

## THE INTERPRETIVE ATTACK ON STATISTICS

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Within sociology over the last ten years there has been a reaction against 'positivistic' methods, in particular against the use of official statistics and social surveys as sources of data.<sup>1</sup> More recently, this interpretive view has itself come under attack from a third position which claims that the 'positivists' and their critics share the same empiricist assumptions.<sup>2</sup> In this paper I want to show that while they vary considerably in subtlety and power, some of the interpretive arguments have considerable force. I shall consider the views of the rationalist critics towards the end of my discussion.<sup>3</sup>

The least persuasive kind of interpretive argument against statistics, rarely stated though sometimes implicit, is the complaint that 'reducing' people to numbers dehumanises them. However, if the notion of dehumanisation has any warrant at all, dehumanisation can only be the product of social forces which seriously affect people's lives, not of forms of analysis which in themselves have no consequences. Of course, statistics may be an aid in repression, especially where this takes a bureaucratic form, but repression can occur and has occurred without the use of statistics.

An argument of a similar character is that statistics represent expert knowledge which is set over against and used to undermine the views of 'ordinary', that is, powerless, people and thereby to manipulate them. According to this view, statistics are simply a technique for, and symbol of, the monopoly of knowledge production by 'experts'. It is certainly worth pointing to the ways in which monopolies of knowledge are established and preserved and the role that statistics may play in achieving this; but this does not mean that statistical knowledge is essential to such monopolies. Nor does it refute the claim that statistics can play an important role in the production and representation of our knowledge of the social world.

Having cleared away some of the less forceful arguments, I want to examine a collection of arguments which seem to me to have rather more cogency. A starting point is provided by the idea that statistics are somehow more trustworthy than qualitatively formulated facts. This is enshrined in the distinction which is often drawn between 'hard' (that is quantitative) and 'soft' (that is qualitative) data. This idea seems to stem from two others. One is that facts are directly apprehended by our senses: something either is or is not the case, and we can tell by looking. Secondly, and this point is quite reasonable in itself, there is the idea that single cases can be misleading: to be sure that something is generally true we need 'the fact' to be observed many times. If, therefore, someone can tell us that something occurred a certain number of times, putting a figure to it, we are more likely to accept the generalisation than if the claim relies on a single observation. Less reasonably, often, the presentation of statistics has the effect of shifting our attention entirely from how each observation was made to how many sightings there have been and what methods of statistical description or inference have been used.

Now the empiricist idea that perception simply mirrors the structure of the world is quite wrong. All observation involves interpretation. Perception is not a passive process of receiving sense-data. The eye and the brain select stimuli and construct patterns out of those stimuli; in part, according to the observer's existing knowledge of the world.<sup>4</sup> No doubt some of the most basic perceptual patterns are genetic, but many are learned. This is demonstrated by the fact that we can learn to perceive significant patterns in scenes which previously appeared irregular or chaotic. Learning to 'read' x-ray slides is a good example of this.<sup>5</sup>

We can suspect, then, that fairly complex processes of interpretation occur in the production of statistics. Furthermore, much of this interpretation will be carried out not by the researcher himself, but by participants in the social world he is studying. Thus, in the case of suicide statistics we rely on coroners' interpretations of the evidence in their determinations of whether or not a suicide has occurred.

In survey research, the researcher frequently relies on the reports of both interviewers and respondents concerning the latter's behaviour, feelings, intentions etc.

Trading on people's interpretations in this way can have a number of important consequences and these have formed the core of the interpretive critique, though the different arguments involved have not always been clearly distinguished from one another.

1. Interpretation involves making assumptions, it involves reliance on prior knowledge about how the world operates. Using statistics produced by others involves relying on *their* knowledge, the nature of which is unknown. Unless their assumptions are investigated a vicious circle can arise :  
 "... if coroners regarded evidence of, for example, depression or social isolation as strong indicators that a suicide had taken place it (is) hardly surprising that statistical analyses based on their decisions would 'discover' such connexions."<sup>6</sup>  
 In other words, observations are theory-laden and if great care is not taken one may be employing statistics which already presuppose the truth of the theory under test - with predictable results. Alternatively, there may be a mismatch between the researcher's definition of the phenomenon under investigation and that, or those, involved in the production of the statistics. This clearly undermines the validity of any conclusions the researcher may draw. This is an argument Douglas (1967) directs against Durkheim's<sup>7</sup> (1952) work on suicide.
2. It may also be that the assumptions about the world on which the interpretations embedded in the statistics are based are wrong. Indeed, there are of course pressures operating in the social world which tend to produce distortion. Firstly, people have *interests* in seeing things, and in things being seen by others, as one way rather than another. To take an obvious example, the family of a suicide victim invariably regard the event as a source of shame and often take steps to persuade

themselves and others that it was an accident. Secondly, people occupy particular positions in the social world which provide them with certain kinds of experiences and information. These, in turn, lead them to develop particular theories about that world which are tied to their practical concerns and to the nature of the situations they routinely face. Thus members of an occupation often tend to share a common taken-for-granted perspective on those social phenomena directly relevant to their task which differs from that of other occupations. We can expect that coroners, enumerators, survey interviewers and others on whom we depend for the production of official and survey statistics will develop such perspectives and these may play an important role in the interpretations they make of the information available to them. Such a perspective, grounded in their practical concerns rather than in scientific considerations, and possibly seriously misleading, may work undetected in the production of the statistics.

3. Another consequence of neglecting the reliance of official statistics and survey research on interpretations by participants is that where one is aggregating across different situations. In using observations by different observers, whether interviewers, respondents or officials, there is a strong possibility that these observers will employ *different* assumptions from one another. Where this occurs one is counting different things and the aggregation is invalid.
4. The validity of the previous three arguments would, I suspect, be accepted by many in the 'positivist' tradition, though they may not regard them as having the same significance as their critics. The final argument, however, is perhaps more contentious and serves to underwrite the importance of the previous arguments. This is the claim that interpretation is of particular importance and of a distinctive character in the observation of *human* action, because it inevitably involves ascribing motives and intentions. One of the central concerns of 'positivism' in the social sciences has been to avoid the invocation of 'subjective meanings' - motives,

intentions, etc. - in the description and explanation of social processes. This is to be avoided because, it is argued, such meanings are not publicly available and therefore are not open to test. Thus, for example, Durkheim sought to explain suicide without reference to the intentions and motives of individual suicides, accounting for variations in suicide rates in terms of variations in the degree of social integration of different social groups. However, the very definition of his area of investigation, suicide, involves the attribution of 'subjective' meanings. He proposes that instead of taking over the definition of suicide held by 'the average intelligence', that is its meaning in commonsense usage, we seek a category of deaths with common qualities 'objective enough to be recognisable by all honest observers.' (Durkheim, 1952, p.42). His alternative definition of suicide is as follows: 'suicide is applied to all cases of death resulting directly or indirectly from a positive or negative act of the victim himself which he knows will produce the result.' (ibid. p.44) Thus, while he avoids defining suicide in terms of intention, he still finds it necessary to refer to the victim's knowledge and thus to ascribe meanings.

The error in all this is the assumption that intentions, motives, etc are subjective and not 'publicly available'. Once one recognises that such meanings are cultural, i.e. *shared* that they are applied by actors on the basis of *convention*, it becomes clear that they are publicly accessible and that, fortunately, there is no need for any attempt to banish them from the account. The implication of this argument is that rather than being simply a methodological trouble, a contaminating factor, as it would appear to be in the context of the previous three arguments, the cultural interpretation of action is an unavoidable tool in the social sciences, and the processes of interpretation employed by the people under study must be central topics of inquiry if we are to describe social phenomena accurately. However, the identification of social meanings is not like the physical description of objects.

Meanings are not tied firmly to particular physical appearances. Even the meaning of a word varies according to the context of its use and the relevant features of the context are not easily specifiable in advance.<sup>8</sup> In identifying meanings one has to employ a cultural knowledge of the society and setting being researched. But one cannot simply rely on first interpretations, these are hypotheses which must be 'tested out'. Furthermore, interpretation of action occurs over time and different kinds of evidence may become available at different times. In other words, the evidence is not necessarily all given at once and the interpretation of previous evidence may have to be revised when new evidence appears. For example, from various features of the manner of a death - slashed wrists, lack of any signs of a struggle, combined with the presence of a note - it may be concluded that suicide has occurred. However, any such conclusion is inevitably more or less provisional. At some later date contrary evidence may arise such as the occurrence of a similar 'suicide', within the same social network coupled with the realisation that someone with access to both scenes of death had some compelling motive for bringing about those deaths. In such cases the previous conclusion would have to be suspended and investigation restarted.<sup>9</sup> The implication of this argument is that the accurate description of social action requires careful investigation and to rely on others' ascription of social meanings is to run considerable risk of mis-description.<sup>10</sup>

One of the conclusions sometimes drawn from this set of arguments by interpretive sociologists is that ethnography (a research method usually involving long term participation and observation in a single setting combined with informal unstructured interviews) is the only valid research method. I certainly think it is true that ethnography has a number of advantages over the use of official statistics and survey research in connexion with the four arguments outlined above.

In the first place, compared to the production of official statistics and survey data, ethnographers short-circuit the retrospective-prospective process of interpretation to a much lesser degree. At the other extreme

is the survey researcher who sets out to collect data on a large number of cases and who, unless he is to take a very long time to collect the data (and that creates the danger of changes occurring in the phenomena under study), must try to make the process of data collection relatively simple and thereby brief. The production of official statistics sometimes involves more intensive investigation than the survey interview but rarely approaches the depth of ethnographic research into particular cases.

Secondly, ethnography facilitates the learning of the culture through the requirement that the researchers observe and participate over a fairly lengthy period of time in the setting or network of relationships being studied.

However, it must be remembered that all research methods short-circuit the retrospective-prospective process of interpretation to some degree. The data produced through ethnography, like that produced in any other way, is the outcome of interpretation and as such reveals much about its mode of production as well as about the case under study. Furthermore, from other points of view, ethnography, at least as currently practised, has certain defects. It would take a very large number of studies, organised and co-ordinated, before any generalisation could be made about a national society, which is a common aim in survey research and the production of official statistics. Also, even within ethnographic accounts of particular settings, one often detects what might be called 'suppressed statistics': there are claims made about frequencies where no indication is given regarding how the count took place. Thus, for example, in his otherwise excellent article entitled 'Dead on Arrival',<sup>11</sup> David Sudnow discusses the way in which those arriving as possible 'dead on arrival' cases at the emergency wards of two American hospitals are differentially treated according to such criteria as age and social class. Yet we are not told how many cases he observed or what kind of sampling was involved or how the correlation was identified and checked. This is not at all unusual in ethnographic studies.

Statistics, even official statistics, cannot therefore be rejected simply on the grounds that they have serious methodological defects. So do all data collection techniques. There are no grounds for outright rejection

of certain methods as 'positivistic' and implicit recommendation of others as relatively unproblematic. We need to set about reconstructing social science using all the methods available, but treating them in a much more sophisticated and reflexive manner than hitherto. And this means that we must be just as self-conscious and critical in our use of those methods favoured by interpretive sociology as those commonly designated as 'positivistic'.

At this point I want to look briefly at the arguments of the radical critics who oppose both 'positivist' and interpretive methodologies and I shall take the work of Hindess (1973) as exemplifying this position. Hindess correctly points to a certain confusion in the arguments of the interpretive critics and to relativistic tendencies in their position. Yet he does not even attempt to argue that the processes Douglas and Cicourel point to are of no importance, indeed he himself shows that theoretical assumptions are built into census categories, though he ignores the significance of the ways in which enumerators apply these categories. Furthermore, the alternative epistemological position from which this critique is launched is obscure: all we are told is that it is rationalist, that it rejects experience as the grounds for knowledge and that it recognises that observation is theory-laden. The latter point is generally accepted even by some of those frequently labelled 'positivists'.<sup>12</sup> The former claim is rather curious. While I can see possible, indeed justifiable, importance being ascribed to the coherence of theories, what other basis for testing the validity of theories can there be than some kind of testing against experience, however mediated by theoretical assumptions.<sup>13</sup>

Given that commonsense theories operate at many different levels and in various different (unknown) ways in the production and use of official and survey statistics, there is a strong argument for ethnographic techniques forming the ongoing base from which surveys and the use of official statistics depart and return. But ethnographic research may well have to start employing such quantitative techniques if it is ever to make large-scale generalisations. Some such combination of methods is one possible solution to the problems currently facing social research.



Equally important, however, is investigation of the process by which findings are produced through the use of different methods. Such research would give us more idea of the threats to validity involved and perhaps enable us to develop methods for minimising or discounting them.<sup>14</sup> This will involve us simultaneously in more intensive investigation of basic social processes such as those involved in everyday social interaction and the effects of social structures and cultures on such processes. In other words, the development of theory and methods is a dialectical process. We cannot, and fortunately do not have to, first perfect our methods before we can develop and test our theories of the social world. The development of each is closely bound up with the development of the other. This can be seen clearly in the interpretive critique of statistics. Equally clear, however, in the work of both the interpretive and radical critics, is the tendency for sound arguments to be overplayed by being formulated in terms of bogus alternative epistemological paradigms. The rejection of such rhetorical strategies is also important if any progress is to be made in developing an adequate social science methodology.

#### NOTES

- 1 See for example J.Kitsuse and A.V.Cicourel: 'A Note on the Uses of Official Statistics' Social Problems (Fall 1963) 11, pp.131-9; A.V.Cicourel: Method and Measurement in Sociology, Free Press, 1964; J.D.Douglas: The Social Meanings of Suicide, Princeton University Press, 1967; and D. Phillips: Knowledge from What, Rand McNally, 1971.
- 2 By the term 'interpretive' I mean approaches which argue that the analysis of social life must begin from an understanding of the meanings embedded in it. Wherever the term 'positivism' or its derivatives have been employed they appear in quotation marks because of the ambiguities surrounding the reference of this term. Unfortunately I cannot avoid trading on its ambiguous commonsense usage in this paper.
- 3 See, in particular, B. Hindess: The Use of Official Statistics in Sociology, Macmillan, 1973
- 4 For a useful account of this see R.J. Gregory: The Intelligent Eye, Weidenfeld and Nicholson, 1970
- 5 For this example see M.J Abercrombie: The Anatomy of Judgement, Hutchinson, 1960

NOTES (cont'd)

- 6 M. Atkinson 'Coroners and the Categorisation of Deaths as Suicide : Changes in Perspectives as Features of the Research Process'  
In C. Bell and H. Newby : Doing Sociological Research, Allen and Unwin, 1977, p.44
- 7 E, Durkheim: Suicide, RKP, 1952
- 8 See D.L. Wieder 'On Meaning by Rule' in J.D.Douglas: Understanding Everyday Life, RKP 1971
- 9 Garfinkel, following Mannheim, terms this the retrospective-prospective character of interpretation: H. Garfinkel: Studies in Ethnomethodology, Prentice Hall, 1967.
- 10 This is my assessment of the import of this argument, one which roughly corresponds to that of Douglas (1967). A quite different conclusion is sometimes drawn by the ethnomethodologists, for example Cicourel (1964) and Max Atkinson; The Discovery of Suicide, Macmillan (1978). They claim that there is no rigorous way to ascribe social meanings and that the only legitimate scientific topic is to study how participants ascribe intentions, motives, etc to one another. They go on to argue that even though it is not done in a rule-governed way, members must necessarily ascribe meanings methodically otherwise they would be unable to communicate with one another and co-ordinate their actions.
- 11 D. Sudnow: "Dead on Arrival" in I.L. Horowitz and M.S. Strong: Sociological Realities, Harper and Row, 1971, pp.225-32
- 12 A good example here is Popper, Conjectures and Refutations Chapter 1. Harper Row 1968
- 13 Hindess crudely identifies both 'positivism' and the interpretive position as treating 'knowledge as reducible to experience' (p.12). In fact there are many way-stages between this crude empiricism and his view that experience has no role to play in the production of knowledge. For a somewhat similar critique of Polantzas see N. Mouzelis: Ideology and Class Politics 1979
- 14 On 'discounting' see I.Deutscher: What we say/what we do, Scott Foresman, 1973.