Present Position and Potential Developments: Some Personal Views

Statistics in the Social Sciences

By HARVEY GOLDSTEIN

[The President of the Manchester Statistical Society, Mr R. L. Harrington, *in the Chair]*

SUMMARY

Various roles are considered for the statistician working in the social sciences. One particular role, that of the statistician as a social scientist, is emphasized and its implications are considered. The paper discusses several case histories and attempts to draw general lessons from these.

Keywords: SOCIAL STATISTICS; SOCIAL MEASUREMENT; STATISTICAL MODELLING

1. INTRODUCTION

To understand how statistics is used and could be used in the social sciences I will describe the different roles which a statistician may assume, and having done that I will argue a case for emphasizing one particular role – that of the statistician *as* a social scientist.

First, in emphasizing one particular role I do not wish to make an absolute value judgement about it. The emphasis arises partly from personal experience and partly because, in contrast to other areas such as medical statistics, I believe there is as yet a serious lack of competent statisticians able and willing to adopt it. Secondly, I am aware that in some of the social sciences my remarks will have less force since quantitative methods already flourish. Thirdly, I will not attempt to cover the whole area of social statistics. Some areas, such as economics, are already partly covered by other conference papers and in any case I would not be confident about extrapolating from areas in which I have worked to those such as social planning which are relatively unknown to me.

2. DEVELOPING STATISTICAL MODELS FOR THE SOCIAL SCIENCES

The first of my roles is that of the statistician who develops or modifies statistical theory for social science applications. The social sciences are distinguished from the natural sciences in many ways, but one of these lies in the nature of the data generated and the theory which underlies the measurements. Without getting involved in philosophical comparisons between measurements in these fields of study, it is worth noting that a crucial distinguishing feature is the far greater disagreement among social as opposed to natural scientists, about just how to make measurements and about what constitutes a valid measuring instrument. Thus, for example, cognitive psychologists, who typically see themselves as closer to the natural sciences than most other social scientists, exhibit little concensus when it comes to measuring cognitive ability, as the history of intelligence testing demonstrates, whereas human biologists generally seem content to measure the physical size of people using a few simple measures such as height or weight.

It would seem to be no accident that one of the major developments to take place largely within the social sciences was the statistical theory of latent variables, which developed out of this lack of agreement. The absence of a clear theoretical guidance as to what would be relevant to measure led to proliferations of indicators, each of which had some relationship with an under-

Present address: University of London Institute of Education, Bedford Way, London WC1H 0AL, UK.

lying concept. The development of mathematical models, in order to identify such a concept precisely, became necessary because no secure substantive models existed. In the early days of factor analysis, statisticians were less involved than psychologists and it was not until the appearance of the Lawley and Maxwell (1963) book with its clear exposition of "respectable" maximum likelihood procedures that this model received attention from the statistical profession. More recently this model has been extended to encompass categorical data sometimes known as the latent trait model, and we see statisticians actively involved in its development (Bartholomew, 1980; Bock and Aitkin, 1982).

Thus, factor analysis was developed in response to a social science discipline need, continuing largely to be applied within social science, with the statistical theory attaining its own autonomous momentum for development. While such vigorous development seems healthy, it does contain certain general dangers.

There is good evidence that the increasing mathematical sophistication of latent variable models has caused some damage, and a good account can be found in the book by Gould (1981). This damage has occurred in several ways. First, the presentation of a problem as essentially mathematical, even if only using relatively simple algebra, can easily make the underlying substantive issues opaque to the non-numerate. This in turn opens up the possibility of mystification, consciously or unconsciously, and to a separation of interests and understandings between the mathematical and the non-mathematical disciplines. Thus, the use of IQ models in educational selection in the UK with its supporting mathematical structures is one area where up until the Second World War the mystique of "innate ability" was widely accepted (Simon, 1953). A more up-to-date version surfaced for a time in the 1970s in the form of a simple latent variable model known as the Rasch Model which was supposed to "objectify" educational testing, but which might be said to have been used rather effectively by the moderately numerate to mystify the hardly numerate (Goldstein, 1979).

Secondly, and of special importance for statisticians, the eventual demystification which perhaps inevitably takes place in such cases, may be very damaging to the statistical profession. The sins of the few often are visited upon the many and erstwhile colleagues may come to trust the profession just a little less.

A second danger is to fail to recognize the nature of the statistical models which originally developed out of substantive applications. The tendency to pursue greater generality and abstraction is of course one of the strengths of statistical modelling, but by the same token mathematical solutions are proposed which inevitably involve *mathematical* assumptions which may or may not be applicable to particular data. We are familiar with the widespread assumption of normal error distributions and applied statisticians are aware of the need to modify this where appropriate and have available an increasing number of techniques for doing so. Less familiar, I suspect, is the ubiquitous use in multivariate analysis, especially canonical, component analysis and scaling, of quadratic constraint functions (see, for example, Healy and Goldstein, 1976), yet in many common applications a choice of alternative function (e.g. linear) may considerably change parameter estimates and interpretations. It would seem to follow that the choice of constraint should be determined on substantive grounds, yet the need to do so hardly seems to be recognized. It is almost as if the existence of one, highly convenient, mathematical formulation is accepted as both necessary and sufficient. Another example, especially relevant to the social sciences, is the basic model for errors of measurement. Mental test score theory, for example, assumes that a test score can be written as

$$x_i = X_i + U_i, \tag{1}$$

where X_i is the "true" score, x_i the observed score and U_i a measurement error. Then, in order to develop the statistical theory it is assumed that

$$\text{cov}(X_i, U_i) = 0. \tag{2}$$

It is not difficult to see that such an assumption is unreasonable in many situations. For

example, in a test designed to discriminate well among low scorers, a large number of subjects will score near the maximum but a few are strung out along the score scale. Yet the edifice of classical mental test score theory rests on this assumption. The basic text (Lord and Novick, 1968) refers to the importance of this assumption early on but pays little serious attention to the consequences of its violation.

Equation (1) of course is a very simple kind of latent variable model and these raise particularly difficult problems, since some of the mathematical assumptions almost have to be arbitrary and in general therefore much more crucial than for example in ordinary linear models where there is an empirical basis for assessing model adequacy from the data themselves; see Goldstein (1980) for a discussion of this point applied to latent trait models. I do not wish to appear unduly nihilistic over the potential utility of such models in the social sciences, but rather to sound caution. If statisticians wish to pursue theoretical models then that is a useful activity. What seems puzzling, however, is why those who pursue an interesting theoretical development sometimes feel that they have to justify it by appealing to potential applications in social science, or indeed else-where. Often, unfortunately, such justifications turn out to be naive as well as unnecessary. Thus, for example, to claim that latent variable models are useful because social science data are largely concerned with unmeasurable or latent concepts, or even that underlying measurement scales ought to be symmetrical (see, for example, Bartholomew, 1980), surely is to confuse mathematical elegance with the substantive world. Such models do have a place in the social sciences and their development is a useful activity, but to oversell them seems unnecessary and potentially alienating for social scientists

My argument can be encapsulated in a remark made to me a few years ago by a highly competent statistician, when talking of a new latent trait model, that "this model is just too good not to be true"!

3. THE STATISTICIAN AS A TEACHER OF SOCIAL SCIENTISTS

In the 1983 directory of Academic Statisticians in the UK and Ireland, out of about 140 departments listed, a rough classification by title produces 23 medical, 13 economic and 10 social, statistically oriented departments (with some double counting). These data underline what is well known—that in many higher education institutions the teaching of social statistics in particular is done either by statisticians whose primary interests are elsewhere or, in many cases, by social scientists themselves who will not necessarily have wide statistical experience.

This is not the place to argue the pros and cons of this situation. Rather I want to discuss the barriers created by the absence of social statistics departments and the consequent lack of statistically active and experienced people who are able to understand the needs of social science students and staff.

, In one sense, statisticians have been too successful in persuading social scientists of the importance of statistical procedures. Our techniques have permeated widely and the demand is catered for by a variety of computer packages. Such programs, however, lack the ability to tell the user how to design her or his study in the first place or how to cope with messy data; precisely the province of the applied social statistician. Naturally, many social scientists have acquired such skills, especially those in the more numerate areas such as geography and psychology, but a great many, if not most, will not have done so. Nor will their needs have been met by general introductory statistics courses—especially and inevitably when given by statistics departments containing little or no social science interests. Beyond a basic understanding, social scientists need experienced guidance, and all too often the best they can do is to follow the SPSS manual and give the instruction "STATISTICS ALL".

This gap between the disciplines leads me onto the final and main section of this paper in which I will argue for the development of statisticians as social scientists.

4. THE STATISTICIAN AS A SOCIAL SCIENTIST

An important division within the social sciences is between those who regard themselves as

numerate and those who do not. This is evident in sociology, education, politics and increasingly in other subjects such as history. The reasons and motives for this are numerous, and across this divide there exists both hostility and incomprehension. At one extreme the non-quantitative social scientist often tends to reject quantitative techniques on the grounds that reality is too subtle to be captured by the crudity of any measurements which could be made and that any attempt at measurement is distorting. At the other extreme, statistically oriented social scientists or statisticians sometimes seem to be claiming that even if the measurements are inappropriate, subsequent manipulation can be expected to extract something useful.

Both these views have some substance. It is the case that many concepts cannot be measured with enough precision or validity to avoid their distortion, and sometimes an insistence on measuring something, simply results in poor measurements. Intelligence seems to be one of these concepts, and social scientists surely are correct to complain that mathematical sophistication, if applied to what they consider are basically invalid primary measurements, far outstrips any substantive justification for making these measurements in the first place.

I have already alluded to another cause for complaint, the exploitation of real data in order to illustrate statistical methodology when the data themselves are of dubious quality. Where such exploitation is confined to the statistical literature and its purpose is clearly understood, there is little to quarrel with. Such is not always the case, however, and the following examples I hope will make this point.

The first is taken from a popular textbook aimed at social scientists (Erickson and Nosanchuk, 1979). Chapters 2–5 give a clear introduction to exploratory data analysis. As a practical example, the authors use data on suicide rates from different countries and carry out exhaustive analyses. Yet, apart from a single sentence about differences in "cause of death" classification procedures, there is no discussion of the considerable measurement problems associated with such a variable. It is as if the authors are saying "these may be inappropriate data from which to draw valid conclusions, but look at how interesting the statistical analysis is".

"If this is what statisticians spend their time doing," one can hear the critics say, "then we wish to have none of it." And they are probably correct, and if such approaches were at all common then the harm done might well outweigh the good. If the textbook encourages social scientists to make inferences from such murky data so long as respectable analysis techniques are used, then there would be cause for concern.

My second example is more complex. In 1976, Neville Bennett published a research report (Bennett, 1976) which compared so called "formal" and "informal" teaching styles among a sample of primary school teachers. In its apparent support for "formal" methods it received much publicity as well as considerable criticism. Partly in response to criticism of the statistical models used, the Social Science Research Council decided to fund a re-analysis of the data carried out by Murray Aitkin, which failed to confirm the previous results, finding little evidence for the superiority of any one style (Aitkin *et al.*, 1981). In the course of this re-analysis several useful models were illustrated and developed. This work not only was elegant and necessary, it has also acted as a model for other researchers, by stressing the need to model the hierarchical nature of the educational system.

Nevertheless, in the welter of discussion about techniques, there has been a tendency to forget about the data. Yet the basic design of the study has been criticized strongly as being seriously flawed. One important criticism was to do with the lack of a proper attempt to control for confounding factors. As Gray and Satterley (1978) suggest, "how one statistically analyses a fundamentally flawed research design is, of course, largely academic".

The social science critic again might point to another example of statisticians producing a sophisticated superstructure on untrustworthy foundations. While I think this would be unfair and miss the point of the re-analysis, the example of this analysis could encourage others to justify their analyses on the basis of sound *statistical* techniques rather than on subject matter grounds.

Another example which is very close to my own interests seems to me to be a case where statistical sophistication has far outrun any possible substantive model, but where statisticians,

or perhaps more correctly psychometricians, have pressed the adoption of their models. I refer to the design and analysis of educational tests. We now have a large array of statistical procedures for selecting items for tests and modelling their behaviour. These procedures rest upon strong assumptions, for example concerning response independence and dimensionality. I have already referred to some aspects of this, but the point to be made here is that among the model development work now under way, attention is given to the autonomous mathematical development with little concern for the educational reality. I am particularly struck when attending meetings in North America by the divide between those who discuss the "educational" requirements of tests including the validity of the assumptions, and those who want to find better ways of estimating model parameters.

What of the other extreme? While anti-quantitative social scientists undoubtedly can find ammunition for their views, to observe and concentrate on a few cases of abuse is hardly good evidence in general. Anti-quantitative attitudes can lead to situations where large amounts of data remain unanalysed, when even simple analyses would provide insight into the subject matter. Occasionally, also there is a rejection of particular techniques because of guilt by association. I once had the salutary experience of being criticized for using a decision theory model on the grounds that similar models had been used by the US military in Vietnam!

A more sophisticated version of these attitudes goes as follows. Statistical techniques rest on certain assumptions. These assumptions embody implicit statements about how individuals or institutions will behave. Thus the techniques embody cultural or ideological positions, so that *any* attempt to use them implies an acceptance of such positions and to the extent that these are not acceptable, we should not be using the techniques. Now I have some sympathy with this view. I believe it applies to some extent to mental test models, but it is also simplistic. The great virtue of attempts to model reality mathematically, is that one does have to be quite explicit about one's assumptions. To dismiss them generally as inappropriate is of course easier ·than attempting to modify them to make them more realistic. The power of statistics to explain and predict the world should not be treated lightly and those who are concerned about poor uses have as much of a responsibility to promote better ones as to protest about the worst.

Having sketched out my concerns I would like to indicate where I think developments should be occurring and who should be involved. My main thesis is twofold. First, I believe that quantitative social science has everything to gain from an increasing involvement of statisticians. Secondly, I believe that this should involve statisticians coming to regard themselves also as being social scientists.

Involving statisticians closely in applied research does not, I think, need a great deal of justification to an audience of statisticians. The involvement, naturally, should be with the substance as well as the methods. I am suggesting, however, that there should be statisticians who work within *a* social science environment and who take *responsibility* for the substantive as well as the statistical aspects. Such a statistician will need to care about the outcome of the issues he or she is studying, whether this concerns the use of factor analysis in the selection of children for different kinds of education or the use of opinion polls to predict election results. I am not suggesting that all statisticians who work with social scientists should become social scientists in this way. Rather I wish to see develop what I think has developed in medicine—a group of communicating statisticians working largely if not wholly in that area. This group should contain those with a strong interest in theoretical modelling as well as those primarily concerned with data analysis for whom the principal excitement is generated by the substantive results.

While not on the North American scale, the social sciences in this country already contain elements of this. It is very evident in the area of survey research where, for example, at the City, Southampton and Edinburgh universities there are groups who span the whole range of theory and applications and who intermingle in creative ways with non-statisticians. In terms of the traditional disciplines, the geographers and psychologists seem well catered for, although in areas such as sociology, politics, education and history as yet there is little. Yet these often are the areas where good statistical advice is badly needed. An example will illustrate my point.

In March 1982 the Metropolitan Police released data on crimes for the previous year. The figures for "robbery and violent theft" were broken down by the victims' judgement of their assailants' colour (where relevant and where reported). Those data which claimed most public attention were the ones which showed more "coloured" than "white" assailants: 55 per cent of the respondents identified "coloured" assailants. Both *The Times* and *The Guardian* newspapers carried similar front page reports of this "finding", the latter being somewhat more sceptical about the figures. It was left to the *Daily Mail*, however, to indicate that these figures needed to take into account the percentage "coloured" population throughout London for valid interpretations to be given. *The Times* in fact published a table where the figures were broken down by area and although even a casual perusal of this table suggests a strong relationship with the percentage coloured population, the news item failed to mention it. Perhaps the most surprising response to these figures came from the Commission for Racial Equality which, as the name suggests, is concerned with eliminating racial discrimination and reducing prejudice. The Commission was quoted as saying "we fully support ethnic monitoring in all fields—we hope the figures will help debate to be more rational and informed" (*The Guardian*, March 11th, 1982).

The police report itself failed to stress the unreliability of the data and the police were reported as being unable to supply population figures (*The Guardian*, January 7th, 1983). Following considerable criticism, however, the Metropolitan Police decided not to repeat the publication and by March 1983 the Home Office itself had produced figures, in response to parliamentary questions, which were fairly detailed and made the necessary caveats about interpretation (*The Guardian*, March 29th, 1983).

The moral of this story is not just another example of statistical abuse, but to show how *official* statistics could actually be published in such a distorted fashion and accepted so uncritically by responsible media. It surely reflects the lack of strong statistical standards in this area despite the efforts of some workers in pointing out the limitations of the available information (Walker, 1983). One of the reasons, I suggest, is the paucity of professional statisticians engaged in this field who care about what is happening. The Royal Statistical Society itself, although it made no immediate response, did convene a discussion meeting on ethnic statistics in 1982 (Royal Statistical Society, 1983) and such activities should help to encourage more statisticians to take a deeper interest.

One final point needs to be made. To commit oneself to integrating with social scientists, or indeed practitioners in any applied field, implies that one cannot take refuge from adopting a position, even a public one, on the substantive issues, in the disguise of a technician. Commitment means what it says and the commitment needs to be visible. Indeed, very often, because of her or his specialist knowledge the statistician is in the best position to defend a piece of work or mount a critique of another.

Having said all this it remains for me to indicate how I think these things are to be achieved. *One* obvious avenue lies through the training which statisticians receive. I believe it broadly to be true that in most higher education institutions the highest status and the most glamour attaches to theoretical rather than applied statistics. Whatever the cultural or historical reasons for this attitude, its persistence must be a hindrance to attracting the ablest students into areas such as social statistics. Nevertheless I am optimistic that this is changing. There are, I think, increasing numbers of social statisticians within universities and polytechnics, and gone are the days when the Committee of Professors of Statistics could debate seriously whether a professor of social statistics was eligible for membership!

Another hopeful sign is the continuing vigour of the Radical Statistics Group which has attracted statisticians with a rather wide range of views and one of whose major activities is to sustain an involvement with current social issues.

One final thought. Perhaps the Society should seek to establish a journal of "Statistics in Society" (Series D). This would not be a rival to series A but perhaps be rather like the initiative between the American Statistical Association and the American Educational Research Association which resulted in the *Journal of Educational Statistics*. "Statistics in Society" would carry general

and research articles but also would contain analyses and discussions of currently important issues. It could bring together those working in the social sciences who have an interest in quantitative methods, from government, higher education and other parts both of the public and private sector. With careful editorship it could command the respect of a wide audience and help to propagate the high standards which are so badly needed.

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INVITED DISCUSSION.

D. J. Bartholomew (London School of Economics): It is a pleasure to record a large measure of agreement with Professor Goldstein especially as regards the need for the greater involvement of statisticians in social science. It is high time the situation was remedied and one might guess that the founding fathers, with their predominantly social interests, would agree. There are also, I am bound to add, a number of matters touched upon over which Professor Goldstein and I have long regarded each other with mutual misgivings but this is not the platform on which to continue that debate. Instead I wish to direct my remarks to a question which is central to the whole paper. It is this: Does social science exist? The fact that pressure was exerted on the Social Science Research Council to drop "science" from its name suggests that there may be quarters where the matter is open to doubt.

The statistician is a scientist or, more exactly, he is an expert in the methodology of science especially as it relates to dealing with uncertainty and variability. Since the social sciences abound in both there is a *prima facie* case for a substantial statistical presence. The possibility of social *science* thus depends on whether the phenomena of interest can be classified and quantified in a meaningful fashion and on whether the patterns and relationships observed are sufficiently persistent. That there are such regularities[1] is well illustrated by Quetelet in his book *Social Physics* and more recently by Sir Maurice Kendall in his Presidential Address to the Society. Though they are often not as clear-cut as in the natural sciences they are sufficient to provide a basis for genuine science. The quantitative study of social phenomena is thus a legitimate field for the deployment of statistical skills. But such a statement needs qualification.

As Professor Goldstein remarks there is a wide range of attitudes among social scientists themselves. The divisions are exacerbated by the intemperate enthusiasms of some social scientists who have assumed the role of part-time statistician. It is tempting, especially for one who works in an institution which spans the whole spectrum of the social sciences, to propose a division of the field. If the term social science were to be reserved for those subjects which are highly quantitative the rest could be lumped together under some such banner as social studies. But the attractions of

doing this fade as soon as the attempt is made, for all branches of social study have a quantitative dimension. Equally, and this is easily overlooked by such as ourselves, all have quite vital aspects which no measuring instrument, however sophisticated, can adequately capture. Social science is thus an activity which represents one among several possible approaches to the understanding of social phenomena. Professor Goldstein's plea for statisticians to become social scientists may thus be accepted with the proviso that the "scientist" part of that designation be given its full weight.

It would take too long to sketch even an outline of what social statistics involves. Two broad strokes must suffice. First, it is one of the most difficult fields of application and, consequently, one where what can be concluded from the data often falls short of the simple message which head-line copy writers crave for. There is often no lack of data but they will be incomplete, of variable quality, and subject to many unknown and uncontrolled variables. There may have been ethical restrictions on what could be collected and unreasonable limits on the time and cost of the investigation. The question of what to measure is often far from obvious and models to elucidate what is being measured are needed if any progress is to be made. Much harm has been done by the indiscriminate transfer of simple techniques inspired by field trials at Rothamsted to social situations for which they are totally inappropriate—a practice which is unfortunately per-petuated by freely available software packages and by scores of introductory texts for social scientists. There is still much fundamental thinking to be done about the appropriate concepts for social scientific work and about making them operational.

My second remark arises from the first; it relates to what Professor Goldstein calls mystification which I take to be the same as "blinding with science". Difficult problems often lead to difficult solutions and this may put them beyond the comprehension of most of us. In theoretical physics this may not bother us but when the subject matter of the research concerns our own interests we are liable to resent being excluded from the discussion by the esoteric language in which it is con-ducted. In spite of this I do not believe that social science, any more than physical science, should be limited by what Quetelet's "average man" can understand. Indeed I would argue that social science is not scientific enough. Within the research community I have heard mathematical physicists chiding social scientists for expecting knowledge to come too easily.

There is no reason to expect that the conceptual framework or the mathematical techniques required to study social phenomena will be simpler than those needed to probe the heart of the matter. I fully agree that much irrelevant mathematics has been imported into social science and that one must be careful about pushing theoretical developments beyond what the data (actual or potential) can bear. But having had such accusations levelled at me in the early days of quantitative manpower planning and having lived to see so-called unnecessary abstractions become practical commonplaces, I am wary of justifying technical inadequacies, or mere timidity, by appeal to high principle.

All of this poses formidable problems of communication, monitoring and control but these are inherent in the scientific enterprise itself. If we decide that scientific activity is a legitimate avenue to knowledge in the social field then we must accept the consequences and seek to devise a social framework within which free enquiry and the public good can both prosper.

The **author** replied later, in writing, as follows.

Having heard his comments, I now feel fewer misgivings about Professor Bartholomew's views than formerly! Seriously, though, I am pleased that he has chosen to emphasize our points of agreement, since these could help to form a basis for statisticians to present social scientists with an agreed view of our role. This discussion, I hope, will continue.