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**A critical epistemology of analytical statistics:  
addressing the sceptical realist**

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Abstract

Some methodologists have challenged the usefulness of statistics by arguing that the ontology implied by their use is inconsistent with the complex ontology of critical realism. Other critics of statistics take a strong social constructivist approach to research methodology. One problem with these sceptics' arguments is that they confuse the method of analytical statistics with the methodology of empiricism. We disentangle the two, and present a constructive argument supporting the cautious use of analytical statistics. The first part of the paper argues the case for an interpretive approach to statistical findings. In the middle of the paper an exemplar is presented showing that multivariate regression results can offer non-intuitive findings, can support non-atomistic interpretations, and can help underpin retroductive explanatory arguments. In exploring the nature of the warranted arguments that can arise after doing analytical statistics, we stress that explanations are emergent and do rest upon the workings of the statistical techniques and practices. We argue against seeing statistical techniques as a 'black box'. Instead, arguments can be developed, with justification resting in part upon the statistical results, in an audience-specific context of argumentation. The data which underlie statistical methods are not factual; the data are more like ficts than facts. Our argument is therefore that warranted arguments can be and are developed by social scientists who may use analytical statistics alongside other methods of research. Details of the argument can be explored further but it is important to establish that the sceptics' arguments are too dismissive of multivariate statistical analysis.

Abstract word count 256

## A critical epistemology of analytical statistics: addressing the sceptical realist

‘Arithmetic had entered the picture, with its many legs, its many spines and heads, its pitiless eyes made of zeroes. Two and two made four, was its message. But what if you didn’t have two and two? Then things wouldn’t add up. And they didn’t add up, I couldn’t get them to; I couldn’t get the red numbers in the inventory book to turn black. This worried me horribly; it was as if it was my own personal fault. When I closed my eyes at night I could see the numbers on the page before me, laid out in rows on my square oak desk at the button factory – those rows of red numbers like so many mechanical caterpillars, munching away at what was left of the money. When what you could manage to sell a thing for was less than what it paid you to make it – which was what was going on at Chase and Sons for some time – this was how numbers behaved. It was bad behaviour – without love, without justice, without mercy – but what could you expect? The numbers were only numbers. They had no choice in the matter.’

--- Margaret Atwood, *The Blind Assassin*.

### Introduction

For many critical realists analytical statistics<sup>1</sup> constitute something of a stumbling block (Sayer, 1992: 190-199; Lawson, 1997: 69-85, 2003: 247; Fleetwood, 2001: 206-209). The principle reason for this is that critical realism takes its original cue from a rejection of positivism in the philosophy of science (Bhaskar, 1975). This rejection, in turn, and extended to the social realm (Bhaskar, 1979), is based upon an ontological critique of both systemic closure and constant conjunction causal regularity. From this sceptical perspective analytical statistics are inappropriate along two lines of argument. They either match a “problematic” method to an indefensible ontology or create an apparent contradiction between a “problematic” method and a more defensible ontology. In either case the sceptical conclusion seems to be that analytical statistics are an inappropriate method on which to place any great reliance in research on aspects of social reality.

The method is problematic for the sceptic both because the significance of the method often requires the assumption that all relevant data are incorporated (a

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<sup>1</sup> In this paper we use a widely accepted notion of analytical statistics as the mathematical process of manipulating survey data in an attempt to reach “well-founded” conclusions which generalise across the region and time-period from whence the data came Marsh, C. (1988). *Exploring Data*. Cambridge, Polity, Dale, A., E. Fieldhouse, et al. (2000). *Analyzing Census Microdata*. London ; New York, Arnold : Oxford University Press. The analytical methods can be seen as including regression as well as some exploratory methods such as factor analysis.

closure in method) and because the manipulation of data by the formulae of analytical statistics tend to be based around the construction of regularities (albeit in a probabilistic sense) between independent and dependent variables by method. Ontology (implicit or explicit) is indefensible if the *presumed* significance of results in the application of analytical statistics is based around some or all of the following assumptions:

1. That there *are* repeated, regular relations between independent and dependent variables of a constant conjunction form (rather than say, a demi-regularity form).
2. That the existence of regularity is sufficient to indicate a relation.
3. That the absence of regularity is sufficient to indicate no relation.
4. That identification of this relation is either sufficient or necessary to provide grounds for adequate description and/or explanation and/or forecasting of events.
5. That this relation is for all-intents-and-purposes enduring or intransitive reaffirming both 4. and 1.
6. That in terms of the focus on 1-5 the system appears to be closed.

Analytical statistics are, however, equally problematic for the sceptic if one makes opposing assumptions to 1-6 in one's implicit or explicit ontology since there now appears to be a mismatch between method and ontology. For Sayer, for example (1992: 194):

My impression is that statisticians see theories as ordering frameworks whose basic building blocks are empirical regularities. The provision of such a theory would only pose anew the problem\m that regularities are not necessarily causal. Given the disjunction between mechanisms and events, a strong correlation (or some other quantitative association) need not imply causation, nor a weak one absence of a causal or structural relation. If a theory is to help solve this type of problem it must postulate causal mechanisms and not merely specify how total variation in the dependent variable might relate quantitatively to variation in the independent variables.

Our interest is in whether it is still possible to provide a philosophical and practical defence of the careful application of analytical statistics in a way that is acceptable to sceptical critical realists, and indeed, to social theorists generally. Two possible routes seem open to provide this defence. First, deconstruct the sceptical case and second, with reference to the first, construct a positive argument for the use of analytical statistics. The former is not simply a matter of showing the sceptics to be wrong concerning the substance or content of argument nor is the latter simply one of supplanting it with correct content. This is because the interesting divides are not necessarily about the broad general

outline of what an analytical statistic is and does. There is little disagreement that analytical statistics do tend to require assumptions of closure by method and do focus on regularising through mathematical manipulation of data. Similarly, the interesting dividing lines are not about defensible ontology. It is certainly the case that there are many mainstream economists and perhaps sociologists and social policy modellers who use analytical statistics allied to an indefensible ontology. It is worth engaging in critique of this. This is not a difficult argument to make; it is simply difficult to get those it is aimed at to listen to the argument. Winning this argument is about strategies of power not dilemmas of how to make a substantive social theory or philosophical argument. The difficult debate is between the sceptics and those who broadly share much (not necessarily all or the details) of their view of defensible ontology – other critical realists, realists, and “soft” social constructivists (Downward 2004; Pawson and Tilley, 1997; Dow 1997, 2002; and see discussion in Sayer, 2000). Thus, the area of apparent contradiction between method and ontology would seem to be the fulcrum of debate of both deconstructing the sceptical case and constructing a positive alternative.

This fulcrum is important because what is at issue is not the question:

*How do analytical statistics do what they do?*

But rather the question:

*How can we interpret what we do and how can we (if we should at all) incorporate it into research?*

This is a matter of what can be done with analytical statistics and not directly the technical aspects of what is done with data. At the same time it is not a functional, instrumental or pragmatic argument. It is rather a clarification of one main area of disagreement that then allows us to ask what difference interpretation makes to the use of closed and regularising method(s) in terms of an open system. This in turn allows the important distinction to be maintained between method and methodology.

We would define a method as a tool or technique. Analytical statistics include such methods as regression and factor analysis. Each method has a definite form and operation that can be distinguished from its particular use and application and from our further understanding (in addition to the nuts and bolts of the operation) of what we are doing when we do so. Our understanding of what we are doing raises issues of methodology. We would define a methodology as a combination of techniques, the practices we conform to when we apply them, and our interpretation of what we are doing when we do so. Methodologies

often have embedded in them assumptions about the nature of reality and underlying or implicit axioms about human behaviour. The distinction between method and methodology is important precisely because it sheds light both on the nature of the sceptical argument and the possibilities of the positive argument.<sup>2</sup>

It is worth pointing out at this stage that the structure of an argument and the implications drawn from it by its proponents are not the same as its content. There are relations between them but they are not the same thing. If they were, then one could not challenge the rationality, logic, or line of reasoning of another, merely the truth or falsity of their substantive propositions (grass is green, God is real). The principle content of the sceptical argument is focussed on closure and regularity. It is structured around identifying the interface between closure and regularity in method and two possible ontologies that accompany it. The formal implications drawn from this are that analytical statistics are “primitive”, inappropriate, misleading, ill-founded and more generally, unpersuasive (Lawson, 1997). The response of the sceptic to analytical statistics is polite rejection. Lawson, for example, has often noted that he remains open to the possibility that someone might provide a plausible argument for analytical statistics -- but that he has yet to see one he could endorse. Sayer notes that “If such methods are to gain any plausibility they must be supplemented by realist appraisals based on qualitative causal and structural analysis” (1992: 193). But Sayer also suggests that the only variety of statistics he feels able to endorse, even with this caveat, is “descriptive statistics” (*ibid.*). He defines descriptive statistics as quantification of only certain social objects amenable to such quantification, such as demographics, where the manipulation neither seeks inference nor claims further explanatory significance for the characteristics of a population from the sample. This is statistics stripped of analytical significance.

The reason why the identification of the fulcrum of argument and the distinction between method and methodology are important is that polite rejection is apparently based on the way the method-ontology interface is interpreted. It is argued that a closed and regularising method is not appropriate for the investigation of social reality in the light of an open system ontology (Lawson, 1989). The assumption *within* Lawson’s point, however, is that methodology cannot effectively transform the way we use analytical statistics in a way that provides some kind of defensible interface between method and ontology. This may seem like a rather laboured point but we would argue that it is significant because it focuses on two questions:

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<sup>2</sup> The epistemological issues that distinguish different methodologies have been commented upon by a number of authors. For a review, see Olsen, in Carter and New (2004). One aim of the present paper is to challenge the commonly noted division of epistemologies into quantitative and qualitative schools.

1. Is rejection of analytical statistics warranted in terms of the form of the sceptical argument?
2. What nuance can be brought to the interface of method and ontology through methodology to change the meaning structure of method and the way in which we use method(s)? Could such a nuance thus affect the effects of closure and regularity in method, and by implication, solve the problem of its relation to ontology?

These questions form the basis of §1-5 hereafter.

In §1 we identify some aspects of the rejection of analytical statistics in order to assess whether rejection is genuinely based on ontological critique that absolutely undermines the tenability of method. We would argue on two related counts that it may not be. First, if one takes the locus of argument directed against analytical statistics much of it, as the sceptics are aware, is actually a critique of practice and attitude rather than method. Clearly, such critique is not in itself a refutation of method since bad practice can undermine any method. The interesting point in terms of the sceptics' rejection of analytical statistics is whether the critique of practice and attitude influences rejection. To explore whether this may be the case we analytically distinguish these aspects and then in a second argument show that the discursive shift from scepticism to rejection seems to rely to some degree on a blurring of the critique of the practices and attitudes of many who use analytical statistics (particularly mainstream economists) and the rejection of the tenability of analytical statistics as a method based in the identification of the ontological limitations of closure and regularity. This in turn blurs the boundary between methodology and method. Since ontological limitation is not the same as a lack of tenability in method, limitation seems to have been translated into rejection by some other discursive means i.e. other aspects of methodology. We illustrate this as a possible classical philosophical error where certainty (rejection) is derived from doubt (ontological limitation).

Arguments in §1 provide us with the opportunity in §2 to explore the nuances of closure and regularity in order to set up a number of criteria, focusing on possible inflections of the link between the regularising method(s) of analytical statistics, an ontology that accepts that the social world is open, and the reconstruction of practice and attitudes. These criteria form the basis of a positive challenge to analytical statistics. Meeting them would provide a defence of analytical statistics that neither dismisses the critical realist critique nor falls into errors of the twin straw men of positivism and strong social constructivism.

The rest of the paper is concerned with beginning to meet this challenge. This is more than the work of a single paper. The point here is to show the plausibility

of at least considering the challenge worthwhile. In §3 we explore one specific statistical procedure (logistic regression) with reference to the criteria in §2. In §4, we illustrate the way one can reach valued interpretive conclusions from a statistical analysis of this kind. Here it seems sensible to frame analytical statistics as an ongoing engagement by the researcher with both method(s) and data in a way that discourses of objective science often disguise. That engagement is itself situated as one element in a methodologically plural research project. Thinking about practice in this way provides grounds for reconceptualising how we think about the products of analytical statistics. One might argue, in accordance with fallibility and on the basis of statistical method that they are neither facts (as mirrors of reality) nor arbitrary fictions. As the informed products of research on some aspect of social reality by a researcher who is also part of social reality in a general sense and intimately engaged in the study of a particular aspect of it, they might best be referred to as contingent assertions of relations, possible descriptions, sources of speculation and sources for explanation, or more concisely, “ficts”. The construction of “ficts” is a research practice which mediates between a method predicated on closure and regularity and an open methodology amenable to investigation of an open social reality. Finally, §5 describes the implications of our argument for closure in two senses: in the causal sense, and in the classification sense.

### **1. Analytical Statistics – Refuted, or Limited?**

The points made in §1 are simply meant to be recognisable as elements of a critical realist critique of analytical statistics. The purpose is not to assert that the proponents of these arguments framed them in precisely this way. The point is to highlight that the philosophical arguments about practice and attitude are analytically distinguishable from philosophical arguments about other ontological limits to analytical statistical methods. This is important because it allows us to raise the possibility that these distinguishable elements colour the inferences drawn from the further critique i.e. that the rejection of the tenability of analytical statistics on the basis of aspects of the ontological limits of closure and regularity owes something to resolvable issues of practice and attitude. To forestall any misinterpretation of what the distinction means in terms of the overall argument it is perhaps also worth stating that we go on to argue that addressing issues of practice and attitude has implications for the tenability of method since it affects the meaning structure of closure and regularity.

#### *Practice makes imperfect*

Closure and regularity raise a fundamental dilemma for critical realists in terms of the sense that they make of analytical statistics. The matching of closure and regularity in ontology and method is one conducive to a position (either by



default or positively affirmed) that data can be a mirror of some aspect of the world. Unlike positivists, critical realists reject this notion of the mirror on the basis that it entails an “ontic fallacy” where objects provide definite knowledge of themselves (Collier, 1994: 104). This raises various general problems, identified in the Western philosophical tradition (Kant 1969: xiv; Alston, 1993) with the issue of the certainty of sense-perception and the elision of interpretation from *knowing* from experience. Since data collection is an intervention, even before an analytical statistic is applied to data, there is therefore the problem of how to justify the research process. Since the statistical method itself is an additional manipulation of that first intervention, it is two steps away from the initial aspect of social reality under investigation (though the intervention itself is of course also an aspect of social reality). Irrespective, therefore, of any further critique, based on closure and regularity, of analytical statistics in terms of the technical aspects of method, critical realists (though clearly they are not alone in this) are led to criticize any use of analytical statistics in some unproblematic “black box” fashion (Sayer, 1992: 194)<sup>3</sup>. From a critical realist perspective the unreflective uncritical use of analytical statistics (the black box) is indicative of an ideological understanding of what it is do science. As such it is open to critique in terms of an implicit ontology and since all positions entail ontology they are thus susceptible to philosophical inquiry on this basis (Bhaskar, 1989: 2). The dominant implicit ontology is that of positivism, or more broadly, any one of a variety of forms of empiricism, where collection, collation and method are collapsed together and the philosophically inarticulate process of research proceeds as though knowing from experience was not a problem.

Importantly, however, at this general level of philosophical critique, since what is under scrutiny is ontology as ideology, what is at stake is actually the *practice and attitude* of s/he who is using analytical statistics. There are two aspects to this. First, there is the general critique of the black box. Because this is about practice and attitude, and also, therefore, in a sense concerned with research habit, opportunity and broad issues of the socialisation of the academic, the black box is a criticism that critical realists often share with non-philosophically inclined commentators of various hues, including those who would claim to be positivists yet seem unaware of the fundamental problems of their position. In the debate on formalism in mainstream economics, for example, Milton Friedman (1991: 36) notes:

The computer revolution has I believe induced economists to carry reliance on mathematics and econometrics beyond the point of vanishing returns... It is enormously time-consuming to gather original data, even from archival sources let alone by direct observation, to piece together different sources,

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<sup>3</sup> Sayer refers to this as use in a ‘cookbook’ manner.

explore in detail their reliability and accuracy, and derive a full understanding of the historical and institutional circumstances under which they were generated. These considerations long rendered abstract theory, including mathematical economics, and doctrinal history the preferred areas for generating a record of publication for professional advancement. More recently, the easiest way to avoid perishing by not publishing is to access an existing data base, download a batch of data to your computer, and put the data through the econometric wringer with one or another of the statistical programs described in the recently added 'Software' section of the Journal. A multiple regression that 45 years ago required three months for a skilled operator of a desk calculator, and 40 hours on the most advanced large-scale computer, today takes me less than 30 seconds on my home computer.

This latter issue about data sources and running multiple regressions brings us to the second aspect of what is actually being criticised at this general level of the ontic fallacy. Sceptical critical realists tend to argue that the articulation of the output of analytical statistics elides the actual process or conduct by which "significant" results were generated. Because many practitioners hold an empiricist view of objective social science where genuine facts will simply emerge from good research and will speak (be affirmed) for themselves, the actual process by which the data has been multiply manipulated ("jigged") is disguised, glossed over, or unstated. The weakness of analytical statistics is therefore revealed by a "theory-practice inconsistency". To state the practices actually engaged in would be to reveal this inconsistency and thus to affirm from the practitioner's own understanding of science that their findings were "unscientific". Lawson, for example, often makes the point that for every published regression analysis we should ask how many others did the theorist run and in what ways did s/he run them, and why were they *not* published or alluded to in the final product?

As with the black box argument, since the general line of critique deployed is that the researcher is insufficiently philosophically aware to acknowledge the consequences of the theory-practice inconsistency, the problem is once more articulated as one of implicit ontology. Again, this appears to be a problem caused by the very nature of the socialisation of the researcher. It is, therefore, a form of ontological critique that is about ideology and thus about practice and attitude. Again, as a consequence, inconsistencies of practice and attitude are a subject of critique (though perhaps not always in the same form) that sceptical critical realists share with others, including, significantly, advocates (some of whom are critical realists) of alternative approaches to analytical statistics (Lee 2002; Ziliak, 2003; Downward, 2004).

For the sceptical critical realist, where the black box argument reveals a tacit positivism that may be directly critiqued in terms of aspects of the philosophy of positivism, the theory-practice inconsistency argument employs a contrastive strategy to highlight a discursive contradiction that further undermines the plausibility of adhering to a positivist philosophy. The purpose of highlighting these two elements in the sceptical critical realist critique of analytical statistics is twofold. First, to indicate that the two elements are analytically distinguishable from further critique – where further critique identifies ontological limits to analytical statistical methods on the basis of technical aspects of closure and regularity. Second, to raise the possibility that these two elements play some role in the inferences drawn from that further critique i.e. that ontological limitation warrants rejection of analytical statistics.

*Rejection, ambivalence, (un)certainty, and doubt*

We would argue that the identification of problems of practice and attitude are *not* in themselves a refutation of method because a method is narrower than a methodology. A methodology is a combination of techniques, the practices we conform to when we apply them and our interpretation of what we are doing when we do so. In social science research textbooks, methodologies are seen as embodying epistemological assumptions, whereas methods can be used by practitioners coming from different epistemic starting-points (Bryman, 1998). Practice and attitude thus have consequences for methodology but methodology should not be conflated with any other form of critique of the technical aspects of a method, philosophical (ontological), or otherwise. This is important because it might be that the significance of a method can be transformed by a reconstruction of methodology. It is not implausible to suggest that methods predicated on closure and regularity can contribute to an open methodology that in turn has something valuable to say about an open social reality. It is not implausible to suggest that an anti-positivist methodology, one informed by a critical realist understanding of ontology, might allow a different meaning structure and significance to be imposed on method. The inverse of this possibility is to suggest that a decisive refutation of method (a warranted rejection) by the sceptical critical realist ought to hinge on the critique of the ontological limits of the technical aspects of method in so far as they can be shown to be *incompatible* with an open methodology and an open social reality. But, in one sense, at least, the sceptical critical realist rejection of analytical statistics is not of this kind because it is framed in terms of an initial ambivalence. Ambivalence implies uncertainty about whether this incompatibility is so. Ambivalence of this kind implies not only deferral of judgement but also begs the question of the critic i.e. are they able to falsify the counter-assertion that a method predicated on closure and regularity is compatible with an open methodology and has something valuable to say about an open social reality? A

sceptic cannot maintain that they have decisively achieved this at the same time as holding that they are willing to be persuaded otherwise. This would turn ambivalence into contradiction. We're not suggesting that sceptics do assert that they have decisively established an incompatibility – the debate is unfinished. The point is that in terms of the pure logic of argument rejection is itself incompatible with ambivalence. To account for rejection, therefore, it would seem that one must look beyond logical entailment and thus beyond argument concerning the technical aspects of the ontological limits of method and think in terms of the counter-attitude of the sceptic to the wider significance of the practice and attitudes with which analytical statistics are entwined. In this sense rejection, seems to rely on the blurring of different aspects of ontological critique, the technical and the ideological. Rejection, even subconscious hostility, would seem to owe something to the socialisation of the sceptic as it situates that ideological critique. Perhaps problems of analytical statistics as method have become enfolded in issues of antagonism regarding the role of analytical statistics in relations of power – such as those in mainstream economics – as they are expressed in methodological critique. If this is the case then the sources of the translation from scepticism to rejection are conducive to the ossification of a position of critique, which in turn invites polarisation. Such a turn of events would be an impediment to constructive debate.

None of the above is to suggest that scepticism is not a valid position or that the ontological critique of the ideological elements of practice and attitude is without basis. The point is that it neither warrants nor accounts for rejection. The identification of a possible ontological limitation does not in any *a priori* sense entail the necessary rejection of a given method. In analytical philosophy the move to rejection would be an inappropriate logical relation between antecedent and consequent, a fallacy of deriving certainty from doubt. This can be illustrated using a well-known syllogism:

1. I know that George Orwell wrote 1984.
2. I do not know that Eric Blair wrote 1984.
3. Eric Blair did not write 1984.

Eric Blair is of course George Orwell's original name. What this suggests is not that the converse of 3 is necessarily true from 1, since this would equally be certainty from doubt in 2, but rather that 3 is conditional on additional information or argument (in this case knowledge of former names of George Orwell). This is a fairly basic philosophical point whose purpose is to open up the possibility that there may be a defence of analytical statistics. It is not a defence in itself. It is not a denial that the identification of ontological limitations provides a basis of critique. Such critiques are relevant to subsequent arguments.

The point is that rejection is an additional step which would require an additional rationale.

## **2. Discussion of Closure and Ontology**

In the previous section we suggested that a warranted rejection of analytical statistics by the sceptical critical realist ought to hinge on the critique of the ontological limits of the technical aspects of method in so far as they can be shown to be *incompatible* with an open methodology and an open social reality. This can be developed further since rejection of a *particular* method might follow if:

- The method were subject to such severe ontological limitations that its application was incapable of shedding light on some aspect of the world.
- There were alternative methods of exploring the same phenomena that did not share its ontological limitations and were not themselves subject to other more severe defects.
- Its ontological limitations were to simply render it incompatible with other aspects of a well-rounded methodologically plural research.

In the most general sense of logical entailment none of these possibilities necessarily flows from the general identification of ontological limitation. This is because they are conditional statements. The nature of their conditionality is compatible with the critical realist insistence on the anti-foundational and analytical distinction between knowledge of reality and reality itself. Just as knowledge is not certain, methods are not perfect. This being so, it is always possible to legitimately mount a case for the use of any given method that is otherwise held to be problematic. This is an important ramification of modern anti-foundational epistemology. To argue otherwise is to create one's own theory-practice inconsistency. Such an argument would tacitly hold that the measure of adequate knowledge or appropriate method was demonstrated conformity or identity with reality – this is the positivist mirror by another name.

Realists such as Goldman (1999) and Alston (1996) argue in harmony with critical realism that it is precisely because knowledge is not certain that justification plays such a strong role in sustaining knowledge claims. We would argue that there is no reason why this licence should not be extended to method. Such a justification, of course, requires a positive case, not just the creation of doubt by highlighting the additional link in a chain of argument that leads to rejection. That case must address the question, why would one not reject analytical statistics? In terms of the anti-foundational point made above, this raises the further question, in what sense and how far can a method *not* conform to broad principles about reality inferred from ontological argument (openness, long-term irregularity, transitivity, depth etc.) and yet still be defended? This can only be

answered in terms of an exploration of ways of working through the link between regularising method(s) of analytical statistics and ontology.

A first stage might be to argue for the application of particular analytical statistical methods in circumstances appropriate to their limitations:

- Analytical statistics might be appropriate in the investigation of aspects of society that tend to exhibit or approximate regularity in their relations in a given milieu at a given time.

This justification is relatively unproblematic for the sceptic since it accords with Lawson's concept of demi-regularity. But it also raises a number of issues. If one already knows that an aspect of society is relatively widespread, such that it exhibits or approximates regularity, it might reasonably be asked what the application of the statistical method adds to the research. Is it adding quantifiable exactness to the range of variation in relatively regular relations? Does the application tease out new and unexpected elements of the relations? Can it highlight the breakdown or points of pressure in those otherwise relatively regular relations that might hint at their collapse or transformation? Does quantification allow us to develop new qualitative understandings? As such, is the application capable of enhancing a methodologically pluralist research?

And what if one does not know that an aspect of society is relatively closed in its operation and approximates regularity in its relations? Is it the case that a method predicated on closure ceases to provide any meaningful insights? If a method explores the relationship between dependent and independent variables, but not all variables are accounted for, or not all the important (most causally significant?) variables are known or accounted for, is it possible to argue that the method is still capable of any of the contributions we highlight above? And, in addition to questions concerning justification that arise from a method predicated on closure, questions also arise on the basis that method constructs regularity through the way in which its formula manipulates data. Is it the case that the regularising function of the manipulation artificially conjoins variables irrespective of their real (if any) relation? Does the form of this conjoining or assimilation by the very process of the manipulation simply undermine the possibility that a given analytical statistical method addresses some aspect of reality? Put another way, is it the case that the regularising function of the method means one cannot distinguish the relative regularity (or lack of regularity) of the aspect of reality?

In the first instance, a good way to address these questions is through a practical step-by-step analysis of the procedures and entailments of a given analytical statistical method. Multiple logistic regression, for instance, is one widely used

technique which we will focus on here.<sup>4</sup> Several points arise in the course of this analysis: the role of closed mathematical systems; the closure of the prior theoretical framework; the ontic status of regularities in the real system and in the mathematical estimates; and the role of the analyst in interpreting results. Iteration between stages is a crucial part of regression *as a methodology*. The researcher examines the data, reads the literature, manipulates data to attempt to operationalise central concepts, generates results, returns to the literature to make comparisons, and keeps iterating around these procedures. The methodology described by Danermark *et al.*, which mentions stages but not steps (i.e. tasks which can be returned to repeatedly and do not come in a strict linear order) is consistent with the analytical statistical methodology described here (Danermark 2002). Our argument is set out below.

However, working through those procedures ought to also generate some further concrete points for a positive case in epistemology for the justification of that method. Several arise in the course of this analysis. In a general sense they tend to indicate that rejection need not follow from ontological limitations *per se*. Instead, careful methodological specification can provide nuance and inflection to the manner in which a particular method expresses what are otherwise highly generalised ontological limitations (in this case closure and regularity). Specifically, eight claims or criteria can be set up to provide for a multi-faceted defence of analytical statistics:

1. Methodological closure need not presume a closure in reality for explanatory significance. Non-identity between the two means that a closed method may still contribute to a realist account of an open system where the degree of openness is not known in advance.
2. A manipulation that constructs regularity need not imply that the basis of that relative regularity is arbitrary or unrepresentative of the aspects of the world under scrutiny. Synthetic epiphenomena highly determined by method itself can occur, but do not necessarily occur.
3. Regularity-seeking analytical statistics are capable of highlighting non-regularity and the breakdown in relative regularity. Analytical statistics can accommodate complexity and contingency.
4. The interpretation of analytical statistics allows non-atomistic inferences about relations.

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<sup>4</sup> By focusing on a regression technique, we avoid the interesting issues that arise with factor analysis and cluster analysis, which are rather different. Logistic regression also can be applied in a multi-level context so we are not avoiding, but rather welcoming, the complexity issues that arise in multi-level causal interpretation of multiple regression. On the latter issue, see Byrne, D. (1999). Complexity Theory And The Social Sciences : An Introduction. London ; New York, Routledge.

5. As a consequence of 1-4, the results obtained through analytical statistics can be counter-phenomenal or unexpected. As such they are able to contribute to a qualitative understanding.
6. The role of the analyst in the initial choice of method(s) and in the subsequent development of the particular research application is highly significant in realising the possibility of 1-5.
7. As a consequence of 1-6, manipulations can contribute to retrodution to causal mechanisms rather than hypostatising<sup>5</sup> variables as chains of events through interpolation.
8. 1-7 imply that an analytical statistical method may be appropriate as part of a methodologically pluralist research project.

The agenda for research implied here is wide-ranging. In the next section we will describe one particular technique and its application. The interpretation based on using this technique can briefly illustrate steps 2-4 in the above list. Then in section 4 we describe how survey data are best perceived not as facts, but as contributing to qualitative understanding (step 5 above) – ‘facts’, one might say. In the concluding section (5) we pull together the argument, noting that steps 6 and 7 are important but recognising that these are areas for further investigation.

### **3. An Illustrative Technique: Logistic Regression**

Logistic regression is an analytical statistic which has grown in popularity for two reasons: Firstly, because the dependent variable in regression can be a simple yes/no binary indicator; and secondly, because computers make it increasingly easy to estimate the models. Logistic regression will be described, first through its underlying concept of odds ratios, and secondly as it appears in a multiple regression context. The exemplar provides a basis for a detailed discussion of the surrounding methodology.

We can use odds ratios of the probability of being employed in London to illustrate. The odds of being employed in London is a ratio of the number employed to the number not employed (i.e. unemployed or not currently working for pay). For non-Londoners, too, there is an odds of participation. The ratio of these two odds – for London residents compared with non-London residents – is the odds ratio. In 2000 this ratio was 1.5, showing that the odds of participating were half again as high in London as in the rest of the country. This

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<sup>5</sup> Hypostatising refers to acting as if, or assuming that, the relations between things were constant.



number 1.5 is a ratio of two odds.<sup>6</sup> An odds ratio is always 1 if there is no difference between the two groups, and can never be negative.

Andrew Sayer and Tony Lawson have both argued that there are limitations to the use of descriptive statistics, with bivariate analysis being better than multi-variate analysis. We disagree with this stated preference. If the odds ratio for employment, outside vs. inside London, is a useful indicator, then the logit equation for employment probabilities with London and other indicators in the equation is likely to be even better. The logit takes the logarithm of the odds of participating as a regression dependent variable. The logarithm is a monotonic transformation that makes the dependent variable have a continuous scale suited to the mathematics of regression. The London odds ratio still emerges as significantly different from the overall odds of being employed, but other complexly interacting factors can be allowed for too.

It is important to realize the limitations of the bivariate odds ratio. If one looked at other incidence statistics using a series of logistic regressions on the odds of being HIV positive, the odds of being mugged, the odds of being married, etc., the 'London effect' could, under certain empirical circumstances, emerge as significant in multivariate analyses where it had been masked in the bivariate analysis. Whether it 'emerges' as significant depends upon the underlying correlations of the various variables. Statisticians are aware of the potential for multi-variate statistics to give results that are (a) superior to bivariate statistics and/or (b) neither visible nor intuitively easy to derive from mere descriptive statistics. The observed association of X1 and Y is affected by the association of X1 with X2 and X3, particularly if X2 and/or X3 is associated with Y but hidden in the bivariate analysis of X1 and Y. Of course it is reality which is influential, and X2 and X3 merely represent real causes by proxy. But the possibility of hidden associations is present whenever excluded variables X2, X3 etc. represent real causes which have effects on X1 or on Y.

For example, consider the participation of men and women in the labour force. In doing so we are not simply seeking either laws or universal patterns, as Cicourel assumed (1964). Instead we are pragmatically seeking to find patterns both within sub-groups and throughout the whole population. These two respective types of patterns may emerge in regression results. For instance if one sub-group has a causal mechanism which others do not have, then identifying this sub-

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<sup>6</sup> The odds were 2.44 to 1 in London, and 3.23 to 1 outside London, for Great Britain. Source: British Household Panel Survey, 2000. British Household Panel Survey [computer file] principal investigator, ESRC Research Centre on Micro-social Change. Colchester: The Data Archive (distributor), 1991/2 to 2001/2. Self-employed people are included together with employed people in these figures.

group with an indicator variable (e.g. mothers, or married people) will help the researcher to notice that pattern. The statistical result (a significant slope coefficient) does not tell us what the cause of the pattern is, but related reading and other types of data can help to illuminate the causal mechanism. On the other hand, if a mechanism is operating generally throughout a population but is counteracted by other mechanisms, then glimpses of a pattern will be shaded and blurred, and that mechanism may be masked.

In logistic regression, in particular, the measurement mode is not assigned *a priori* except in so far as cases (in this case people) are identified as bearers of data. Even here, the 'case' may bear not only the weight of causes that operate on or through persons, but also structural relations between larger classes of people and institutional factors that affect such a person. The variables in the regression are of a variety of levels of measurement (not all are continuous). The dependent variable in particular is qualitative in character. We make two translations of the act of 'entering' a labour market. First, we allow people to declare how they have done that; secondly, we group these declarations into the larger categories 'active' and 'inactive', and thirdly, we transform this binary categorisation into a new continuous variable, the logit of activity. The logit is defined as the log of the odds of being active. The odds of being active are defined as:

*the ratio of the probability of being active to the probability of being inactive.*<sup>7</sup>

This ratio is always positive. We take its logarithm, giving a new number on a wider scale ranging from negative to positive values. The logit, i.e. the log odds, is not constrained to be between 0 and 1. The logit values are related to the odds ratios that can be calculated for every pair of variables. The resulting multivariate analysis is known as logistic regression.

Participation can be defined as reporting either self-employment or employment in the British Household Panel Survey interview for the year 2000. Non-participants include full-time students and those who are retired, doing family care work, disabled, or unemployed. In the age group 16-59 for women and 16-64 for men, 7% of the working-age respondents were fulltime students, 4% of people were unemployed, and 4% were retired. 14% of women and 0.5% of men were doing family care work. 4.5% of the working-age respondents were recorded as being on long-term sick leave or disabled.

Detailed regression analysis provides numerous alternative model specifications, and for each a list of coefficients can be set out in a table. One equation, for instance, summarising some results in this particular case, can be represented as follows:

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<sup>7</sup> This differs from the probability of participation, which is the ratio of the number who participate to the total size of the population.

$$\text{log of the odds of employment} = -1.47(\text{LTLI}) + 0.27 * \text{London} + .61 * \text{Ddegree} - 0.76 * \text{Noqual} + 0.92 * \text{Wife} + .61$$

Each number shows whether the odds of being employed are raised or lowered by the presence of a given characteristic. In this equation, the following definitions are used.

LTLI = Long-term limiting illness (specifically, the person reports that they are unable to do some forms of work due to an illness or other disabling condition)

London= Lives in Inner or Outer London or the rest of the Southeast

Degree = Has a degree and/or a higher degree

Noqual= Has no qualifications i.e. no CSEs or O-Levels or other qualifications

Wife = Is married or cohabiting, and is female

Many other factors could be allowed for but are ignored here since our aim is only to illustrate what can be done and how it can be interpreted. (For fuller estimates, see Walby and Olsen, 2002). There is not space here for a detailed interpretation.

In this equation, there is an implicit base case of people who are not LTLI, don't live in London, have some O-level qualifications, and are not wives – including all men. Most statisticians would interpret the coefficients serially: people with LTLI are less likely to be employed, those in London are more likely, etc. This approach would be empiricist if it were not engaged with theoretical frameworks and asking the question of why these coefficients have been measured. The retroductive questions include: What social, economic, and political factors need to be taken into account in interpreting these findings? what theories from different disciplines, including demography and human geography as well as those already mentioned, need to be considered possible candidates to assist in the interpretation? to what extent do the findings help resolve differences of explanation between the disciplines and their theories?

In this analysis we have taken as an *a priori* assumption the possibility that some of the independent variables are causally related to the outcome. Unpacking this, the *a priori* ontological assumption is that real causal mechanisms in society are reflected in the values recorded for X, Y, etc. These causal mechanisms are likely to overlap in reality. Statistical analysis proceeds using three steps initially: using the data to reflect and test the claims in the literature; using a pluralist approach to widen the analysis (e.g. to include demographic factors, labour-demand factors, and historical-regional factors); and exploring the data further to

discover demi-regularities not already described. Iteration between these three steps occurs. The interpretation is not necessarily individualistic (note the appearance of the person's marital relationship in our exemplar), is not necessarily nomothetic (note that the interpretation of a London coefficient need not lead one to adduce a *law* of Londoners' higher labour-force participation), and is not necessarily reductionist (there is no need to use a utility-maximisation approach to these findings, for instance). 'London' is not even the causal mechanism for the observed association; we know merely that we must explore why there is an association of London residence with a higher tendency toward labour-force participation. Most importantly, it is possible to recognise that the causes of labour-force participation overlap and are complex, leading to multiple configurations, rather than simplifying the causal interpretation of the equation. Furthermore, by estimating a whole series of models, ranging from simple bivariate models to those with many variables and some interaction terms, users of regression attempt to explore the complexity of the real world that underlies and precedes the collection of these specific data.

In conclusion, critics of statistics argue that statisticians seek only regularities and assume closure in reality. In this section we showed that methodological closure can be assumed, and regularities within the data-set can be sought, without assuming closure in reality. Mathematics and manipulation could also be used to show that things are changing over time, rather than assuming that they are constant over time.<sup>8</sup> One should not assume that statisticians are regularizing or universalistic; it depends on who is doing the art and practice of statistics and how they do it. To seek regularities is not necessarily to reify regularities.

#### **4: An Analysis of Data as Ficts in the Absence of Closure**

In this section we re-assess the nature of the data used in analytical statistics. We deconstruct the usual technique, examining what kind of 'closure' is presumed. We argue in favour of opening up the black box of regression technique, and at the same time actively stressing that the data themselves are merely ficts not facts. Ficts are potentially fictional and not necessarily concretely true. We conclude that a whole new methodology of interpretation is needed, differing from the usual textbook approach in so far as the texts assume that the data are factual and provide firm ground for the interpreter's arguments.

We define methodological closure as involving three elements. It would mean, firstly, that a set of variables are self-contained and of sufficient interest in themselves (for a given stage of research); that regularities may be found to exist

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<sup>8</sup> This is the appropriate approach for realists to use in proceeding with their work although it is not necessarily recognised as such by some realists. It is also not necessarily the same process of logic that is used by current economic statisticians; for a review, see Ziliak for a discussion. (\*).

between and among the variables; and that these regularities are to some extent separable, i.e. they highlight differentiable parts of reality whose separation has some continuity or duration in time.

We need not assume closure in reality when using the assumption of methodological closure. Closure in reality is rather different, and would involve (again) three elements: a non-permeable boundary to the system being examined; separable causal mechanisms; and no emergent properties. If closure existed in reality, the system could be scrutinized part by part without loss of knowledge about its whole operation. Realists have argued convincingly that this type of closure does not exist in social systems (Sayer 1992; Lawson 1999; Lawson 2003) and that if scientists assumed such closure they would be making an error of conflation (Bhaskar, in (Archer 1998); (Bhaskar 1975)). The conflation can be precisely described as confusing methodological closure with real closure of systems.

To start with a mathematical system, such as a data matrix and equations estimating some patterns within that system, and then say that it tells us about the real world is not to argue that the two things are identical. There is an analogy with structure-agency dialectics. Whether we refer to structure and agency as a duality (Giddens 1986) or more properly to a dialectic of structure and agency, we avoid conflating the model with the real world. We separate structure from agency for the purpose of learning about the world, but structure is not actually separable from agency in the world. Therefore Giddens' point that the two are intrinsically related is correct, whilst Archer's attempts to separate them and to explore their separate/linked nature is also helpful.

In the case of analytical statistics, separating out variables and slope coefficients, or the odds of an outcome broken down by types of actor, are useful separations which have heuristic use. They can lead to insights about the real, complex social system.

Having said that, a variable is still not to be equated with a causal mechanism. Variables are not facts, nor are they factual in nature. A better mapping of causal mechanisms onto empirical data would label the variables as 'facts', and the relations between variables as 'associations between facts'. The literature on statistics routinely notes that associations do not imply causation. To establish causation, one must develop an argument taking account of both contextual factors and specific mechanisms; evidence about the context and mechanisms may arise from within the survey data or from without, e.g. from case study evidence or qualitative data. In analytical statistics, a number can be assigned to the association between variables. The apparent exactness of this number does not reduce its fictive character.

However the knowledge claims that we construct, using such numbers, may be argued to be true. The argument will combine induction (argument from evidence) with inference (argument which abstracts from the detail) and retrodution (argument which explains what conditions in reality may have or could have led to these observations. Thus although inductive arguments do not have an absolute truth status (i.e. true-false polarity), nor do they have deductive truth since some premises are not necessarily true (e.g. there may be measurement error or misconceptions in the variable definitions), they can still be valid arguments (Fisher 1988).

Figure 1: Ficts and Reality

REALM:	OBSERVATIONS and FICTS (in grey):
Empirical	<div>Variable → Mapping → Variable</div> <div> <math>X_1</math> → association of .6 → Y            (where <math>X_1</math> is an indicator of A, and Y is an indicator of B)         </div>
Real	numerous mechanisms and the whole context;  the tendency for A to cause B

In summary, an inductive argument using analytical statistics can lead to true knowledge claims. A fict, playing a role within such arguments, is not a fact but rather an attempt to represent reality. Having ficts also makes it possible to observe an association of ficts without that association being an unproblematic essence. The phenomenon X may not be an essential cause or mechanism. The statistical manipulation can provide a starting point for counterfactual thought experiments. Statistical analysis can inspire philosophical and theoretical reflection.

An exploration of the nature of ‘ficts’ can be begun here. First, we are pointing to a polarity of facts vs. ficts, which stresses ways in which ficts may be un-true as descriptors of reality. For instance, a classic caveat about measurement error falls in this category. Given a scale of measurement, responses may deviate from true measurement (as for instance Likert scales inevitably do). In nominal variables, for a given categorization, there may be errors in the mutual exclusivity imposed by the data-collectors. For example, marital status may be

allocated the categories married vs. single, when separated people may arguably lie in both groups, but the marital status variable forces them to be placed in just one category.

A second way in which records may be untrue descriptions of respondents is when the categories of measurement do not extend appropriately (in reality) across the whole sample. An example illustrating this problem is the variable 'housing tenure' in the UK. In England and Wales certain forms of social housing now exist which are not identical to those forms legally recognized in Scotland. Therefore surveys whose sampling frame crosses the Scottish border force the social housing sector into encompassing categories (although the UK Census deals with this matter more carefully) which are either overly general or simply inaccurate.

A third way in which measurement error converts data into fictional accounts might be called the 'missing missing values' (*sic*). Here respondents who would prefer not to answer a badly posed question feel coerced into answering. Their response would be better coded as a 'missing value' but instead does get recorded, e.g. a step-parent who wants to argue that they are not the parent of the child in their household, but who gets recorded as the parent since they play arguably similar roles. All three examples of measurement error are carefully treated in texts on questionnaire design.

These examples of error lead to the second major fictive aspect of survey data: narratives and claims bear truths, but items of record-keeping dissociated from people cannot in themselves bear truths. Information, data-records, and marks on pieces of paper are not, in themselves, either knowledge or arguments about society. If we were to claim that a whole data-set was true as a description of its cases, the claim would be false (due to the errors already listed). If we argue that it is 'true enough', using pragmatic logic, we move into an arena of justified *arguments*. In this arena, it is not the *data* which are true or false, but the arguments about them. The usual statistical practice is not to say generally that the data are true, but rather that the statements in the interpretative conclusion are warranted. It is not necessary for all the data to be accurate records for these conclusions to be warranted.

Closely related to this point, survey data have ontological limitations. Data sets in survey matrices overly simplify the world into a set of homogenous and comparable cases, with comparable characteristics. The non-nested complexity of the real world is not well represented in this simple format. For instance persons are nested within households within regions, but persons also relate to other persons via their employment in a firm, which is not nested within a region but rather cuts across regions. The complexity of social relations, and the

important social relations which we try to represent in social analysis, are necessarily reduced through record-keeping in the survey-data format. Multiple tables with keyed indexing to relate them to each other do not adequately convey the richness of the social world's complexity. It is not necessary to argue that data *represent* the world for them still to be useful in warranted arguments. Thus their fictional ontology, which often appears reductionist, does not justify a complete rejection of the act of interpreting statistical results.

A third area of fictionality in the data tables is that the meaning carried in the survey data may be objected to by the respondents who are its subjects. Meanings can be contested in any social arena. In social surveys we have the question-construction stage, when pilot studies are used to try to develop questions that get at the 'usual', common, and hegemonic meanings of the recorded actions or things. Then there are the data-manipulation procedures, which do not change nor even depend upon the meanings; and finally we reach the interpretation stage. At this last stage new meanings can be read into the data. In our labour-force example, a new meaning of being a 'wife' would perhaps be that a woman who is a wife is induced to get paid employment. How this happens is open to further exploration. But for any particular wife, this tendency is not necessarily actualized. The women in the survey might also disagree with our interpretation.

The detailed diversity of real people implies that the data themselves can be 'wrong', that interpretations are likely to be contested, and that the meanings 'in' the data are not essential but rather are contestable. Survey data don't bear meanings the way textual data can be argued to bear meanings. In any case, meaning-realism (in which social meanings have real essences; see Roth, 1987) is not a necessary assumption for either survey research or for a programme of interpretation of statistics.

The hermeneutics of survey data may be an interesting area for further research (Elliott, 1998). For current purposes we have established that data may have a fictive character. The association between two variables, too, has no essential meaning. These are the tools with which warranted arguments can be built. But only after the building has occurred can the truth or goodness of the argument be assessed.

One reason why statisticians often do not engage in such reflection about warranted arguments is that they are socialized through their training into not considering counterfactuals. They also rarely have training in ontology, and their epistemological training is limited to notions of validity and generalisability that are rooted in empiricist habits. Texts such as Johnston and DiNardo (1997); Mukherjee, White *et al.* (1998); Greene (2003) illustrate the tendency toward an



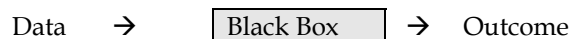
empiricist focus. Empiricism conflates the two realms shown in Figure 1; it assumes that empirical data are what we are studying. By not differentiating the world from the data, and conflating the two together, empiricism oversimplifies the task of interpretation. We are indebted to the detailed analyses by Lawson (1999; 2003), Bhaskar (1975), and Sayer (1992, 2000) which has revealed the multi-faceted epistemic problems which result.

Counterfactual work in analytical statistics requires comparative statistics, and can usefully bring out findings about minority groups vs. the majority, or different populations which have common causal mechanisms. The class structure of different countries, and different policy regimes which together generate different employment outcomes, illustrates the possibilities for comparative statistics. To throw out statistics as a method would involve discarding all possibilities for such comparative work, yet such work has been very illuminating in many disciplines.

Logic is used to make warrantably assertable statements based upon both mathematics and empirical record-keeping (e.g. variables). The resulting knowledge claims form a reasoned argument of which one example, in very truncated form, was given in section 3. The empirical warrant for the interpretive claims arises as claim 5 of our argument (see section 1). There is a logical link between the use of maths and facts along with the creation of supervenient claims which are, arguably, true (Morgan, supervenience piece, \*\*).

The mapping shown in Figure 1 seems to be arbitrary. Mappings of this kind are found in both bivariate and multi-variate data-handling practices and they can appear as a black box. The mapping seems to create a fiction (an epiphenomenon) which is a mirror of its own method, not a mirror of reality. Instead of a Fordist model of mathematical analysis, we need a post-Fordist notion of mathematical interpretive practice. Figure 2 illustrates the Fordist approach, and Figure 3 the post-Fordist approach.

Figure 2: Empiricist Analysis



The black box is objectionable because it appears to be impersonal, uncontrolled, ungrounded in reality, and unexplained. For non-mathematicians, all statistical analysis seems to possess these qualities. Our view of the process is rather different (see Figure 3).

Figure 3: Reality-Based Analysis

Reality → Empirical and Mathematical Mappings → Warranted Outcomes

The interpreter is engaged, personal, grounded in reality, and located in a particular space-time position. They are within structures yet attempting to view those structures. They are agents (actors in Archer's terminology; (Archer 2000)). The post-Fordist approach would be creative; would use malleable techniques, i.e. with methods tailored to aims; and would produce mutating outputs. The product can be matched to the needs of different users and listeners. This tailoring implies that seeking laws would be unjustifiable. The real content of the black box of procedures can also be revealed in different ways to different audiences.

The illustrative statistical results presented here support our claim that learning can occur through the interpretation of analytical statistics. Three types of learning were noted here; corresponding to claims 2, 3, and 4 of the earlier section 1:

2. The association of high employment probability with living in or near London is not necessarily a synthetic epiphenomenon, i.e. it is likely to have real causes. The observed associations could be, but are not always, figments created through the correlation method that constructs regularities.

3. Logistic regression can accommodate complexity and contingency through the use of multi-variate models, mainly by introducing dummy variables and estimating alternative model specifications.

4. In interpreting the regression, we can use non-atomistic inferences about relations, e.g. the husband-wife relation (represented here via the variable 'wife') and having small children (not shown here, but commonly used, e.g. Walby and Olsen, 2002).

The development of regression interpretations occurs through iterative reflection, further reading and study, manipulation using sophisticated modes of operationalisation, and multi-disciplinary theorising. Methodological pluralism (Williams, 2000) is reflected in our proposed methodology. The realist approach tends to be highly multi-disciplinary because it assumes that parsimonious models would oversimplify the world. Thus although triangulation of primary data types was not used in this paper, it is strongly recommended as part of the methodology advocated here (Flick, 1992). Methodological pluralism in general implies that data triangulation will be a welcome source for more rich retroduction, compared with simply using secondary survey data alone (Carter and New, 2003).

## 5. Conclusions

In this paper we began by describing the position of those critical realists who are sceptical about multi-variate statistics (notably Sayer and Lawson). Some underlying assumptions of this sceptical argument were shown to be false. Then a positive case in favour of using analytical statistics as part of a mixed-methods methodology was developed. An example of the interpretation of logistic regression was used to show that the interpretation need not be atomistic or reductionist. However, we also argued that the data underlying such interpretations are 'ficts', i.e. are not true in themselves, and cannot be considered to be accurate or true descriptions of reality. Instead, the validity of the interpretations of such data are what social scientists should argue about. Therefore what matters is how warranted arguments are built by the researcher who uses statistics. Our argument supports seeking surprising findings; being aware of the caveat that demi-regularities do not necessarily reveal laws; and otherwise following advice given from the 'sceptical' school. However the capacity of multi-variate statistics to provide a grounding for warranted arguments implies that their use cannot be rejected out of hand by serious social researchers.

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