

Yet another book tumbles from the careful editorship of that "hardest" of the quantitative heavies, Hubert M. Blalock. The topic this time is measurement and the problems involved in getting at concepts and scales indirectly through the medium of the questionnaire. Immediately, let me say to all those of you who are now leaning back, silently running through Chapter I of Cicourel's Method and Measurement and feeling thoroughly sure that you know all the pitfalls and criticisms, that it's time you got your heads down and read how the quantitative heavies themselves are tackling this, perhaps the thorniest of the social scientists' problems.

The book is a collection of articles which address themselves to difficulties in three major areas of measurement theory, and, more importantly, attempt some solutions, instead of sitting on the sidelines, carping. The first section deals with new ways of collecting and interpreting more accurately responses from people, ranging from problems of scaling to those of measurement "error". Long and Wilson introduce a method known as a non-metric unfolding technique which converts an ordinal scale of items which have been ranked by individuals into an interval scale to which stronger statistical tests may be applied. The article concentrates on an exposition of the procedure rather than any justification for it; it only manages to squeeze out of the data more than has gone in by the rejection of any "errors" where intransitivity occurs (the Escher Steps situation where a respondent may, for quite valid reasons, prefer A to B, B to C and C to A). In this section also, D'Andrade shows that memory "error" is not random, and that systematic distortion occurs when observers try to recall the traits associated with different subjects. Re-analysing existing data, he discovered that the correlations found between the traits afterwards proved to be due more to the observer's conception of "what trait is like what" than to covariation in the behaviour of the subjects, especially after a period of time has elapsed.

The second section is entitled "Multiple Indicator Approaches", which contains a series of rather staid articles on some of the technical problems concerned with reliance on several "indicators" of a trait or concept where no one single "indicator" is found to be adequate. Throughout the entire section, although there is much discussion of measurement "error", measuring it, randomising it, there is no fundamental questioning of what is actually is; it seems to me that the ball has to be thrown back into the court of the measurement theorists - there is nothing here in substance that Combs didn't say. An article by Sullivan, for example, deals specifically with "criteria for the selection of indicators, given several from which to choose"; (he concludes that you prefer multiple indicators which produce the most random (uncorrelated) measurement error and which relate to only one construct). But why have we got several indicators from which to choose - surely this indicates sloppy theorising?

The final section deals with the statistics of ordinal measurement, a very important topic in a subject area where it is doubtful if any real interval scales exist at all. Wilson reviews the behaviour of different measures of association and concludes that Goodman and Kruskal's γ , Somers' d_{yx} and a new measure, e , are uniquely appropriate for testing 3 different types of ordinal hypothesis, hypotheses of no reversals (if x increases y does not decrease), asymmetric relations (if x increases, y increases) and strict relations (if x increases, y increases, and if x does not vary, y does not vary) respectively. Properly formulated rules for the use of appropriate measures of association are a vital counter attack to the more usual practice of running a "STATISTICS ALL" instruction on a computer package and then picking the measure producing the most dramatic result to report.

It's all careful stuff, and certainly contains technical advances upon already existing body of methodological theory (if I may be permitted such an excruciating term). But on reading it, it is doubtful whether our doubting eth-meth will feel inspired that after all there may be something in quantitative social science.

The book has tumbled, but into whose hands will it fall? Black asserts in his introduction that the "chapters in this book are not highly technical" and quite correctly says that the problem of communication between the technician and the social scientist will only really be resolved when they are one and the same person. (When this occurs, I feel certain that we will see much more exciting advances than those documented in this volume.) But the fact of the matter is that, on this side of the Atlantic at least, there are very few statistico-sociological hands for this book to fall into. New ideas in both theory and methods are not usually communicated by text-books, but arise through the introduction of individuals to actual practical examples of their usage and by the enthusiastic proclaytising of the in their departments or units. I am worried that the mathematician/ will fail to recognise some of the important issues that are in this book, and that the sociologists will simply not understand it.

Cathie Marsh