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# When theory fails? The history of American sociological research methods

TIM MAY

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Jennifer Platt, *A History of Sociological Research Methods in America 1920–1960*. Cambridge: Cambridge University Press, 1996. 372 pp. £40.00. ISBN 0 521 44173 0.

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The development with which we are concerned is in both the practice of method and in its conceptualisation and theorisation; these cannot be dissociated from the wider sociological setting. (271)

So begins the conclusion to Jennifer Platt's book on the development of American sociological research methods in the period 1920–60. Interpersonal relations, graduate students and resultant social and academic networks; the funding of research by foundations, government and private individuals; an analysis of relevant monographs, articles and textbooks; the use of archival material and unstructured interviews with many of those people involved with the development and practice of research methods: these form the empirical basis for the study.

A 'history of ideas' approach would normally be adopted in considering this subject. This would take the form of a discussion of philosophical, socio-theoretical, or methodological issues, against which research practice would be evaluated. However, this study opens by stating its aim of combating the 'naive assumption' 'that theoretical positions determine or summarise the whole of sociological practice' (1). It does not subscribe to a 'normative

agenda' whereby histories are written of exemplary studies that conform to particular agendas. Overall, it seeks to avoid the error of 'reading off' particular studies against underlying theoretical positions which, under close examination: 'cannot be causally responsible for them' (6).

A chapter which examines writing on method then follows. Starting with monographs, taken as new and significant contributions to the field and noting the problems associated with these evaluations, some clear trends emerge. First, a more detailed discussion of issues of method was apparent as the discipline became more institutionally established. Second, up to the 1940s a quantitative/qualitative, humanistic/scientific controversy was apparent; represented, for example, by Znaniecki's distinction between cultural and natural systems and method of analytic induction on the one hand and Lundberg's dismissal of all methods of data collection, other than quantification, as 'mystical', on the other. Third, social work concerns were apparent in the earlier years, to be taken up by commercial research agencies and institutions towards the end of the period. Fourth, pure qualitative work diminished in the postwar period and questions of experimentation and measurement predominated. Yet this change also represented the situated practicalities to which practitioners of sociological research responded: for example, the wealth of data produced by the 'American Soldier' research conducted during the Second World War.

In relation to textbooks of the time (which may be seen as an indicator of trends within the discipline) a number of parallel themes emerge, a feature given particular prominence due to the hiatus in production from the pre- to postwar period. Again, matters of measurement, scaling and discussions of variables become more prominent, with a decline in case studies, and life histories. At the same time, the topics of 'fieldwork' and 'participant observation' begin to emerge. Whilst observational studies were conducted during the 1920s, they were not seen to involve access to meaning production in the manner with which its practice is now associated. Interest centred not on the means of data collection, as it increasingly did in postwar times, but on what was collected. In this sense it could be seen as connected with matters of accuracy and its systematic nature, more in line with natural, rather than social, scientific discourse. It was not until the late 1940s and early 1950s that it became advocated as a method in order to access the meanings of particular communities, groups and practices. Collectively, these issues, together with 'exogenous' factors such as the Depression, the Second World War and increased governmental demands for the production of population statistics and opinion polls, led to significant changes in writings on method.

Chapter 3 is devoted to the subject of scientism. As the author notes, this term is often used in a pejorative sense to encompass positivism, definitional operationalism and empiricism. Positivism, in particular, seems to be used in such an indiscriminate way – normally as a term of abuse – that its meaning

has become increasingly devoid of substance. To this extent, an examination of actual research practice in pre- and postwar times is to be welcomed and the chapter questions the assumed connection between scientific theory, methodology and associated writings, and developments in method. For example, interest in Durkheim was due not to meta-sociological concerns, but to the use of his work in relation to comparative method and multivariate analysis. This mediation of ideas in the service of innovatory research practice was also apparent in respect of the Vienna Circle.

Those such as Giddings, Chapin and Ogburn, possessed more influence. Their striving for objectivism is seen to derive from changes in academic culture engendered by a growing sense of a lack of shared values in the interwar period, accompanied by attacks on social science in the name of religion. As such: 'it will be noted that the content of the works of these writers owes little to philosophers' (77). Similarly, Lundberg and Dodd appear as major influences on scientism. As Lundberg wrote on the inapplicability of the scientific method for dealing with language and meaning production:

. . . the symbols and symbolic behavior by and through which man anticipates future occurrences or 'ends' can be objective and tangible 'empirical facts' as any other phenomena whatsoever. (quoted, 79)

Lundberg also wrote on the applicability of the categories of 'attraction' and 'repulsion' in energy flows as relevant to the study of social phenomena. Dodd's work on 'sociometry' may be summed up by the following phrase: if a social phenomenon cannot be measured and given a statistical value, it does not exist. His 'S-Theory' aimed to heighten methodological precision rather than generalize and to this extent encompassed 12 basic concepts with a further 400 derivatives for the purposes of comprehensively classifying social phenomena. Yet, the reaction within sociology to their ideas was mixed. Lundberg, for example, was regarded both as a 'philosopher' and, by Hubert Blalock, as an 'extreme operationalist', whilst Dodd's complex and idiosyncratic symbolism was not thought to be worth the investment in terms of the insights it might yield. Thus, if a consensus emerged during this period that sociology was a science, an agreement on content was not apparent. Therefore, questions are begged regarding the relationship between theory and practice. This is the subject of Chapter 4.

For the author theory is not seen to guide, or cause, practice. In the absence of this elective affinity, what are the determinants of 'practical methodological outcomes'? In order to impute theoretical commitment to practitioners it would be necessary to show (1) that they hold those beliefs independent of their research and (2) how such beliefs translate into research practice. The practical problems encountered are that researchers do not necessarily write general statements on such preferences and when they do so, their practice is not necessarily in accordance with their precepts. Platt thus argues that

discourses about practice need to be separated from those of practice itself: 'In the absence of appropriate evidence, imputations of theoretical commitment come from the observer rather than the actor, and so are normative rather than empirical' (108). Further, in pursuance of (2), even if practitioners hold such beliefs they would need to be fully committed to and conscious of them, to implement them with consistency in appropriate contexts and to select their methods without giving due thought to, or indeed encountering impediments from, practical constraints. As Platt concludes: 'I may be excessively cynical, but I find these further conditions implausible as universal empirical generalisations' (109).

In search of an alternative explanation, she turns to writing on methods. An analysis of journal articles over the period, whilst not assuming that they are representative, allows for reports of practice which might conform to particular theoretical commitments. For this purpose 'major' journals are examined: *American Journal of Sociology*, *American Sociological Review*, *Social Forces* and *Social Problems*. Again, no evidence is found and it is suggested that 'theory is led by practice rather than vice versa' (128). This came as no surprise to some of her respondents who had a more pragmatic and even instrumental approach to methods in terms of their appropriateness to the problem or issue under consideration. Thus, in the case of the relationships between functionalism and the survey, Weber, symbolic interactionism and participant observation, Marxism, feminism and method, supposed causal links between methods and philosophical, political and theoretical preferences are found wanting. The message is not that theory is irrelevant, simply that it cannot explain the genesis or adoption of particular methods.

In terms of such situated pragmatics of social research, Jennifer Platt reports that Goffman said his earlier methods were determined by a lack of funding. In this sense, research funding and the use of research methods become inextricably linked. In a chapter devoted to this very topic, causal accounts of the relationship between funding, state interests, methodological commitment and research outcomes, are again questioned. An examination of private funding and the Rockefeller Foundation's support of institutional centres of research activity then follows. For instance, the Laura Spelman Rockefeller Memorial fund, consolidated into the Rockefeller Foundation in 1929, distributed some \$112 million over six years; the main recipients being the universities of Chicago, Columbia, North Carolina and Harvard, as well as the Social Science Research Council (SSRC).

When it came to government funding, an outlet for rural sociology came from the Division of Farm Population and Rural Life in the US Department of Agriculture. A division of this office, the Division of Program Surveys, was started in the late 1930s to collect interview data on farmers. Then, when the USA entered the war, it became responsible for more systematic data collection on the civilian population with Rensis Likert being employed to

develop a more 'scientific' basis for the work. One sociologist involved at the time described this work as 'dustbowl empiricism' (152). In addition, the New Deal enabled the collection of large data sets outside of government, whilst research was conducted on behalf of the USAF and the US Army. One researcher, upon asking an Air Force colonel why they should subsidize research when he had just said it was not important, received the following reply:

... he said suppose there's a war, and we lose the war, and after the war there's an investigation of why we lost the war – well, we think it essential for us to be able to show that we consulted with all the available experts, even sociologists! (Gross quoted, 156)

Platt concludes from her examination that whilst capitalist interests were undoubtedly present, they do not provide a sufficient explanation for research practice outcomes. If Fisher's (1993) concept of contradictory class location is insufficient, organizational and occupational interests become of relevance. It is these factors which produced an increase in quantitative work. Foundation funding was not creating this state of affairs but: 'taking the advice of leading academics who identified this as unmet need' (196).

Another plausible explanation for the development of sociological research methods lies in the social structures of academic life. Here, the influence of 'schools' in historical accounts predominates. Some associations are then provided: for example, Merton and Lazarsfeld at Columbia; Burgess, Blumer and Hughes at Chicago; Dodd and Lundberg at Washington and Howard Odum at North Carolina. In the case of each of these, a divergence of influences and methods is found which does not permit the attribution of a core set of ideas in order to explain research practices. On the other hand, both the onset of war and the age of academics and researchers related to developments in methods.

There is not a simple dismissal here regarding the use of explanatory categories against which to evaluate historical data, nor a denial of the role of causal accounts. Jennifer Platt's aim is to distinguish adequately between descriptions and causal propositions in order that data can be subject to test:

Some of the problems noted seem to arise because authors start with preconceptions about the existence of particular groupings, rather than defining what they are interested in and then looking for instances; this can put them at the mercy of the processes of the social construction of perceived 'schools', which arguably might better be treated as part of the subject matter for study. (237)

The question has now changed: how to account for the creation of particular images of this history given the findings? This is the subject of a chapter in which it is noted that selective memory of events, studies and the actions

of particular individuals can serve to legitimate and identify claims made within contemporary practices. Here, for example, associations between longevity, gender and reputation emerge. Women, for instance, may outlive their husbands and thus organize events, writings and conferences in their memory. As a result, even if the women were of the same or greater intellectual stature in their lifetime, a memory of the males' contribution may last longer. In terms of race, W. E. B. Du Bois found racism an impediment to his academic career and later turned to political activism, the result being that he is remembered for the latter, rather than the former, despite the scholarly and exceptional nature of his work.

As for memories of exemplars, a leader in this period was Whyte's *Street Corner Society*. However, the methodological appendix to this book (published in 1943), was not added until 1955 and then for teaching purposes. This changed the perception of a book which had clear behaviouristic and positivistic orientations (Whyte was taught by Hughes and Warner, but also learnt statistics from Stouffer and Ogburn). The dominant interpretation of the work was not in accordance with the actual practice of its author. As he has written:

From 1948 on, working with students on surveys, I came to recognise that, while the method had limitations, it also had important strengths. Many years later, in the course of our research programme in Peru, I became convinced not only of the importance of integrating surveys with anthropological methods but also that the study of local history could enrich our knowledge. (Whyte, 1984: 20)

Selective interpretations fulfil the needs of particular groups or schools of thought for the gaining of respectability and identity through intellectual lineage. As Platt notes (255) in considering this issue, it is not whether a study meets the criteria of a particular category or not, but whether it is a good example of the use of a specific method, or set of methods, that should be of interest.

In a similar manner, reputations grow beyond context. The 'Chicago school' has come to be seen, through the accounts of its own members as well as others, as a distinct entity. Nevertheless, there were those, such as Jane Addams, centred around the Hull House settlement, who were at first seen as sociologists and then, in the course of the academic ascendancy of the university, redefined as social workers. There were also those women in academic posts who, with the establishment of sociology within the university, were placed in departments such as 'household administration' and 'home economics'.

In her conclusion, Jennifer Platt returns to an explanation of the development of sociological research methods. It is not paradigms or influential schools of thought following a set of theoretical principles or programmes of

research which explain the outcomes, but, as mentioned earlier, factors such as the Depression and the Second World War, or the postwar GI Bill, in interaction with internal disciplinary developments in research methods. Thus, whilst funding was of importance, it did not constrain developments in data collection. Research became more quantitative, but those who sought to maintain a qualitative/quantitative divide when the boundaries became more blurred, reconceptualized the differences with the result that participant observation, as we now know it, became a distinctive category.

In the unfolding of this detailed study, the boundaries between internal and external accounts of disciplinary accumulation of knowledge and resultant practices, break down. Boundaries are so often drawn in order to meet particular categories or agendas. Yet upon close examination departments are not homogeneous centres pursuing particular research lines; differences exist between and among them. The division of labour between theorists, methodologists and empirical researchers was as real then as it still appears:

. . . research methods may on the level of theory, when theory is consciously involved at all, reflect intellectual *bricolage* or *post hoc* justifications rather than the consistent working through of carefully chosen fundamental assumptions. Frequently methodological choices are steered by quite other considerations, some of a highly practical nature, and there are independent methodological traditions with their own channels of transmission. (275)

The practice of sociological research methods has to be examined on its own terms. Its development cannot be separated from the wider context in which it is produced. Nevertheless, it 'has its own dynamics, which cannot be fully subsumed under other heads' (275). These 'other heads' include those which theorists, philosophers and historians of ideas, invoke.

Overall, the book is a welcome counterbalance to those trends in historical writing to which the author alludes. There is a clear problem in applying to these developments concepts that render little justice to their dynamics and resultant subject-matter. That said, at the same time as wishing to avoid unhelpful dichotomies between, for example, theory and research, the author tends to keep reminding the reader of the poverty of 'theoretical' approaches to the subject. Perhaps this reflects a perceived need to correct those tendencies within the discipline to which she alludes. However, the study tends to be written not in terms of what we might learn from past developments for present practices, but in order that an examination of the past should not be conducted according to pre-established theoretical principles. This somewhat polemical tone leaves the reader with a sense that more could be learnt from this detailed investigation. This is clearly an aim of the author, but one not explicitly fulfilled.

The above might well be due to the contingency of her approach becoming



an overcorrective to what are seen as the fixed generalizations that necessarily come from theoretical allusions. Yet this is not an either/or choice. As Thomas McCarthy puts it: 'contingency is opposed to necessity and not to universality'. Further, that 'universality should not be confused with infallibility' (Hoy and McCarthy, 1994: 231). In that sense, the study is close to falling into the same trap that it seeks to avoid. Reading practices retrospectively against principles renders little justice to their dynamics, but the situated pragmatics to which the author alludes tend to appear as instrumental necessities.

This poses a more general question: can historical studies be conducted without employing ideas from the very traditions which Jennifer Platt criticizes? For instance, in the chapter on writing on methods, it is inevitable that the topic of the conceptualization of methods should arise. In a footnote, the author writes that she 'has a problem here which is shared with those she appears to be criticising' (45). Discussions of this type, given the tone of the book, should have been in the main text. However, the author is committed to her approach:

History has often been approached from the angle of systematics, whether by intellectual preference or because of the pragmatic demands of teaching. Perhaps the time has come to try approaching it with the presupposition of incoherence, eclecticism and lack of pattern? (236)

At the same time, in an allusion to the 'strong programme' in the sociology of science, she takes it as a 'methodological imperative' to explain every outcome 'independent of its intellectual merit' (5). Here one is reminded of Foucault's injunctions to take history as comprising a series of 'events'. They represent a 'breach of self-evidence' where the temptation is to invoke constants, or a uniformity which imposes itself on otherwise disparate happenings (Foucault, 1991). These events, however marginal, are not taken as surface manifestations of deeper underlying structures. Instead, it is the study of cultural practices without a reference point outside of history itself. This absence of a regulatory theoretical framework is a fundamental part of the strategy through which histories of this type may be studied and written. History is then analysed in terms of relations of force, strategic development and tactics (Foucault, 1980).

The study does allude to schools in terms of their actual internal dynamics, but this discussion centres on Lundberg and his circle. Given this, the history often appears as one of reasoned problem-solving, yet accompanied by opportunistic responses to changing social, economic and political conditions. Is this overcoming an internal/external account of the development of research methods? What of the component of anticipatory decision-making on the part of academics and funding agencies? Therefore, whilst

Jennifer Platt questions Fisher's analysis, she does so by arguing that funding agencies were manipulated by academic interests. This tendency to overcorrect leads to a series of absences: for example, in the chapter on structures of academic life, one gets no sense of the ways in which struggles for academic capital in relation to the availability of funding, produced particular outcomes (see Bourdieu, 1988). One of the few insights one gets into these internal dynamics is in relation to the propensity for networking manifested, for example, in baby-sitting circles and, as noted, those women pioneers who were redefined or marginalized as the result of the institutionalization of empirical work.

These absences lead the reader to ask a series of questions; in particular, was this development a mixture of opportunism, the search for truth, social reality, academic status, or dispassionate and disinterested work in general? When the reader looks for such a discussion in the text, it proceeds to examine the functions of myths and exemplars in historical allusions to this period and repeats the point regarding the explanatory poverty of a category-led approach to the subject. This is disappointing given that the author has written of the need to investigate 'the kind of "connecting theories" which play an essential role in inference. If some methodologists were to work on these problems significant advances in understanding could be made' (Platt, 1981: 64).

These points noted, no book can seek to exhaust a topic and this study is, without doubt, an important contribution to our understanding of an area of sociology colonized in ways that can serve as much to obscure, as to enlighten, our understanding of its development. It is hoped, therefore, that this study will provide a basis for further work by the author in pursuance of some of the questions raised here, as well as those she has raised herself.

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