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## **The Coming Crisis of Empirical Sociology**

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## Bios

**Mike Savage** is Professor of Sociology at the University of Manchester where he is Director of the ESRC Centre for Research on Socio-Cultural Change (CRESC). He has written widely on social stratification, urban sociology, and historical change, and is currently completing a book exploring the cultural and political history of the social sciences in post war Britain. His recent publications include *Globalisation and Belonging* (with Gaynor Bagnall and Brian Longhurst, Sage 2005) and *Rethinking Class: Culture, Identities and Lifestyles* (edited with Fiona Devine, Rosemary Crompton and John Scott, Palgrave 2004).

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## **Abstract**

This paper argues that in an age of *knowing capitalism*, sociologists have not adequately thought about the challenges posed to their expertise by the proliferation of 'social' transactional data which are now routinely collected, processed and analysed by a wide variety of private and public institutions. Drawing on British examples, we argue that whereas over the past 40 years sociologists championed innovative methodological resources, notably the sample survey and the in-depth interviews, that reasonably allowed them to claim distinctive expertise to access the 'social' in powerful ways, such claims are now much less secure. We argue that both the sample survey and the in depth interview are increasingly dated research methods which are unlikely to provide a robust base for the jurisdiction of empirical sociologists in coming decades. We conclude by speculating how sociology might respond to this coming crisis through taking up new interests in the 'politics of method'.

## **Keywords**

History of sociological methodologies / survey methods / in-depth interviews / politics of method / transactional data / geodemographics / descriptive sociology

## Introduction

In this paper we suggest that sociology faces a coming crisis, which has not yet been sufficiently appreciated or understood. Although much has been written about *theoretical* worries concerning the status of the 'social' in an age marked by globalising, mobile and dynamic relations, where 'social', 'technological' and 'natural' processes intersect and hybridise profusely (Gane, 2004), this is not what primarily concerns us here. Rather, our focus is on the changing significance of *empirical research* and the claims to jurisdiction that sociologists can make around their methodological repertoires. Our concern is that in the years between about 1950 and 1990 sociologists could claim a series of distinctive methodological tools that allowed them to claim clear points of access to social relations, in the early twenty-first century social data is now so routinely gathered and disseminated, and in such myriad ways, that the role of sociologists in generating data is now unclear. Fifty years ago, academic social scientists might be seen as occupying the apex of the – generally limited – social science research 'apparatus'. Now they occupy an increasingly marginal position in the huge research infrastructure that forms an integral feature of what Thrift (2005) characterises as *knowing capitalism*; where circuits of information proliferate and are embedded in numerous kinds of information technologies. In an era where capitalism has begun to 'consider its own practices on a continuous basis...to use its fear of uncertainty as a resource...to circulate new ideas of the world as if they were its own...to...make business out of, thinking the everyday' (Thrift, 2005: 1) what is the role of the empirical sociologist? To use the argot of currently fashionable actor network theory (Latour, 2005), is academic sociology becoming less of an 'obligatory point of passage' for vast swathes of powerful agents? And if so, how can the discipline best respond to this challenge?

## Some Intimations of a Coming Crisis

Our sense of this impending crisis has crept up upon us as we have gone about our work in recent years. For Savage an early sign was in 2004 when he attended the ESRC Research Methods festival. With colleagues Gindo Tampubolon and Alan Warde he was enrolled in a session designed to popularise social network methods. He talked about an ESRC funded research project which mapped the personal connections and ties of members of three voluntary organisations using social network analysis. The project had proved time consuming and intensive. A lot of time had been spent finding three organisations prepared to participate, a postal questionnaire had been sent to 320 members in total, with a very high response rate. Many members had been interviewed face-to-face to ask detailed questions about their social networks. Thirty life histories had been conducted. The resulting intensive study of the member's social ties was amongst the most detailed ever carried out in the UK (see Ray *et al.*, 2003; Warde *et al.*, 2005). During the Festival he talked to other participants interested in social network methods. It turned out that one enthusiast was not an academic but worked in a research unit attached to a leading telecommunications company. When asked what

data he used for his social network studies, he shyly replied that he had the entire records of every phone call made on his system over several years, amounting to several billion ties. This is data which dwarves anything that an academic social scientist could garner. Crucially, it was data that did not require a special effort to collect, but was the digital by-product of the routine operations of a large capitalist institution. It is also private data to which most academics have no access. To be sure, we can cavil about its limits. It does not tell us what the callers actually talked about. We can emphasise our superior reflexivity, theoretical sophistication, or critical edge. Fair enough – up to a point. Yet, the danger is that this response involves taking refuge in the reassurance of our own internal world, our own assumed abilities to be more ‘sophisticated’, and thereby we chose to ignore the huge swathes of ‘social data’ that now proliferate.

For Burrows the realisation of the coming crisis occurred in 2005. He was carrying out fieldwork within a range of sites where the analysis of such swathes of ‘social data’ is big business – the geodemographics industry (6, 2005). It soon became apparent within the context of interviews with the designers of such systems that they not only had routine access to myriad sources of commercial social transactional data (Evans, 2005) but that they successfully merged this with public data sources such as the Census, Electoral Rolls, the Land Registry and so on in order to produce highly sophisticated socio-spatial maps at a level of detail and granularity hitherto not possible within the academy and without having to consider many of the ethical constraints which condition the work of academic researchers. Not only that, but they were using the rhetoric of sociological discourse (‘ideal types’, ‘weltanschauung’, ‘habitus’, ‘urban factorial ecology’, ‘globalisation’ and so on) as a analytic justification for their practices (Burrows and Gane, 2006). In essence a parallel and largely unknown (to academic sociologists) world of ‘commercial sociology’ was being revealed that certainly did not seem to lack sophistication.

The crisis though does not just manifest itself at the level of data collection and analysis; there is also the question of the use and dissemination of research information. This realisation has recently struck Savage in his role as the Director of the ESRC Centre for Research on Socio-Cultural Change (CRESC). Part of the pitch for funding was that the research capacity of the ‘cultural sector’ was limited, and did not engage with the methods and ideas developed in the academy. With this in mind one of the Centre researchers, Dr Andrew Miles, co-ordinated an ‘outreach’ project examining the research needs and issues of those working in the sector (see [www.cresc.ac.uk](http://www.cresc.ac.uk)). It soon became apparent that it would be wrong to adopt a ‘deficit’ model where it is assumed that there is no research taking place in these sectors and that academics need to provide a service which is otherwise missing. Indeed, far from it. There is plenty of research taking place in the cultural sector, but it does not depend very much on academic intervention. Cultural institutions have impressive databases, mailing lists, research projects and interventions. They have a range of ‘rules of thumb’, models and practices which are informed by extensive research co-ordinated by consultants and partners as well as ‘in-

house'. For the most part, the kind of academic research carried out in the name of culture is largely irrelevant. The ideas of Bourdieu and Foucault, indeed all the glorious flourishes of the cultural turn, do not – with a few exceptions - speak to the workaday needs and interests of such institutions. Once again, in such a situation it would also be possible to be precious and condescending to those who work in the sector, and bemoan their limited awareness, their instrumentalism, and so forth. However, our main point is that from their perspective, the research they do generally meets their needs: it is productive and is 'effective' in its own terms.

### **Historical comparisons**

These anecdotes speak to the fact that in the early twenty first century, the research of academic sociologists appears somewhat peripheral to the multifarious research circuits which are implicated in the constitution of a *knowing capitalism*. This is a novel situation. Gibbons' *et al's* (1994) account of the rise of 'Mode 2' knowledge over 'Mode 1' knowledge notes the rise of transdisciplinary and applied knowledge over internally validated academic knowledge. Bauman (1988) has illuminatingly explored the decline of 'legislative' knowledge in a new culture of capitalist consumerism. Yet both these posit a world where there was at least some deference to the internal authority of academic expertise. In fact, we would argue, following Abbott's (1990) insistence of the way that professional expertise is constituted by its practical abilities to diagnose; it was the practical devices that social scientists developed that commanded interest. From the pioneering example of their role in the social survey and community studies (see Bulmer *et al.*, 1991; Osborne and Rose, 2004), academic sociologists were highly innovative in conducting applied research and persuading a range of institutions of the effectiveness of their research repertoires. We need to remind ourselves that forty or so years ago, in the absence of routine data gathering and analysis conducted by institutions themselves, academic sociologists were remarkable methodological innovators (See more generally Rose (1990) and Osborne and Rose (1999)). When *New Society* was launched in 1962 with its messianic concern to demonstrate the importance of social research, it pioneered the practice of sending out a questionnaire to all its readers, was deluged by responses, and reported the results in its pages (*New Society*, 7<sup>th</sup> March, 9<sup>th</sup> May 1963). Such user questionnaires are now so routinely implicated in our daily encounters that we often forget that it was social scientists who invented this technology just four decades ago. We are told that the results of this *New Society* questionnaire were discussed by the (Conservative) cabinet, so interesting and innovative was the idea of getting your readers to volunteer their own thoughts: cabinet ministers, including Enoch Powell, were certainly happy to contribute columns and reviews to *New Society* in its early years. When Goldthorpe and Lockwood (1963) wrote a speculative article about the implications of affluence for the working class in *Sociological Review*, they were summoned to the Department of Science and Industrial Research and given a large research grant on the spot – no peer reviewing required - to fund them to carry out a survey, which led to the most celebrated sociological study ever carried out in Britain

(Goldthorpe *et al.*, 1968/69). When Frankenberg conducted the first sustained ethnographic study on the British mainland (*Village on the Border*, 1957), his work was featured in all the national media and he became a (minor) celebrity. The anthropologist, Lupton became the first Director of the Manchester Business School because of his pioneering use of ethnographic methods to understand shop floor working relations (Lupton, 1963). And so on and so forth. In 1963, when the Sunday Times published a colour supplement, the first colour magazine to be published in the UK, *New Society* saw it as an incursion into its own jurisdiction, and reviewed it in patronising style, praising aspects of its attempt to write accessible features on social issues (*New Society*, 14<sup>th</sup> Feb 1963) – but making it clear that *New Society* was a long way ahead in promoting accessible social commentary. Which academic social scientist today, gazing at the acres of shelf space devoted to colour magazines, would feel able to speak with such an air of superiority? And *New Society*, of course, is nowhere to be seen.

It is not, therefore, that in the past, there was unthinking deference to academic authority (for other examples, see Dirks (1999) on anthropologies role in Indian colonial government or Mitchell (2002) on the role of the social sciences in constituting the Egyptian ‘economy’ during the 20<sup>th</sup> century). It was rather that that such social scientists invented and supported research ‘technologies’ which allowed access to the ‘social’ in ways which a range of interest groups found valuable. This is the social role of sociology, not in terms of its ideas or theoretical schools, or the stature of its leading spokespeople, but in terms of the importance of its empirical research technologies.

### **Some Manifestations of the Crisis**

#### Quantitative Methods

Some influential commentators (Goldthorpe, 2000; Halsey, 2004) see the sample survey as the core methodological resource of sociology, its great and enduring contribution to the scientific study of society. The sample survey, it is claimed, and so we tell our students, allows us to generalise and predict through revealing enduring regularities by the use of inferential statistics. Through inference we can be confident that questionnaires on a limited number of people have more general resonance and can form the basis of scientific sociology. Now, there is no question that the national sample survey was a remarkable innovation at its inception. Rather than relying on a decennial census of every household, hugely expensive and time consuming, social trends could be assessed on the basis of more parsimonious methods. Small scale, local, sampling began in the UK in the early twentieth century, and national sample surveys began in the 1930s (in the form of opinion polling, on which see [Osborne and Rose \(1999\)](#)). The Government Social Survey was inaugurated during the Second World War, and the post war years saw the dramatic expansion of national sample surveys, with academic social scientists playing a key role in their propagation and development. The creation of key governmental ‘technologies’, such as the official inflation rate, came to depend on



survey research (in this case, based on the Family Expenditure Survey). Such a momentum continued well into the 1980s, in large part informed by the advocacy by Sara Arber, Angela Dale and others in the 'Surrey School' for 'secondary analysis' (Arber *et al.*, 1988) and the genesis of the British Household Panel Study (BHPS) in 1991 which allowed sample surveys to be used for the analysis of longitudinal change.

However, the sample survey is not a tool that stands 'outside history'. Its glory years, we contend, are in the past. One difficulty is that in an intensely researched environment, response rates have been steadily falling, and it is proving more difficult to obtain response rates of 80 per cent or more which were once thought normal. People no longer treat it as an honour to be asked their opinion, but instead see it as a nuisance, or even an intrusion. These problems are, however, not overwhelming because survey statisticians have developed methods for estimating the attributes of 'the missing', and it still remains possible to generalise on the basis of biased samples. A second problem concerns the way that surveys rely for their sampling frame, on the empty homogeneous space defined by national boundaries. The survey emerged as a key device for imagining the nation, and in a global era of mass migration, this also marks a serious limit. Even the most ambitious comparative research relies on comparing discrete national samples (for instance [Inglehart and Welzel \(2005\)](#)). A third telling issue is the proliferation of survey research in private companies, especially in areas of market research. Such survey research now has very limited reference to academic expertise. Unlike public surveys which form the bedrock of academic statistical expertise, the fact that their data is commodified is central both to their market and their purposes. One of the most important of such surveys is the British Market Research Bureau (BMRB) survey, which has a very large sample size which parallels the largest public surveys and asks questions every month. Yet, the BMRB has hardly been used by academic sociologists (for an exception, see Savage *et al* (1992)). The data is not subject to the kind of multivariate analysis preferred by social scientists but are displayed visually through forms of cluster analysis, so making the results accessible to a wide audience in corporate marketing departments. Very few socio-demographic variables are used, and class continues to be measured through largely discredited (by sociologists) market research categories. Even though no self respecting academic sociologist would dream of using such measures, and even though the official National Statistics Socio-Economic Classification has developed a refined and elaborate means of classifying occupations, this message seems largely irrelevant to the powerful bastions of market researchers.

To be clear, our point is not to bemoan the limited knowledge or ignorance of market researchers, nor to claim that academic social scientists should copy the private sector in some kind of deferential way, but rather to point out how key agents in the research apparatus of contemporary capitalist organisations, now simply don't need the empirical expertise of quantitative social scientists as they go about their business. But this point needs to be pushed further. Most powerful institutional agents now have more effective research tools than sample surveys. As we have already noted, they can

draw on the digital data generated routinely as a by-product of their own transactions: sales data, mailing lists, subscription data, and so forth. When sample surveys became popular, from the 1940s, they met resistance from those who insisted on the need to research 'whole populations', usually in the form of intensive case studies. The idea of 'abstracting' individuals from contexts and manipulating their responses to questionnaires statistically was an idea which was not easily embraced. As late as the early 1950s, the hybrid mix of anthropologists, surrealists and sociologists who ran Mass-Observation insisted that their methods of eliciting narrative accounts of purchasing decisions was more valuable than the abstracted accounts provided by survey researchers (Hubble, 2006). Anthropologists and psychologists, strongly influenced by Lewin's (e.g. 1951) field theory thought it essential to explore the dynamics and relationships between all the parties in specified social settings, and this conception informed early social network methods. Looking back, we can see how sample survey researchers won this battle because they were able to demonstrate the effectiveness of their technology to powerful stake holders in the context of the 1950s. And, although they raised the flag of science in winning this battle, we might also note that cost effectiveness was on their side.

However, in the current situation, where data on whole populations are routinely gathered as a by-product of institutional transactions, the sample survey seems a very poor instrument. To give a simple example of the merits of routine transactional data over survey data, Amazon.com does not need to market its books by predicting on the basis of inference from sample surveys the social position of someone who buys any given book and then offering them other books to buy which they know on the basis of inference similar people also tend to buy. They have a much more powerful tool. They know *exactly* what other books are bought by people making any particular purchase, and hence they can immediately offer such books directly to other consumers when they make the same purchase. Hence the (irritating, though often tellingly useful) screens offering 'Other people who have bought x have also bought y' which confronts the Amazon customer. Similar principles are used by supermarkets through data gathered by their loyalty card schemes where they can identify for any given customer – without knowing anything very much about their personal, 'social', characteristics – what other kind of goods they might be liable to buy if they buy, for instance, organic bananas. They can hence bypass the principles of inference altogether and work directly with the real, complete, data derived from all the transactions within their system

Insofar as there is one variable within this new body of work that is viewed as essential, it is not any of the usual sociological suspects - social class, ethnicity, stage of the life course, gender, educational attainment and so on – rather, it is all of these (and others) but as manifested through residential location. From an analytic point of view this represents a radical collapsing of these very common sociological variables onto postcode locations. The exact mixture of the sociological 'elements' that become 'fused' together in each category of these socio-spatial 'compounds' is empirically determined

by the statistical purchase each gives in explaining small-scale spatial variations in consumption patterns (Burrows and Gane, 2006). It turns out that knowledge of the spatial location of someone is increasingly an important proxy for all manner of sociological information; indeed to the extent that there is no need for other social measures. Richard Webber, one of the pioneers, has concluded that

‘the type of neighbourhood in which a consumer lives is a significantly more predictive piece of information than any person or household level discriminator’ (Webber, 2004: 1).

There are some arenas in which the sample survey will continue to be a central research tool because of the limits of transactional data. One challenge is posed by those ‘outside the grid’, and sample surveys in some cases are better able to represent the missing, ‘representative’ population. The British Crime Survey, for instance, is valuable precisely because it is able to show that the ‘real data’ gathered by the police as part of their auditing process understates crime as experienced by individuals. However, given the problem of non-response to surveys, it is not clear that they necessarily will continue to have such advantages: other approaches include data capture methods where data bases are compared to see which populations are missing from one data source but appear in another so that knowledge of those who might be missing from any one database can nonetheless be garnered (Pleace and Bretherton, 2006). It is perhaps telling that even in the heartland of ‘political arithmetic’ sociology, the study of social mobility, there has been recent interest in using transactional data (in the form of marriage registers, see [Miles \(1999\)](#)), or data collected by genealogists, often using web based methods ([Prandy and Botero, 2000](#)).

Let us be clear: the sample survey continues to be an important research resource, especially with respect to longitudinal analysis. However, we need to recognise its historicity, and the way that the more recent technologies allow different, more descriptive data to be deployed in new and powerful ways. The sample survey came to enjoy a certain pre-eminence in a situation where the principles of statistical inference had been developed and the technologies for the conduct of surveys invented and data deriving from routine transactions could not be easily collected, stored and manipulated. This state of affairs existed between about 1950 and 1990, but decreasingly applies. It is unlikely, we suggest, that in the future the sample survey will be a particularly important research tool, and those sociologists who stake the expertise of their discipline to this method might want to reflect on whether this might leave them exposed to marginalisation or even redundancy.

### Qualitative Methods

Our polemic thus far may strike a ready chord amongst the majority of UK sociologists who are critical of quantitative approaches and prefer qualitative methods. However, any complacency here is very misplaced. The socio-technical changes we outlined at the

beginning of this paper also have implications for those in the profession committed to more qualitative styles of empirical research. Just like the survey, there is also a history to qualitative methods. A comparatively unusual feature of British sociology is its embrace of the 'in depth interview' as its preferred research method. Halsey (2004) shows that 80 per cent of qualitative papers published in the British sociology journals in 2000 used interviews, a proportion which has steadily increased from about 50 per cent in the early 1960s. No other national tradition of sociology gives the in-depth interview such pre-eminence. In the United States, the majority of sociologists analyse survey data, but the minority of qualitative sociologists predominantly conduct ethnographies or observational research and steer clear of (what they sometimes perceive as) 'lightweight' interview methods. Those American sociologists who use in-depth interviews tend to specialise in publications for the popular market (e.g. Bellah *et al.*, 1985). When Americans do use in-depth interviews they tend to interview many more people than is the case in the UK - often between 100 and 200 - or provide a comparative twist, for instance by conducting interviews in different nations (as with Lamont (1992) who interviews in the American Mid West, New York, and two areas of France, or Bellah *et al* (1985) who interviewed in numerous American neighbourhoods). By contrast the British use of in-depth interviews tends to be smaller scale and focused on a particular (sub-) population of interest (though there are exceptions, for instance Devine (2004) and Savage *et al* (2005)).

Why this particular, and unusual, emphasis on the in-depth interview in Britain? The interview, as most famously discussed by Foucault (1976), originated as a 'confessional' technology, with its remit expanding from the church confessional to large arenas of professional practice during the Victorian period. Interview methods thus became incorporated into the professional practice of social workers, bank managers, doctors, personnel managers (in their recruitment practices) and psychologists, and as Rose (1990) has shown, came to play an important role in the defining the jurisdiction of the 'psy' disciplines. For most of the early years of the twentieth century, social researchers would not normally interview respondents, but – insofar as they conducted empirical research at all - would rely on the opinions of influential agents, whose opinions were taken to represent those of others. However, although it was sociologists who pioneered the use of these methods in allowing popular narratives to be made 'public', the routine use of such methods in all forms of contemporary journalism, from the colour magazine to the Oprah Winfrey show marks a clear shift of expertise away from the academy. The in depth interview remains a useful device for allowing respondents to reflect on their practices, histories and identities, by suspending such practices in order to allow people to 'account for themselves' (Gilbert and Abell, 1983). Yet, whilst it might be a valuable resource for eliciting people's reflexive identities (see notably Bourdieu 1999), it is not clear it has so much value in researching the kinds of myriad mobilities, switches, transactions, and fluidities that are claimed to make up contemporary social life (Urry, 2003).

The key mechanism by which the interview method became imported into sociology was through British sociology's reaction to Parsonian functionalism from the 1950s. Taking issue with functionalist accounts of reference groups, norms, and values, British sociologists used material gleaned from interviews as a means of teasing out people's own versions of their salient social values, especially when focusing on working class or under privileged groups. This had few counterparts in other nations. The influence of Michael Young, Peter Willmott and especially Elizabeth Bott, who had links to, or worked at the Tavistock Institute in the early 1950s was crucial here. Bott's influential argument that '(w)hen an individual talks about class he [*sic*] is trying to say something, in a symbolic form, about his [*sic*] experiences of power and prestige in his [*sic*] actual membership groups and social relationships, both past and present' (Bott 1971: 163), was dependent on using the interview as a means of eliciting people's worldview as an object of sociological concern (see more generally, Savage 2006). Bott's work excited large numbers of sociologists to use in-depth interview methods as a means of *understanding respondents own conceptions of the social order*. Michael Young, Paul Willmott, Colin Bell, John Goldthorpe, David Lockwood, Ray Pahl, Brian Jackson and Dennis Marsden all came to employ such methods in their studies of community and class in the 1960s, from which point this method became a core and enduring feature of sociological methods in the UK.

The point to note, however, is that the value of such in-depth interviews, when removed from their original critique of functionalism by showing how norms and values were rooted in everyday social relations, needs justification. It was widely agreed by the early 1970s that Bott's belief that it was possible to show how people's attitudes to class were related to their social experience proved an empirical failure (Bulmer, 1975). Although there were certainly innovative ways in which in-depth interviews were deployed, it is now not very clear what the significance of the in-depth interview is in an age of *knowing capitalism*. Certainly, it still has a role in generating valuable accounts of actions that can be used either as analytic resources in their own right (Wooffit, 2005) or as inferential resources able to inform mid-range typifications of social actions. But as a tool for generating sophisticated understandings of the diverse *weltanschauung* that pertain in contemporary societies we are not so sure. Not only are the worldviews of diverse populations now routinely presented to us in the popular and new media in such a manner that their summary characterisation by sociologists is no longer as necessary (or as interesting) as once it was, but some of the social transactional research technologies discussed above are now also able to produce nuanced representations of the life worlds of quite specific populations groupings for example. Geodemographic systems such as Mosaic uses 61 detailed ideal typical qualitative '*weltanschauung*', characterised and precisely mapped in social space (Burrows and Gane, 2006) – which are derived not from extensive qualitative interview data but by data fusion techniques that draw upon sources such as statistical cluster analysis, digital photos, focus groups and so on.

## Rethinking the Repertoires of Empirical Sociology

We have argued that the repertoires of empirical sociology need to be rethought in an age of *knowing capitalism*. This call goes far beyond the now familiar demand for more methods training but asks for greater reflection on how sociologists can best relate to the proliferation of social data gathered by others, which we currently largely ignore. We do not think it is a satisfactory critical response to shrug these issues off through invoking our sophistication in relation to social theory. The kind of sociological theorising which presents synthetic accounts of social change is certainly interesting to a (relatively) wide audience and keeps sociology in the public eye. This explains the appeal of the writings of Giddens, Bauman, Sennett, and Beck, for instance. However, for all the claims to novelty, this kind of sociology is really just a revival of the kind of sociology that existed in the first half of the twentieth century under the leadership of Spencer, Geddes, Branford and Hobhouse. It is a kind of sociology which does not seek to define its expertise in terms of its empirical research skills, but in terms of its ability to provide an overview of a kind that is not intended to be 'tested' by empirical research. The problem with this kind of sociology is the way that it can become unwittingly complicit in teleological visions of social change, where the past is mobilised in pursuit of a narrative account seeking to identify the present through its relationship to a possible future. It may be that this is the best possibility sociology has to define its public role in an age when – if our arguments are correct – its empirical resources seem problematic, but if so, we should be clear that this does mark an abandonment of the vision of empirical research which was central to the expansion of sociology in the post-war years.

Another response is to focus on specialised areas where sociologists can claim to know a great deal about specific topics of interest, or where they have a specific specialised expertise on offer. In an era when journalism is retreating from detailed social investigation, this is no doubt an important venture. However, this involves specialisation of research interests and makes it difficult to retain a systematic, holistic sociology where it is essential to relate diverse aspects of the social together (see also Webster, (2005)). Running through this paper is our interest in an alternative vision, where sociology seeks to define itself through a concern with research methods (interpreted very broadly), not simply as particular techniques, but as themselves an intrinsic feature of contemporary capitalist organisation. This interest in the 'politics of method' involves sociologists renewing their interests in methodological innovation, and reporting critically on new digitalisations.

We do not have the space here to explore in detail what this might involve, but we see it as drawing on the arguments of writers such as Pickstone (2002), Latour (2005) and Abbott (2000) who argue – for different reasons – that we abandon a sole focus on causality (which we are very bad at) and analysis and embrace instead an interest in *description and classification* (see, for example, the exemplary Bowker and Star (1999)). If we see the power of contemporary social knowledge as lying in its abilities to conduct minute description, we can better situate our concerns as exposing these descriptions, challenging them, and presenting our own descriptions. In such a process we need a

radical mixture of methods coupled with renewed critical reflection. Such a call for a descriptive sociology does not involve sole reliance on narrative but seeks to link narrative, numbers, and images in ways that engage with, and critique, the kinds of routine transactional analyses that now proliferate. Rather than seeking refuge in our own, internal debates, this involves casting our net wide, critically engaging with the extensive data sources which now exist, and not least, campaigning for access to such data where they are currently private. Through this means, we can renew the critical project of sociology through challenging current practices in the collection, use and deployment of social data.

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