

## REALISTIC MODELS? CRITICAL REALISM AND STATISTICAL MODELS IN THE SOCIAL SCIENCES

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### 1. Introduction

My aim in this paper is to question the scepticism of critical realist philosophers of science in relation to the use of statistical methods in social science research. By arguing that statistical analysis is inevitably 'deductivist' in nature (Bhaskar, 1998a; Lawson, 1997, 1998, 2001; Pratten, 1999), I believe that critical realists merely reinforce the influence of empiricism<sup>1</sup>. Moreover, by confining their criticism of statistics to the social sciences, these writers adopt an unwarranted anti-naturalist stance. In contrast, I will argue that critical realism can help to resolve a number of philosophical problems in relation to the specification, assessment and interpretation of statistical models. Social scientists are increasingly aware of these issues (Cliff, 1983; Hayduk, 1987, 1996; Hedström & Swedberg, 1998; McKim & Turner, 1997; Mulaik, 2001), and it is therefore timely to reconsider how their concerns might be addressed from *within* the framework of critical realism. I am

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<sup>1</sup> Lawson (1999: 224) states: "... my central claim with respect to contemporary mainstream economics is that it is most accurately characterised as deductivist. By deductivism I understand a mode of explanation which involves deducing the explanandum from a set of initial conditions plus regularities that take the *form* 'whenever this event or state of affairs then that event or state of affairs'" (italics in original; cf. Lawson, 1997: 17). As evidence he points to the 'bulk of econometric modelling' within mainstream economics (p. 227). Bhaskar (1998a: xi), like Lawson, paints a monolithic picture of 'deductivism', which he describes as the 'Popper-Hempel theory of explanation'.

in agreement with the principal tenets of critical realism, which I believe to be one of the most promising forms of scientific realism to have emerged in recent years, and the criticisms that I will make in this paper are therefore intended to strengthen rather than undermine this philosophical approach<sup>2</sup>.

I will confine my attention to the most important issues raised by recent debates and will concentrate on causal modelling approaches that use Structural Equation Modelling (SEM), a powerful and flexible family of statistical models currently available (Bentler & Wu, 1995; Bentler & Weeks, 1980; Bollen, 1989; Byrne, 1994; DiLalla, 2000; Loehlin, 1992; Schumacker & Lomax, 1996)<sup>3</sup>. Because of the potential of these models, debates about the relationship between statistics, theoretical models and real structures and processes have a particular significance for SEM practitioners. In this paper I will focus in particular upon 'omitted and included variables bias', tests of 'goodness of fit' and the causal interpretation of model coefficients. I hope in this way to bridge the gap between the relatively abstract prescriptions of critical realist philosophers and the more concrete concerns of applied researchers (cf. Hands, 1999: 181; Harré & Madden, 1998: 120).

In order to anticipate one source of criticism, I would like to stress

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<sup>2</sup> Psillos (1999) defines 'scientific realism' in relation to three fundamental theses: (1) the world has a definite and mind-independent natural-kind structure; (2) scientific theories are truth-conditioned descriptions of their intended domain, whether observable or non-observable, and theoretical terms have putative factual reference; (3) mature and successful scientific theories may be regarded as well-confirmed and approximately true of the world.

<sup>3</sup> Structural Equation Models combine qualitative, theoretical insights regarding causal mechanisms, on the one hand, and quantitative data, on the other, permitting the evaluation of complex hypotheses involving networks of cause and effect relationships. They contain a regression equation for every outcome variable and a corresponding 'error term' that captures the variance not explained by the explanatory variables. Complex models may be estimated, in which variables that are outcomes in one equation appear as causes in another. The power of Structural Equation models comes from the great variety of theoretical models that may be specified, including first- or even second-order latent variables. Because Structural Equation models draw on theoretical knowledge, they are in principle testable: the hypothesised relationships between the variables in the model imply a pattern of covariances between the observed variables and model adequacy can be assessed by confronting this theoretically implied covariance matrix with the observed variances and covariances.

at the outset that I do not seek to privilege quantitative techniques over qualitative methods. I believe that the choice of methodological approach should be dictated by the nature of the research problem rather than by the methodological preferences of the researcher. In many cases, the most effective approach will involve the application of two or more methodological techniques, followed by the 'triangulation' of research findings. Thus, my sole aim in this paper is to demonstrate that statistical analysis – and causal modelling in particular – are in principal consistent with critical realism.

## 2. Critical Realist Philosophy of Science

Critical realism has attracted the attention of social scientists in recent years by offering attractive solutions to the problems associated with both positivist and hermeneutic philosophies of science, particularly in relation to social change, the 'structure/agency debate' and the ontological status of social structures. Indeed, many social scientists are only familiar with scientific realism through their contact with the work of critical realists. However, critical realism also espouses a number of positions which distinguish it from other realist philosophies of science. Bhaskar (1975), the founder of CR, provides a transcendental argument that takes the success of the natural sciences as its premise and goes on to argue that in order for this success to be possible, the natural world must have a stratified ontology of causally efficacious 'generative mechanisms' that operate in 'open systems' as well as under laboratory conditions.

Bhaskar (1979) extends these ontological conclusions to the social realm, whilst nevertheless insisting on the specificities of social structures (e.g. their 'concept dependence'). However, Lawson denies that an analogous transcendental argument can be applied directly to economics (1997: 56), because this discipline – like the other social sciences – does not have the demonstrated success required for a transcendental underpinning. Although this is not the main focus of this paper, I believe that an alternative, transcendental argument is possible and that this can ground the possibility of naturalism. The key to this is to use the success of *everyday human interventions* in the social world as the premise for the derivation. Social actors are (generally) able to coordinate and plan their behaviour and to act in socially appropriate and meaningful ways, and

this entails the existence of relatively stable and enduring mechanisms and structures. As Kitcher (2001) argues, realists can rely on

our everyday methods for correcting our perceptions of the world around us, taking the successes of our physical, physiological, and psychological theories to reveal the limitations of our perceptual powers. The judgments we make on this basis are, of course, fallible. But unless we relapse into global skepticism there's no reason to maintain that they are not true... . (p. 191)

Like other realists, critical realists insist on the possibility of choosing rationally between rival theories. Bhaskar (1975) links this with the concept of 'explanatory power', arguing that the most powerful theories are those that explain the widest range of phenomena. For critical realists, theoretical development mirrors the stratification of the ontological domain: each account of a generative mechanism contains 'gaps' or 'black boxes' which may subsequently be explained by positing the existence of additional mechanisms at a 'deeper' or more fundamental level (Bhaskar, 1979: 15, 17). Higher-level structures, mechanisms and phenomena – including human behaviours and interactions – are 'emergent' from, but not reducible to lower-level ones (Bhaskar, 1979: 32).

Although generative mechanisms frequently refer to unobservable entities and processes, critical realists argue that the explanatory adequacy of our hypotheses about these mechanisms can be evaluated by investigating their observable effects. Indeed, it is only to the extent that generative mechanisms have observable effects that theoretical knowledge of them is possible. Thus, a theory with high explanatory power is one that has the capacity to explain a wider range of phenomena than its rivals, as well as being consistent with the available evidence<sup>4</sup>. Bhaskar & Lawson remark that

we can (provisionally) accept that theory which can accommodate the

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<sup>4</sup> Lawson states that "competing theories with non-empirical referents can be assessed according to their relative explanatory powers ... with respect to 'observables', i.e. according to their relative successes in illuminating a range of empirical phenomena" (1999: 238).

largest range of phenomena (typically expressed as contrastive demiregs) upon which it bears. This remains a context-dependent affair, but entirely feasible. (1998: 14)

Bhaskar (1989) provides two criteria for the assessment of explanatory power, the first of which clearly recalls the work of Imre Lakatos:

A theory  $T_c$  is preferable to a theory  $T_d$ , even if they are incommensurable, provided that  $T_c$  can explain *under its descriptions*, almost all the phenomena that  $T_d$  can explain under its descriptions, plus some significant phenomena that  $T_d$  cannot explain. (p. 73; italics in original; cf. pp. 19, 32)

This formulation is rather vague, however, particularly in light of Bhaskar's second criterion: a theory is preferable to another

if it can either (a) identify and/or describe and/or explain a deeper level of reality; and/or (b) achieve a new order of epistemic (explanatory and/or taxonomic) integration, or at least show grounded promise of being able to do so. (1986: 82)

But, as Peacock (2000) points out, the first explanatory power rule implicitly includes vertical explanations, so it is hard to see what the second criterion actually adds. The main challenge is therefore to operationalise the notion of explanatory power by showing what this involves in practice.

Bhaskar's notion of explanation relies on a causal account of the relationship between unobservable and observable entities. To explain a given phenomenon is to describe a generative mechanism which, were it to exist and operate as hypothesised, would account for the phenomenon. The notion of 'ontological stratification' implies that social or natural phenomena occurring in open systems are co-determined by a number of distinct mechanisms, which (in the terminology used by Bhaskar) may exist without being actualised and may operate without being observed. In contrast, empiricists deny the possibility of gaining accurate knowledge of unobserved mechanisms and this is the main contrast between empiricism and realism. According to semantic empiricists, for example, the only meaningful *language* that scientists can use is restricted to observable things, properties and events, and epistemological empiricists

sustain, in analogous fashion, that the only scientific *claims* that are justified concern observable entities (Kitcher, 2001). Whereas the former position founders on the impossibility of distinguishing between observational and theoretical vocabularies, the latter may be countered by showing that, unless our claims about things we cannot observe were approximately true, the success of our attempts to control our natural and social environment would be nothing short of miraculous (Kitcher, 2001)<sup>5</sup>.

### 3. The Attitudes of Critical Realists Towards Statistical Analysis

Critical realists are often sceptical of statistical analysis, dismissing quantitative research methods as worthless, simplistic and misleading<sup>6</sup>. For example, Bhaskar (1998c) states that

[o]n the account of laws advanced here they cannot be identified with constant conjunctions of atomistic events or regarded as reporting correlations between either independent or equivalent variables. On the contrary, they must always be grounded in some conception of an explanatory mechanism and ascribed, as tendencies, to specific kinds of things. (p. 98)

Archer argues that statistical techniques fail to give adequate consideration to the specificities of social systems:

In social realism it is quintessential that society is an open system: and not in the milk and water terms of those methods' textbooks warning about the difficulties of 'controlling for extraneous variables'. (1998: 190)

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<sup>5</sup> Whereas empiricists take unobservables to be epistemically inaccessible, constructivists regard all objects as epistemically inaccessible (at least if we conceptualise these as realists do). The most plausible realist counter-arguments emphasise that when we perceive, we are in causal contact with the objects of perception, and "although this contact is mediated by our having certain kinds of psychological states, we don't perceive by perceiving those states (or their contents)" (Kitcher, 2001: 157).

<sup>6</sup> The only exceptions to this that I have found are the conference paper by Doug Porpora (1998) and an article by Amit Ron in the *Journal of Critical Realism* (November 2002).

Lawson (1998) attributes the failure of mainstream economics to the

often quite irrelevant, typically formalistic, methods and techniques which economists naively and unthinkingly wield in a forlorn hope of thereby gaining illumination of a social world that they do not 'fit'. (p. 169)<sup>7</sup>

In contrast to these writers, I believe that 'sets of equations' – whilst not equivalent to theoretical models – can be extremely useful in assessing the explanatory power of social science theories<sup>8</sup>. In common with Hoover (1998) and Porpora (1998), I believe that statistical research techniques are compatible with realism (and with critical realism in particular), as long as the rather indirect relationship between the empirical adequacy of statistical models and the explanatory power of theoretical hypotheses is understood. Porpora stresses that

... even in open systems, regularities detected by analytical statistics can be as indicative of active mechanisms as are regularities detected in the experimental laboratory. No more actualism is implied in one case than the other. What distinguishes realism from positivism is not that they run regressions and we do not but how we run regressions and the significance we attach to them. (1998: 4-5).

The failure of critical realists to appreciate the importance of methodological pluralism has (ironically) led them to embrace an empiricist account of statistical analysis at the very moment when many

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<sup>7</sup> Hands (1999) and Koch (2001) take Lawson to task for assuming that deductivism (i.e. Humean empiricism) adequately characterises contemporary economics. Koch (2001) argues that mainstream economics does not rely on the notion of 'constant conjunctions'; instead, hypotheses refer to 'modified event regularities' and explicitly acknowledge the role of 'limiting conditions', a formulation that Lawson himself has explicitly accepted in the past (Lawson, 1997: 27-28; cf. Koch, 2001). However, these authors do not address the question of whether statistical techniques are consistent with critical realism.

<sup>8</sup> In this paper I will use the term 'theoretical model' to refer to the substantive hypotheses developed by the theorist – typically in linguistic form – in an attempt to account for a given phenomenon. In contrast, I will use the term 'statistical model' to refer to the mathematical model that operationalises these theoretical hypotheses in the form of a directed graph (path diagram) or a set of equations.

applied researchers are themselves questioning empiricism. For example, many psychologists bemoan the lack of theoretically informed statistical models that can be used to analyse the determinants of children's well-being (Brooks-Gunn et al., 1997). Because individual, family and neighbourhood factors are mediated by specific situational variables, a theoretical account of the underlying generative mechanisms is crucial in order to specify these relationships correctly. Unless the causal effect of the above factors is correctly identified, external interventions to support families cannot be effectively designed, and this practical orientation cuts against empiricist interpretations of statistical models. Interestingly, many applied researchers have more or less spontaneously adopted a realist interpretation of statistical models in terms of underlying mechanisms, in contrast to the empiricist focus on events and their regular co-occurrence (cf. Ron, 2002). Similarly, researchers who use Structural Equation Modelling techniques often state explicitly that their aim is to evaluate theoretical hypotheses involving mechanisms and structures. Rather than treating their statistical models as 'fictions' or 'instruments', these researchers actively seek to construct models that reproduce real processes and structures, including unobserved variables, cross-level influences, reciprocal relationships, interactions, feedback loops and contextual effects (Chan, 1998; Duncan et al., 1997; Hayduk, 1987, 1996; Schumacker & Marcoulides, 1998). The flexibility of the statistical theory behind Structural Equation Models means that background assumptions can often be tested explicitly and rigid distributional constraints can be relaxed. Given the enormous potential of these developments, it is disappointing to read that statistical models should, in Bhaskar's view, be "totally discarded"<sup>9</sup>.

I believe that this hostility towards statistical methods reflects the methodological preferences of Bhaskar, Lawson and other critical realists, in favour of a case-based, qualitative approach to social explanation. Indeed, there is a long tradition within economics of rejecting statistical methods along with neo-classical economic theory, as if these elements

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<sup>9</sup> "Humean theories of causality and law, deductive-nomological and statistical models of explanation, inductivist theories of scientific development and criteria of confirmation, Popperian theories of scientific rationality and criteria of falsification, together with the hermeneutical contrasts parasitic upon them, must all be totally discarded" (Bhaskar, 1998d: 225)

of the 'mainstream' were inseparable. For example, Keynes wrote that

[if] we were dealing with ... independent atomic factors and between them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling their laws of their action. (quoted in Lawson, 1997: 304)

I will show in this paper that each of these objections can be dealt with satisfactorily by statistical modelling techniques. I am therefore in agreement with Boylan & O'Gorman when they argue that

[p]articularly in the case of econometrics, which critical realists associate with a Humean methodology, we fully acknowledge that it does not live up to the exaggerated claims made on its behalf. However, there is no reason why it cannot be uncoupled from this Humean association. (1999: 143)

#### **4. Answering Critical Realist Objections to Statistical Analysis**

In order to show that statistical techniques of analysis represent a viable research strategy, I will address the specific objections that have been raised by critical realists. The first of these objections concerns the relationship between statistical models and substantive theoretical models. Obviously, we can only defend the relevance of statistical models if these models can be shown to provide a satisfactory representation of specific theoretical models. Whereas theories are typically expressed in linguistic form, statistical models necessarily take a mathematical form. Thus, Lawson (2001: 377) argues that the problems in contemporary economics can be attributed, in large part, to the "need to express everything in the form of mathematics". Similarly, Bhaskar argues that

... the conceptual aspect of the subject matter of the social sciences circumscribes the possibility of measurement in an even more fundamental way. For meanings cannot be measured, only understood. Hypotheses about them must be expressed in language, and confirmed in dialogue. (Bhaskar, 1998d: 226)

In response, it is important to realise that theories expressed in linguistic form can often be given a mathematical form and vice versa, and numeric variables can encode qualitative information about attributes, states and relationships as well as events (cf. Cowen, 1998: 132). For example, a theory that includes unobservable theoretical concepts as well as complex causal relationships can be translated into a set of structural equations. To the extent that certain assumptions must be made in this process, this in itself does not invalidate the statistical model, but merely implies that the resulting conclusions are dependent upon the plausibility and sensitivity of the assumptions. As more powerful techniques of statistical analysis have become available, more realistic assumptions can be made regarding the distribution of variables, non-linearity, non-additivity and forms of social, spatial and temporal dependence.

At the same time, it is important to recognise that the specification of a statistical model does not render the theoretical model superfluous. Statistical models typically do not reproduce every nuance of the original theoretical model and indeed this is not necessary in order to provide pertinent information on the explanatory power of the theory. Mathematical 'formalisation' can actually *enhance* theoretical clarity by spelling out the empirical consequences of a theoretical hypothesis, and Lawson's derogatory use of this term is unjustified (Lawson, 1998: 169). Indeed, many *qualitative* researchers use causal flowcharts whilst developing their theoretical hypotheses (Miles & Huberman, 1994). These graphs can easily be translated into mathematical form; for example, in statistical model-building, the construction of a 'path diagram' often represents an intermediate step between the theoretical model and the specification of a statistical model.

The second objection that critical realists have made to statistical models concerns the validity of the assumptions implied by these models. Examples of such assumptions include linearity, additivity, no serial correlation, homoscedasticity and multivariate normality (see Bollen (1989) and Bentler & Wu (1995) for a detailed description of the assumptions made by Structural Equation Models). Some of these assumptions – such as the assumptions of multivariate normality and homoscedasticity – can be relaxed in Structural Equation models, due to recent developments in statistical theory and due to the robustness of the Maximum Likelihood estimator to deviations from normality (Bollen & Stine, 1990; Browne, 1984; Browne & Cudeck, 1992; Satorra, 1990).

The assumptions of linearity and additivity should be tested, but these assumptions are often plausible and perfectly appropriate. Non-linear relationships can often be approximated by linear models to a high degree of accuracy, and researchers who routinely test for interactions between variables will realise that these are far from ubiquitous. Thus, Manicas (1998) is not justified in concluding that statistical techniques based on the assumption of additivity are 'almost totally *meaningless*' (p. 334; italics in original). Similarly, in relation to model assumptions, the flexibility of causal modelling techniques facilitates non-standard models incorporating multilevel or local dependence structures (Newsom, 2002).

The third issue that must be addressed is the alleged dependence of statistical models on observed variables. In fact, *unobserved* variables can also be included in Structural Equation Models, providing an additional means of bridging the gap between theoretical and statistical models (Bollen, 1989; Byrne, 1994; Dunn et al., 1993; Loehlin, 1992). It is simply not the case that statistical models are confined to the realm of 'superficial appearances'. Latent variables, whilst not directly observable, can be identified on the basis of their observed effects and may be used to represent complex, multifaceted concepts that would otherwise be impossible to measure.

The fourth issue is the alleged 'atomism' of statistical models, which Lawson again views as inevitable. But it is important to realise that this is not an automatic consequence of statistical research methods but is due to *theoretical* weaknesses. At any rate, once it is understood that statistical modelling does not assume constant conjunctions of atomistic sense data this accusation loses its force. Arguably, the best way to overcome atomism in the context of statistical models is to situate individual variables within an adequate theoretical framework. For example, in the study of family relationships, social psychologists have developed statistical models that account for the complex dependence between individuals and between different dyadic relationships within the family. Thus, the similarities between husbands' and wives' evaluations can be modelled explicitly, as can the dependence generated by gender (e.g. father-son and father-daughter relationships, as compared to mother-son and mother-daughter relationships). Familiarity with the work of social psychologists such as Sandra Murray, Rod Conger and David Kenny confirms that statistical models can be powerful tools in the study of social relationships.

The fifth issue involves the ‘open systems’ argument: statistical techniques are not valid in the social sciences, it is argued, due to the lack of significant closures in the social domain<sup>10</sup>. But Lawson has argued (and Bhaskar concurs) that even in the context of open systems, ‘demi-regularities’ encode patterns that are attributable to the effects of social mechanisms:

A demi-regularity ... is precisely a partial event regularity which *prima facie* indicates the occasional, but less than universal, actualisation of a mechanism or tendency, over a definite region of space-time. (1998: 149).

Benton (1998) points out that even in the laboratory, closure is not always obtained, and a precondition for laboratory experiments is typically the prior detection of at least some effects of a mechanism (Lawson, 2001: 381-2). Moreover, the non-experimental *natural* sciences such as geology, astronomy and evolutionary biology have been highly successful in formulating theories with high explanatory power. It is therefore possible to argue that the statistical analysis of non-experimental data on the basis of a theoretical model and appropriate modelling assumptions represents a valid research strategy in both the natural and social sciences. Hoover (1998) echoes this point when he states that:

Openness is relative. Uncontrolled, non-experimental situations, not just in astronomy, may be closed enough to deliver regularities of varying degrees of precision and reliability. Conversely, no experiment is perfectly immune to outside influences. Closure too is relative. And, in large measure, closure is secured using regularities readily to hand in the world as instruments. (1998: 14)

It is therefore possible to deal with the objections raised by critical realists in relation to the mathematical form of statistical models, their

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<sup>10</sup> A ‘closed system’ is one in which one or more mechanisms are effectively isolated or screened off from extraneous influences, enabling the scientist to study their operation in isolation from potentially confounding factors. Bhaskar assumes that ‘closure’ is routinely obtained in the laboratories of natural scientists but that society is fundamentally ‘open’ due to the complexity and inter-relatedness of social structures and due to the effects of social interaction, social change and human agency (1979: 59).

assumptions, their alleged dependence upon observed variables, their 'atomism' and their ability to provide insights into the operation of generative mechanisms in 'open systems'. As well as responding to these objections, I believe that it is possible to provide a positive account of causal modelling that indicates its potential role in social science research. In the following paragraphs I will attempt to provide such an account and to provide a more complete response to philosophical critics of causal modelling techniques.

I believe that the associations between events, attributes, actions and beliefs that are recorded by the covariances between variables can be interpreted as equivalent to 'demi-regularities':

In fact, a great diversity of non-experimental means of empirical control and correction, as well as adaptations of experimental methods themselves, have been developed in these sciences. This is true just as much of the historical social as of the historical natural sciences. (Benton, 1998: 310)

Covariances are more complex than the binary 'contrastives' that Lawson discusses: rather than comparing one case with another, where the cases differ in just one respect, we are instead examining distributions of cases where these distributions are jointly determined by a range of factors and where the effect of one mechanism can only be identified by controlling *statistically* for other influences. Lawson's examples of demi-regularities include cases that are much more conducive to statistical modelling than to 'Mill's method' of contrasts, such as the concentration of women in secondary sectors of the labour market and the small proportion of children from poor backgrounds in the UK who continue into higher education (Lawson, 1998: 151). These patterns involve contrasts along several continuous dimensions that can only be analysed accurately using statistical techniques. Moreover, it is hard to imagine how we might go about evaluating alternative theories in relation to these phenomena without using statistical models. Lawson's objection – that statistical models must either explain 'everything back to the big bang' or else nothing at all (2001: 384) – overlooks a third option: constructing parsimonious but powerful explanatory models that focus on the most important proximal causes.

However, Lawson (1997, 2001) partially anticipates this response,

arguing that “econometricians concern themselves with attempting to determine constant event conjunctions ... *of a probabilistic sort*” (1997, p. 69; italics added). He then equates ‘well-behaved probabilistic functions’ with ‘constant conjunctions’, declaring that:

[i]n identifying the mainstream economics project as deductivist I claim little more than that it relies upon, i.e. regards as essential, those results, claims, hypotheses, etc., which assume the ‘whenever event (or state of affairs) x then event (or state of affairs) y’ form, *or a probabilistic equivalent*. (2001, p. 372; italics added)

But probabilistic models do not assume closure, as they base their inferences on the observable patterns in the relationships between variables, patterns that are similar to Lawson’s ‘demi-regularities’ (cf. Lawson, 1998: 157).

A further objection is possible, however, and Olsen (1999) pursues this by arguing that statistical models such as the classical regression model assume closure by sustaining that the omission of any relevant variables ‘inevitably’ gives rise to bias in the estimated coefficients (p. 4). But this is misleading, as the probabilistic form of the regression model – which includes an ‘error’ term that incorporates the influence of omitted variables as well as random measurement errors – does not imply closure, and because the omission of causally relevant variables does not inevitably lead to biased estimates of other coefficients (Pearl, 2000). For example, if a variable that is not correlated with any other explanatory variable is omitted from a model, the regression coefficients for the variables included will not be biased. Judea Pearl (2000) uses graph theory to prove that even if an omitted variable is correlated with an explanatory variable in the model, bias is not inevitable. Therefore, we can distinguish between the probabilistic *form* of the classical regression model and the specific conditions that may give rise to bias. Furthermore, it is obvious that inclusion of the most important causal factors will reduce the scope for bias and it is not unreasonable to assume that, if we account for the principal causal influences, the effects associated with the remaining variables may cancel out. All of these observations can be extended from the classical regression model to more complex techniques such as Path Analysis and Structural Equation Modelling.

Olsen also suggests that multicollinearity places limits on the

interpretability of regression coefficients, as “[i]f the  $X_i$ ’s are correlated then the  $\beta_i$ ’s may be exaggerated and unstable” (1999: 9). This is, once again, misleading, as the regression coefficients (i.e. the unstandardised ‘betas’) only become unstable in situations of near perfect collinearity (i.e. when one variable represents an almost perfect linear function of one or more other variables) (Achen, 1982; Fox, 1996; Stevens, 1996). This is unlikely to occur if reasonable sample sizes are used and if the specification of the model is conceptualised carefully. For example, rather than including a number of variables that are highly similar, it often makes more sense to treat these as indicators of a single latent variable. Rather than including ten variables that measure different facets of ‘self-esteem’, for example, we should treat these as indicators of a latent variable and include only the error-free latent variable in the model.

The importance of maintaining a distinction between theoretical and statistical models becomes clear when we consider the issue of ‘underdetermination’. One of the justifications that empiricists provide for their refusal to draw causal conclusions from statistical models is the existence of models that are mathematically equivalent to the original model (i.e. they imply an identical pattern of means, variances and covariances with the same number of restrictions on the variance-covariance matrix) but nevertheless differ from that model in substantive terms (e.g. in terms of the direction of certain effects) (MacCallum et al., 1993). Spirtes et al. (1991) and Pearl (2000) provide algorithms for locating alternative models using graph theory. But Kitcher (2001) points out that when alternative models are generated by an algorithm rather than by a rival theory, realists have nothing to fear from the notion of ‘underdetermination’ (p. 196). The mere possibility of locating equivalent models does not in itself cast doubt on a model; after all, algorithms can locate alternatives to theoretical hypotheses involving observable entities that empiricists themselves readily accept<sup>11</sup>.

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<sup>11</sup> As I noted earlier, if generic forms of philosophical scepticism are set aside, underdetermination does not raise serious problems for causal modelling. Nevertheless, as Kitcher (2001) observes, this will sometimes override the realist licence to accept theoretical claims as correct, underlining the corrigibility of all such claims.

## 5. Structural Equation Modelling and the Statistical Assessment of Causal Hypotheses

The modular nature of mechanism-based accounts is fully compatible with statistical techniques such as Structural Equation Modelling. Just as individual mechanisms can be combined theoretically (or ‘concatenated’) in order to provide a more complete account of a phenomenon, Structural Equation models can incorporate variables or structures that represent distinct generative mechanisms. Each direct causal path in the statistical model may be treated as a ‘black box’ and, in a future model, substituted by a more detailed sub-model. Indeed, this is often how theoretical progress occurs within applied social science research. For example, we may construct a statistical model to estimate the effect of the neighbourhood context on children’s cognitive development; in this case, the intervening mechanisms are not specified and we have a ‘black box’. In a subsequent model, however, we may construct a model in which neighbourhood of residence determines the quality of the social and cultural environment, which in turn impacts on children’s cognitive development. The factors that now mediate between the two original variables explain the nature of the mechanism in question.

As I mentioned above, the choice of variables to include in a statistical model and the relationships between those variables should be determined in accordance with the researcher’s theoretical hypotheses. The explanatory power of the theory may be judged by examining the empirical adequacy of the statistical model across a range of different datasets, by looking at the pattern of residuals, by inspecting the estimated coefficients and by considering evidence from other sources. Of course, this evaluation is conditional upon the internal consistency and scope of the theoretical model. The explanatory power of theoretical models depends not only on their ability to shed light on the patterns observed in specific datasets, but also their ability to generalise to other datasets and their consistency with existing qualitative research findings. Finally, it is important to note that a complex statistical model with poor overall fit may contain model components that fit well. The estimated coefficients relating to these parts of the model may be reliable even if the model as a whole is flawed.

The main focus of statistical models is often on the use of regression coefficients to evaluate causal hypotheses. Judea Pearl (2000) shows that

estimated regression coefficients from Structural Equation Models can, in certain circumstances, be interpreted counterfactually as a measure of the sensitivity of a given variable to a (hypothetical) experimental manipulation of its causes, conditional upon the assumptions implied by the form and structure of the model. For example, a model might suggest that increasing parental income by 5,000 EUR will, on average, improve a child's reading score at age 10 by 5 per cent, holding other variables constant (such as parents' social class position, educational attainments and neighbourhood of residence). Obviously, experiments of this sort cannot generally be carried out, but regression coefficients can nevertheless be interpreted in these terms<sup>12</sup>. The key insight of Pearl and other methodologists is that experimental control is not necessary in order to identify the existence and effects of social mechanisms, as long as we know how these relate to the other factors that operate in a given context.

This brings us to one of the core issues in current debates about causality, namely the question of when regression coefficients can be interpreted causally. As long as we remain within the framework of empiricism, we will never have a warrant for describing theoretical models as 'approximately true' (i.e. in the realist sense outlined by Psillos (1999)), for treating latent variables as representations of real mechanisms or for drawing causal conclusions from statistical models. For example, positivists reject the very notion of causality, equating theory with a deductively-related body of law-like propositions and viewing Structural Equation Models as an algebraic object that is void of causal content. For example, Muthen (1987: 180) argues that "[i]t would be very healthy if more researchers abandoned thinking of and using

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<sup>12</sup> Where an experimental set-up is created by chance, this can obviously provide a valuable opportunity to study social processes. The Gautreaux Assisted Housing Program in the United States is a good example (Rosenbaum, Kulieke & Rubinowitz, 1988; Kaufman & Rosenbaum, 1992; Rosenbaum, 1991). This programme allocated suburban and inner-city housing to disadvantaged applicants on a random basis. Comparing the educational and labour-market trajectories of children from families who were moved to the suburbs with those who were moved to inner-city neighbourhoods, massive differences in school drop-out rates were observed, as well as big differences in college enrolments and employment history.

terms such as cause and effect” (quoted in Pearl, 2000: 137)<sup>13</sup>. As a consequence, applied researchers often assume a schizophrenic stance, using an *implicitly* causal language when reporting the results of their models, whilst assiduously reassuring the reader that statistical models can never provide reliable information on causation<sup>14</sup>. Therefore, one of the most important contributions that critical realism can make to Structural Equation Modelling is to underpin a *causal* interpretation of these models, conditional upon modelling assumptions.

Clearly, one of the most important assumptions that we must satisfy before drawing causal conclusions from a statistical model is that we have eliminated the possibility that estimates of causal effects may be *confounded* due to shortcomings in the specification of the model. I touched upon this issue earlier, where I noted that the omission of a causally relevant variable from a model can, in certain circumstances, lead to omitted variables bias. It is also important to recognise that the *inclusion* of causally irrelevant variables can also lead to biased estimates (‘included variables bias’). This implies that valid causal conclusions can only be drawn if a model is correctly specified, in the sense that *all* causally relevant variables have been included and all irrelevant ones excluded. But, as I argued earlier, not all mechanisms are equally important. If we include the most important proximal factors, then the scope for bias is reduced. Many critical realists – perhaps due to their lack of experience of applied research – have a tendency to place all mechanisms on the same level and to assume that considerable bias is inevitable.

Secondly, by focusing on the diachronic aspects of theory development we can identify a means of progressively improving model specification in accordance with the CR notion of the ‘logic of scientific discovery’. Common-sense ideas and ‘lay theories’ regarding causes and

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<sup>13</sup> Pearl (2000) also cites Holland (1995: 54): “I am speaking, of course, about the equation:  $\{y = a + bx + \epsilon\}$ . What does it mean? The only meaning I have ever determined for such an equation is that it is a shorthand way of describing the conditional distribution of  $\{y\}$  given  $\{x\}$ .”

<sup>14</sup> For example, Kerlinger (1979) concludes his statistical analysis by stressing that “[e]ven though we used expressions like ‘accounted for’ and ‘effects’, causal implications, while perhaps inescapable because of language connotations, were not intended...” (quoted in Manicas, 1998: 328).

effects provide a starting-point, and if a published model has shortcomings, then the onus is upon other researchers to demonstrate that an alteration to its specification will give rise to changes in model estimates. Cross-case comparisons can also shed light on important omitted factors.

## 6. Structural Equation Models and the Notion of Explanatory Power

It is now possible to return to the issue of explanatory power and to provide further guidelines for theory assessment. If we take the principal tenets of critical realism seriously, then it is inappropriate to employ a null hypothesis of exact fit when assessing model fit, as we know from the very beginning that our models cannot represent *all* of the mechanisms that operate in the real world, but only the most important of these<sup>15</sup>. As I have already indicated, common modelling assumptions such as linearity and multivariate normality, whilst defensible in many cases, certainly involve a degree of simplification. This suggests that a more descriptive approach to model assessment is required, perhaps using alternative measures of model fit. A critical realist approach to causal modelling would therefore not rely on the use of hypothesis tests to establish scientific ‘laws’, but instead treat model fit in a more descriptive manner as just one means of evaluating explanatory power. As Porpora (1998) suggests,

[i]n its use of analytical statistics, positivism mistakenly conflates evidence and explanation, but there is no reason for realism not to disentangle the two. When they are disentangled, analytical statistics emerge not as Andrew Sayer (1984) characterises them as “primitive tools as far as explanation is concerned” for the simple reason that analytical statistics are not explanatory tools at all. Rather than being explanatory tools, analytical statistics – including regression – are

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<sup>15</sup> The notion of ‘goodness of fit’ is discussed in a vast statistical literature, although few methodologists provide a comprehensive treatment of the epistemological and ontological issues raised by this concept (cf. Bentler & Chou, 1992; Bentler & Yuan, 1999; Bollen & Long, 1993; Browne & Cudeck, 1992; Mulaik, 2001).

evidentiary tools, enabling assessment of explanations. (p. 4)<sup>16</sup>

It is important to remember that a poorly-fitting model does not necessarily invalidate the theoretical hypotheses that guided model specification, as the source of poor fit may be confined to just one part of the model or the measures used to operationalise key concepts may be flawed. Equally, a *fitting* model does not imply that we have constructed a true representation of one or more generative mechanisms, as models can fit for the wrong reasons. This is why I emphasised in an earlier section the need to reproduce modelling results in new contexts, with new datasets that differ as much as possible from the original ones. Finally, model fit should always be evaluated relative to the degrees of freedom of the model<sup>17</sup>.

The absence of decisive statistical tests for evaluating the adequacy of theoretical models lends weight to the ‘Lakatosian’ elements of critical realism. Lakatos (1978) shows how theoretical development and change can occur even in the absence of such tests by focusing on the organisation of theoretical hypotheses into ‘scientific research programmes’. These programmes have an internal structure, being organised around a small number of ‘core’ hypotheses that lend them coherence, around which a ‘protective belt’ of auxiliary hypotheses forms. Lakatos argues that it is always possible to preserve the core of a research programme – and scientists often remain loyal to a specific

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<sup>16</sup> Porpora seeks to avoid providing overly prescriptive methodological advice to researchers, preferring the ‘methodological anarchy’ recommended by Feyerabend (1978), whilst justifying judgmental rationality on the basis of the ‘weight of argument’, a conclusion that is considerably more subjective than that provided by this paper.

<sup>17</sup> The ‘degrees of freedom’ of a model represent the number of dimensions in which the theory-inspired model can differ from the observed data. When we evaluate a Structural Equation Model using global fit indices, for example, we are only evaluating the parameters that we have constrained in some way. Increasing the number of parameters that are estimated from the data does not generally lead to a more testable model (Mulaik, 2001). Therefore, a model that is more parsimonious and constraining (i.e. one that imposes more theoretically-inspired constraints) has greater scope for poor fit. Mulaik provides the following, ‘objectivist’ formulation of this issue: “Good fit alone is not sufficient to insure the objectivity of the model and its pre-specified parameters. The model must also have many degrees of freedom to be highly credible as representing something objective” (2001: 219).

strand of research even in the face of contradictory evidence – by altering the auxiliary hypotheses (by relying on *ad hoc* explanations, for example). However, such a resolute theoretical defence will tend to erode the coherence of the research programme as a whole, leading to internal inconsistencies and reducing both its range of application and its ability to explain new phenomena. In the field of Structural Equation Modelling, a similar process occurs when *ad hoc* adjustments are made to a statistical model on the basis of the observed data. This ‘data-fitting’ approach can nearly always generate a fitting model, but the price paid is very high in terms of robustness. This is why models that have been assessed using a variety of different datasets have greater explanatory power than those assessed using just one: data-fitting gives rise to unstable models that do not generalise.

The evaluation of explanatory power should therefore reward parsimony, internal consistency, scope, empirical adequacy and generality. Bhaskar (1979) embraces this Lakatosian idea, arguing that historical materialism, for example,

can only be justified by its fruitfulness in generating projects encapsulating research programmes capable of generating sequences of theories, progressively richer in explanatory power. (p. 53)

In descriptive terms, the concept of ‘explanatory power’ suggests that when the scientific community goes over to a new theory it generally does so on the basis of an assessment of explanatory power, and that this assessment is made in a rather complex and context-dependent manner. Furthermore, in normative terms, it suggests that between two alternative theories we should choose the one with greatest explanatory power.

## 7. Conclusions

In summary, it is important to acknowledge that critical realist philosophy of science has helped social scientists to defend a realist approach to social theory at a time when intellectual trends have been pushing mainly in the opposite direction. However, as this philosophical current has increased its influence within economics, politics and sociology, the need to address anomalies in the writings of the founders has become more

pressing. There is also a real need to explore the commonalities between critical realism and other realist philosophies of science (cf. Kitcher, 2001; Psillos, 1999) and to draw out their implications in relation to research methods. This paper has tackled this latter task by providing an internal critique of the writings of critical realists such as Roy Bhaskar and Tony Lawson and demonstrating that their antipathy towards statistical research methods is not inherent in their social ontology and is not implied by their account of science. Rather than weakening this philosophical framework, I believe that a changed stance on this issue can increase the relevance of critical realism and sharpen its criticism of mainstream economics.

I have also tried to show that critical realism can make a significant contribution to statistical research methods. Above all, I believe that critical realism can ground the possibility of drawing causal conclusions from statistical models, a possibility that was first raised by Wright during the early years of the 20<sup>th</sup> century (Wright, 1921, 1923), that found an echo in the work of Haavelmo after the Second World War (Haavelmo, 1943) and was systematised by Pearl during the 1990s (Pearl, 1996, 2000). This would be of great significance to applied social science research and would enhance the status and relevance of critical realist philosophy of science.

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