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From methodology to methodography?

Martyn Hammersley

Abstract

This article examines the character of a small but detailed observational study that focused on two teams of researchers, one engaged in qualitative sociological research, the other developing statistical models. The study was presented as investigating 'the social life of methods', an approach seen by some as displacing conventional research methodology. The study drew on ethnomethodology, and was offered as a direct parallel with ethnographic and ethnomethodological investigations of natural scientists' work by Science and Technology Studies scholars. In the articles deriving from this study, the authors show how even the statisticians relied on background qualitative knowledge about the social phenomena to which their data related. The articles also document routine practices employed by each set of researchers, some 'troubles' they encountered and how they dealt with these. Another theme addressed is whether the distinction between quantitative and qualitative approaches accurately characterised differences between these researchers at the level of practical reasoning. While this research is presented as descriptive in orientation, concerned simply with documenting social science practices, it operates against a background of at least implicit critique. I examine its character and the closely associated criticism of social research methodology and conventional social science.

Keywords

Science and technology studies, ethnomethodology, 'social life of methods', methodology

In the last 20 years, a reflexive turn has been announced in social science, involving the application of analytic approaches from the field of Science and Technology Studies to social science research itself:

> 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'. (Mair et al., 2013: 1)

The small body of work I examine in this article locates itself within this tradition, as involving 'the application of sociology to sociological work' (Greiffenhagen et al., 2011: abstract). However, a slightly more accurate characterisation would be 'the application of ethnomethodology to sociological work'. As such, it offers a useful opportunity to explore the relationship between ethnomethodology and mainstream social science.[1]

Of course, reflexive attention to social science practice has a lengthy history, in various forms. 'Natural histories' of particular research projects have long been produced, generating a huge literature (see Hammersley, 2002) – albeit one that seems to be largely neglected. And, in the 1960s, Alvin Gouldner (1970), Robert Friedrichs (1970) and others sought to develop a 'sociology of sociology'. Meanwhile, in the 1980s, anthropologists and others began to pay increasing attention to the role their work played in processes of imperialism and social reproduction, especially through the ways in which they represented the people they studied in their written accounts (Clifford and Marcus, 1986). And the discursive practices employed by other types of social scientist were also explored, including in sociology (Brown, 1977 [1989]; Edmondson, 1984) and in economics (McCloskey, 1982; Samuels, 1990).[2] What is distinctive about the most recent reflexive turn is the particular analytic approaches that have been deployed, and the commitment on the part of some to

Emeritus Professor of Educational and Social Research, The Open University, Milton Keynes, UK

Corresponding author:
Martyn Hammersley, Emeritus Professor of Educational and Social Research, The Open University, WELS, Level 2 Stuart Hall Building, Milton Keynes MK7 6AA, UK.
Email: m.hammersley@open.ac.uk

empirical investigation of colleagues' research practices. Also significant is that, rather than reflexivity being deployed in order to inform social science practice, at face value at least this new approach (or set of approaches) is simply concerned with studying how social science operates in the realm of the very phenomena that it studies, its similarities to lay reasoning, and to forms of data collection and analysis that have somewhat different goals (e.g. that which serves companies' needs for marketing information, or governments' requirements for policymaking) (Savage, 2013). Furthermore, 'methods are increasingly being seen as productive or "performative," that is, as "enacting" the very societies, cultures and systems of exchange they offer accounts of [. . .]' (Mair et al., 2013: 2).

In this article, I focus on a particular study in this tradition, one that draws on ethnomethodology (reported in Greiffenhagen et al., 2011, 2015; Mair et al., 2013, 2016). These authors investigated the work of two sets of researchers: a team of sociologists deploying qualitative analysis methods, and one of statisticians developing quantitative modelling procedures for social science colleagues. The authors present their work as an ethnomethodological investigation of social science, concerned with detailed documentation of actual practice.

They describe the fieldwork involved in their study as follows:

> Over the five months of the project (November 2009–March 2010), we conducted several interviews with individual researchers from both units (17 with Realities, 10 with BIAS),[3] attended workshops and talks (2 and 3), sat in on a variety of group meetings (12 and 2), and observed analysis sessions in which researchers worked on data together (3 and 6). We also had a variety of different kinds of written accounts to work with as well (working papers, websites, official documentation and the like). While the analysis sessions provided our focus, our capacity to make sense of what was going on within them was directly informed by what we learned in these other ways. (Greiffenhagen et al., 2011: 96)

In their first article, 'From methodology to methodography', the authors begin from a common assumption within research methodology that qualitative and quantitative research 'involve incompatible ways of reasoning about the social world'. They state that their aim is to study research methods in practice so as 'to gain insight into how social scientists reasoned through their research problems and to assess the extent to which this aspect of their work could be broken down along qualitative or quantitative lines'. Presenting this work as joining 'a growing body of research into "the social life of methods" (Law et al., 2011)', they highlight studies of the interactional achievement of survey interviews (e.g. Houtkoop-Steenstra, 2000; Maynard et al., 2002; Suchman and Jordan, 1990), but also investigations of the practices employed in qualitative interviews (e.g. Greiffenhagen et al., 2011: 94; Hester and Francis, 1994; Rapley, 2001: 94).

The authors then examine 'two brief episodes taken from meetings':

> The first centres on two researchers involved in analysing and drawing conclusions from an interview transcript, the second centres on two researchers in the process of checking a statistical model built to combine temporal and spatial data at the small-area level. (Greiffenhagen et al., 2011: 96)

In the first example, the qualitative researchers are examining some of their data about families and local relationships, and comment that what one informant says about his relationship with his neighbour does not match with the usual sociological understanding of community life. The authors summarise their analysis of this episode as follows:

> what we see over the course of the four extracts is a glimpse of an *analysis-in-the-making*, the transformation of an odd comment in response to a question in an interview into one of the keystones of a sociological analysis of 'associations in place' through the alchemy of social scientific reasoning practices. (Greiffenhagen et al., 2011: 100)

The statisticians' work was concerned with creating 'generic models able to capture the degree of local variation among small geographical areas over time, establishing both the common trend across areas as well as those areas which departed from or "bucked" that trend' (Greiffenhagen et al., 2011: 100). The focus of the discussion examined here was on what was taken by the statisticians to be a potential problem in one of the models they had developed; an asymmetry between the strands of the model dealing with the common trend and local variation. They are concerned with whether this

> is simply a result of the statistical software they are using rather than technical, substantive or methodological decisions. The researchers start to think about possible ways to correct this asymmetry, but note that the most obvious ways of proceeding (i.e. introduce symmetry) would result in 'quite a substantively different model'. (Greiffenhagen et al., 2011: 102)

The authors present these data as giving access to an 'unfolding history, a glimpse of the *model-in-the-making*'. They comment,

> The end-products of statistical research are typically designed to stand-alone, to wear their logical structure on their sleeve, but here we are looking at something that is not a finished product; it is still a live issue. [. . .] Models do not build themselves any more than they interpret themselves; it is neither a predominantly mechanical nor purely deductive process. Of course, some standard techniques are involved; they are not starting from scratch. But choices still have to be made, and these are frequently based on intuitions, hunches and ideas of what is needed that have not yet been fully rationalized. The researchers are not following a pre-specified template, this is not the 'beginning of [. . .] rails invisibly laid to infinity' (Wittgenstein, 1953: §218) along which the modelling process glides without effort. (Greiffenhagen et al., 2011: 103)

The authors conclude their discussion by summarising their aim as documenting:

> 'the work that makes the methods work', [. . .] the things that members have to do – as far as they are concerned – to adequately deploy their elected method, but which are not normally articulated as part of the explication of how the method is followed.

And, speaking to the issue of the distinction between qualitative and quantitative research, with the former often described as 'interpretative' by contrast with the latter, they comment that 'we can observe that the reasoning in which the researchers are engaged in *both* cases involves "interpretation"'. The authors do not deny the existence of differences in the reasoning deployed by the two groups of researchers, but they argue that

> [. . .] the inferences the researchers made in the course of their exchanges began at different points, were leveraged in different ways and led them in very different directions because they belonged to distinct lines of inquiry into quite distinct problems each with their own local and disciplinary histories. Embedded in and addressed to distinctive 'problem situations', it was their differences that provided the researchers' purchase on their particular problems and gave their research its character as research of a particular kind. It was these diffuse differences, and not a generic separation down qualitative and quantitative lines, that mattered to doing the work. (Greiffenhagen et al., 2011: 104)

Finally, the authors insist that

> There is no intention to match the practices of either group against anyone's ideals of method so as to attribute methodological failings (or successes) to them. Acknowledging that these were specific moments in ongoing projects, what we have attempted to bring out is that the described practices are constitutive aspects of producing sound research for all practical social scientific purposes (cf. Anderson et al., 1985: 136). (Greiffenhagen et al., 2011: 104)

I will outline the other two substantive articles produced by these authors much more briefly, since they cover some of the same ground. In 'Methodological troubles as problems and phenomena', Greiffenhagen et al. (2015) focus on how the two sets of researchers dealt with 'troubles' that they encountered in the course of their analytic work, how these arose locally on particular occasions in this work, and how resolving them was part of 'the practical accomplishment of method' (p. 462). In 'Statistical Practice: Putting Society on Display' (Mair et al., 2016), the authors show how the statisticians relied on background knowledge about the social phenomena to which their data related, and sometimes engaged in sporadic investigation to check this, as well as orienting to whether their models would be found useful or how they would be used by social science colleagues.

Discussion

As a preliminary to examining this research, it is worth noting that ethnomethodologists have often displayed an ambivalent attitude towards conventional forms of social scientific work (see Hammersley, 2018: ch3). On one hand, they have sought to distinguish their approach as a radical alternative to it; and as part of this, they have often questioned its scientific credentials, primarily on the grounds that it trades on common sense assumptions rather than relying solely on technical methods and analytically grounded inferences, in the way that it has sometimes claimed to do. Moreover, ethnomethodological work has often been put forward as a way of studying the social world that avoids this problem (see Zimmerman and Pollner, 1970). For example, Button et al. (2015: 13) write that

> The social sciences feed off and, curiously, at the same time seek to rival common-sense, thereby producing a *disjuncture* between the social world as known and understood by social scientists and the social world as it is known and understood by society's members. Ethnomethodology, by contrast, recognises that common-sense knowledge of social doings is the very *bedrock* of social life and makes it a topic of study in its own right.

These authors describe the social sciences as producing 'abstract and general descriptions that *hover above* social life as it is ordinarily encountered by the very people engaged in society's day-to-day business' (p6). And they echo Garfinkel's suggestion that 'the social sciences are "talking sciences" essentially occupied with the business of "shoving words around"' (p144).'

On the other hand, Garfinkel (2002: ch2) often insisted that ethnomethodology is 'incommensurable with, and asymmetrically alternate to' conventional social science, rather than competitive with it. Indeed, he suggested that describing 'professional and lay sociological practices' is the main task of ethnomethodological investigation, as illustrated by his analysis of jurors' deliberations and of coding practices carried out for research purposes (Garfinkel, 1967).

Greiffenhagen et al. explicitly locate their work in this second enterprise, and they abjure any criticism of the researchers they studied, even though they point to the ways in which those researchers drew on common sense understandings and practices. At the same time, they do hint at criticism of social research methodology. And while, in places, they could be interpreted as proposing methodography as a *supplement* to methodology, the title of their first article, and some of their discussion there and elsewhere, suggests that they are proposing that methodography should *displace* methodology. The authors report that research on 'the social lives of methods':

> reflects dissatisfaction with programmatic doctrinal statements of the aims of the social sciences wedded to meta-reflection, critique and inter- and intra-disciplinary jostling and one-upmanship. Rather than using idealized conceptions of social science as decontextualized standards to judge what social scientists do, the

> focus has been on understanding the scale, range and diversity of the social sciences' practical entanglements in social and cultural life, showing that the social sciences do not merely record, but are productive, helping to bring into being and stabilize the very phenomena they depict [. . .]. (Greiffenhagen et al., 2015: 461)

At one point, without any clarification, the authors refer to 'the mythological conception of the methodology of the social sciences' (Greiffenhagen et al., 2015: 480).

A first task in examining the intended status of their work must therefore be to identify the differences between methodography and methodology. These appear to include the following:

1. Where methodology relies, at best, on researchers' informal impressions of how they do their work, methodography involves detailed documentation of exactly what is done.
2. Methodography is not *self*-documentation but documentation of others' practices.[4]
3. The descriptions produced take into account aspects of practice that would be overlooked as unimportant by methodologists.
4. Methodography's concern is simply with *describing* social science practices, not with evaluating them or prescribing what researchers ought to do.

On the basis of this, it would seem that there are at least a couple of reasons for the authors' questioning of conventional methodology. First, that most of it is not based on careful investigation of what researchers do, and therefore often has a remote or distorted relationship to their actual practice. This parallels criticism, from Science and Technology Studies (see, for instance, Law and Lodge, 1984), of philosophers' assumptions about the work of natural scientists.[5] The second argument is a deeper one, and more directly related to ethnomethodology. This is that the problem with methodology is that it makes generalisations about the practice of research, rather than taking account of its contextually constituted character. Furthermore, methodology formulates rules that are to be followed, thereby assuming that research can be reduced to formal procedures that govern behaviour, rather than recognising that rules are invoked in situationally specific ways in the course of practice – in other words, that their character is performative not representational. I will examine each of these arguments in turn.

The authors set up a contrast between a standard view of social science, characteristic of methodology, and that which is embedded in their own work and other investigations of 'the social life of methods'. They write that

> Social science methods (fieldwork, interviewing, surveying, analysis, writing, etc.) are less and less being seen (or, at least, are less likely to be 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.

Later, they label the view they are opposing as 'positivistic conceptions of method' (Mair et al., 2013: 1–2).

We can start by noting that the characterisation of social research methodology deployed here – as assuming that researchers can 'step outside the societies and cultures they study', and as requiring that they view societies 'objectively from afar' – is a significant misrepresentation. This account may have been true in the 1950s but it is certainly not true today. A great deal of the current methodological literature deals with qualitative approaches that are premised on the idea that researchers are part of what they study, and these approaches frequently reject the principle of objectivity, or fundamentally reinterpret it (see Hammersley, 2011: ch4). Nor does this literature generally assume that methods are 'discrete technical devices'. So, the social research methodology that is being rejected here is a caricature.[6]

We should also note that when Science and Technology Studies (STS) scholars criticised philosophy of science, much of the force of their criticism hinged on the fact that the philosophers were not, for the most part, themselves engaged in doing scientific work. But the situation is quite different with social research methodology; the literature dealing with this has been produced almost entirely by practising social scientists, and is often based on their experience in carrying out research. So, this objection to methodology presumably relies primarily on distrust of social researchers' own informal knowledge of what they do. While I believe there can be grounds for this, at face value it stands somewhat at odds with ethnomethodology's valorising of members' practical understanding.

I will now turn to the second argument against methodology: that it makes general, and prescriptive, claims – rather than recognising the situated, reflexive character of all accounts, including those of social researchers. The argument here is that, rather than researchers following some template of good practice, they necessarily constitute what would be good practice in and through their actions at each point of the research process. The authors report that

> Garfinkel is not asserting the impossibility of social science, but rather pointing to an alternative conception of what it consists in, one in which 'social science practice' is not determined by, or coextensive with, 'rules of good procedure'. That is not to say 'rules of good procedure', i.e. methods, have no relevance in social science, but, rather, that they should not be treated as decontextualized descriptions of research practice – where one substitutes for the other. Instead, as we have tried to show, their relation to research practice is contingent, they are invoked and brought into play locally, 'for all practical purposes', by researchers in the course of what they do and as part of resolving the troubles they encounter along the way. (Greiffenhagen et al., 2015: 479)

While Garfinkel may not have asserted the impossibility of social science, it should be noted that the authors cite a book in support of their argument, co-authored by one of them, whose title is *There is No Such Thing as a Social Science* (Hutchinson et al., 2008).[7] Aside from this, it would seem to follow from what they write that methodological statements which are not directly involved in the activity of some ongoing research investigation must be viewed as superfluous, at best. Moreover, any methodological writings that *are* judged to be directly involved in the knowledge-production process cannot be treated as providing general and/or prescriptive accounts but rather must be seen as glosses, produced on particular occasions for particular purposes; they are to be viewed in terms of their functioning, not as sources of information or guidance.

I will not assess this argument in full here (see Hammersley, 2018, 2019a), but focus solely on the fact that it leads to a performative contradiction. Methodography, as exemplified in the work of Greiffenhagen et al., claims to provide an account which accurately represents features of the work of the social scientists studied, ones which exist independently of that account and are quite general in character: that there is no sharp, fundamental difference in the reasoning of qualitative researchers and statisticians, and that what differences there are arise from the particular investigations in which these two sets of researchers are involved; that research practice does not amount simply to following rules; and so on. Yet, on their own argument, these conclusions must themselves be treated as superfluous, in the same manner as much methodology. They cannot even be viewed as glosses since they were certainly not produced as an integral part of the knowledge-production process in which the researchers studied were engaged; the authors were external observers who adopted a quite different perspective on that process from those directly involved in it.

This performative contradiction arises from applying the ethnomethodological argument that social processes reflexively construct themselves as what they are to ethnomethodological work itself. Yet, if this is not done, we have a version of what Woolgar and Pawluch (1985) referred to as ontological gerrymandering. While those authors take this to be an existential inevitability to which the only genuine response is continually to acknowledge it, this is not the response of most ethnomethodologists; nor is it the stance adopted by Greiffenhagen et al. But, as a result, it is not clear what the ground on which their investigation could stand is.

The problem here stems from the very core of ethnomethodology – its insistence that language use is not a matter of conveying information about pre-existing phenomena, but rather is performative in character: language is used to *do* things rather than simply to make statements about them. However, it is necessary to point out that offering descriptions, for various purposes, is one of the things that language can be used to do; indeed, it is what Greiffenhagen et al claim to be doing. A second source of the problem is that ethnomethodology proposes that the meaning of all utterances is treated as indexical: it is not a product of some set of rules operating on the basis of a dictionary of standard meanings; instead, ethnomethodologists treat meanings as constituted entirely in and through processes of social interaction on particular occasions. This challenges any notion of language use as representation, in two respects. First, the referring process itself is not stable – there are no rules of translation whereby the meaning of a statement made at one time and place can be related to what the same statement (in lexical and grammatical terms) uttered on another occasion means. The relationship between the two meanings of the statement must be assumed to be undecidable, though it can of course always be formulated, in one way or another. The second challenge to representation is that there appears to be an ontology operating here whereby the existence of phenomena can be no more than their being constituted as meaningful objects on particular occasions during the course of social interaction (see Zimmerman and Pollner, 1970). In other words, there is nothing 'outside' what we might call the social process of meaning constitution, just as for Derrida (1976) there could be 'no outside to the text' (pp. 158–159).[8]

We should note that this implies a fundamental difference in orientation between the authors and the researchers they studied. There are other likely differences in assumption too. For example, Greiffenhagen et al. (2015) declare that a key feature of ethnomethodology is 'a steadfast refusal to privilege sociological perspectives on the social world' (p. 463). They report that

> in treating 'sociological reasoning' as a ubiquitous feature of everyday life, Garfinkel was not seeking to elevate sociology to the status of a universal science but to undermine attempts to draw a demarcation line between sociological analyses and ordinary forms of practical reasoning by showing their 'vulgar' grounding. (Lynch, 2000; Sharrock, 2001)

By contrast, the researchers that the authors studied cannot avoid assuming that, through engaging in intensive investigation based on specialised methods, they are capable of producing answers to questions about the social world that are more likely to be true than those of lay people. If they were not to hold this assumption they could not apply for funding to do their work, or present their findings under the heading of social science, at least not with any integrity.[9]

What is involved here is not simply a disagreement between advocates of methodography and methodology, then, but one between ethnomethodologists and mainstream social scientists. Furthermore, it is not a superficial disagreement, but approximates to what Lyotard (1984: 65–66) calls paralogy, a fundamental incommensurability (see Hammersley, 2019b). At best, conventional social research is viewed by ethnomethodologists as a form of practice that is self-constituting, not only in the sense of determining what counts as 'good practice' but also co-producing the very phenomena it documents. By contrast, it is clear even from the small amount of data that the authors cite that the researchers they studied believed that they were aiming at accurate representations of phenomena that existed independently of their work.

There is a second problem with Greiffenhagen et al.'s work, which, at face value, is of a more mundane kind. This concerns its value: whether it produces worthwhile knowledge. This is a question that all research must face; though, of course, there is considerable scope for disagreement about the value of any findings, and indeed about how worth is to be judged. Nevertheless, this is not simply an arbitrary matter: the fact that there may not be a consensus, or that there is no means of *calculating* value in a conclusive way, does not mean that any judgement is as good as any other. Judgements must be assessed on the basis of the grounds available to support them, with efforts made to traverse differences in view about the relative solidity of grounds. So, what follows below is my assessment of this research, and I indicate why I have come to the conclusions I have.

I suspect that, if mainstream social scientists read these articles, most of them would conclude that they do not offer much news. The response might be similar to audience reaction to a presentation given by the statisticians the authors studied:

> The audience had been unwilling to engage with what made the research innovative in methodological terms because they were unable to identify any particularly visible substantive reward in doing so – they could not see what was in it for them. (Mair et al., 2016: 68)

As this quotation illustrates, a response tells us as much about the responder as it does about that to which they are responding. Nevertheless, I think there are some reasons for such a response in the case of the work I have been examining here. For instance, the fact that the qualitative-quantitative distinction is a crude one that does not capture the details of actual practice is widely recognised, even in the methodological literature that the authors cite.[10] And the claim that quantitative work relies on qualitative knowing is also far from new (see Campbell, 1978). Similarly, the fact that one cannot do good research by following a set of methodological rules has long been acknowledged (see Bell and Newby, 1977).

But perhaps Greiffenhagen et al.'s work should be judged in terms of *ethnomethodological* news values, rather than those of mainstream social science?[11] I am on even more uncertain ground here, but there seems to be a contrast between what these authors provide and what is characteristic, say, of work in conversation analysis. The latter provides cumulative knowledge of interactional devices that people employ, and of how these function to deal with the various problems that can arise in social interaction. It is not clear to me that these articles offer much of this kind. Instead, like a great deal of other ethnomethodological work (Hammersley, 2018: 81–84), the authors seem mainly to reiterate and illustrate pregiven assumptions of ethnomethodology.

For these reasons, my response to this body of work, albeit a very small-scale project, is one of disappointment. Careful, external investigation of the practices of social scientists and statisticians is a very promising venture. But, in my view, what would be required for this to be worthwhile is the description of those practices within the framework of a set of key methodological concerns – focusing, for instance, on how research questions are selected and formulated; how sources of data and methods are chosen, and on the basis of what considerations; what counts as evidence, how it is produced, what is treated as sufficient evidence; how the risk of speculative or biased interpretations is guarded against; how any gap between the assumptions built into models and what happens in the contexts to which those models are intended to apply is to be reduced; and so on. As I have noted, the authors *do* address the relationship between qualitative and quantitative approaches, but their discussion of this does not go much beyond what is already in the literature. Similarly, while they point to statisticians' reliance upon qualitative knowing they do not consider *in detail* just how even these statisticians acquired and deployed this type of knowledge.[12]

What I am recommending here does not undercut the distinction between methodography and methodology. The sorts of investigation I have just outlined could be solely concerned with what social scientists actually do, feeding very usefully into separate methodological discussion about the relative advantages and disadvantages of particular methods, correcting and supplementing methodologists' assumptions about social science practice. However, what I have proposed here *is* almost certainly at odds with an ethnomethodological orientation, at least in its 'radical' forms (Lynch, 2016).

Conclusion

I have examined three articles reporting an ethnomethodological investigation of social scientific and statistical practice. In doing so, I raised some questions about this approach and its relationship to conventional social research and methodology. While the authors present their work as simply describing the practices of the researchers they studied, they seem to promote this 'methodography' as displacing methodology. I argued that, in important respects, they caricature the literature in that field, and that ethnomethodology does not provide a sound basis for methodography. This is because of an inherent performative contradiction: while insisting on the self-constituting character of any practice, including social science, the authors do not apply this to their own account of social science practices. Instead, this is offered as accurately representing general features that such practices possess independently of their investigation.

I also raised questions about the 'news-value' of these articles: I suggested that they do not tell us much new about social science practice. And, in my judgment, even how much news they offer in ethnomethodological terms is an open question. Work in conversation analysis has produced a great deal of new knowledge about how talk-in-action is organised, but here the authors do not identify specific analytical devices that social scientists use. For the most part, their findings reiterate a common ethnomethodological theme: that rules cannot *govern* behaviour, so that *work* is involved in applying analytic methods, routine problems needing to be resolved. It seems to me that developing the field of investigation that these articles open up requires a rather different approach from that adopted in this body of work, one which abandons the radical claims of ethnomethodology and treats methodography as a *supplement* to methodology.

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ORCID iD

Martyn Hammersley https://orcid.org/0000-0001-6842-6276

Notes

1. I have examined this more generally elsewhere, see Hammersley (2018: ch3).
2. For further discussion, see Mair et al. (2013). See also Hammersley (1993a, 1993b).
3. ‘Realities’ and ‘BIAS’ were the names of the two research teams.
4. It is perhaps worth noting that ethnomethodology does not rule out self-documentation: see, for example, Robillard (1999) and Anderson and Sharrock (2018).
5. Interestingly, this echoes a philosopher’s criticism of the philosophy of science: Toulmin (1953). I am grateful to Michael Mair for reminding me of this.
6. This is true with much of the rest of the literature on ‘the social life of methods’.
7. For a response to this, see Hammersley (2017).
8. Ethnomethodologists may deny this ontological commitment, though I believe that they are on weak ground to do so (Hammersley, 2018, 2019a). Another option would be a methodological interpretation; here, the idea that the social world is ongoingly constituted in and through processes of social interaction would be treated as a working hypothesis adopted to discover whether it opens up an interesting field of research possibilities. However, without ontological or epistemological assumptions, or conclusions, the point of the investigation would be unclear and the status of any phenomena ‘discovered’ uncertain.
9. Presumably, the attitude of ethnomethodology is that social science practices are available for descriptive study in the same manner as those of astrologers, water diviners, propagandists, and so on. Social scientists themselves cannot, of course, be so indifferent to the character and status of their work. And ethnomethodologists cannot legitimately ignore the question of the justification for their own investigations. In fact, it seems to me that the newsworthiness of the key ethnomethodological themes – that social scientists rely on common sense knowledge and practices, that rules do not apply themselves, and so on – is entirely parasitic upon the existence of a conception of rationality which assumes that procedures *can* supplant practical judgement. While, as I pointed out earlier, much social science today does not rely on this conception of rationality, a great deal of policymaking within governments and large organisations arguably does. There is little sign, however, that ethnomethodology can serve as an effective corrective to this. Furthermore, ethnomethodologists’ contrast between this conception of rationality and ‘ad hocing’ involves a false dichotomy, it tends to reinforce the fallacy that, if rules cannot be followed rigidly, what is good or bad judgement can only be a matter of what is given these labels.
10. The authors themselves refer to the mixed methods literature, where this view is widespread, but it is much more generally recognised.
11. Although it is worth noting that two out of the three journals in which the authors’ articles were published were not directed specifically at this field but at mainstream social scientists.
12. In my view, Mair et al. (2016) glorify this element of the statisticians’ work by applying the adjectives ‘ethnographic’, ‘anthropological’ and ‘Geertzian’, thereby obscuring and neglecting the differences between how the statisticians and qualitative researchers go about ‘qualitative knowing’.

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Author biography

Martyn Hammersley is Emeritus Professor of Educational and Social Research at The Open University, UK. He has carried out research in the sociology of education and the sociology of the media, but much of his work has been concerned with methodological issues surrounding social enquiry.