

CRITICAL NOTE

SOCIAL SURVEY RESEARCH AND
POSTGRADUATE TRAINING
IN SOCIOLOGICAL METHOD

MARTIN I. A. BULMER

'Every cobbler thinks leather is the only thing. Most social scientists, including the present writer, have their favourite research methods with which they are familiar and have some skill in using. And I suspect we mostly choose to investigate problems that seem vulnerable to attack through these methods. But we should at least try to be less parochial than cobblers. Let us be done with the argument of participant observation *versus* interviewing—as we have largely dispensed with the arguments for psychology *versus* sociology—and get on with the business of attacking our problems with the widest array of conceptual and methodological tools that we possess and they demand. This does not preclude discussion and debate regarding the usefulness of different methods for the study of specific problems or types of problems. But it is very different from the assertion of the general and inherent superiority of one method over another on the basis of some intrinsic qualities it presumably possesses'.¹

This note has been inspired by a proposal of the Social Science Research Council that 'one or more academic centres in this country should develop a training and research programme along similar lines' to that of the Detroit Area Study at the University of Michigan,² and that such a programme would be located within one or more Sociology departments, if it followed the D.A.S. model. This paper is not concerned to discuss Area Studies, but rather to examine the case for social research training at the post-graduate level integrally linked to a social survey research organization. Its basic premise is that the form taken by institutional arrangements for postgraduate training in sociological method should reflect a clear aim in relation both to postgraduate training in sociological method in particular, and to postgraduate education in sociology in general. (This note does not discuss the teaching of general methodology/philosophy of social science but this is assumed to be an integral part of postgraduate teaching of sociological method.)

Three main objectives of the scheme proposed are identified:

- (a) To establish a 'highly efficient means of providing young social scientists with an all-round training in the planning and execution of survey research'.
- (b) To provide staff members with an opportunity to pursue social science research.
- (c) To supply 'social science data of value' to authorities and communities in the area surrounding the university where the programme is located.

The comments here are directed principally to the first of these aims.

The primary aim of the programme, good postgraduate training in survey research methodology, is one for which there are distinguished American models and for which there appears to be a need in Britain. Writing seven years ago, Hannan Selvin commented on the extent to which Britain lagged behind American, and even European, provision for the teaching of quantitative social research.

'I was surprised to find', he wrote, 'that the British not only lag far behind America in their teaching of methodology and in their training for research but that they are also far behind many European countries . . . There is some instruction in methodology in Britain, and there are research institutes, but only one of the three major universities I visited has both these activities, and in that university (London), neither activity involved any member of the sociology staff. The backwardness is all the more surprising in view of the long British tradition of empirical social research (Graunt, Sinclair, Farr, Booth, and Rowntree are only a few of the names that come to mind) and of the pre-eminent place that England occupies in the development of modern statistics as a set of tools for research (it is enough to cite Pearson, Yule, and Fisher) . . . British sociology may well ponder the reasons for its failure to exploit its past opportunities and the changes that might be made in the future to bring it in step with sociology elsewhere'.³

One of the possible American models which Selvin described was the Michigan D.A.S. The students involved take a single course in introductory methodology, and a double course centred around a group research project, on which each year a staff member directs the class in a study of interest to him. The students benefit from working with an experienced scholar. Moreover, the execution is linked with the very large general-purpose Survey Research Centre within the University. ('The staff includes 75 professional research workers with doctoral degrees, drawn from a variety of social science disciplines, and 75 pre-doctoral research assistants. Allowing for part-time interviewers, etc., the total staff is equivalent to about 300 people, full-time'.⁴) Each D.A.S. study is carried out on a large probability sample of metropolitan Detroit (700-800 interviews in recent years), with some of the interviewing being done by the professional staff of the Survey Research Centre. In this way, graduate students within the Sociology Department are exposed to the highest standards of design and data collection, and benefit from collaboration with a large professional survey research organisation.

The form which the training takes follows the standard procedures of social survey research. The students' work includes preliminary relevant reading, problem formulation, question drafting, pretest analysis, sampling, interviewing, and data processing. In addition each student writes an analysis paper based on part of the collected data.

The value of such research may be estimated both from the published work which the D.A.S. has produced, and from the association with the Survey Research Centre which it enjoys. The latter is a major centre for research on survey methodology, and works like L. Kish's *Survey Sampling* and R. L. Kahn and C. F. Cannell's *The Dynamics of Interviewing: Theory, Technique, and Cases* have been written by members of its staff.

The intrinsic worth of the Detroit Area Study as a model for postgraduate training in survey research is not, therefore, in doubt. Nor is the appropriateness of American sociological models for research training at issue. Karl Popper has directed our attention to 'a very simple and decisive point, but nevertheless one that is not often sufficiently realised . . .—that we cannot start afresh; that we must make use of what people before us have done in science . . . We must carry on a certain tradition'.⁵ The question is: which tradition should be our model, and what are the criteria for choosing between different traditions?

The criteria for choosing between different traditions are, it is assumed, grounded in both general conceptions of the discipline as a whole, and in ideas about the form which postgraduate education in sociology should take. The direction taken by courses in sociological method should relate meaningfully to other parts of the postgraduate course, and an adequate assessment of the appropriateness of pursuing a programme of survey research training must answer the question of how it would fit in with other postgraduate courses. This is particularly so in the present state of the discipline. Faced with uncertainty about the direction in which sociology is going, surely the institutional arrangements for postgraduate training should be as flexible, open-ended, and potentially creative as possible, and not exclude from the outset certain possibilities.

If the future of sociology lies in the fruitful and creative interplay of theory and method, what are the implications for post-graduate education in sociology of the inter-disciplinary nature of the S.S.R.C. proposal? If 'the distinctive offering of sociology to our society is sociological theory, not only researched description,'⁶ how precisely would an inter-disciplinary survey research unit mesh with a postgraduate course which placed an important emphasis on the contribution of sociological theory to the formulation of sociological problems? Does the inter-disciplinary nature of the proposal not imply a quite unacceptable attenuation of the specifically sociological-theoretical element in such postgraduate education? How far would such a training scheme promote the interplay and integration of theory and method, and how far would its inter-disciplinary nature hinder this specifically sociological aim? A proposal to institute survey research training without making sure that an integral part of the course is concerned with how to translate theoretical ideas into researchable problems would seem to run a particular danger of emasculating postgraduate work in sociology.

Looking at sociological method as such, however, is sufficiently problematical, and is our concern here. Clichés about contemporary-sociology-in-flux apart, different sociologists tend to favour different methods for investigating the social world. Very occasionally, doubts about certain methods are expressed in print. In relation to social survey research, at least two kinds of criticism have been made. At a general level, criticism has been levelled at the methodological assumptions underlying such research. A. V. Cicourel, for example, suggests that too little attention is paid to the considerable difficulties of trying to elicit and quantify cultural meanings by means of structured interviews and multi-variate analysis.⁷ The practice, for example, of correlating attitudinal responses with a range of 'face-sheet' variables (sex, occupation, income, education, etc.) involves a series of assumptions about the way in which such structural variables influence social action. As Bennett Berger has suggested in discussing one such variable, age,⁸ it may be necessary to treat such attributes as culturally variable as much as structurally determinant. To do so, however, must greatly complicate the economical and efficient procedures of social survey analysis.

At a more specific level, arguments have been directed against the supposed limitations of social survey methods, as opposed to other methods. Howard Becker and Blanche Geer, for example,⁹ compared interviewing and participant observation and argued for the superiority of the latter in being more attuned to social actors' 'native languages', in permitting the probing of matters interviewees are unwilling or unable to talk about, in providing a means of cross-checking items of data, and in permitting the observation of changes in behaviour over a period of time.

This article provoked a rejoinder from Martin Trow,¹⁰ who pointed out the ultimate unfruitfulness of polemical arguments about the value of one method as against another. Whether or not he understood Becker and Geer correctly, his basic point is a just one. While discussion of the uses and limitations of particular methods is valuable, assertions of the general and inherent superiority of this or that method are unhelpful. The test of a particular method must be its value in practice for the sociologist. General criticisms of social survey research *per se* are thus beside the point. All methods have their weaknesses, but also can be shown to yield useful results in practice.

The proposal for social survey research training should not therefore be ruled out on *a priori* methodological grounds. It does not follow, however, that the proposal is necessarily well-conceived from a sociological point of view. For the proposal, in suggesting the establishment of training in one particular sociological method, would seem to place crucial limitations on the nature and scope of both training in sociological methods *and* on postgraduate education in sociology in general. If methods of research flow from sociological problems, in the sense that teaching methods is concerned with understanding how to develop research strategies appropriate to the problem under investigation, this flow may be hindered or stultified by a prior commitment to one particular method. (An alternative, complementary, formulation would

be that methods of research are chosen in relation to the nature of the social phenomena being studied. The same methods or combination of methods will not necessarily be equally appropriate in different contexts; for example, in the study of the effects of mass communications and of informal relations within formal organisations.)

In relation to the general aims of postgraduate education, can one prescribe which theory, or which method, is likely to be most fruitful? The teaching of sociological theory—whether in terms of the main historical figures or not—rarely focuses exclusively on one thinker or one school, to the exclusion of the others. The limitations of institutionalising social survey research training are not intrinsic to the method, but stem from the concentration on one method, when catholicity would seem more appropriate.

Two recent texts in sociological method provide an instructive contrast. Galtung's book¹¹ takes as its basic organising principle the data matrix, and builds around it a very sophisticated (though in places uneven) discussion of the whole range of quantitative social research procedures. Denzin's more recent book,¹² on the other hand, is written from a symbolic interactionist perspective, and takes as its basic concern the social interaction of the sociologist with those whom he studies. Although uneven also, over-compressed, and inadequate in its treatment of quantitative procedures, Denzin seems to achieve a more adequate overview of the range of sociological methods at present in use. An illuminating postgraduate course in method could be based on using both books side-by-side.

The S.S.R.C. proposal, however, implies a course which would use only Galtung (or its equivalent) as a text. That its limitations would be considerable is suggested by two reviews of Galtung which deserve quotation at length. Both reviewers recognise the very considerable value of Galtung's book, but both comment also on its particular view of the range of sociological research as a whole. Ironically, their implication is that the S.S.R.C. should not be setting up a programme of survey research training, but promoting and systematizing *qualitative* social research!

R. Boudon concludes a very favourable review¹³ with the following comments:

'Galtung does not deal with sociological research as a whole but with one particular though obviously important aspect: quantitative research. However, it seems to me that from an epistemological point of view qualitative methods are a field where exploration is urgently needed. This is one of the two main reasons why I do not believe Galtung's book represents a *theory of social research*, for such a theory would have to make allowance for the totality of the methods of observation, analysis, and explication of social facts. On the other hand (and this is my second objection) it would demand that the relationship between the research methods and the objects of research would be analysed. But Galtung is not at all concerned—save in some passages, e.g. where he examines the limitations of survey research (p. 148 ff.)—about the relationship between the methodology of sociology and its objects. This is due to the fact that the emphasis throughout is on the formal characteristics of techniques and methods. Besides, the importance attached to the idea of a data matrix results in giving a possibly over-excessive emphasis to types of research the results of which may be presented in the form of a data matrix. But there is a good deal of research which cannot be presented in this form—this is true not only of qualitative research but also of a lot of quantitative research related to secondary analysis.

Perhaps Galtung is over-attached to the current fashion of representation—but one that should be re-examined—according to which the survey is the mode *par excellence* and ultimately the only model of sociological research'.

A. P. M. Coxon has made very similar remarks in a review in this journal¹⁴ of Galtung and of the Blalocks' recent symposium.¹⁵

'Many sociologists are worried that methodologists have developed what has been termed the 'variable and variance' approach to the virtual exclusion of the 'situations' modes of explanation (known under other titles as the Columbia versus the Chicago schools, or behaviourism versus interactionism), and have largely neglected the relational aspects in the analysis of social behaviour. Both Galtung and the Blalocks tend to confirm this suspicion, but it arises for interesting reasons. Such texts must be based on where

sociologists are now—i.e. largely motivated on simple 'variable-centred' statistics and *The Language of Social Research*, or else eschewing formal and quantitative analysis and depending upon sensitizing concepts and insightful description. These two positions are not genuinely irreconcilable, of course, but any new methodology must presumably take account of both. Very many 'behaviourist' approaches are restricted to social (as opposed to sociological) variables, suffer from diminishing returns in the explanation of a dependant variable, use models of analysis which make ludicrously strong requirements of the data, and seem to let the inherently sociological problems slip through the net. Correlatively 'interactionists' have been tardy in developing appropriate measurement systems and other formal systems for unambiguously stating models of situations and interaction, and have usually let rigour disappear in a cloud of verbal subtlety.

This suggests a further possibility. How far does the S.S.R.C. proposal confuse survey research with sociological method *in toto* (i.e. treat the two as synonymous)? Although this confusion is still perpetuated in certain courses and certain textbooks, it has no basis whatever in the actual *practice* of sociologists. Social survey methods are one among a number of methods employed by sociologists. Refining the quantitative/qualitative dichotomy, at least six different types of sociological method may be distinguished, between which there are clear and significant differences. The distinctions are analytic, and different methods are frequently combined in particular pieces of research:

- (i) *Social Survey Methods*, described by Hyman¹⁶ and Moser and Kalton.¹⁷
- (ii) *Field-Work Methods*, using participant observation, reviewed by Junker¹⁸ and in the McCall and Simmons Reader.¹⁹
- (iii) *Personal Documentary Methods*, using life histories, letters, diaries, etc., evaluated by Gottschalk *et al.*²⁰ and Becker²¹.
- (iv) *Historical Approaches*, using the methods and materials of the historian for sociological purposes.²²
- (v) *Unobtrusive Measures*, using official statistics and other sources of non-reactive data, described by Wcbb *et al.*²³
- (vi) *Experimental Designs*, discussed by Campbell and Stanley.²⁴

What are the implications of this diversity for postgraduate training in sociological method? How can one do justice to the variety of methods without spreading the effort too thinly? Are there, in particular, any traditions other than the Michigan D.A.S. to serve as a model? Other contemporary American research institutes—the Bureau of Applied Social Research at Columbia University, the National Opinion Research Centre at Chicago—are representative in different ways of American leadership in quantitative social research, and would not seem to be very different from the Michigan model. However, a different, and older, American tradition might be worth considering, particularly as it involved a strong commitment to research in the area around the University. The research carried out in the Department of Sociology at the University of Chicago in the twenty years from the end of World War One still represents an impressive achievement in substantive terms, and contributed significantly to the advancement of sociological method. In a sense it is now part of the history of sociology, representing one of the earliest organised programmes of empirical sociological research. Has it any contemporary relevance?

Substantively, the work influenced by the Chicago tradition is still of importance in at least five different fields. In the study of deviance, Thrasher, Shaw, Anderson, and Sutherland are still read. In urban sociology and community studies, Park and Burgess's *The City* and Roderick McKenzie on human ecology remain of importance. In race relations there are a number of valuable studies: Wirth's *The Ghetto*, Stonequist's *The Marginal Man*, and monographs by Park, Pauline Young, Franklin Frazier, and C. S. Johnson. In the sociology of education, Willard Waller's classic, *The Sociology of Teaching*, is a product of the Chicago tradition.

Fifthly, the sociology of social problems was first developed through works such as *The Polish Peasant in Europe and America*, Thomas's *The Unadjusted Girl*, and Mowrer's *Family Disorganization*.

Nor was the emphasis exclusively upon empirical studies. Park and Burgess's *Introduction to the Science of Sociology* interpreted at least certain aspects of European social thought to an American audience, with a particularly Simmelian flavour. Park's main interests were by no means naïvely empirical, as Ralph Turner's discussion about him in the admirable *Heritage of Sociology* series emphasises.²⁵ His concern with social control and collective behaviour is reflected in his three-volume collected works.²⁶ Can one describe as 'empiricist' a man who developed the concepts of marginal man, social distance, social role, self-conception, accommodation, assimilation, or natural areas? Others such as Thomas, Ogburn, and McKenzie made theoretical contributions to the understanding of particular substantive fields. Wirth, in particular, was sensitive to value problems and helped to translate Mannheim's *Ideology and Utopia* for an English-speaking audience. More diffuse, but nevertheless significant, was the influence of George Herbert Mead from the Philosophy Department.

In terms of methods, research at Chicago was both innovative and catholic. Three methods in particular were developed strongly in different ways. Unobtrusive methods of research underpinned the theories of human ecology, and made extensive use of census tract data and other quantitative information on the city to establish generalisations about urban structure. Personal documentary methods were employed to illuminate a very wide range of sociological problems from the point of view of those involved. And field-work methods were central to the study of specific urban milieux. Historical techniques were not emphasised to the same extent, except by Wirth, although Park placed considerable emphasis on the contribution of history to an understanding of sociological phenomena.

The Chicago tradition did not lead to the development of social surveys of the modern kind. It is possible that they were thought inferior. Thomas, at least, once expressed the view that 'interviews in the main may be treated as a body of error to be used for purposes of comparison in future observations'.²⁷ Park, however, saw himself as carrying on the line of Booth and Rowntree, as his own justification for local studies—'The City as a Social Laboratory'—makes clear.²⁸ Everett Hughes argues that the contribution of the Chicago School consisted, in part, in continuing the social survey tradition, but leading it in a new direction, that of a more self-conscious and acute theoretical analysis.²⁹ Modern social survey research is a post-Chicago development and, in terms of method, was at first a reaction against the particular emphasis on certain other methods at Chicago. But it remains an interesting question which approach is closest to the concerns of Booth and Rowntree. At least in terms of the analysis of social problems, it may be argued that Park's creative approach seems more fruitful than the rather mechanistic and utilitarian servicing role which is at least implied in the S.S.R.C. proposal.³⁰

Where does this leave us? Is it enough to say that the Chicago tradition is now *passé*, its methods superseded by more rigorous ones, its naïve conceptions of sociology replaced by more adequate views of the relationship between the sociologist and his society, its monument being the *Heritage of Sociology* series? Much the same kind of comment—with different emphases—could be made about Graunt, Petty, Quetelet, Le Play, Booth, Rowntree, and others in the Pantheon. That we look back at traditions with the benefit of hindsight does not rule out comparing them with contemporary models, particularly if what they contributed seems to be valuable. On these grounds the Chicago tradition would seem to have some relevance both to area studies, and to the improvement of methods of sociological research. It is not a perfect model, and its lack of emphasis on social survey methods need not be emulated, but sociologically it seems at least as plausible as the Michigan D.A.S.

It is true, in a sense, that sociology at Chicago could not help being a success. It was a new subject in a young university, the largest and indeed the dominant sociology department in the country for 20 years; what comparison is there with a modest training programme in survey

research methods? Moreover, Chicago was fortunate in the leading figures it attracted—Thomas, Park, Burgess, McKenzie, Faris, Wirth—and to a considerable extent owed its success to them. Such creative drive cannot be formed by institutional arrangements. Yet perhaps these two objections also provide the hint of an answer—that the Chicago tradition derived its impetus from a common commitment to attack a range of sociological problems using a variety of methods, and *not* from a prior institutionalized commitment to one particular method. The range and scope of the achievement is indeed unlikely to be repeated, but one may be able to learn something from the form which that particular achievement took.

Notes

1. M. Trow, 'Comment on "Participant Observation and Interviewing: a Comparison"', *Human Organisation*, 16, 3, 1957, pp. 33-5.
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3. Hannan C. Selvin, 'Training for Social Research—the Recent American Experience', in J. Gould (ed.), *Penguin Survey of the Social Sciences 1965*, Harmondsworth: Penguin, 1965, pp. 73-95.
4. Jeremy Mitchell, 'Support for Social Science Research', *S.S.R.C. Newsletter* 8, March 1970, p. 14.
5. K. R. Popper, *Conjectures and Refutations*, London: Routledge, 1963, p. 129.
6. B. G. Glaser and A. L. Strauss, *The Discovery of Grounded Theory*, London: Weidenfeld, 1968, pp. 30-31.
7. A. V. Cicourel, *Method and Measurement in Sociology*, New York: The Free Press, 1964, esp. Chapter IV.
8. B. Berger, 'How Long is a Generation?', *British Journal of Sociology*, XI, March 1960, pp. 10-23.
9. H. Becker and B. Geer, 'Participant Observation and Interviewing: a Comparison', *Human Organisation* 16, 3, 1957, pp. 28-32.
10. 'Comment on "Participant Observation and Interviewing: a Comparison"'. The exchange, with Becker and Geer's rejoinder, is reprinted in W. J. Filstead (ed.), *Readings in Qualitative Methodology*, Chicago: Markham, 1970, pp. 133-152.
11. J. Galtung, *Theory and Methods of Social Research*, London: Allen and Unwin, 1968.
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13. R. Boudon, book review in *Acta Sociologica*, Vol. 12, No. 3, 1969, pp. 161-163.
14. A. P. M. Coxon, review article in *Sociology*, Vol. 3, No. 2, May 1969, pp. 247-250.
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17. C. A. Moser and G. Kalton, *Survey Methods in Social Investigation*, revised edition, London: Heinemann, 1971.
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19. G. J. McCall and J. L. Simmons, *Issues in Participant Observations: a text and a reader*, Mass.: Addison-Wesley, 1969.
20. L. Gottschalk et al., *The Use of Personal Documents in History, Anthropology and Sociology*, New York: S.S.R.C., 1945.
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22. S. M. Lipset and R. Hofstadter (eds.), *Sociology and History: Methods*, New York: Basic Books, 1968.
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24. D. T. Campbell and J. C. Stanley, *Experimental and Quasi-Experimental Designs for Research*, Chicago: Rand McNally, 1966.
25. R. H. Turner, 'Introduction' to *Robert E. Park on Social Control and Collective Behaviour*, Chicago: University of Chicago Press, 1967, pp. ix-xlvi.
26. *The Collected Papers of Robert E. Park* edited by E. C. Hughes, C. S. Johnson, J. Masuoka, R. Redfield, and L. Wirth: Vol. I, *Race and Culture*; Vol. II, *Human Communities*; Vol. III, *Society*; Glencoe, Ill.: The Free Press, 1950, 1952, and 1955.
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29. E. C. Hughes, 'Introduction', to B. Junker, *Field Work*. See also his article 'Robert Park' in T. Raison (ed.), *The Founding Fathers of Social Science*, Harmondsworth, Penguin, 1969. On sociology at Chicago more generally see: R. E. L. Faris, *Chicago Sociology 1920-32*, Chicago: University of Chicago Press, 1970.
30. Cf. the distinction made by M. Janowitz between the 'Enlightenment' and 'Engineering' models of the relation between sociological knowledge and the society in which it is produced, in his article 'Modèles Sociologiques et action sur la société', *Sociologie du Travail*, Vol. 10, No. 1, Jan-March 1968, pp. 36-49.

Biographical note: MARTIN I. A. BULMER, born Newcastle-upon-Tyne, 1943; B.Sc. (Sociology), London School of Economics, 1967; Junior Research Fellow in Sociology, 1968-70, Lecturer in Sociology, 1970, University of Durham.