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Social studies of social science: A working bibliography

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Abstract

The social sciences are currently going through a reflexive phase, one marked by the appearance of a wave of studies which approach their disciplines' own methods and research practices as their empirical subject matter. Driven partly by a growing interest in knowledge production and partly by a desire to make the social sciences 'fit-for-purpose' in the digital era, these studies seek to reinvigorate debates around methods by treating them as embedded social and cultural phenomena with their own distinctive biographical trajectories – or “social lives”. Empirical studies of social scientific work and the role of methods within it, however, remain relatively scarce. There are several reasons for this but, for one thing, it can be difficult to find examples of how such studies might be undertaken. This contribution draws together a literature scattered across various social science disciplines and their sub-fields in which social science methods have been studied empirically. We hope this working bibliography will provide a useful resource for those who wish to undertake such studies in the future. We also hope to show that the more recent literature can be connected to, and stands to be informed by, a much broader literature. We do not pretend that our bibliography is complete and comprehensive but we do think it represents a starting point for those who wish to pursue these issues for themselves.

Keywords: social studies of social science; social life of methods; ethnomethodology; conversation analysis; anthropology; mixed methods; sociology of knowledge; data; analysis; methodology; qualitative methods; quantitative methods

Introduction

Although it was foreshadowed in many respects, and we could point to previous moves in this direction (e.g. LaPiere 1934, Benney & Hughes 1956¹, Sorokin 1956, Toulmin 1958, Winch 1958, Mills 1959, Hammond 1964), there have been a series of attempts to reappraise the 'problem' of method and research practice in the social sciences in recent decades, beginning in the late 1970s and gradually gaining momentum from there. Social science methods (fieldwork, interviewing, surveying, analysis, writing, etc.) are less and less being seen (or, at least, are less likely to be

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claimed to be seen) as discrete technical devices or armaments that enable those who deploy them to step outside the societies and cultures they study so as to view them objectively from afar. Instead, they are increasingly being treated as part-and-parcel of those societies and cultures, and constitutive elements of the knowledge-making practices that operate at their very centre. Taking a lead from the sociology of scientific knowledge and science and technology studies (Bloor 1976, Lynch 1993, Maynard & Schaeffer 2000), the general thread running through this body of work, put simply, is that method is what connects the social scientist into the lives of their societies and cultures², it is not what enables the social scientist to detach themselves from them and thereby escape their ‘messy’, ‘earthly’ entanglements (Law 2004, Latour 2007). Indeed, methods are increasingly being seen as productive or ‘performative’, i.e. as ‘enacting’ the very societies, cultures and systems of exchange they offer accounts of (see e.g. Briggs 2007, Mackenzie, Muniesa & Siu 2007, Majima and Moore 2009, Benzecry & Krause 2010, Savage 2013). Thus, and to take just a few examples, opinion polls are treated as producing that which they measure, i.e. public opinion, surveys, surveyable populations, interviews, interviewable subjects and economic analyses, markets: ‘public opinion’, ‘population’, ‘the subject’ and ‘the market’ are claimed not to pre-exist their deployment but are themselves ‘brought into being’ in and through their use (as in, e.g., Mackenzie, Muniesa & Siu 2007, Ruppert 2007, Law 2009).

Not only have the ‘technical’ practices of social scientists been reconfigured, in a reflexive move, as objects of inquiry, those working within this particular area of research – an area we might loosely term ‘social studies of social scientific research practices’ given their wide disciplinary cast – have also begun to actively advocate experimentation and a move away from established methodological prescriptions and ideas as a consequence of undertaking their inquiries (Fisher & Marcus 1986, Bourdieu & Wacquant 1992, Evans & Foster 2011, Mason 2011, Back & Puwar 2012, Gane 2012, Lury & Wakeford 2012). What we have seen, as a result, is an interest in the ‘social life of method’ (Law, Savage & Ruppert 2011, Savage 2013) being increasingly coupled to a concern for the ‘politics of method’³ (Clifford & Marcus 1985, Savage 2010, Adkins & Lury 2012) as well as an interest in how the social sciences have, at various points in time, set about finding ways of making themselves ‘relevant’ (Burawoy 2005, Savage & Burrows 2007, Rose, Osborne & Savage 2008, Savage 2013, Evans & Foster 2011, Gane 2012). Partly a response to a distinct *politicisation* of method in various areas of social life⁴, these reflections on method as political are also predicated on the idea that the social sciences are increasingly methodologically flatfooted (Beck 2005, Burawoy 2005, Adkins & Lury 2012, Gane 2012, Lury & Wakeford 2012), no longer able to keep pace with the speed with which contemporary social and cultural life is changing. In some cases, these changes, particularly those connected to ‘digitisation’ and ‘big data’, are seen as changes to the nature of ‘the empirical’ itself (see e.g. Adkins & Lury 2009). The social sciences, partly because they refuse to acknowledge their embedded and participant status by clinging to positivistic conceptions of method as detached, are said to be making themselves peripheral to – or “voluntarily estranging” themselves from, in Latour’s formulation (2010: 148) – that which provides *their* animus: social and cultural life.

These literatures may move in a variety of directions and from a variety of different starting points but one strand that links them is an interest in how social science has been practiced, is being practiced and will be practiced, and all that may follow from

that. As we found out when approached about the possibility of undertaking a comparative fieldwork study of reasoning work in quantitative and qualitative research in 2009⁵ (see Greiffenhagen, Mair & Sharrock 2011), however, it is difficult to locate examples of how such studies themselves might be practiced. ‘Model’ studies are difficult to find, particularly in an area of research where the contributors are scattered across many different fields and have addressed quite different concerns and audiences (Guggenheim & Krause 2012)⁶. As a consequence, beyond the work we were most familiar with, work in ethnomethodology and conversation analysis, we found ourselves reading across different fields of inquiry – primarily, but not solely, sociology, anthropology, history, archaeology, philosophy and the sociology of scientific knowledge/science and technology studies – in order to get a sense of how such studies had been approached by others. Unlike social studies of science, where debates spanned disciplinary divides from the start, social studies of social science have been only been fitfully ecumenical. While there are lines of ‘cultural’ exchange across social scientific diasporas (Barth 1967), by and large researchers have not taken up or responded to the insights that have emerged from studies located in fields seen as distant, and therefore of only tangential relevance, to their own.

In the spirit of cross-disciplinary dialogue and debate, we thought it would be useful to provide the list of references we have compiled in the course of our research as a resource for further discussion and work in this area. We make no claim to completeness or comprehensiveness – we are sure that those who consult it will spot many omissions, blind spots and oversights. It is, however, a *more* complete list of references than we have found anywhere else. We do not wish to claim sole credit for this as we have built on the work of Elisabeth Simbuerger (2009), the researchers in the Methods Lab at Goldsmiths College, London, Paul ten Have’s EMCA (ethnomethodology and conversation analysis) website and contributors to the Lang-Use mailing list, all extremely helpful when it came to identifying the studies we present. Beyond that, we have done a lot of digging, using reference lists within books, articles and reports to locate additional materials. New bibliographic tools have been useful but most important were old-fashioned spade work and actually reading what we found because searches frequently turned up materials that appeared relevant at first glance but on closer inspection were not.

Social Studies of Social Science: An Overview

What, in overview, can we take from the exercise as a whole? We can see two things at work from quite an early stage: firstly, an interest in putting the social sciences in – and attempting to establish empirical accounts of how they are shaped by and depend for their intelligibility upon but also how they shape and lend intelligibility to – social, cultural and historical contexts, an interest which traces back to, among others, Weber (1978 [1922]), Mannheim (1936) and Schutz (e.g. 1963); and we can also see, secondly, attempts to treat what social scientists do as itself a form of social and cultural practice capable of, and likely to be illuminated by, being studied like any other. Ethnomethodological and conversation analytic studies from the 1960s and 1970s, while never a unified front and with important differences in the programmes of Garfinkel, Sacks and Cicourel very much in view, were amongst the first to seek to conduct research of this kind empirically, something ethnomethodologists and conversation analysts have continued to do in the period since⁷. Then, from the 1970s on, and perhaps most prominently in anthropology where questions relating to what

anthropologists ought to study, the ways in which they ought to study it and how they ought to present the results of that work acquired a particular visibility and force (Clifford & Marcus 1986), self-scrutiny increased across social science disciplines. For instance, Bourdieu's accounts of his struggles with structuralism in *Outline of a Theory of Practice* (1977, a subject he returned to in *The Logic of Practice* (1990)) and Rabinow's *Fieldwork in Morocco* (1977) offered new ways of thinking about the work of the researcher, here the ethnographer, and their role in the production of knowledge⁸. What might be thought of as 'ethnomethodological' themes also began to surface in the anthropological literature at this point, as in Marcus' early paper 'The Ethnographic Subject as Ethnographer' (1980, compare Moerman 1969, Sharrock 1974, Sharrock & Anderson 1982). While these initial developments in anthropology – as in an earlier and less well known sociological literature (see, e.g., the list above) – were predominantly 'auto-critical', attention shifted in the 1980s to the question of ethnographic authority and, more specifically, ethnographic authorities, most famously Malinowski. While taking up the problem of writing as its leitmotif, as in the enduringly influential *Writing Culture* (Clifford & Marcus 1986, but see also, e.g., Marcus & Cushman 1982, Herzfeld 1983), this was never solely about the representation of research practices *simpliciter* but also about recasting ethnography itself as a co-production emerging from interactions between anthropologists and those they studied that took place within particular social, cultural and political contexts. Although not an entirely new theme, (see, e.g., Miner 1956), the problem identified was that the nature of ethnographic research had been obscured by the authoritative narratives produced by anthropologists. As a consequence, ways had to be found of experimenting with method – methods of fieldwork, methods of analysis, but also methods of writing (e.g. Fisher & Marcus 1986) – in order to overcome the ideological, political and indeed empirical failings of the past⁹. These are themes which have continued to exercise anthropologists, leading to, among other things, reconceptualisations of the 'field' and consequently ethnographic practice (Gupta & Ferguson 1997) along the way. The concern with developing an ethnographic orientation to fieldwork has also recently been the subject of a series of lively debates in archaeology, a field closely allied to that of anthropology, particularly in the US (see, e.g., Edgeworth 2006 and below). We would also note that a series of high profile disputes which either began or were revisited from the 1980s onwards – e.g. Redfield versus Lewis, Mead versus Freeman, Obeyesekere versus Sahlins – and which were organised around 'hostile readings' of evidence, inference and knowledge claims in specific ethnographies, have provided a different, rather more antagonistic, dimension to the reflexive turn within recent anthropological work (see, e.g., Heider 1988, Freeman 1989, Heider 1989, Rhoades 1989, Orans 1996, Borofsky 1997, Freeman, Orans & Côté 2000).

This work gelled with developments in the philosophy and sociology of the natural and social sciences. Building on the ideas of Ludwig Wittgenstein (1953), Peter Winch (1958), Thomas Kuhn (1962), Michel Foucault (e.g. 2002a[1966], 2002b [1969]), Jacques Derrida (1966), David Bloor (1976), Nelson Goodman (1978), Ian Hacking (e.g. 1968, 1975, 1983) and others, sociological researchers began to undertake empirical studies of the natural sciences in the late 1970s, e.g. Latour and Woolgar (1986[1979]). Although not specifically concerned with the social sciences, from an early stage the problem of 'symmetry' in such accounts quickly came to the fore. If 'facts' are products, what is the status of social scientific accounts of their making? Are they not constructions too and, if so, how can the social sciences be

exempted from similar forms of study?¹⁰ It is perhaps this question that the recent wave of studies on the social life of methods takes up most directly. However, 'laboratory studies' were not the only way in which the work of the social sciences was beginning to be reappraised. Although normally cited for its theoretical and substantive interest, Foucault's work (e.g. 1975, 2002a, 2002b) helped set the stage for a new body of studies of enumerative, measurement and classificatory practices, particularly within statistics. Foucault's work, along with that of Wittgenstein, has been cited as a major influence by Ian Hacking who took up the issues Foucault had started to excavate and delved deeper into the historical emergence of the sciences of probability (see e.g. 1990, 2002). Hacking's histories, in turn, link with other historical and sociological work by Donald Mackenzie (1981), Theodore Porter (1986, 1996), Alain Desrosières (e.g. 1998), Geoff Bowker and Susan Leigh Star (1999), and others (e.g. Stigler 2002, Espeland & Stevens 2008). Taken collectively, this body of work has given rise to studies that seek to tell us something about the way in which statisticians and their collaborators make things knowable – features of statistical practice statisticians themselves have addressed but from different though equally interesting angles (see, e.g., Chatfield 2002).

These issues have been taken up in many innovative ways since the 1980s, but three alternative approaches that emerged from the early work discussed above still tend to provide the point of departure for contemporary research. The 'field' as it stands might thus be seen as organised around (1) historical-genealogical studies, (2) 'auto-critical' and 'reflexive' studies (something which incorporates 'confessional' accounts of the past and present), and, finally, (3) studies of 'live' social scientific practice, mostly fieldwork, interview or survey based. Although it would be misleading to characterise these studies as narrowly fixed on one aspect of research at the expense of others, across these areas we find different areas of research practice provide the analytical focus for investigation. Broadly speaking these are: (1) studies of the production of the raw materials of research, i.e. data, and the ways in which the social world is made into an object of social scientific inquiry; (2) studies of analytic work, the ways in which data can be and are variously turned into findings and so are made to speak of and to social worlds; and (3) studies of writing, de/inscription and representational work, including such things as critical-exegetical examinations of the 'discursive' constitution of social facts, de-constructionist dissolutions of authoritative 'writing'/depiction and studies of, e.g., writing itself as a practical activity. We have then and in sum, studies of data collection, analysis and 'writing up' in all their complexities¹¹ undertaken either through a process of reflection on first-hand experience, through studies of the artifacts social scientists work with and produce, or by following researchers themselves to find out what doing research involves¹². These three areas of practice have been examined differently by those who adopt one of the different approaches to the social study of social science we identify above. There is no straightforward way of mapping between, say, fieldwork studies and the practice of data collection, or historical studies and the practice of writing, giving rise to some complex and interesting cases in what is an increasingly diverse field. We hope that diversity comes across strongly in the bibliography.

Methodology

It would be difficult to describe the long, slow accretion of materials over a four year period through the ongoing search for further examples and the pursuit of leads as an

explicit methodology. Nor do we wish to lend it a retrospective coherence by offering a reconstructed logic of what was involved (Kaplan 1968). We were not operating with formal inclusion and exclusion criteria. Rather, we were sure that we would know what we were looking for when we saw it, with relevant materials appearing in and through the process of being sought out (Garfinkel 1967, Hill and Stones-Crittenden 1968). Nonetheless, we have tried to ensure that the lists we provide below include empirical work rather than theoretical discussions and programmatic statements: what we think of as the closest cousins we could find in the social sciences of the more established body of laboratory studies and histories of natural science practices. Discerning the difference between an empirical study and a theoretical or philosophical treatise is not always easy. The dividing line between the two can be difficult to judge, as in cases where programmatic statements are tied into empirical claims about the way research actually is or has been practiced (see, e.g. Back & Puwar 2012 or Savage 2013). By and large, however, we have tried to exclude work which is straightforwardly (meta)theoretical or falls within the purview of the (institutional and material) history of ideas and disciplines rather than the historical study of research practices – a different kind of exercise¹³. Although we have tried to tread that line carefully, many of our selections, and the rationales guiding inclusion and exclusion, will undoubtedly appear idiosyncratic to some.

We are also aware that there are many possible ways in which a bibliography of this kind might be set out. We could differentiate the work we list by disciplinary background, theoretical orientation, methodological approach, methods employed, focus of inquiry, kinds of data, scope of the study, field, audience, and so on, and also by whether we choose to focus on those undertaking the study or those being studied. We have opted to organise the results of our ongoing work in the following way. We begin, firstly, by listing what we are terming social studies of social scientific practice, past and present, including work on statistical practices; secondly, we list ethnomethodological and conversation analytic work on the same topic; and, thirdly, we list anthropological contributions, including recent ethnographic studies of archaeological fieldwork – three areas we see as sufficiently different to one another to warrant separate treatment. Following this, we change tack slightly and present a list of empirical contributions to debates around a particular approach to research: ‘mixed methods’¹⁴. We are sure further, complementary lists could be provided for areas like history, geography, cartography, socio-legal studies, archival scholarship and many, many others – and we would welcome them. We are also aware that our list provides a limited guide to an increasingly innovative body of quantitative studies of social science research practices, including those that employ bibliometric techniques and novel forms of data-mining. However, these are not areas we have had the opportunity to explore ourselves and so we leave the task to others.

Finally, our lists are of course selective and betray our own leanings, preferences and practical purposes in seeking out these studies in the first place. However, we have not excluded work that takes a different approach to our own. With Paul Feyerabend (1973), we see disagreements, differences and disputes as healthy, helping to stimulate thinking and sharpen argument and analytical clarity. We have enjoyed engaging with this body of work for that very reason and we hope those who use this working bibliography will do so too.

Notes

¹ Benney and Hughes put things very well: “[T]he interview, as itself a form of social rhetoric, is not merely a tool of sociology but a part of its very subject matter. When one is learning about the interview, he [sic] is adding to sociological knowledge itself.” (1956: 137-138)

² Though rarely, we would add, in clearly delineable or expected ways, see e.g. Beck 2005.

³ An idea which connects to debates around ‘ontological politics’, see e.g. Law & Hassard 1999 but see also Anderson & Sharrock 2013 and Lynch 2013 for rejoinders.

⁴ As demonstrated by, among the many possible examples, the ongoing controversies around climate change (e.g. Rogers & Marres 1999, Edwards 2010, Rajão & Vurdubakis 2013), the aetiology of HIV/AIDS (Green 2009), techniques for counting votes in the United States (e.g. Hiltgartner, Lynch & Berkowitz 2005, Martin & Lynch 2009), randomised control trials (Goldacre 2008, 2012), the seismologists sentenced for manslaughter for failing to predict an earthquake in L’Aquila, Italy (Jones 2012), or the French psychiatrist given a suspended sentence because one of her patients committed murder after being cleared for release (BBC 2012) – all cases where questions of method and the warrant for scientific claims have become public concerns (see also Beck 2005, Latour & Weibel 2005). Disputes around social scientific analyses of the 2011 riots in the UK – when Boris Johnson complained he had “heard too much sociological explanation and not enough condemnation” – might seem to provide a more directly social scientific case in point (Cooper & Nichols 2011), but it is worth noting that, in the other cases mentioned above, it was the repercussions of acting on claims based upon the application of particular methods that generated the controversy – in this sense ‘the politicisation of method’ goes hand-in-hand with the ‘socialisation of science’, the linked claim that the sciences as a whole are inside not outside society and thus are socially, culturally, politically and economically accountable and in a variety of ways. Other controversies within social science disciplines are perhaps more illustrative. We might think here of anthropology and both the Yanomami scandal and its fall out (Borofsky 2005) as well as the ‘human terrains system’ controversy centring on the role of anthropologists in facilitating the ‘war on terror’ (see, e.g., Forte 2011) where particular ways of doing anthropological research and the potential/alleged/real harms associated with them were subject to considerable scrutiny. The election of Napoleon Chagnon, the main protagonist in the Yanomami controversy, to the US National Academy of Science, alongside its support for military research of the kind exemplified by the human terrain systems programme, were recently cited by Marshall Sahlins as his main reasons for resigning from the same body in a show of public protest (2013a, 2013b). Social scientists have found or sought to make themselves parties to public disputes of different kinds too. Legal cases where social scientists have been called upon and asked to defend their claims as experts provide a particularly interesting example (see, e.g., Ruse 1986). A number of sociologists, including Diane Vaughan, Steve Fuller and Simon Cole among others, have participated in public inquiries or trials as expert witnesses and with varying degrees of success. While Vaughan’s work on the Columbia disaster board of inquiry was lauded (Vaughan 2005, 2006), Fuller and Cole had a much harder time negotiating cross-examination and convincing judge or jury that their ‘expert’ contributions added anything meaningful to proceedings (see

e.g. Lynch & Cole 2005, Gorman 2006, Cole 2006, 2009, Lynch 2006a, Lynch 2006b, 2007, 2009b, 2009c, Fuller 2006, 2008a, 2008b, 2009 as well as, e.g., Bal & Mastboom 2007). There have also been instances where social scientists with opposing views have found themselves pitted against each other in court (Peyrot & Burns 2001). Analyses of these cases give us interesting insights into knowledge-making and knowledge-claiming practices in the social sciences and the degree to which they survive legal examination and deconstruction.

⁵ The research was part of a National Centre for Research Methods Collaborative Fund Project, and involved work with the Realities and BIASII NCRM Wave II Nodes.

⁶ In Lewis Coser's summary: "It is indeed curious, as Robert K. Merton has remarked, that while historians of science have increasingly come to use in their own work sociological conceptualizations and methodological tools, historians of sociology have largely been remiss in this respect, if one can judge from the dearth of any serious sociological study of sociologists. This suggests an extension of the well-known impression that sociologists feel most at ease studying status inferiors, be they workers or students, army privates or thieves, while they find it much more difficult to study status superiors, whether top managers or university presidents, generals or senators. But apparently sociologists find it still easier to study status superiors than to study themselves." There are several reasons for this reticence. On the difficulties of studying 'thinking work' of the kind social scientists routinely engage in, see e.g. Garforth 2012. On the 'village politics' that social scientists studying other social scientists can become embroiled in, see Williams 2002.

⁷ The interest in social science methods as practiced is clear in Sacks (1963), Cicourel (1964) and Garfinkel (1967, Hill & Stones-Crittenden 1968, Garfinkel & Sacks 1970) on the manner in which social scientists make their phenomena of interest available for study (see also Turner 1974). From these early works, ethnomethodology has retained an ongoing commitment to explications of vernacular forms of practical sociological reasoning in the sites and settings – ordinary and specialised – those forms of reasoning are 'indigenous' to, including those in which professional sociologists do their work. Ethnomethodology is often thought of as being sceptical about the claims of social science to adequately capture the social world but as Benson and Hughes put it, it "is not that [ethnomethodology claims social science] ... fails to make 'adequate' reference to the world, rather what becomes of interest is the manner in which it does so" (Benson & Hughes 1991: 125). This involves examining, in detail, the "myriad of practical decisions, judgements and interpretations ... [which] have to be made to get the [often unpromising] material to speak" (Benson & Hughes 1991: 122), part of the enduring rationale guiding social studies of the social sciences.

⁸ For a particularly useful interrogation of the reflexive turn in anthropology, see Macbeth 2001.

⁹ Interestingly, we see here, but at a much earlier stage than in the recent sociological literature, how an interest in treating research as a practice engaged in by particular people in particular places at particular times in particular ways is connected to attempts to break with those practices and find new ways of doing research.

¹⁰ Latour and Woolgar, for instance, included a picture of the roof of the laboratory Latour had undertaken his fieldwork within as part of their account (1986: 93), asking their readers whether they had questioned its inclusion or conceded its propriety. This attempt to problematise the accounts being offered from within as they are being produced has itself become a familiar technique (e.g. Gilbert & Mulkay 1984, Ashmore 1989).

¹¹ This characterisation opens up a variety of related aspects of what researchers do for study. For instance, attending to how researchers learn to engage in and organise research as a practical activity, and the numerous unremarked skills and competencies required of a researcher as part of that, is particularly instructive, enabling us to see aspects of research work often taken-for-granted (see here Goodwin 1994, 2006). The adoption and use of different devices and tools by social science researchers, as well as how they subsequently incorporate them within their routine research practices, provides a useful focus in this regard (see, e.g., Lofgren 1990, 2013, Lury & Wakeford 2012, Back & Puwar 2013, Krajowski 2013, Savage 2013). Alternatively, it is also possible to examine particular moments in the career of a research project and examine what they might have to tell us about social scientific work. Review processes, including ethical review, have a particular interest in this regard, not least because they are one place where social scientists must describe and explain the practicalities of their work to (not always sympathetic) others, modelling their research in ways that enable its logic to be seen and evaluated (see, e.g., Camic, Gross & Lamont 2011, Stark 2011, Nind, Wiles, Bengry-Howell & Crow 2012, Coffey, Robison & Prosser 2012 but also Hacking 1983).

¹² It is worth stressing that texts are not the only things that social scientists produce and make use of in the course of their work, but take their place alongside numbers/statistics, visual artifacts (photographs, videos, tables, diagrams, graphs, maps, pictures and so on), audio recordings of many different kinds (as in the ethnographic, compositional and documentary work of Rupert Cox for instance, see e.g. Cox 2008, 2010) but also such things as models and, increasingly, code, databases, algorithms, software programmes and online or virtual 'spaces'. They may even include contributions to the built environment as well as experimental sites and settings (see e.g. Gieryn 2002, 2006 and Bates 2010). Social studies of social science research practices have extended to considerations of all manner of productions and thus are far from being text-centric in their concerns.

¹³ Thinking about, e.g., sociology sociologically can involve different kinds of things: it can be treated as an occasion for theorising, for methodological reflection, for engaging in sociological analysis or as furnishing opportunities for empirical inquiry. Again, while far from discrete or always easily distinguishable activities, we have tried to focus our bibliography on studies of sociological practices. This is not because we question the value of other ways of approaching the question of the status of the social sciences but because we have had to make our task more manageable. We have, therefore, left out the considerable literature on the 'sociology of sociology', 'metasociology' and critical sociologies of knowledge, including work that takes up the contributions of the likes of Furfey (1965), Habermas (1967), Abrams (1968), Friedrichs (1970), Gouldner (1970), Coser (1971), Giddens (1971) and many others. For recent work in this area, see, e.g., Wagner & Wittrock 1991, Heilbron, Magnusson & Wittrock 1998, Calhoun 2007 and UNESCO 2010. Equivalent

literatures will be found across the social sciences but the studies we are primarily interested in are not ‘metatheoretical’ exercises (Ritzer 2006) although they may well have (meta)theoretical implications.

¹⁴ When set alongside the others, ‘mixed methods’ may appear slightly out of place. However, it is an interestingly ‘reflexive’ area – with a number of those who describe themselves as mixed methods researchers undertaking studies of how researchers practically mix methodologies in the course of doing their work. In that sense, it provides a good sense of how contemporary researchers have approached the task of conducting studies of methods, methodology and research practices. The question as to why questions of method, methodology and research practice have been such a concern in mixed methods is itself of interest. As those who have engaged with the literature will know, a great deal has been written on the subject of mixed methods but it, somewhat paradoxically, remains a rather nebulous pursuit. Mixed methods is a field of research in the process of organising itself, of finding a suitable narrative, and, by that, trying to demonstrate its coherence and stake out its own place in the landscape of the social sciences. This has involved providing back-stories that, among other things, show off its particular values, its coherent theoretical, epistemological and ontological underpinnings (i.e. the idea that mixed method operations are justifiable philosophically), its venerability, etc., and give those new to the field some idea of its scope, its boundaries and its prospects. Such activity has also been about providing rules, recipes and how to guides – something which makes the literature seem overly preoccupied with what might be thought of as the ‘proper’ way of doing mixed method research. Despite these ongoing debates over its character, mixed methods research is increasingly being spoken of as a definable methodological ‘kind’ and those involved in research that falls within that kind are trying to move beyond a programmatic phase. As a research culture very much in the making, practitioners are faced with certain problems less prominent in more established areas, one of which is knowing what those who call themselves mixed methods researchers actually do. Tashakkori and Teddlie (2010: 271), in an account of their rationale in putting together the *Handbook of Mixed Methods Research*, say the following: “We initiated the Handbook in the late 1990s ... with some trepidation and uncertainty about its salience ... We asked ourselves, “Doesn’t everyone do this?” ...” Establishing what ‘everyone’ does has proven far from straightforward, however, so those in mixed methods are in the process of pinning down what mixed methods researchers, at least, do – a considerable task in and of itself.

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