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by Jennifer Platt

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Jean-Michel Chapoulie's approach is sophisticated and richly suggestive. The main thrust of his argument deals with his concern for intellectual methods that avoid the implicit hypotheses of "conventional history," in which excessive personal familiarity with the field sets the trap of presentist approaches and disregard of causal factors not internal to the discipline as now defined. He sees the solution in a focus on the activities of the going concern(s) which produce sociological work rather than just on their products, and in attention to the criteria of argument and rhetoric exemplified in those products. Below, I concentrate on the former, and raise some issues about the possible implementation of his programme.

One can, as I have tended to do in the past, define what is needed as the sociology of past sociology, rather than as good historical work on past sociology. Why should it not be taken for granted that there are social processes, not only individual intellectual achievements, determining or contributing to sociological outcomes, and that the social institutions involved require study? Why should work on our own discipline be exempted from the general requirement that empirical statements rest on systematic data, collected by methods that are transparent to the reader? It is much less likely that sociologists will explicitly disagree with such statements than that they will, as Chapoulie points out, unselfconsciously write in ways which do so implicitly. I have sometimes admired an author's confident, magisterial tone – and then realised how much this owed to a reliance on a "diffuse knowledge of the environment" [Chapoulie 2009], and the absence of any explicit and systematic data

with explanation of their lacunae and difficulties. Chapoulie goes beyond such observations to suggest an intellectual framework specific to the needs of the history of the social sciences.

“Historical sociology” has become a recognised specialism, though one typically dealing with broader topics than the history of sociology. John Goldthorpe [1991] has raised one of the problems suggested by “grand” historical work by sociologists such as Theda Skocpol, who by the scale and chronological range of the questions they address have been driven to rely on using as secondary data work by historians who were not necessarily addressing the same problems, and in consequence have often taken such indirect interpretation of the primary data farther than it can bear.¹ To agree the problem is not to solve it, and there are intrinsic difficulties in relating micro and macro levels. Goldthorpe urges the merits of staying within the range of topics where it is possible to generate one’s own evidence. But that conclusion, as he recognises, only applies if the question addressed is one posed in terms of a general theory rather than of specific historical situations. That draws attention to some real conflicts of approach, summarised by Adams *et al.*:

To the extent that historical sociologists underline the fundamental historicity of the categories and concepts of social life (...) they will inevitably be at odds with social scientists seeking universal covering laws [Adams *et al.* 2005, 25].

Chapoulie here seems to incline to the historicist choice, but I hope he might agree that the question of universal laws can be addressed after we have a careful historical description.

Where to Start? What to Include?

Chapoulie advocates the inclusion, in the history of sociological work, of other disciplines, and of activities such as social journalism and reform movements, works seen as unscientific or which have not received recognition, unimportant authors, etc. He argues that otherwise we are at the mercy of actors’ changing folk norms of what to count as sociology. But one does not have to reject that approach to recognise that folk norms are undoubtedly part of the descriptive scenery of what we study, whether they are considered as effects or causes, and that to ignore them may risk imposing one’s own equally unhelpful conceptions. The extent to which there

¹ Goldthorpe does not mention the converse problem in work on the history of sociology: some good historians who are not sociologists have, in my judgment, written weaker history because they are simply not familiar enough with the sociological work they write about, and so they too have relied heavily on secondary sources without adequate critical perspective on them.

is an institutionally and intellectually separate “sociology” has varied from one time and place to another, and so perhaps the most useful starting point is a question, rather than an assumption, about the extent to which it can realistically be treated in isolation from its wider social setting.² To say that intellectual work done under other auspices is “really” – or even just “equally” – sociological, by whatever criteria one chooses, seems in itself presentist. But to note that it has features in common with what in other times and places has been formally “sociology” can still lead to worthwhile research questions: if they coexist, why is one called sociology and the other not? How are local traditions of division of academic labour involved? If one is earlier than the other, was the former socially an ancestor of the latter, or did that develop independently? And so on.

There seem to be in Chapoulie’s proposal two partially separable themes, each of which has at least occasionally been exemplified in practice. One is the recommendation that the problems in defining where the boundaries of sociology lie be dealt with by setting particular sociological outputs in a total context going beyond the intellectual content of research done by those formally labelled as sociologists.³ (Maybe some work by sociologists working as public intellectuals should be reclassified as journalism?) A stronger version of this recommendation would be to see sociology as part of a broader phenomenon, rather than other forms of work as part of sociology. Some academic sociologists have maintained that investigative work such as that of journalist Barbara Ehrenreich [2002] on minimum-wage jobs and living standards is better sociology than that produced by their colleagues. Both her work and the empirical work of professional sociologists on class and the labour market could then be treated as members of some such category as “social investigation and commentary.”

Examples of sociological work along the lines advocated might be:

- Camic’s work on the significance of within-university relations with other disciplines to the differing conceptions of sociology’s task in early US sociology departments [Camic 1995];
- Scott and Husbands’ work on the Sociological Society (later the Institute of Sociology) in Britain, a body which sat on the borderline between academia

² Chapoulie applies this point particularly to the Chicago School, and it seems likely that this is indeed a very strong – maybe an unusually strong? – case where the two cannot meaningfully be separated [see also Platt 1992]. A question not raised is why the conventional history is so often of a single discipline in isolation; however, his reference to the solidity of the market for it suggests that this may owe a lot to the perceived needs of teaching students on degrees defined as in that discipline.

³ Interestingly, this has been done without question for theorists and founding fathers treated as such; Marx was certainly not formally a sociologist, and it has been pointed out that for much of his career Weber was an economic historian, while a number of those used as prominent theorists in recent times have been philosophers by trade.

and extra-academic activities in the 1930s and closed in 1955, and its leading figures Branford and Geddes [Scott and Husbands 2007];

- Connell's analysis and promotion of "Southern theory," located at the periphery and often by authors not regarded as sociologists [Connell 2007].⁴ (Of course part of the point there is not so much to describe what has already happened as to advocate what should happen in the future).

This recommendation offers a challenging but inspiring agenda, though perhaps one unduly ambitious in its demands. One could almost always work inwards to the micro level or outwards to larger systems, but is that always necessary? I am reminded, at the level of caricature, of the addresses we used to write inside school atlases, which conventionally ended "The World, The Solar System, The Universe." One does not need to get as far as The Universe to give an address sufficient for the delivery of a letter, and some boundaries are needed if particular projects are to be completed. Consider the difference, for instance, between inquiry into the career of the influence of a single US article from the 1970s, and into factors affecting the institutionalisation of sociology in Latin America. Each is a perfectly reasonable historical topic, yet it seems unlikely that the phenomena require all the same factors to be taken into account to understand and explain them – though openness to that possibility would certainly not be a disadvantage

The other theme is about things which *are* part of the total range of recognised sociological activity, and suggests that it will be fruitful to look at the full range of ordinary or less successful sociological work, not just (the presumed ancestors of) currently fashionable or already famous parts. Potential examples of this:

- Turner's use of the career of the once prominent but now little-known academic sociologist Ellwood to throw light on his whole period and on the reasons why later he sank from sight [Turner 2007];
- Work on the history of introductory textbooks in sociology – not a highly respected genre, but the most widely diffused form of publication [Platt *et al.* 2008];
- DeCesare's work on sociology in US high schools, a level of teaching usually ignored in history, but one both consequential for later stages and indicative of mundane assumptions about the content and purposes of sociology [DeCesare 2005].

It is only by knowing something about such matters that the contexts which first produced better-known phenomena, and then made them salient, can be under-

⁴ Her argument is analogous to Chapoulie's, in that it too shows how what present themselves as abstractly universal ideas are in fact ones that have arisen in and reflect specific circumstances.

stood. Possibilities not exemplified above which could be added to these lists include historically neglected areas such as how sociology has been taught in higher education, the careers of sociologists inside and outside the academy, research for national and local government, think tanks and commercial research bodies such as polling agencies and market research companies. In addition, a major activity of sociologists is the production of graduates in sociology, relatively few of whom will formally become “sociologists” themselves, so that the nature and career of that product could appropriately be included under this head. To such pursuit of contextual ramifications and filling of substantive gaps one could in principle also add novel theoretical approaches.⁵ For instance, how about an application of rational choice theory to the history of sociology?⁶ The shortage of public data on individual sociologists’ earnings might be a problem, but there are useful archival materials which include correspondence with publishers about royalties, as well as foundation records about grants. This would be an interesting counterbalance to the cultural turn!

Activities and Knowledge from Personal Experience

For Chapoulie a salient problem is the effect of too much familiarity, leading those writing on historical topics to draw excessively on personal experience and general knowledge. There is perhaps some tension between his critique of historical work which draws on what a participant knows without the effort of serious historical work on systematic data and, later in the paper, his recognition that there is a role for “the ethnographical knowledge which each one of us can have acquired.” I am happy to stand up for the potential value of the latter, as long as it is treated critically as participant observation data. Peneff [2009], in his fascinating new book, has made the case for the primacy of observation as the only mode of data collection which gives direct access to the data. Arguably some of us have occupied roles which offer an unusually advantageous ethnographic vantage point: officer of a learned society, academic dismissed on political grounds, journal editor, member of first cohort of feminist or gay activists, advisor to minister... (It is not only one’s own personal ethnographic experience that can be drawn on to supplement more systematic sources; those who have occupied such roles can share what they have observed.) This does not fit the case where the data of interest are publications in themselves, not just as indicators

⁵ The only instance I can recall of serious application of a worked-out theoretical position – from a theorist – to empirical data about the history of sociology is in chapter 8 of Kilminster 1998. Surely there must be others – but I suspect not many.

⁶ Maybe Gary Becker has done it already? A recent paper on the migration of leading physicists to the destinations with the highest R&D expenditure is suggestive [Hunter *et al.* 2009].

of what they report or is assumed to lie behind them, but it serves the purpose of drawing attention to the often merely intermediary status of documents. In addition, saturation in historical materials from a past time and place can give one something like an ethnographic “feel” for it which at least helps one to interpret its individual documents better than one would with more narrowly focused knowledge.

Informally-ethnographic knowledge can find a place as data on what participants “knew” at the time. With luck, there may even be inconsistent accounts available for the same events or period, which will facilitate the tracing of the experiences and networks that have led to different bodies of tacit or taken-for-granted “knowledge.” (I have seen a prominent theorist’s brief outline of the history of sociology since World War II which made no mention of Lazarsfeld; no doubt he could find an omission equally shocking to him in any such draft by me). This illustrates a blessing and a complication of historical work on the discipline: its reflexive possibilities. The stories that our informants tell are analysable as stories that are told about the discipline, rather than just as recitals of historical facts – *and so are our own versions of the history*. Burrow [2007] has written an inspiring history of histories. He shows what topics and problems have been treated as appropriate, what the concerns have been that have come to the fore, what issues of method have been raised and what stances different writers have taken on them, what has been taken for granted and what as requiring explanation. The perspective implied is more widely applicable, and could help provide some of the critical distance required.

A practical problem in aiming to start from activities rather than products is simply that the products themselves are relatively easily accessible, but carry little of the data on the activities involved in their production, so we need to look elsewhere for those. Some sources are commonly available – college catalogues, conference programmes, minutes of committees, constitutions and lists of officers of learned societies – because they are routinely produced, widely diffused and at least semi-public; such other matters as private letters on academic topics, course reading lists or lecture notes, original research data such as field notes or interview transcripts, details of the division of labour in research, may never have existed and/or are not usually in the public domain. One practical reason for the tendency to limit historical work to the academic sphere must be simply its predictable provision of some forms of relevant documentation. There are, indeed, temptations to use what comes to hand: editorial board memberships as a measure of disciplinary status, the titles of association sections as an indicator of the general pattern of research interests, SSCI citation counts as a measure of intellectual influence. It would seem wasteful indeed not to make use of these, though their adequacy as measures of the desired concepts can be very questionable.

But even in academic life and its research publications some things are seldom documented, though they may directly affect the character of the intellectual work done. I remember a mature student, who came to university from a background of left political commitment and trade union activism, and who, on reading a sociological work, immediately identified the leftist sect with which its author was affiliated. I could not do the same; I was not aware of the sect's existence, the author's membership was certainly not publicly recorded, and to place the work correctly required experience of the differences treated as politically important by members of small ideologically committed groups. For some authors their political or religious beliefs have affected, or indeed been inseparable from, their sociological work, but it may take such direct knowledge to perceive, or infer, this if it is not explicitly stated. Where documents are available, they may be misleading. I have the testimony of a person actively involved in a controversial episode in British sociology that several of the actors were at the time for political reasons making written public statements, but offering quite different personal views in private conversation on the matter. That does not make the documents less significant interventions at the time, but it certainly alters their meaning. Some such matters may have been diffused on informal gossip networks – but commonly-known gossip is not always correct (even if the prevalence of its contents is nonetheless of intrinsic historical interest), and later access to it is problematic.

Chapoulie's thoughts are particularly valuable in that they suggest good questions to ask around any research topic. As practical guidance they should, however, probably be interpreted not so much as direct instructions to the individual worker, but as aimed at the needs of the field as a whole rather than of each project, and as applicable in different ways to different topics. Diversity of approaches is a strength, not a weakness, as long as it does not follow from work in group traditions which ignore each other, since we learn from criticism. Every strategy has its weak points, so a combination of approaches may allow their complementary strengths to emerge; eclecticism and opportunism maximise the use of scarce data resources. Choices from the toolbox need to depend on the problem and the data available; we have to make the best of what we have got, and data with evident limitations, as long as those are taken into account, are better than none. Nobody can do everything, but each of us can aim to bring a well-made brick to the wall of our shared knowledge.

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Abstract: Critical against conventional disciplinary history of social science, this essay presents an analytical framework stemming from historical research of the author on Chicago sociology and french sociology after 1945. The proposed point of departure for research in the history of social sciences is similar to the basic perspective of history (as proposed by Lucien Febvre) or sociology of work in Everett Hughes' style: social sciences are to be considered as social practices whose primary ends are the production of texts, with historical and thus changing properties. Investigations must look at every categories concerned directly or indirectly (or even in abstentia) with the production of social sciences: researcher, concurrent researcher of other specialties or disciplines, scholarly and learned institutions, those who finance research, general audiences, etc.). Heterogeneous elements must be taken into account: the documentary sources, the way of processing documentation, the rhetoric, the categories of analyses and questioning, the social contexts in which the research is carried out, the biographical experience of producers, the contexts of publication and reception. As an illustration of the possible extensions of this approach, a rapid presentation is given of the use made in research on the Chicago tradition of certain distinctions to analyze the diffusion of works and the relations between generations of researchers.

Keywords: history of sociology, social journalism, sociological activities, personal experience.

Jennifer Platt is emeritus professor of sociology at the University of Sussex. Her main research interests are in the history of sociology and in aspects of research method. She has been president of the British Sociological Association and the Research Committee on the History of Sociology of the ISA, and Chair of the Section on History of Sociology of the American Sociological Association. Her most recent book is *The British Sociological Association: a Sociological History* (2003).