

EDITOR'S THOUGHTS

Sitting down to edit the Radstats Newsletter certainly brings back memories - most prominently, of the last time one sat down to edit the Radstats Newsletter! So I looked back to RS 27(May '83), to find that the issues facing the group do not appear to have changed that much.

On recruitment, I understand that both the Health and Education Groups have interested new people in their current projects - the Health Information Network, and the pamphlet on "myths" in education policy, respectively. The Health Information Network idea seems to be especially successful in offering people a range of possible contributions between "pamphlet co-author" and merely attending meetings. See the Health and Education Sections of this Newsletter.

In terms of making our "political work" into "paid work", this was perhaps a relatively challenging area in which to make progress. However, John Lintott reports on the progress of Alternative Social Trends, and John Abraham brings us up to date on the development of the proposed Radical Statistics book - both exciting projects that could benefit from your help (in a very specific way, in the case of AST)! See the "Books to be Produced" section.

In terms of moving on from the critiques of statistics, esp official statistical data, to the radical use - or re-use - of statistical data or techniques, Cecilio Mar Molinero's article produced as a result of using statistics in several campaigns against school closures in Southampton shows a masterful use of statistical and O.R. techniques.

Though there are no articles about the developing world in this issue, there will be a meeting on "Statistics in Nicaragua" at the A.G.M., and the Group of the same name will be producing a Radstats pamphlet, by the same name - if you see what I mean.

No one has taken up Roy Carr-Hill's challenge in the last Newsletter to demystify the concepts of 'risk' and 'relative risk', but, as these seem to be used more and more in public policy discussions - usually to convince us that we shouldn't worry about something like Chernobyl, or Sellafield - I hope that someone or some group may take up these issues soon.

Two final points : (1) As I think you will already know from a previous mailing, the AGM is in York on 28 Feb. and 1 Mar.; part of the leaflet is reproduced on page 34.

(2) A general election is imminent, and whatever your views on the quality of the process, its outcome is likely to affect the working and everyday lives of most, if not all, of us. If you would like to think of how you can have an influence the discussions around the campaign, there is at least one idea in this Newsletter aimed at that, viz. the Education Group pamphlet. There will no doubt be discussion of other such ideas at the AGM.

Hope to see you there!

Jeff Evans

Science and Sociological Practice

Steven Yearley, Open University Press, paperback

The issues Steven Yearley examines in this book are raised in a clear and lucid fashion, and will be read with profit by those interested in the methods of the social sciences. The book's scope, however, is far narrower than the title or the claims made in the cover would imply. For, despite taking seriously, and clearly expounding the familiar vicissitudes of the once 'Standard' empiricist philosophy of science, the author persists with one of the main tenets of the old 'standard' view which, it might be thought, had suffered the same fate as all the rest. This is the notion that 'method' in science, or social science, is a matter which can be considered independently of the content of theory.

So the 'Post-empiricist' philosophy of science on which attention is concentrated in this book is broadly Lakatos's theory of 'Research Programmes'. The connection is rightly noted between a theory of scientific rationality, and the broader question of a theory of human conduct as that of rational actors. What is not remarked upon is the mismatch between these - even if both were correct - in terms of the level of theory involved. While any theory of scientific rationality is going to be a non-empirical, meta-scientific normative theory prescribing a rational method, the more general theory of human conduct to which it relates is considered by Yearley to be a 'naturalistic', presumably empirical and falsifiable scientific theory. The central conundrum of the book is that the normative, meta-scientific theory of what counts as rational scientific activity which the author favours is falsified: - empirical studies of scientific practice reveal that scientists themselves do not adhere to the principles of rationality which it is hoped can be expanded, from the idealised example of scientific activity, to embrace the explanation of human conduct in general. Now if there can be a purely prescriptive account of scientific method, independent of the content of scientific theory, then it ought not to be falsifiable - any more than the prescriptive precept 'Stealing is wrong' is falsified by the existence of thieves. This conundrum, in other words, calls into question the very possibility of a prescriptive theory of scientific method which is independent of the content of social theory.

Once this conundrum is reached, there are a number of possible routes that could be taken. One would be to count the 'rational man'(sic) theory as having been refuted, as e.g. Hindess does (1). Another would be to loosen up the theory so that action is simply assumed always to be rational (much as a determinist assumes events always to be caused) - it is just that, in puzzling cases where we would not have

acted in this way, we simply do not know the agent's reasons, but we take it that there must have been a reason for the action, even so. (This is what, in his subtle way, Davidson urges (2).) The latter course is, in effect, to make the assumption of rationality of agents into a necessarily unfalsifiable, 'metaphysical' precept for doing social science, parallel to but different from the equivalent notion of 'cause' or 'causal law' in natural science. Natural scientists take the failure of a prediction to be a potential falsifier of some particular causal law or other and never consider that it might potentially falsify the general supposition that there are causal laws. In fact Yearley attempts to go as far down this road as he can without finally abandoning the notion that the hypothesis of the rationality of action might be 'naturalistic' - i.e. a 'falsifiable-in-the-long-run' theory in social science.

I do not think he can have his cake and eat it in this way. Particular hypotheses about the rationalia of actions which are rational, I would agree, can be assessed as empirical hypotheses on the basis of evidence. Equally, it will be an empirical question whether rationalia are the right constructs to employ, in contrast, say, to physiological constructs, in the explanation of some particular instance of human activity - as it clearly is in a court of law when the question is raised whether it is the guilt or the illness of the accused that should be determined. Rationality, however, unlike being governed by a causal law (of an invariable, or a statistical, form) is a notion which admits of degrees. This is because it is a normative, regulatory notion. We should not be surprised if scientists are not as rational as they should be - no one is. But, at the same time, the 'standards' of rationality which we may presume to obtain - and, inevitably, to be often broken - in any given social situation are not fixed once and for all, but vary and evolve relative to the circumstances. In this way, too, they differ from being governed by a causal law. While our ideas about causal laws may change, the laws themselves do not. By contrast, when our ideas about precepts of rationality change, then so do the precepts themselves. There are circumstances - economic competition is a prime example - where there seem to be 'automatic' penalties for failing to optimise 'rationality'. In most social circumstances, however, there are no such penalties, and even in the economic case it is fairly clear that the standards of 'rationality' enforced by the automatic 'penalties' built into the economy cannot be divorced from the historical and social specificities of the forms of economic exchange and production, nor from the general level of understanding of those 'mechanisms' possessed by the participants.

Rationality, then, is not a notion that can be deployed in the explanation of human conduct in a manner analogous to the deployment of the notion of 'causal connection' in the explanation of phenomena in natural science. Equally, it cannot be a purely empirical construct in an explanatory theory. Granted, it can be an empirical question whether or not a particular instance of action is rational, but particular failures by people to be rational could not invalidate the notion of rationality itself, because

it is a normative notion. If, by 'naturalistic' is meant a form of enquiry which has some methodological kinship with natural science, then explanations of actions in terms of 'rationality' are not naturalistic. No matter how far the term 'rational' is loosened up to allow divergent patterns of action to be equally 'rational in their own terms', and no matter how tolerant we consequently become regarding the 'rationality' of the practice of 'naturalistic' theory construction in the natural sciences - causality and rationality remain distinct. For, if there are any causal connections between natural phenomena, then these unalterably obtain independently of how or whether these are described by human beings. By contrast, if there are any rational connections between circumstances and human actions in those circumstances, then these depend precisely upon how or whether these are described by human beings, and change when those descriptions change.

These remarks of mine do not begin to uncover the complexities and difficulties which surround the notion of rationality. A number of works listed in the bibliography of Steven Yearley's book, but to which he gives little attention in the text, do, however, raise some of these issues. Science and Sociological Practice is a valuable work for the clarity with which it raises the central conundrum of the apparent lack of rationality in the practice of our supposed ideal of rational activity - natural science. The author's methodological perspective, however, reveals its weakness in the later chapters of the book, because by retaining the hope of a 'naturalistic method' he refuses to accept the conclusion which, it seems to me, we must draw from this central conundrum. That is that any account of the methods by which human beings know and explain the world cannot fall to have implications for a substantive account of what human beings are like, in order that they can be capable of knowing and explaining things. It seemed, at one time, that 'methodology' could stand back from the content of natural science. It cannot stand back from the more general question of what counts as a science, because it is itself a social science - and an account of a sphere of human activity which must simultaneously describe and guide practice. Any methodology, whether or not it cares to recognise this, carries with it a tacit account of human nature - the nature of those who attempt to know and explain the world around them.

(1) Hindess B. Philosophy and Methodology in the Social Sciences, Brighton, Harvester, 1977.

(2) Davidson D. Essays on Actions and Events, Oxford, Clarendon, 1980.

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