains firmly allied to isolating event-regularities. Indeed, one can argue that this focus is even more sharply drawn in approaches such as Hendry’s because probabilistic factors—or the representation of the DGP in Hendry’s language—naturally become much more prevalent in the presentation of econometric results (see also Pratten (1999)). Moreover, in both the AER and Hendry approaches predictions are still treated as the mirror image of explanations, and vice versa through the ‘symmetry thesis’ noted above.

4. CRITICAL-REALIST CONCERN WITH ECONOMETRICS: A DISCUSSION

4.1 Economics and reality

The above analysis outlined, in turn, some of the main aspects of the ontology and epistemology of critical realism and the key properties of econometric analysis. Attention now turns to the specific critical-realist concerns with econometrics. Once these are clearly stated and understood a response can be made.

A useful starting point is chapter 7 of *Economics and Reality*. Here, Lawson begins the discussion at the general level discussed in section 2. More direct criticism is then targeted at the estimation of parametric functions. Thus, Lawson also argues that the criticism may not apply to the ‘determination of summary statistics of a body of data’ (p. 69), or to...
case studies, interviews and so on. In effect, he redefines econometrics in terms of the latter techniques (p. 221).

In contrast, Lawson’s objections to parametric functions are made with reference to a reformulation of the Lucas critique. The original critique argued that optimizing behaviour evolved in the light of policy interventions. Such structural instability undermined econometric analysis. Lawson argues that this result is indicative of the lack of applicability of the closure conditions noted earlier. It is clear, therefore, that these conditions need further examination. This is now undertaken in connection with the ‘twin’ branches of statistical inference referred to in the discussion of econometric methods.

4.2 The legitimacy of empirical measurement

We begin with a discussion of the legitimacy of empirical measurement, which is a precondition for statistical estimation. Econometricians have underplayed this issue. For example, Hendry (1995a, p. 345) writes, ‘[t]he assumption that economic events are measurable is not unreasonable’. Conversely, 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 involves the non-additive nature of social science material.

The basic conditions required for performing any mathematical (and statistical) operation are quite clearly expressed by Allen (1962). Objects must belong to the same set, which requires that they exhibit formal ‘equivalence’, i.e. reflexivity, symmetry and transitivity. Reflexivity essentially implies that objects are defined unambiguously. 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 form

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5 This illustrates that as well as offering new philosophical ideas and concepts the framework of critical realism has looked to assimilate a variety of specific criticisms within its domain. Other examples of this follow in the next sections. Utilising Lakatosian phrasing this suggests that critical realism is ‘progressive’ in its outlook. What its ‘protective belt’ might comprise remains to be seen. In one sense this paper probes hard for some response.

6 Naturally, these assumptions are invoked in neoclassical economics, e.g. to ensure that the conditions required of preferences in consumer theory are logically robust. Likewise, the most basic of descriptive and inferential statistics (e.g. frequency counts and non-parametric tests respectively) require this assumption.
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 the case, however, in cardinal number systems.

As implied in the discussion above, in the most widely used econometric methods interval or cardinal measures of data are employed to define the dependent variable. One main reason for this, of course, is that mainstream economic theory—which is based on an epistemology of predicting events vis-à-vis claims of ceteris paribus—requires that changes in the intensity of variables be measured.\(^7\) Nonetheless, even in attempts to capture qualitative features, in short, as Georgescu-Roegen (1966, 1971) notes, the necessary conditions for quantification imply an appeal to qualitative invariance, namely ‘[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)).

Therefore, a basic element of critical-realist concern with econometrics should lie in the presumption that social objects are not like this; they change irreversibly and are thus not qualitatively invariant. Unfortunately, critical realism has hardly discussed this in the context of economics. But, of course, this is well known in the Post Keynesian literature. In his criticisms of Tinbergen (in 1939), amongst other things, Keynes (XIV, pp. 309–11) warns that there are problems of inappropriately defining econometric proxies for specific economic concepts. Keynes criticizes Tinbergen for not being concerned with the units in which profits are measured. However, the point is more fundamental than this. From an explicit, but non-economic, critical-realist perspective Sayer (1992) argues that one should recognize the concept dependence of social objects, i.e. the notion that they depend upon their meaning to society (p. 30).

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. From the point of view of Post-Keynesian economics, students of Keynes will also recognize many of the arguments above. For instance, it has been noted that Keynes appears to

\(^7\) 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)).
adopt an atomistic approach in the *Treatise on Probability*, although he might well have altered this view (Bateman (1987), Winslow (1993)). He certainly discusses the conditions necessary for quantification and statistical inference in the *Treatise* and after (Keynes, XIV). Keynes is clear, however, that quantification requires atomic entities (XIV, p. 286).

### 4.3 Statistical inference

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

In the final analysis, as discussed above, econometric methods 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, however, is presented differently in the various approaches. For example in the AER approach, drawing upon Haavelmo, there is no presumption that the distribution necessarily captures real properties of the economic system. As discussed earlier this helps to explain why econometrics is often described as instrumentalist reasoning (Lawson (1989)). In contrast, for Hendry the notion of a DGP appears to be a literal possibility (Hendry *et al.* (1990)). There is thus scope for debate over the ontological ‘character’ of the probability function.\(^8\)

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\(^8\) This claim has led Pratten (1999) to claim that Hendry is a ‘realist’, although probably in the Humean sense (see Hendry and Richard (1982, p. 363)). Indeed, the *existence* of the DGP was the major point of contention between Lawson (1981, 1983) and Hendry (1983). Lawson’s critique concerned apparent claims by Hendry that his iterative approach could lead to the discovery of the ‘true’ DGP *and* in equation form. Hendry (1983, and subsequently) has reiterated the equation form of the DGP but has shown some inconsistency on the question of ‘truth’ (cf. Davidson *et al.* (1978, pp. 663, 686)). More generally, as discussed in Downward and Mearman (2000), econometricians have exercised direct concern with the shape of the distribution of the dependent variable in attempts to measure qualitative relationships. Moreover, for Bayesian econometricians like Leamer, the probabilistic mechanism must ultimately exist as a subjective entity.

\(^9\) Even non-parametric methods appeal to independent and identical drawings in making inferences. In this respect they differ from parametric approaches in not prescribing the form of probability distribution. Ontologically speaking, therefore, the differences are one of detail not kind.
Hendry, however, ultimately argues that the ‘proof of the pudding’ of the existence of an underlying invariance upon which to base inferences ‘is in the eating’ (Hendry (1995a, p. 18)). Thus, despite these differences it could be argued that, in general, the approaches share a latent or explicit instrumentalism. This is important because instrumentalism implies that ontological and epistemological claims are formally synonymous. Critical realism argues that this commits an epistemic fallacy through the conflation of the object and subject of analysis. The econometric approaches noted above thus could be said to redefine but not avoid this fallacy.

Having outlined the main aspects of econometric methods, and the associated critical-realist concern with the general assumptions that are made in both producing estimates of parameters and drawing statistical inferences, the generally problematic logic of econometric inference can now be more forcefully restated in critical-realist terms. It is argued 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 ICC is equivalent to assuming the underlying homogeneity of nature (Lawson (1989, pp. 240–41)) and the atomistic combination of objects. This is required to ensure that data points can be legitimately combined in calculating regression coefficients and associated statistics, and that the variables can, in turn, be legitimately combined according to the functional form of the model. The 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. Despite apparent differences of emphasis between econometric approaches, therefore, measurable probabilities ultimately perform the same nomological function in econometric inference. In the literature thus, econometrics, as so defined, remains tied to the analysis of, and search for, event-regularities and is, for critical-realists, inextricably linked to instrumentalism. Given that instrumentalism is typically cast as opposed to realism, and given that critical realism regards the search for event-regularities as essentially misguided, its critique of econometrics must be regarded as significant.

4.4 Empirical work and critical realism: the need for a modified position

The discussion above, however, can be generalized beyond the specific econometric approaches discussed. Drawing upon the critical-realist
critique, logically speaking, any 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. Finally any form of statistical inference will entail the ECC. Presented this way, the argument confirms Bhaskar’s claim that

The applied scientist is an instrumentalist and a conservative, the pure scientist is a [critical realist] and (at the highest level) a revolutionary. (Bhaskar (1978, p. 120))

Now an important consequence of this argument is that this criticism also applies to Lawson’s (1997) redefinition of econometrics noted earlier. The implication is that admitting the possibility of statistical analysis of one sort means that it is not straightforward to rule out any particular method of analysis. Thus, critical realism’s fundamental critique of econometrics might imply that the proposed alternative methods are equally subject to that critique.

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. Put more positively, and in a manner consistent with the critical-realist rhetoric, the dual between instrumentalism and realism needs to be transcended and a finer grade of distinction developed between the uses of empirical methods. The solution, we would argue, lies in minimizing the well thought out but general problems of empirical work identified by critical realism. In this sense, 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. It is argued that in this regard Post Keynesianism can aid the development of critical realism.

5. THE PRACTICE OF CRITICAL-REALIST EMPIRICAL WORK: SOME PROPOSALS

The task therefore is to establish legitimate reasons for using econometrics whilst remaining sensitive to critical-realist criticisms of econometrics. Significantly, these reasons also reflect the breaking down of the duality of ‘estimation’ and ‘inference’ as implied in modern econometric
approaches. However, an alternative ontological perspective—one informed by critical realism—implies a different logic of inference.

A useful starting point, as alluded to earlier, is that many of the arguments raised by critical realism have been aired elsewhere historically. In this sense the arguments against econometrics do not reflect particularly unique themes. From a Post Keynesian perspective, it should be noted that Keynes was well aware of the conflict between philosophical and practical needs. It is also worth noting that, along with Keynes, many early econometricians or applied economists were aware of the problems of applied econometrics (Lawson, 1989). It follows that lessons can be learned from deliberating upon how these issues have been approached in the past. Inasmuch as critical realism has become a framework within which these criticisms have been restated, it follows that it can provide a framework within which potential solutions might be discussed.

5.1 Administered prices as retroduction

An interesting example from which discussion can begin is Gardiner Means's work on administered prices. The discussion above suggests that a key feature of critical-realist thinking is that explanation proceeds via a process of retroduction. This involves moving ‘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’. It is clear that Means, an early contributor to what is now regarded as Post Keynesian pricing theory, shared this epistemological objective (Downward (2000)).

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. To explain these patterns in the data, Means provided a detailed discussion, driven by case studies, of the business enterprise—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 than market prices in, for example, the agricultural sector. Means also constructed by eye a simple ‘regression’ analysis to calculate a relationship between the magnitude of price changes and industrial concentration as a proxy for administered pricing processes. The slope coefficient indicated that in a period of declining prices generally, i.e. 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.

While Means’s econometrics were very rudimentary, it is clear that they were useful to the research in helping to specify phenomena to be explored and contribute to the broader historical explanation of pricing he offered. Significantly, Lee and Downward (1999), using modern econometric methods and Means’s original data, broadly confirm his initial insights. It is contended here, therefore, that regression-based analysis can contribute to the specification of ‘demi-regularities’ from which causal analysis can proceed, drawing upon more historical and qualitatively based analysis. This apparently straightforward idea does contain some important ramifications, however. On the one hand, qualitative work plays an integral and explicit part in drawing inferences. This approach is therefore differentiated from other econometric approaches such as Hendry’s, in which qualitative (and theoretical) factors are relevant only prior to inference. This argument is elaborated below. On the other hand, our interpretation of Means moves beyond redefinitions of econometrics such as Lawson’s (1997), because regression calculation, at a minimum as sample-specific descriptive analysis, is an entirely consistent part of this approach. In short, in this paper we argue that retroduction should involve the ‘triangulation’ of types of empirical analyses. Further elaboration and justification of this claim can be offered.

5.2 Retroduction as triangulation

To begin with, as implied earlier many research techniques differ more in emphasis than in kind. In this respect it should be noted that both qualitative and quantitative work invokes closure (Downward (1999, 2000), Downward et al. (forthcoming)). For example, for two reasons, even an appeal to qualitative evidence implies the assumption of intrinsic closure. First, in the limit, all empirical discourse relies on some means of collecting, collating and ordering data. ‘Collecting’ together insights to produce a stylized interpretation is identical in this respect. Second,

10 The positive slope is based on a regression line rising from a negative intercept on the ‘magnitude of price change’ axis. During the Depression all prices fell, i.e. price changes were negative, but the fall was less pronounced for more concentrated industries, as measured on the other axis.
however, appeal to conceptual descriptions such as ‘administered pricing’ in a generalized manner implies the invariance of the essential qualities of the pricing process.

This explains why Lawson’s attempt to redefine econometrics is problematic. More generally we suggest that the conventional dualistic distinction between quantitative and qualitative work, offered for example by the critical realist Sayer (1992, p. 114), is not particularly helpful and, indeed, is misleading. 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 observations.

This is not to say that quantitative and qualitative 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 involve, for example, a literary shift from nouns referring to states, to verbs describing processes. It remains therefore that ‘qualitative’ work by definition has more flexibility, e.g. in identifying the sources and reasons for changes in structures. However, recognizing these different features of empirical research implies that ‘estimation’ and ‘inference’ would necessarily intertwine in explanations that draw upon a variety of methods.

These arguments also suggest how differences in the use of econometric techniques may be understood in a more subtle way than currently dominates the literature. It is clear, for example, that Means’s early use of regression analysis as a sample-specific description differs from (and is technically inferior to) that of, say, a cointegration analysis motivated by Hendry’s approach to econometrics. If nothing else 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 additivity, the constancy of data points etc., the inferential emphasis is different. The latter approach appeals nomologically to a probability distribution per se. The former appeals to ‘real’ aspects of business behaviour and a particular causal mechanism.\(^\text{11}\)

\(^\text{11}\) A related argument can be constructed from Finch and McMaster’s (2000) argument that non-parametric methods of analysis may be useful, from a critical-realist perspective, as a means of checking the robustness of categories and interpretation. One could easily see that work of this nature might thus be a bridging point for further statistical analysis of the kind advocated in this paper as part of a triangulation strategy of inference.
While this argument is different from that, say espoused by Lawson (1997), significantly it retains the claim made by critical realists that empirical work cannot be used to discriminate between hypotheses per se. 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. However, it follows that researchers interested in applied work and eager to participate in policy discussion should not ignore econometric analysis but researchers should design a research approach that exposes the sensitivity of their insights other than by appeal to probabilistic criteria.

From an explicitly Post Keynesian perspective, Keynes (VIII, XIV, pp. 285–320) is also instructive, as he articulates the need for triangulating insights. As noted earlier, it is without doubt that Keynes argued that quantification of probabilities required reference to atomic entities. Moreover, Keynes’s (XIV, pp. 306–20) criticism of Tinbergen’s econometric analysis can be cited as a precursor to critical-realist concerns. Yet, more generally Keynes emphasized the importance of rational belief rather than knowledge as a basis of argument (Keynes (VIII, p. 10)). 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 common across various contexts then this indicates its relevance. 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.

Thus, from an operational perspective, once again this suggests that various empirical insights should be triangulated. The added contribution of critical realism is to suggest strongly that this should have an ontological aspiration. Consequently, while descriptive and historical analysis might themselves be suggestive of the causal mechanisms, 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, p. 370) was increasingly pessimistic of the
ability to discriminate between theories with regression methods, he 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 charting, in the form of ‘bunch maps’, the behaviour of parameters when minimizing the errors in each variable sequentially. Moreover, Frisch argued that other, less parametric, methods for analysing economic data could be employed as alternatives to the bunch map approach. Examples of these were characteristic root analysis, principal 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 referred to in various econometric approaches such as Hendry’s as discussed above, 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, e.g. 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-realistic empirical analysis, they need not be ignored. Their legitimacy, as with other insights, resides in the triangulation. This, it is argued, is a significant addition to the critical-realistic approach.

In closing this discussion it might be claimed that triangulation is already a common practice and/or an implicit aspiration subscribed to by, for example, neoclassical economists and applied econometricians. This may well be; however, it is not at all apparent in the vast literature on applied economics. The evidence is stark. First, there appears to be a relative absence of alternative methods, such as the case study, in orthodox literature. Blinder et al. (1998) claim a novelty of approach against this background. Second, claims of triangulation in for example Hendry might be based on text such as: ‘while understanding the probability structure of hypothetical cases is an essential tool, it is only the first of many necessary but insufficient tools for the econometric analysis of empirical evidence’ (Hendry (1995a, p. 16)); and ‘[e]stimation methods or estimators are a second important tool in our tool kit and ... [are] ... necessary but insufficient for solving the model discovery problem’ (pp. 16–17). However, such appeals are made prior to, and in the aid of, purely econometric analysis. Third, an implicit assumption of our approach (and indeed Keynes’s) is that econometric analysis is both fallible and subject to critical-realistic objections. Although
Hendry admits fallibility, in that he does not make strong claims for the ‘truth’ (in various senses) of his models, there is nothing in his approach that regards econometrics as inherently problematic that requires triangulation as a strategy. Thus, the approach proposed here is critical of both critical realism and econometrics, whilst rejecting neither.

5.3 Econometric predictions?

It is clear from the above discussion that econometrics potentially can perform many tasks. Most obviously it could be used to codify the empirical level (as demi-regularities) and to both facilitate and help to legitimize 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 realist literature two commentaries exist. One, such as Sayer (1992, p. 133), argues that prediction is a ‘curve fitting’ exercise with no attempt to model actual processes closely. Another, e.g. put forward by the Post Keynesian Dow (1990, pp. 351–52), emphasizes the need to account for structural change.

These two positions appear to stress opposite intentions. Sayer appears more concerned with the prediction of events per se. In contrast Dow’s substantive methodological point 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, along with their implications, will of necessity always be open to revision. Likewise this discussion suggests how, for example, simply addressing ‘regime shifts’ or ‘structural breaks’ as in, say, Hendry’s econometric approach does not fully address criticalrealist concerns.

In this sense, along with the quantitative–qualitative and estimation–inference duals, a modified perspective on critical realism removes the ‘explanation’/’prediction’ dual that has characterized econometric discussion. More importantly it clearly demonstrates the important difference

12 However, this is not to say that critical realism holds to a ‘symmetry’ thesis: clearly it cannot in open systems.
that critical realism makes to economic discourse. What is encouraging and significant, though, is that both critical realism and Post Keynesian economics, i.e. philosophy and applied economics, appear to be able to benefit from—and contribute to—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.

6. CONCLUSION

Mathematical and statistical methods remain a potent source of controversy in economics. Post Keynesianism has become part of this controversy by embracing critical realism and econometric methods, and yet critical realists effectively reject econometric methods. However, in this paper we argue that critical realism itself exhibits tensions on the question of econometrics. Specifically, it is argued, critical realism has tended to rule out econometrics except in very specific conditions, yet espouses other methods that might be problematic for similar reasons. These reasons focus on the ontological requirements for quantification and inference. Crucially, we argue that 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 non-duality of quantitative, i.e. econometric, and qualitative, i.e. interpretative, methods. Thus, while critical realism impacts on Post Keynesianism, the relationship is not one-way. In fact via a constructive dialogue, Post Keynesianism feeds back to critical realism. Consequently, we argue that empirical methods have an inevitable and indeed key role to play in economic inferences. The applied researcher wishing to face up to the philosophical criticisms made by critical realism should make sensible use of whatever empirical methods are at disposal. Drawing on Keynes’s arguments, the important guideline for a researcher is that insights need to be triangulated. The added feature of the critical-realist literature is that endeavours should be explicitly guided by ontological matters in producing inferences. Thus, methods such as interviews and case studies that help to establish context-specific understanding further, by exploring the meaning and mechanisms of particular processes, need to be allied to other methods that begin to explore their generality in the sense that similar demi-regularities might be detected. In sum, in echoing Chick’s (1998) comments on the applicability of formalism to open systems, we argue that ‘[econometrics] is fine, but it must know its place’ (p. 1868).
This represents a modification of the critical-realist position and a strengthening of Post Keynesian practice.

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