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# A Critique of Statistical Modelling from a Critical Realist Perspective

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## Abstract

*Most published research in information systems is underpinned by a positivist or empiricist philosophy. This generally involves the collection of quantitative data and its subsequent analysis using some form of statistical modelling, often multivariate such as regression. Alternative paradigms, such as interpretivism, often critique such statistical analysis on the grounds that the social world is inherently different to the material world. However, this often leads to a strongly anti-realist position which denies the existence of any forms of external social structures. The purpose of this paper is also to put forward a critique of traditional statistical modelling and analysis but from a different direction - critical realism. This maintains the ontological reality of social systems whilst recognising the cultural and historical epistemological limits to our knowledge of them.*

## Keywords

Critical realism, critique, information systems research, statistical modelling

## 1. Introduction

Surveys of the IS literature (Cheon, Grover & Sabherwal, 1993, Nandhakumar & Jones, 1997, Orlikowski & Baroudi, 1991, Walsham, 1995) agree that the majority of information systems research that is published, especially in N. American oriented journals, is generally of an positivist<sup>1</sup> nature and, more specifically, relies on some form of statistical analysis and modelling. Mingers' (2003) survey concluded that nearly 50% of empirical research published in the top IS journals employed observation, experiments, surveys or simulations and would thus involved some sort of statistical analysis. When positivistic case study research was included this proportion rose to 75% (over 90% in the case of the top journal Information Systems Research).

Despite this positivistic hegemony, alternative research approaches have been proposed and to some extent employed (Galliers, 1992, Goles & Hirschheim, 2000, Nissen, Klein & Hirschheim, 1991). These generally come from an interpretive or subjectivist perspective (Avison & Myers, 1995, Harvey & Myers, 1995, Myers,

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<sup>1</sup> Positivism is one species of a wider scientific philosophy known as empiricism. Broadly, this can be characterised by: an emphasis on empirically observable events that can be measured and are independent of the observer; the assumption that observation is relatively pure and unproblematic; an acceptance of the Humean view that causality can only be interpreted as constant conjunctions of events; the view that knowledge consists of universal laws generalised from the empirical data and used for predictive purposes.

1994) although there is also work within the critical tradition (Ngwenyama & Lee, 1997). Interpretive researchers tend to be very critical of positivism, and statistical analysis in particular, on the grounds that the social world is inherently different to the material world and is in essence a human social construction not able to be quantified and captured in statistical models. However, this often leads to a strongly anti-realist position which tends to deny the existence of any forms of external social structures.

This paper puts forward a critique of traditional statistical modelling and analysis but from a different philosophical paradigm – that of *critical realism*. This has two aims:

- To provide a detailed and coherent critique of statistical modelling as it is currently understood and practiced within an empiricist perspective. This should serve as an antidote to the current dominance of this research approach. The critique is based both on an acceptance of some of the commonly stated interpretive arguments but also on the grounds that empiricism is based on a very reductionist view of causality and ontology.
- To recognise that statistical analysis should not be thrown out entirely, as interpretivism would have it, but can be utilised critically within a practical and realistic framework that encourages combinations of extensive and intensive research methods (Lawson, 1997, Layder, 1993). It is argued that this will go beyond the sterile standoff between positivism and interpretivism and produce much richer and more coherent information systems research results.

## 2. Critical Realism and Information Systems

The significance of critical realism (CR) for information systems has been pointed out by Dobson (2001a, 2001b), Mutch (2002, 1999) and Mingers (2002). It is a sophisticated philosophical position that aims to develop a middle way between empiricism, which defines science very narrowly in terms of empirically observable and measurable events, and the many forms of conventionalism or interpretivism which highlight the limitations on our knowledge of the world and tend thereby to diminish the reality of the world itself. For introductions and overviews see: Archer et al. (1998), Sayer (2000), Mingers (2000), and Ackroyd and Fleetwood (Ackroyd & Fleetwood, 2000).

In broad terms, critical realism seeks to re-establish a strong form of realist ontology. That is, an acceptance of the existence of structures and mechanisms, some of which may be non-physical and unobservable (for example social structures). These mechanisms are seen to causally generate the actual events that occur, some of which are observed empirically. In taking this view it stands against both empiricism and interpretivism. Empiricism is underpinned by a very limited understanding of causation - what is called, following Hume (Hume, 1967), a constant conjunction of events. Empiricism is also reluctant to accept the existence of entities or structures that are not physical and observable; it restricts science to the domain of empirical event. Taken together, these result in a very impoverished view of the real world. Interpretivism provides many cogent criticisms of the traditional empiricist assumptions that observation was relatively unproblematic and that we could gain true knowledge of the world. CR largely accepts these limitations on a naïve realism but maintains that, despite this, there is an independent external world and that we can gain reliable (if not provable) knowledge of it.

Put more formally, the main tenets of critical realism are: to re-establish a realist view of *being* in the ontological domain whilst accepting the relativism of knowledge as socially and historically conditioned in the epistemological domain (Bhaskar, 1978); and to argue for a critical naturalism in social science (Archer et al., 1998, Bhaskar, 1979). This is based on the following arguments:

- Ontologically, the existence of a domain of structures and mechanisms, events, and experiences (the *Real*). These structures have causal powers or tendencies the interplay of which leads to the occurrence (or absence) of particular events (the *Actual*). These structures may be physical, social, or conceptual, and may well be unobservable except through their effects. Some, but not all, of the events will be observed or experienced by people and thus become *Empirical*. Events and experiences themselves have causal properties.
- Epistemologically, the recognition that our knowledge is always provisional and historically and culturally relative – we do not have observer-independent access to the world – but that this does not make all theories or beliefs equally valid. We can accept epistemic relativism without accepting judgemental relativism.
- Methodologically, the view that science is not essentially about discovering universal laws, purely predictive ability, or the simple description of meanings and beliefs. Rather, it is centrally concerned with *explanation, understanding, and interpretation*. It moves from some phenomena (or its absence) that has been observed or experienced, to the postulation of some underlying mechanism(s) or structure(s) which, *if they existed*, would causally generate the phenomena. Efforts are then made to confirm or refute the proposed mechanisms. This leads to an emphasis on methodological pluralism and in depth explanation.

The main implications for information systems research are: i) that research should be neither wholly positivist, nor wholly interpretivist since both are limited in their own ways. Positivism reduces real structures to empirical events; interpretivism reduces being to our knowledge of being. ii) It should aim for explanation and understanding not just of how things are, but why they are as they are. It should be intensive and deep rather than extensive and shallow.

### 3. The Critical Realist Critique of Statistical Modelling

Statistical analysis is a form of modelling that explicitly recognises the existence of uncertainty in a set of data. It is conventionally seen as having two possible roles - descriptive and inferential. Descriptive statistics is simply concerned with summarising the main characteristics of a dataset, particularly highlighting any patterns (and anomalies) that may not be immediately obvious. Inferential statistics goes beyond the data as given, recognising that it is likely to be only a sample of all possible values (the population), to draw inferences from the sample to its underlying population.

From a CR perspective, descriptive statistics is unobjectionable and, indeed, very useful. If patterns exist within some set of observations (be they quantitative or qualitative) then there must be some underlying structures, mechanisms, or constraints generating them and this may prove a good starting point for a CR investigation. We must be aware, of course, that the patterns in the data cannot be assumed to simply

reflect an underlying reality. Critical realism recognises that the *process* of observation or, as we would call it, data *production* inevitably imposes itself on the results. This is especially so within IS research where much of the data we work with is produced on a routine basis within an organization, often for purposes different to the actual study, and with highly variable (and uncertain) levels of quality.

At first sight, inferential statistics would also appear to be compatible with critical realism in moving from actual observations to something underlying them, even if it is populations rather than mechanisms or structures. However, when we look at what is actually meant by this within statistics we find a very impoverished and empiricist viewpoint. An informal sample of “business statistics” textbooks typical of those used on both undergraduate and postgraduate IS courses shows that a standard definition of the purpose of inferential statistics is “to make inferences (predictions, decisions) about certain characteristics of the population based on information contained in a sample” (Mendenhall, Reinmuth, Beaver & Duhan, 1986, p.7). A sample is “a subset of measurements selected from the population of interest”. This is already quite a limited aspiration – concentrating on making *predictions* about a defined *population* based only on *measured* data. However, it becomes even more so when we look at the definition of the underlying “population” - the set representing all measurements of interest to the sample collector” (Mendenhall et al., 1986, p. 4). This is typical of the empiricist approach to science - collect some data and then describe and perhaps predict it in terms of some mathematical models. The real world is lost all together in favour merely of that which can be measured, with no attempt at *explanation* at all.

Moving now to the practice of statistical modelling, there is clearly a wide range of univariate and multivariate methods available. In order to make the critique more precise we shall take multiple regression as typical of a range of statistical techniques and one that is commonly used in IS research. Some discussion of other methods is given later. We shall discuss a range of criticisms of statistical modelling that have been made from a CR perspective by a variety of authors (Fleetwood, 2001a, Lawson, 1997, Mingers, 2000, Olsen, 1999, Pawson & Tilley, 1997, Porpora, 1998, Ron, 1999, Sayer, 1992). Again, in order to sharpen the discussion we shall take as representative of current good practice in statistical modelling two papers by Fildes (Allen & Fildes, 2000, Fildes, 1985)<sup>2</sup>. These provide surveys of the state of the art and detailed recommendations, in a structured way, to inform statistical practice. Whilst the papers are primarily concerned with statistical modelling in econometrics, this is perhaps the applied discipline which has most fully developed practical statistical modelling, and I would maintain that the arguments hold equally for information systems research.

### 3.1 Empiricism and Causation

The implicit philosophy of statistical modelling is inherently empiricist, embodying a view of causation that is successionist rather than generative. It is based on the Humean (Hume, 1967) conception of constant conjunctions of events which assumes that events occur in regular sequences and that these can be effectively quantified yielding a set of related variables, often over time. From these data mathematical equations can be generated that represent semi-universal laws. No other form of causation can be inferred from statistically significant results – they only imply

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<sup>2</sup> The following should not be taken as criticism of these particular authors as they are to some extent sympathetic (personal communication).

association (Abbott, 1998). In contrast, CR emphasises the importance of generative causation – i.e., moving from observed data associations to the interacting causal mechanisms that underlie them rather than simply a wider population of unobserved data.

One of the main general points that Fildes makes is the importance of the early stages of model building, especially in terms of using previous research and developing a theoretical model before going on to a data model. In this he is following the arguments of Hendry *et al* (1990) that much econometric practice has been a-theoretical in developing empirical models independently of theory and using *ad hoc* criteria to choose between the many competing models. Fildes suggests that a theoretical model can be developed from a wide range of sources – what has been done before, theoretical work, prior research, unexplained findings or discussions with experts. This approach would certainly be applauded by critical realists, particularly against the alternative of an entirely data-driven approach.

However, the applause must be tempered when we see that the main purpose of this stage is not the identification of possible underlying structures or generating mechanisms, but simply large numbers of potential (theoretical) variables. This whole approach only makes sense with a particular view of causality. Fildes does discuss the issue of causality, claiming that an appropriately specified model is as rigorous a definition of causality as you can get. However, this is not satisfactory from a CR perspective. First, that the model will inevitably lack ontological depth. All the variables, and the factors they represent, must occur at essentially the same level of aggregation – organization, industry, economy – whereas the actual world is a complex inter-weaving of different structures at many different levels (Fleetwood, 2001b). Second, despite Fildes protestations, regression does only rely on *association* between variables. Such association could be the result of a genuine causal relation, but could equally stem from a mutual relation with a third, underlying, variable; a causal relation in the wrong direction; or indeed sheer coincidence. Third, the underlying assumption is one of stable event regularities (Humean causation). Although seldom explicitly discussed, Hendry (1990, p. 184) accepts that “I am a Humean in that I believe we cannot perceive necessary connections in reality. All we can do is set up a theoretical model in which we define the word ‘causality’ precisely,

### 3.2 Stability and Closure

Major assumptions have to be made about the closure and stability of the system under study. Extrinsic closure is the assumption that those factors not included in the model will not change, or will not have a substantive effect on the model if they do. In practice, there will be many variables excluded from the analysis for a variety of reasons. It could be because data is not available, or because the factors are not measurable; or because a factor is not operative at that point but could become so; or because the investigators are ignorant of it. These may well have significant effects on the phenomena being analysed (Liu, 1960). Moreover, social systems are never closed but always open to historical change and accident (what is sometimes called ‘path dependence’ (David, 1986).

Intrinsic closure is the aggregating assumption that individual elements are inherently identical, and linearly additive with no emergent properties or non-linear dynamics. A

further point here is that the data itself is often extremely unreliable, as much a reflection of its process of production as some real factor. Fildes recognises that this does cause problems for the modeller “the level of aggregation in a model, the number of hierarchies to be included ... are not so self-apparent. ... Nor is the choice of causal framework straightforward. ... To complicate matters further, systems-based arguments suggest the possibility of higher aggregate-level characteristics” (Fildes, 1985, p. 552). Unfortunately, “it seems that little guidance is to be found in either the systems or the econometrics literature with regard to systems definition.” (p. 553).

The next move is from the theoretical model, such as it is, to the data model and here more major problems occur. The theoretical variables will have to be measured in some way and those that have not been thought of, or cannot be measured, will have to be excluded. The model itself will have to be constructed on the assumptions of *closure* – that the excluded variables will together form relatively small random errors, and *stability* – that the relations underlying the data will remain constant. Little is said by Fildes about these major difficulties other than “a modeller should attempt to include difficult-to-measure variables” (p. 555) and, on stability, “On the face of it a prime requirement for effective forecasting is stability of the model over time.” (p. 559) Some of the many problems with regard to measurement, generally acknowledged by Fildes, are: factors that are important but cannot be measured in principle; factors that cannot be measured directly, but for which there may be proxies; factors that can be measured but for which the measurements may be unreliable, or have missing data; factors for which there may not be sufficient data; factors that may be measured differently across observations; factors that are themselves difficult to forecast; data rarely conforms to the statistical assumptions underlying the methods.

### 3.3 *Ad hoc* Nature of Modelling

Any model is always under-determined by the data. Partly because of the previous point, many different sets of variables, subject to different sets of relationships, will be compatible with the data. It is generally not possible to choose between them on purely statistical grounds and so all kinds of other, judgmental, factors come into play – *ad hoc* criteria, personal belief or intuition, ‘experience’, potential usefulness, robustness, etc. These undermine the supposedly ‘scientific’, observer-independent approach. Often the form of the model is chosen more for its mathematical properties, i.e., its tractability, than its realism.

Having arrived at a set of data, often based on what is available as much as anything else, the next stage is the building of the model itself. Many decisions have to be made in developing a model: its functional form (e.g., linear or non-linear, absolute, differenced, transformed); single equation, structural equations, or vector autoregressive (VAR); what lag structure to use if any; which outliers to delete; what to do with missing data; and which variables to include or exclude.

Here, the major critique is that the model is always under-determined by the data and therefore choices are made in an *ad hoc* way, often based neither on sound statistical theory nor on domain-specific theory. That this is the case is amply demonstrated at many places in Fildes’ paper. He does give a general modelling strategy but it is clearly the case that experienced modellers could easily come up with significantly different models based on the same set of data thus undermining claims to researcher-

independent objectivity. This has been demonstrated empirically by Magnus and Morgan (1999) who conducted an experiment in which an apprentice had to try to replicate the analysis of a dataset that might have been carried out by three different experts (Leamer, Sims, and Hendry) following their published guidance. In all cases the results were different from each other, and different from that which would have been produced by the expert, thus demonstrating the importance of tacit knowledge in statistical analysis.

Magnus and Morgan conducted a further experiment which involved eight expert teams, from different universities, analysing the same sets of data each using their own particular methodology. The data concerned the demand for food in the US and in the Netherlands and was based on a classic study by Tobin (1950) augmented with more recent data. The teams were asked to estimate the income elasticity of food demand and to forecast per capita food consumption. In terms of elasticities, the lowest estimates were around 0.38 whilst the highest were around 0.74 - clearly vastly different especially when remembering that these were based on *the same sets of data*. The forecasts were perhaps even more extreme - from a base of around 4000 in 1989 the lowest forecast for the year 2000 was 4130 while the highest was nearly 18000 (see Magnus and Morgan (1999, Figure 4 p. 297))!

### 3.4 Statistical Modelling as A-Theoretical

Much statistical modelling is highly a-theoretic. That is, it does not draw on already available theory, even in disciplines such as IS or economics where much is available, but instead restricts itself to simply describing or summarising the data in a concise way. (Cooley & LeRoy, 1985). First, there is the question of statistical theory. There is much available, and it is certainly covered extensively in textbooks, but as Fildes demonstrates it is often ignored in practice and there is little empirical evidence of its actual value in modelling. Second, and more important from a CR perspective, is substantive or domain theory that may be relevant to the situation. Here, Fildes identifies two different approaches to modelling – specific to general and general to specific (although they could more typically be called extrapolative and causal respectively). In the former the main emphasis is on modelling a variable purely in terms of its own past history (auto-correlations). Occasionally, one or two other “explanatory” or dummy variables may be added. This gives a conceptually simple model and a well-defined modelling process but, as Fildes points out, “Little attention is given to the specification of the system in which the variables are defined and measured. Even less attention is given to earlier work, empirical or theoretical” (p. 566).

The approach Fildes recommends, general to specific, does attempt to draw on previous work, although it is usually previous empirical studies rather than theory directly. But he recognises that there is a strong tendency to concentrate attention on the data that is available, perhaps using a variety of techniques, rather than trying the harder task of generating new data on the basis of available theories. However, several econometricians are much more critical arguing that econometrics is, in general, profoundly a-theoretical (Cooley et al., 1985, Hendry et al., 1990, Hoover, 1988, Koopmans, 1947). One of the comments made by the assessors of the Magnus and Morgan experiment was: “{a}nother worrying aspect is the shift towards a more inductive approach, a shift away from the economic underpinning in favour of more



elaborate statistical methodology” (Barten, Cramer, Hashem Pesaran, Schmidt & Wickens, 1999).

### 3.5 Significance Testing

There are many serious problems with the prevailing approach to statistical analysis - the “null-hypothesis, significance-testing” procedure (NHSTP) but see Chow (1996) for a defence of this approach:

- The null hypothesis (H<sub>0</sub>), that which is assumed to be the case, could almost never be true.
- The null hypothesis (H<sub>0</sub>) also includes the many assumptions of the model which are also highly unlikely (especially in the case of regression).
- The alternative hypothesis gives no specific information about the situation. In reality there are many possible hypotheses and the choice of H<sub>0</sub> and H<sub>1</sub> are essentially arbitrary.
- The chosen significance level is essentially arbitrary and therefore so is the likelihood of a significant result. The same is true of sample size.
- The results do not give the probability of H<sub>1</sub> being true, nor do they relate in any way to practical importance or significance.
- In practice, results tend to be either obviously significant, or not, and thus not requiring a test. Or, if they are in the grey area a larger sample is needed.

### 3.6 Model Accuracy

Generally, the main purpose of building a regression model is to be able to make forecasts or predictions with it. Yet the empirical evidence is that econometric models do very poorly in *ex ante* forecasting (Hutchison, 1994, Kay, 1995, Leamer, 1983, Rosenberg, 1992, Smith, 1995). That is, forecasts made before the event, using where necessary forecasts of the independent variables, as opposed to *ex post* forecasts using the actual values of the independent variables. Even in the latter case performance on holdout samples is much worse than within the sample on which the model was estimated. Sherden (1998), in a provocative book about the difficulty of predicting the future, has documented how poorly forecasters in a whole variety of fields perform: “Of these sixteen types of forecasts, only two - one-day-ahead weather forecasts and the ageing of the population - can be counted on. The rest are about as reliable as the fifty-fifty odds in flipping a coin” (p. iii). Even Fildes (2000), in a partly critical review of the book, accepts that much of the criticism of economic forecasters is justified.

Fildes himself has carried out three major reviews of the accuracy of model-based forecasting. The first (Fildes, 1985) summarised all the comparative literature that could be found up to that point. It compared forecast accuracy for “causal”, i.e., regression models, against two other approaches – simple extrapolative and judgmental. In the comparison with extrapolative 80 papers were examined. The conclusions were fairly clear and depended on whether the forecasts were *ex post* or *ex ante*. With *ex post* forecasts, where all the independent variables were known, results generally favoured modelling rather than extrapolation. However, with *ex ante*

forecasts (the actual situation in practice) results were more mixed with the models often performing less well despite sometimes having judgmental “corrections” made to the actual forecasts to reflect developments not currently in the model. The results on judgmental forecasting (from only a few comparisons) were even less clear with no particular advantage to modelling emerging.

Allen and Fildes’ (2000) paper synthesised these studies together with a major comparison by Armstrong (1985) and post-1985 studies. The results, if anything, are even less clear. With regard to extrapolative models, Armstrong claims that they are better than model-based in the short term, but Allen and Fildes argue the relative performance is similar regardless of time horizon. In terms of *ex post* and *ex ante* the later evidence seems to contradict the earlier, but this depends very much on which studies are included. With regard to judgmental forecasting, the results suggest that causal methods will *only* be superior where there are many observations available and large changes in the environment are likely. Again, however, the results can be interpreted in different ways.

Fildes and Stekler (2000) provides a detailed consideration of the accuracy of macroeconomic forecasting in the US and the UK. Their review is best summarised in their own words: “We have found that most forecasters fail to predict recessions in advance and sometimes fail to recognise them contemporaneously. Forecasters also seem to make systematic errors such as underestimating growth during periods of economic expansion, overestimating it during declines, under-predicting inflation when it is accelerating and over-predicting when it is decelerating. Despite the *relatively large errors observed*, almost all forecasts are superior to the predictions that could have been obtained from naïve models and *often* better than those generated by times series models. ... In addition, there is little evidence that forecasts have improved over time” (p. 29-30, my emphasis). Unfortunately, these studies do not generally discuss the *absolute* accuracy of any of the forecasting methods nor which methods may be appropriate in which circumstances.

We can perhaps summarise the state of play with Allen and Fildes (2000) conclusion: ‘A well specified econometric model should forecast at least as well as the naïve no-change method.’ (p. 348). Given the problems, outlined above, of the availability and reliability of data, the difficulty of developing a ‘well-specified’ model, and the significant costs in time and expertise involved, then this is surely damning with faint praise.

## 4. The Place of Statistics within IS Research

Despite this critique of statistics as conventionally conceived, there is a significant role for statistical description and analysis within CR perspective.

At the beginning of a research project producing and analyzing data will be valuable in gaining a clear appreciation of the situation. Such analysis can begin to identify the major relationships and constraints that will be of importance, but can only ever give a partial picture. More specifically, some statistical techniques are very good at identifying patterns in large sets of data. For instance, supermarkets have vast stores of data about our shopping habits but uncovering unexpected relationships requires sophisticated data mining or multivariate statistical techniques.

Such modeling is what was described above as descriptive rather than explanatory. That is, its aim is to generate a compact representation of the patterns and relationships of the data itself, without going beyond that to trying to explain the underlying causal mechanisms at play. This is essentially the second phase of research, *analysis* – which is really the main plank of critical realism's retroductive methodology. Generally, this phase requires theoretical understanding and imagination to come up with potential explanations rather than detailed modeling. But there are modeling techniques that can be valuable. Within statistics, methods such as factor analysis and path analysis do aim to go beneath the surface to draw out latent variables or causal connections.

The next research phase often involves assessing competing causal explanations, or of potential changes to the situation that may be desirable or feasible. Here again modeling can be useful. Statistical methods such as analysis of variance or covariance, or correspondence analysis can be used to test for the effects of hypothesized structures (or demi-regs as Lawson (1997) calls them). Whilst we do not expect to find universal regularities with the social and organizational world, nevertheless underlying causal structures may well give rise to differences or contrasts that are relatively enduring over space or time. Such differences could relate to ways of structuring organizations; ways of producing goods or services; different types of information technology, or methods of IS development. Statistical tests can be used to detect the existence of such contrasts or to check the efficacy of possible changes.

The fundamental point is that statistical modeling does have a serious role to play within IS research but, theoretically at least, that role should always be seen as one of assisting the general critical realist approach of better explanation and learning, rather than simply description or prediction. And, the inevitable limitations of such models to that which may be quantified means that they can only ever be part of the whole research project.

## 5. Conclusions

This paper has been primarily concerned with the nature and role of statistical modelling in information systems research. It may seem as though it has largely been negative and critical, but we would like to make it clear that our concerns are not so much with statistical modelling itself, but with the predominantly empiricist understanding of the modelling process. From a critical realist perspective statistical modelling can be very useful as a way of discovering patterns of events that reveal the presence of underlying structures; conceptualising, and exploring the behaviour of possible generative mechanisms; and conducting analyses to test out possible explanations. They should not, generally, be seen as simply driven by, or descriptive of, available sets of data and should be part of wider investigations that include the softer, qualitative, aspects of research situations.

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