

Ethnomethodological criticism of ethnography

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Abstract

Ethnomethodologists have made some fundamental criticisms of conventional forms of ethnography. For example, it has been argued that they fail to examine the processes through which the phenomena studied have been constituted, and that they lack rigour because they rely upon unexplicated common-sense knowledge. In my view, these criticisms have not been given sufficient attention. This article outlines them in detail and then goes on to provide an evaluation. It is concluded that they do not provide a sufficient basis for the radical-re-specification of the focus of inquiry that ethnomethodologists propose. However, they do raise issues to which ethnographers should give more attention.

Keywords

construction of social phenomena, ethnomethodology, ethnography, reflexivity, rigour in qualitative research

Despite the fact that some ethnomethodological work has taken the form of ethnography (for instance, Sudnow 1967, Zimmerman 1970a, 1970b; Bittner 1967a, 1967b; Anderson et al., 1989; Travers, 1997), to a large extent ethnomethodology has developed separately from conventional ethnographic studies in anthropology and sociology. Indeed, it has sometimes been highly critical of them. We can see this, early on, in Wieder's (1974) account of 'the convict code', which amounted, in effect, to a critique of previous research on that topic – much of which had been ethnographic. Criticisms also arose in debates between an ethnomethodological approach to scientific work and 'laboratory ethnographies' (see Lynch, 1992). Perhaps the sharpest disagreement has occurred in the field concerned with studying human-computer interaction, where conventional ethnographic work was described as actually 'harmful' (Crabtree et al., 2011). But there are other examples of criticism too: one of the main sources I will use here is Anderson and Lee's (1982)

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'Taking Professor Gluckman seriously', in which the authors discuss their own sociological projects in light of the tradition of anthropological participant observation research represented by Max Gluckman.¹ Much like Wieder, they argue that they found themselves unable to carry out their projects in the conventional ethnographic manner because of fundamental problems, ones that they believe apply to all work of this kind.

These criticisms have rarely been addressed by conventional ethnographers, at least in any sustained fashion. Indeed, it has been much more common for them to take over ethnomethodological arguments, or interpretations of these, for their own purposes; without addressing the major differences that exist between the two approaches. An evaluation of these criticisms is my task in this article, but first I need to spell them out.

The critique

There are several closely interrelated criticisms. The first is that, despite its claim to 'adopt the native point of view', or to take an 'appreciative stance', conventional ethnography typically formulates what is going on in the settings studied in the terms of anthropological or sociological theory, rather than in those deployed by participants. And it is suggested that this opens up a gap between the ethnographic account and indigenous ones. Indeed, it is argued that, not only is the perspective adopted often at odds with participant understandings, but the accounts produced by ethnographers frequently *explain away* those understandings, implicitly or explicitly.

Anderson and Lee (1982: 298–299) argue that this involves claiming unwarranted authority for an academic perspective: 'The researcher's version, constructed as it is from an unanalysed borrowing of descriptive apparatus from members of the field, potentially stands in opposition to some members' versions of "how it is", and yet can offer no good reason for its claimed superiority'. And Bittner (1973) provides an interesting variant of this argument. He suggests that the ethnographer's marginal status, arising from the fact that he or she seeks to be both insider and outsider, tends to undercut a proper understanding of how participants experience their world. For the ethnographer it is one of many possible cultural worlds, but for the participants, often, it is 'simply the way the world is'. Thus, 'the field worker [. . .] forever confronts "someone's social reality"'. And even when recognising that 'to them this reality is incontrovertibly real [...] in just the way that "they" perceive it', the ethnographer 'knows that to some "others" it may seem altogether different, and that, in fact, the most impressive features of "the" social world is its colourful plurality'. Therefore, ethnographic accounts underplay the features of 'depth, stability and necessity that people recognise as actually inherent in the circumstances of their existence'. As a result, Bittner claims, cultures are rendered in intellectualistic terms as 'constructions that partake more of the character of theoretical formulation than of realistic description' (Bittner, 1973: 123). Pollner and Emerson (2001:127) suggest that this problem arose, in a particularly illuminating way, in ethnographic studies of scientific knowledge, with some of these tending to treat science as fabrication, portraying the knowledge it produces as 'constructed' and as on a par with other possible 'constructions', which was sharply at odds with the assumptions of the scientists themselves.

A second criticism is that most conventional ethnographic work neglects to examine the fundamental *nature* of the phenomena being studied. Thus, Garfinkel et al. (1988;

cited in Pollner and Emerson, 2001) criticised 'analytic ethnography' for importing sociological concerns rather than examining how the phenomenon being studied – in their case, once again, the practice of natural scientists – is itself ongoingly constituted. Another formulation of this criticism is that there is a failure to attend to what are the most distinctive features of the activity being investigated; so, for instance, Becker (1963) has been criticised for studying jazz musicians without giving much analytic attention to how they go about playing the music (Tolmie et al., 2013: 228; but see Faulkner and Becker, 2009).

The complaint here is that what is neglected is not simply a topic that could have been given attention but, instead, one that is essential: the very nature of what is being investigated – how these phenomena are constituted as the things that they are. There is a strong parallel here with the arguments of Husserl and other phenomenologists regarding the failure of philosophers to examine how phenomena initially appear to us, and the constitutive processes involved in generating our ordinary, largely taken-for-granted, experience of the world. It is claimed that, through studying these processes, the essential features of the types of phenomena we experience can be identified. Thus, Husserl (1970) argues that failure to carry out such foundational investigations has led to a tendency to misunderstand the character of scientific knowledge, neglecting that this is necessarily based upon ordinary experience: as a result, such knowledge is falsely viewed as capable of replacing experiential understanding, on the grounds that it captures underlying reality as against mere appearance. Ethnomethodologists' arguments are very much along the same lines, even though they differ from phenomenologists in their conception of how ordinary experience is generated, treating it as the intersubjective product of practical social interaction in the world, rather than of subjective psychological processes occurring within individuals.

A third criticism, in many ways the core one, is that conventional ethnographic work lacks appropriate rigour: that it is speculative in character. Thus, Anderson and Lee report that in the course of their own inquiries, 'we encountered methodological problems that we were unable to resolve whilst retaining the original object of our endeavours. Whilst we could have proceeded to fulfil our object, we would not have been able to do so in a way that satisfied our desire for rigour; we would not in our view have been able to claim warrant for our observations' (Anderson and Lee, 1982: 286). And they suggest that this failure to achieve rigour is true of conventional ethnographic work generally.

The fundamental problem identified here is that conventional ethnography relies upon common-sense resources in an unexplicated way. For example, as Pollner and Emerson (2001) note, it treats identity categories – such as those relating to gender or ethnicity – as natural facts, whereas ethnomethodology has proposed that they are *accomplishments*; as exemplified by Garfinkel's (1967: Chapter 5) analysis of Agnes's work in constituting herself as female, or Wieder and Pratt's (1990) discussion of American Indian identity. Thus, Anderson and Lee (1982: 290–291) argue that participant observers are faced with people who are continually constituting one another as people of particular kinds (notably as performers of various social roles); and that, in their descriptions, ethnographers themselves necessarily do the same, often drawing on the categories used by informants for this purpose. Moreover, much the same is true as regards the attribution of particular features (cultures, types of knowledge, motivation)

to particular categories of actor, groups, or individuals (Moerman, 1968; Sharrock, 1974; Jayyusi, 1984; Sharrock and Watson, 1984).

Anderson and Lee claim that 'this unanalysed involvement of the participants in furnishing the results of the researcher's enquiries, because unexamined and perhaps unexaminable, renders the would-be scientist's analytic description into the status of polemic or scientifically unwarranted argument' (1982: 289). There seem to be several grounds for this charge. First, it is suggested that, contrary to their claim to have developed scientific concepts, ethnographers' categorisations are no different in character from those of members; indeed, as already noted, ethnographers often simply take over the latter. Thus, it is claimed, the epistemic status of their accounts cannot be superior.

A second problem is that the categorisations of both members and ethnographers are, necessarily, pragmatic and situationally variable; in other words, 'they shift and change according to contingent circumstances and occasioned relevances'. As a result, they are 'equivocal when viewed in terms of the scientific and rigorous standards imposed by the practical requirements of sociologists insisting on the objective nature of the task involved in defining the field' (Anderson and Lee, 1982: 291). Any object can be described in multiple true ways, and participants must select from among these on the basis of what is appropriate for the occasion, given the activity in which they are engaged. Yet, social science, in purporting to produce *general* descriptions and explanations that apply *across* situations, cannot rely upon situationally variable categories; these do not meet its requirements.

There may be a further problem identified here too. Anderson and Lee (1982: 291) remark that:

The choice that both of us faced when encountering the problem was whether to accept [...] the arbitrary nature of our own [categorising] practices, or to make [categorising] practices ([participants'] and ours) the topic of our enquiry in order to see what such practices achieved, and to see what place such practices had in the very achievement of the order that we had chosen to study.

There is a hint here that since ethnographers, like other social scientists, are not primarily concerned with participating in the social world, but rather with studying it, their selection of relevant descriptions cannot but be arbitrary, in a way that those of participants are not. This is because, rather than reflecting the constraints that arise out of practical engagement, ethnographers' selection of categories for analytic purposes derives from a sociological or anthropological agenda that does not arise from such engagement. In these terms, conventional ethnographic accounts are necessarily distortions.

In summary, one of the main ethnomethodological criticisms of conventional ethnography is that it is insufficiently rigorous: ethnographic accounts are speculative or 'arbitrary', in that they are at odds with the participant understandings that constitute the phenomena they are investigating; indeed, ethnographers impose external interpretations on these. At one level, the complaint is that ethnographic accounts are little different in epistemic status from the accounts put forward by ordinary people, while being falsely presented as superior through a spurious appeal to scientific authority. This appeal is spurious because these accounts rely upon common-sense categories that are situationally

variable, whereas science requires categories that apply across situations. At another level, ethnographic accounts involve distortion through the imposition of artificial sociological concerns and concepts. Button et al. (2015: 133) summarise this second argument as follows: '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 label conventional ethnographic work as 'interpretive or scenic ethnography' (2015: 13), describing it as providing abstract accounts that 'hover above' processes of social interaction, by contrast with the concrete descriptions of constitutive practices that ethnomethodology supplies.

As this makes clear, ethnomethodologists argue that rigour can only be achieved by a re-specification of the focus of inquiry. They claim that the problems facing conventional ethnography stem from a fundamental feature of human social life that it ignores: that participants necessarily engage in artful practices which *constitute* the very phenomena that ethnographers treat as given. In the words of Anderson and Lee (1982: 290): 'how members in social settings (including [the] participant observer) invoke their common-sense methods in order to describe those settings is crucial in determining the nature of the objects and items that make up the settings'. Moreover, the fact that, in this process, participants display what they are doing in the course of doing it provides the opportunity for rigorous investigation that ethnomethodology exploits. This means that how the phenomena being investigated are ongoingly constituted in-and-through members' practices can be studied without relying upon unexplicated common-sense resources, or on situationally variable descriptions and explanations. In short, it is argued, a shift in focus to explicating the use of common-sense methods by participants allows social phenomena to be studied scientifically.

A fourth criticism, again closely related, is that, like many other social scientists, ethnographers appeal to values, norms, perspectives, interests and so on, as causal factors in explaining people's actions. Yet, in doing so, they fail to analyse the complexities involved in 'adhering to a value', 'following a norm', 'acting in accordance with a perspective', or 'serving an interest'. Instead, they treat the relationship between these factors and the actions they are taken to explain as an external or mechanical one, equivalent to the triggering of a gun; whereas, in fact, how people invoke such rules is necessarily situationally variable, it depends upon what the occasion demands. As a result, such rules cannot form part of a scientific explanation because the latter requires determinate, universal, not situationally variable, relationships. Once again, though, ethnomethodology points to a solution to this problem: if the task is explication of the methods by which social phenomena are constituted, what is required is *description* not explanation; and so causal analysis is not involved, and this problem is avoided.

Finally, drawing on the work of Stoddart (1986), Pollner and Emerson (2001: 129–30) note the implications of ethnomethodology for ethnographic texts. They highlight the ways in which, while ethnographers' intellectual authority depends upon their having first-hand contact with a setting, the accounts they produce tend to omit their own activities. As a result, these accounts are presented as if they amounted to unmediated

revelation of the character of the settings investigated: the textual (and other) work of the ethnographer goes unmentioned, indeed is systematically hidden.

Evaluating the ethnomethodological critique

In this evaluation, I am not *primarily* concerned with whether the portrayal of conventional ethnography on which ethnomethodological critics rely is accurate, though I will touch on this. Rather, my interest is in the cogency of the grounds on which their criticisms rely. I will address the criticisms more or less in the order in which I presented them, though as I noted they are interrelated in various respects.

The first criticism concerns the nature of the questions that conventional ethnographers address, and the concepts that they employ. It is claimed that these are usually imported from sociology or anthropology, rather than being those of the people being studied. One aspect of this criticism could be formulated as the objection that, despite its claim to adopt an ‘appreciative’ stance, in practice ethnography does not live up to this because, very often, it describes and explains people’s actions in conceptual terms that are at odds with their own understandings.

This criticism is weak, I suggest. First, the questions sociologists address are often ones that also preoccupy lay people, including the people being studied. It is true that sometimes these issues may be framed in different ways from those prevailing in the setting concerned, and that occasionally ethnographers address matters that are of little or no interest to most lay people. But there is no sharp break between sociological issues and lay concerns, and this is especially true of ethnographic studies. Much the same is also true of the concepts that ethnographers employ – very often these are refinements of ones that are available in the society they are studying or in other societies – indeed, ethnomethodologists criticise them for this, as we have seen.

Secondly, we should note that the argument that ethnography must seek to grasp ‘the native point of view’ has not usually been taken to entail that it must not offer accounts that differ from those of participants; indeed, there are good reasons for resisting this conclusion. Much of the rationale for ethnography, and for social science generally, lies in its ability to explore perspectives that are different from those of participants, as well as to assess the likely validity of various descriptions and explanations to a greater degree than participants usually do, or can do. This is possible because of the distinctive leeway and resources that ethnographers have for collecting and evaluating evidence, and in producing descriptions and explanations. At the same time, what is involved here is not *wholly* different from the work of journalists, social workers, or detectives, who also engage in sustained forms of inquiry for purposes that may be at odds with the activities of the people they seek information from and/or about. Of course, it can be questioned whether the work of social scientists is of value, by comparison with that of these other occupations, or in more general terms, but ethnomethodologists have not, to my knowledge, provided an explicit assessment of this; which would need to be compared with a similar assessment of the kind of work that they recommend and practice.

Furthermore, while some ethnography, for instance that labelled ‘critical’, may put forward its findings as correcting or even replacing participant understandings, many ethnographers are more circumspect in their claims regarding the superiority of the

knowledge they produce. They often recognise the different purposes of social science as against those built into the practices in which lay people engage, and that perspectives are differentially appropriate according to the purposes involved. Specifically in relation to Bittner's version of this argument, we must note that ethnographers *do* frequently seek to convey just how the world is experienced by participants, in relevant respects, as *part* of their accounts. And while it is true that they are inclined to present this as by no means the only way in which the same phenomena could be experienced, this does not necessarily imply an intellectualist understanding of culture (for instance they are able to recognise differences in sensibility, and in fact much cultural analysis takes this form).

It is also important to note that, in some respects, this first criticism also applies to ethnomethodology itself. After all, the latter focuses on matters that are largely taken-for-granted by participants, and which they generally believe there is no need to examine. Furthermore, the ethnomethodological account of how the phenomena of everyday experience are constituted is at least potentially at odds with how people think of those phenomena from within the natural attitude. So, the relationship between the perspective adopted by ethnomethodology and those of lay people is open to many of the same questions as that between conventional ethnography and lay perspectives, perhaps even more so. This is a point that is effectively acknowledged by Garfinkel: he argues that lay people would necessarily have no interest in, and no means of accessing, the findings of ethnomethodological studies, at least insofar as lay orientations remain within the natural attitude (Garfinkel, 1967: 7–9).

The second criticism advances a more fundamental methodological complaint: that conventional ethnography, like social science generally, does not give attention to how social phenomena are constituted as what they are. If we follow up the argument about studying jazz musicians, it is true that Becker (1963) largely takes for granted how particular forms of music are constituted as 'jazz', indeed how particular sounds are taken to be 'music', and even what is involved in producing this music. Ethnomethodologists argue that this is a problem because it means that the very nature of what is being studied has not been determined.² Here, ethnomethodologists are sometimes interpreted as proposing that foundational work on such issues is a necessary preliminary to more conventional investigations, and this is true of some (see, for instance, Cicourel, 1964). However, generally speaking, they argue that the situationally variable ways in which social phenomena are constituted means that no such foundation could be provided. In these terms, ethnomethodology represents a replacement for conventional social science, not a foundational supplement to it.

This second line of argument relies on the idea that each type of phenomenon has an essential character, lying in the constitutive work that produces it. Something like this is also to be found in some versions of conventional ethnography and other kinds of qualitative research: here the assumption is that the goal of inquiry can be to capture the phenomena studied exhaustively, whether patterns of activity in a setting or participant experience (Hammersley, 1992). However, there are good reasons to believe that all inquiry is necessarily partial, in the sense that each study addresses specific research questions; and where different questions are addressed even in relation to the same phenomenon somewhat different accounts will result.³ Given this partiality, no form of inquiry can involve the reproduction or unmediated 'display' of participant experience or practices.

However, the ethnomethodological version of this argument is unique, since it seems to rely upon a distinctive ontology, according to which social phenomena – indeed, strictly speaking, all phenomena that we experience under the natural attitude – are constituted solely in-and-through processes of practical social interaction. So, for instance, whether or not suicides have occurred, in particular cases, is determined entirely by the decisions of coroners (see Sharrock, 2001; Hammersley, 2018a). The implication is that it makes no sense to question this decision by asking whether the person *really* committed suicide, precisely because it is a socio-culturally constituted phenomenon. Of course, family, friends, or others may dispute a coroner's decision, but in doing so they have no alternative but to rely upon similar resources to the coroner: was there a suicide note; is the manner of death compatible with suicide; did the person have any reason to commit suicide, here relying upon common-sense ideas about what these would be; was the person depressed prior to the death occurring; etc? What happened is determined by these processes of sense-making: facts are socially constituted, they do not exist independently of the processes through which this occurs.⁴

It should be clear that this ontology raises deep philosophical issues. Moreover, a stance that is so dramatically at odds with our ordinary understanding of the world surely requires strong justification, and this has not been provided.⁵ Instead it is usually put forward by ethnomethodologists as a 'study policy' (see for instance Sharrock and Anderson, 1986). But, if this is what it is, there needs to be clarity about why this policy rather than others (for instance that of conventional ethnography) ought to be adopted, and about the status of any findings: are these themselves *constituted* by that policy?

The third criticism I outlined is also methodological, but this time concerns analytic rigour: it is argued that because ethnographic accounts necessarily rely upon members' methods and common-sense knowledge, and because they employ categories that are situationally variable, they cannot meet the requirements of science. In part, this seems to be an internal critique, given that, at least until the last third of the twentieth century, ethnographers usually insisted on the scientific character of their work. However, there is also a contrast being made here with the more rigorous approach that ethnomethodology is held to represent. In addition, as I noted, it sometimes seems to be suggested that sociological and anthropological categories are 'theoretical', in the sense that they do not derive from practical engagement with social activity, and are therefore artificial, producing 'arbitrary' or simply speculative accounts.

As regards the argument that ethnography's reliance upon common-sense resources means that, by definition, it cannot be rigorous, we should note that this adopts a conception of rigour taken over from some US sociology and 'behavioural science' of the 1940s and 1950s; and, beyond that, from positivist philosophy of science. We might reasonably ask, not only how relevant this conception is to the work of ethnographers (since, generally speaking, they do not share it), but also whether there are alternative methodological conceptions that set the requirements of rigour differently, and in so doing render ethnography, and perhaps conventional social science more generally, justifiable? Since the 1950s, conflicting accounts of the nature of science have been developed by philosophers (see Couvalis, 1997), several of which could serve this purpose – in that they do not require scientists to avoid all reliance upon common-sense resources, only to suspend those whose validity or effectiveness is open to reasonable

doubt.⁶ On the basis of this, it can be argued that there is no *essential* problem with relying upon inherited common-sense assumptions, or using mundane practices of understanding. This is because a *corrective process* is involved: errors can be discovered; and social science develops and systematises this process of error-correction for its own purposes. While this capacity to check our understandings cannot guarantee the validity of the knowledge produced, applying it means that the conclusions reached are more likely to be true than otherwise. And this fallibilist position can, in principle, be applied to all kinds of scientific knowledge.⁷

Furthermore, we can turn the argument round to ask whether ethnomethodology itself can avoid reliance upon unexplicated common-sense resources. Of course, it does not appeal to the positivist conception of rigour in justifying its own approach, but it *does* often insist on a form of literal description (analogous to that of Husserlian phenomenology) that purportedly avoids reliance upon these resources, in the sense that its aim is to 'display' or 'exhibit' sense-making practices. However, it seems clear that ethnomethodology cannot live up to this conception of rigour: some ethnomethodologists and conversation analysts have acknowledged that reliance upon common sense cannot be entirely avoided in ethnomethodological work (Turner, 1970: 177; Pollner, 1987: 148; Lynch, 1993). This is because in seeking to explicate *some* practices it necessarily relies upon *others*. Furthermore, there is no reason to assume that the set of practices is finite, so that there would ever come a point of full self-explication. Of course, if the sort of fallibilist conception of science I hinted at earlier were adopted, then this problem would be removed; but ethnomethodology's own susceptibility to this problem undercuts its critique of conventional ethnography, and fallibilism potentially reinstates the latter's scientific status.

The second strand of argument about rigour claims that ethnography, and social science generally, cannot but rely upon the same sorts of context-dependent and situationally variable assignment of categories that are characteristic of participant understanding; and that this is at odds with what is required if the kind of general, theoretical conclusions characteristic of science are to be produced. Here, care is required in determining exactly what terms like 'context-dependent' and 'situationally variable' mean; and why, if ethnographers necessarily employ descriptions with these characteristics, this undercuts their claim to produce superior knowledge (of particular kinds) to that of participants. We might also ask: is this a distinctive problem with *social* categories or does it apply to all categories? Does natural science escape this problem, and if so how? If it does not, does this mean that it can claim no superiority for the knowledge it produces over our everyday understandings of the physical world? Finally, we should also note that people in everyday life make generalisations across situations, so are we to regard these as automatically spurious?

There are at least two senses that can be given to the term 'context-dependent', though ethnomethodological discussions sometimes seem to conflate them. The first refers to the fact that what sense is made of a situation, how it is described and explained, depends upon purposes: these carry relevancies, both as regards what sorts of things are to be noticed and within what frame they are to be understood. Clearly, practical understanding of a phenomenon is 'situationally variable' in this sense because if we approached the 'same phenomenon' with different purposes, for example in playing a different social

role, the sense we would make of it, and how we would describe and explain features of it, could be very different. This kind of context-dependence clearly applies to all forms of inquiry, including both natural science and lay forms of social investigation: here context is defined by governing purposes and the perspectives associated with these.

The second notion of context-dependence refers to the way in which how one feature of a situation is understood will be shaped by how others are understood, by participants and analysts alike. For instance, use of the word 'nigger' is likely to be interpreted differently depending on *who* is using it, *who* is being addressed, and *what type of utterance* is involved. Its use by Dick Gregory as the title for his autobiography, or among black youth, is likely to be understood very differently from its use by whites in disparaging black people; though, of course, the two are not unrelated (see Kennedy, 2002). Similarly, a violent act may be interpreted differently depending upon what other acts preceded it, for instance according to whether it was unprovoked or made in self-defence. Conversation Analysis has of course emphasised the significance of sequential patterns in conversation, which also draws on this sense of 'context'.

Both these kinds of context-dependence operate, though the relationship between them is complex. However, it is significant that Garfinkel treats the relationships involved within a context as 'reflexive', by which he means that none of them can serve as an anchor: each feature is made sense of against a background that can itself only be understood on the basis of the sense given to its features, with equal mutual dependence holding throughout. In other words, it is claimed that this reflexivity involves a hermeneutic circle (Heritage, 1984: 83; Watson, 1997: 55). However, this would only be true if we insisted that in order to serve as an anchor, thereby tending to determine the meaning of other features, an indicator would need to be apodictic in epistemic status.

The effect of this requirement is to flatten out the differential plausibility of most if not all indicators we are likely to encounter. Yet in everyday life we neither insist on apodictic indicators nor treat all indications as equally reliable. Take the classical example of whether what we see in the corner of a darkened room is a coil of rope or a snake. None of the signs we could use to reach a conclusion is absolutely reliable: even if we poked the 'rope/snake' with a stick and this prompted no reaction, we would still be cautious in how we approached the object (is it a snake in 'hibernation' or asleep, is it a seriously ill snake that could still be dangerous?). Nevertheless, we would treat evidence of movement in response to our provocation as a very strong indication that it is a snake not a rope (though not absolutely conclusive evidence – perhaps even if it rears up and puts out its tongue we might ask whether we are hallucinating?). As I suggested earlier, there is a process of self-correction involved: we have a spiral not a circle, and one that is grounded at least to some extent in our experience of reality. Moreover, I suggest that this is very much in line with Husserl's analyses of the process of perception, at least in his initial development of phenomenology.

So, I suggest that ethnomethodologists have not provided a convincing case that contexts are reciprocally constituted in and through processes of social interaction in a circular fashion – with nothing behind, or beyond, this. Such an ontology is at odds with everyday experience, and while it would be foolish to insist that all experience is reliable, it is surely very questionable to adopt a point of view that requires us to abandon most of

the assumptions on which we routinely rely in making sense of the world. Indeed, it is doubtful whether any attempt to do this could be sustained.

The final strand of argument about rigour, which may not be shared by all ethnomethodologists, claims that the categories employed by sociologists and anthropologists are artificial because they are not generated in the course of practical engagement with the social world but rather through contemplation of it, where the aim is solely to describe and explain its features. A first point to be made about this is that sociological and anthropological work does not take place outside of the social world, any more than does that of natural scientists, a point that ethnomethodologists emphasise when they note how it relies upon common-sense assumptions. And this is particularly true of ethnographers: they participate in one way or another in the settings they investigate. What *is* true is that the analytic categories they develop do not relate directly to this practical engagement – they are not designed to facilitate it, even though they come out of it. Instead, ethnographers aim at producing sociological descriptions and explanations. Yet this, too, is an activity that takes place *within* the social world, not in some otherworldly realm, and surely therefore has its own form of practical engagement.

We should also note that many of the ideas that lay people put forward about the world in which they live are not directly implicated in practical engagement in the manner that is privileged by ethnomethodology – in other words, they do not play a *constitutive* role in relation to activities in which those people are engaged. What ethnomethodologists may be drawing on here is a distinction that Schutz (1945) made between the pragmatic character of the world of daily life (in which ‘acts of working’ ‘gear into the outer world’), and the disengaged character of scientific cognition. But I suggest that this distinction is false, that what we have are multiple social activities all of which involve social engagement, but of different kinds and serving different purposes (Hammersley, 2018a). Along these lines we can reasonably ask: why is the activity of social scientists any more problematic than the ways in which the investigations of journalists and police detectives are not isomorphic with the activities of the people with whom they deal?

This third strand of ethnomethodological criticism regarding rigour certainly points to significant questions about the nature of sociology and anthropology as disciplines, and their relationship to the knowledge that lay people produce and employ in the course of their activities, and the views they express about the world. But it relies upon the questionable distinction, just outlined, for which ethnomethodologists offer little evidence, and which is actually at odds with their insistence elsewhere on a stance of ‘indifference’ towards conventional social science. This is one of several fundamental antinomies that seem to be intrinsic to ethnomethodology (see the conclusion in Hammersley, 2018b).

The fourth area of criticism by ethnomethodologists I identified concerns whether such factors as rules, perspectives, or interests can be treated as playing a causal role in relation to human action. The response of many ethnographers to this issue would be to deny that they are engaged in causal analysis. However, while they may not be seeking to discover universal laws, they certainly make claims about causal relationships, in other words about what produces, influences, or shapes what (Hammersley, 2014: Chapter 1). At the same time, in my judgment the argument of ethnomethodologists here is excessive. In denying that rules can act as causes they seem to rely upon what might be called the billiard ball model of causation, where one object has a direct impact on

another, with determinate and measurable effect, and where background conditions can be assumed to be generally fixed. But why should we restrict the meaning of 'cause' in this way? It is clearly an inadequate model, not only in the context of social science but even in much natural science. What is required, I suggest, is clarification of what social causality involves, and how it can be investigated. I suspect that this would greatly weaken the ethnomethodological challenge to this aspect of ethnographic work.⁸

The final criticism, concerned with how ethnographers construct their texts, also points to an important set of issues. However, they are ones that have been given considerable attention by ethnographers since the 1980s, when Stoddart wrote his piece (see Atkinson, 1990; Hammersley, 1993); even if they have not been resolved satisfactorily. At the same time, it is important to raise questions about a key assumption that appears to underpin this ethnomethodological criticism: that, for ethnographers to be justified in appealing to first-hand contact as a ground for their intellectual authority, this contact must have been unmediated. On the contrary, ethnographers can recognise the role they themselves play in generating their accounts without treating what they produce as storied fictions. Furthermore, there is no reason why these texts must be presented in such a way as to make explicit all of the devices, cognitive and literary, employed in generating them. As in other kinds of inquiry, these only need to be highlighted to the extent that they involve a significant risk of misrepresentation. In other words, there is no reason to adhere to a notion of full transparency, whereby an account must not only present its content but also explicate fully how that content was acquired and has been constituted. This is an ideal that is impossible to realise, and it is unnecessary. It is also one that ethnomethodology itself cannot realise, it too involves the construction of texts using cultural resources. Nor, for reasons I have just explained, should we assume that a greater degree of 'transparency' is always desirable. Of course, here, by contrast with some advocates of 'reflexivity' amongst qualitative researchers, ethnomethodologists are not calling for an explication of the biography, presuppositions and commitments of the researcher, her or his social positioning in relation to neo-colonialism or patriarchy, etc. Rather what they are proposing is that in displaying the practices that constitute the social world in and through the activity of 'members', they are simultaneously explicating the methods that they have used to display those practices: that the two sets of methods are identical. But this argument relies upon the questionable ontology I outlined earlier.

Conclusion

In this article I have identified a series of criticisms that ethnomethodologists have made against conventional forms of ethnography. Examining these, I suggested that they rely upon assumptions that require interrogation, especially since the demands they set up often cannot be met by ethnomethodological research either. This is true, for instance, of the conception of rigour they adopt, which is conceived as requiring no reliance upon unexplicated common-sense resources.

At the same time, these criticisms highlight features of ethnographic practice that involve genuine problems, ones that are by no means entirely resolved. One of these is the question of how what is produced by ethnography is different from the accounts generated by participants. In my view the answer is that ethnographers are freer to

explore alternative ways of conceptualising the phenomena being investigated, because they are not constrained by the requirements of other kinds of practical action. And I regard this as a potentially (though not *necessarily*) positive feature. Certainly, I think ethnomethodologists are wrong to treat it as entirely negative. Moreover, ethnographers have more time and resources for developing and testing the validity of their accounts than is usually the case for participants. They are thus able to adopt a sustained focus on a specific set of factual questions, to assess the likely truth of potential answers (rather than being concerned with their usefulness for various purposes, their political or ethical implications), to search for relevant data, to formulate explanations explicitly and in a systematic way (for example seeking internal consistency among the elements), and to check hypotheses against plausible competitors. Closely related is that they have the discretion to recognise that a question is currently unanswerable. As a result, in principle at least, their conclusions should regularly reach a higher threshold of likely validity than most lay accounts. However, this is only possible for certain types of question, and it seems to me that many of the questions that ethnographers, and other social scientists, actually address are not open to rigorous investigation even in this sense. Nor is how they go about their work always sufficiently rigorous (Hammersley, 2014). In these terms, the ethnomethodological critique carries weight.

At the same time, Pollner and Emerson (2001: 130) have noted the ‘double-binding’ character of ethnomethodological criticisms of ethnography. On the one hand, it is accused of taking over participant understandings rather than gaining adequate distance so as to be able to document those understandings. On the other hand, there is the claim that it does not get close enough to participants’ activities, since the ethnographer remains an observer in the sense of employing ‘external’ categories of understanding; rather than acquiring the knowledge and skills involved in the activities through learning to become a competent practitioner, and limiting the analytic task to explicating those activities. Moreover, ‘extreme resolutions’ of this dilemma are likely to result in either explosion or implosion: either the ethnographer becomes embroiled in a potentially endless process of self-explication, or, alternatively, ‘goes native’, to the extent of not being able to produce an account different from those of participants. There is a genuine dilemma identified here. But it needs to be dealt with pragmatically, not by seeking to occupy the radical high ground. Ethnomethodologists’ criticisms of ethnography are all aimed at justifying the re-specification of social science they believe to be necessary. But, in my view, they have not established their own position securely enough for the proposal of such a radical remedy to be warranted.

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Notes

1. Their article appeared in a volume celebrating his work.
2. Garfinkel (1986: vii) suggests that neglect of the constitutive work producing social phenomena is not a ‘fault’ of conventional social science because it is a ‘systematically produced feature of ordinary society’, and such a science necessarily belongs to this society. But there

is a fundamental ambiguity within ethnomethodological writings between a critical and an 'indifferent' attitude towards conventional social science.

3. This cannot, of course, validate contradictory accounts.
4. Here, Garfinkel (2006 [1948]: 102–107) seems to have taken to heart a key argument put forward by Schutz (1945: 535): that meaning is constituted entirely retrospectively. Schutz writes: 'Meaning [. . .] is not a quality inherent to certain experiences emerging within our stream of consciousness, but the result of an interpretation of a past experience looked at from the present. Now with a reflective attitude. As long as I live in my acts, directed toward the objects of these acts, the acts do not have any meaning. They become meaningful if I grasp them as well-circumscribed experiences of the past and, therefore, in retrospection. Only experiences which can be recollected beyond their actuality and which can be questioned about their constitution are, therefore, subjectively meaningful'. This appears to imply that even if, *per impossible*, the person who committed suicide could give an account of what had happened this too would necessarily rely upon cultural resources regarding what counts as suicide, as against murder, etc; and it would constitute the death as belonging to one or other category, rather than simply reporting 'what actually happened'.
5. See Searle (1995, 2010) for a very different approach to dealing with these philosophical issues.
6. Indeed, this position has a long history, see, for example, Rescher (1978). See also Haack (2007).
7. Of course, the process of error-correction in social science is by no means unproblematic, in principle or in practice, and not just in the context of ethnographic work but more generally. But my point is that the scope for this process is present, however weakly it is actually deployed.
8. For an attempt to address this issue, see Hammersley (2014: Chapters 1 and 2).

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