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Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

### Empfohlene Zitierung / Suggested Citation:

Hammersley, M. (2019). Understanding a dispute about ethnomethodology: Watson and Sharrock's response to Atkinson's "Critical Review". *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, 20(1), 1-20.  
<https://doi.org/10.17169/fqs-20.1.3048>

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## Understanding a Dispute About Ethnomethodology: Watson and Sharrock's Response to Atkinson's "Critical Review"

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**Key words:**  
ethnomethodology;  
ethnography;  
perspectivism;  
incommensurability

**Abstract:** Since its emergence, ethnomethodology has been subject to a succession of disputes, prompted both by external commentaries and by internal divisions. Often, the external commentaries have been rejected as displaying gross misconceptions about the character of ethnomethodology, and these misconceptions have frequently been all too evident. In this article I examine a less well-known case where the external commentary—a "critical review" by Paul ATKINSON—displayed considerable understanding of, and indeed appreciation for, ethnomethodological work; albeit alongside some criticism, and an argument for the fruitfulness of combining elements of ethnography and ethnomethodology. It also connected with some disputes internal to ethnomethodology. The response to this review was, nevertheless, sharp rejection. This was on the grounds that ethnomethodology is fundamentally different from the "constructive analysis" characteristic of conventional ethnography, and qualitative research more generally. The arguments on each side make this a particularly illuminating dispute.

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## 1. Introduction

It has been a feature of the history of ethnomethodology that its adherents have often emphasised its radical distinctiveness from other kinds of social science. A key aspect of this has been a tendency to underline its differences even from those sociological approaches to which it has frequently been regarded as close—by both external critics and more sympathetic commentators—such as symbolic interactionism and interpretive ethnography. [1]

This radicalism has been evidenced in a number of disputes over the years (for instance RAWLS, 1985; ZIMMERMAN & WIEDER, 1970). However, the dispute I will be examining here is less well-known than others because, while one side of it was a published article (ATKINSON, 1988), the other (WATSON & SHARROCK, 1991) remained unpublished: it was a conference paper that was only circulated informally and that is not easily available. Furthermore, whereas other disputes of

this kind have tended to be marred by fundamental misunderstandings of ethnomethodology on the part of outside commentators, what is at issue here is more profound. ATKINSON's primary aim was as much to promote a broader interest in ethnomethodology on the part of qualitative researchers as to engage in critique; even though he does make some criticisms. [2]

ATKINSON argues that some elements of ethnomethodology/conversation analysis (EM/CA) can be, and to some degree have been, integrated with interactionist ethnography, but he recognises that no "grand synthesis" (1985, p.117) is possible. In response, WATSON and SHARROCK (1991) present EM/CA as a separate, self-sufficient enterprise, and one that is superior in key respects to conventional sociological perspectives; which they describe as seeking "to re-invent the (square) wheel" (p.1)—in other words, they view them as stuck in a futile rut. [3]

While this dispute took place a long time ago, there are important connections to discussions now taking place *within* ethnomethodology, centred on claims that some current work inspired by it, particularly in the field of conversation analysis, has moved away from GARFINKEL's original radical orientation and towards rapprochement with other approaches (BUTTON & SHARROCK, 2016; LYNCH, 2016, 2018 [2016]). In short, the dispute examined here raises fundamental, and pressing, questions about the character of EM/CA and the prospects for mutual engagement with other forms of qualitative research. [4]

I will begin by outlining the dispute, and in particular the arguments of WATSON and SHARROCK (1991), going on to assess these in order to consider what scope they allow for mutual dialogue. [5]

## 2. An Outline of the Dispute

ATKINSON's article was published in the *Annual Review of Sociology*; a series that is more or less central to what we might call mainstream Anglo-American sociology.<sup>1</sup> WATSON and SHARROCK's response to ATKINSON's review took the form of a conference paper whose title—"On the Provision of 'Ethnographic Context' in Ethnomethodological and Conversation-Analytic Research"—did not make explicit that it was a reply to a previous paper. Moreover, it was presented at an international conference on current work in EM/CA. So, this unpublished paper was not addressed to ATKINSON and mainstream sociologists directly, but rather to fellow ethnomethodologists and conversation analysts. Indeed, the authors explain that their concern is with the need to defend EM/CA from external attack, and how this can be done. ATKINSON's review is presented by them as an example of "criticisms by constructive analysts" that "are often misguided to the point of prejudice", and of the tendency of mainstream sociologists to "marginalise" EM/CA (WATSON & SHARROCK, 1991, p1). [6]

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1 There had been an earlier ethnomethodological contribution to this series by Aaron CICOUREL (1981), and there was a later one by MAYNARD and CLAYMAN (1991) on "The Diversity of Ethnomethodology".

While WATSON and SHARROCK's announced focus is on one of the central issues in ATKINSON's review—whether ethnographic context is a useful supplement to EM/CA work—their main emphasis is on the distinctiveness of ethnomethodology: its coherence and self-sufficiency. They argue that even "sympathetic" outsiders like ATKINSON have failed to recognise its character. In response, they present arguments designed to insulate EM/CA from "incorporation" into conventional sociology and anthropology. They present it, in effect, as an embattled enclave.<sup>2</sup> [7]

In line with this, early on WATSON and SHARROCK (1991, p.1) declare that: "our objective is to consider not so much why, but how ethnomethodology and conversation analysis have been so consistently marginalized and to critically examine the devices in orthodox sociologies ('constructive analysis') whereby this marginalization has been brought about". This makes clear that they will not be engaging with ATKINSON's review on its own terms but will examine it primarily from the point of view of how it can best be countered, so as to defend EM/CA. Indeed, at one point they declare that their "only interest in addressing his work" is in the features of his argument that "are typical of many constructive-analytic conceptions and critiques of ethnomethodology and conversation analysis" (p.5). [8]

WATSON and SHARROCK begin by making two preliminary points. Firstly, critics (presumably including ATKINSON) do not recognise that what is offered in many ethnomethodological studies are initial investigations, rather than fully developed, finished conclusions; and that ethnomethodologists are, themselves, by no means uncritical of one another's work. Their second point is that the critics fail to offer an alternative approach that will solve the problems to which EM/CA is addressed. The implication is that criticism of ethnomethodological work frequently amounts to attacking those who are doing their best to find a way of dealing with difficult problems that the critics themselves refuse to face, or fail to deal with effectively. [9]

The authors then turn specifically to ATKINSON himself, positioning him as "a member of a group of British ethnographers who [...] claim to criticise ethnomethodology and conversation analysis from a sympathetic and friendly position, offering advice and support" (WATSON & SHARROCK, 1991, p.3). Use of the word "claim" here, at the very least, implies that—whatever the intentions of these ethnographers (of whom I am one, I should acknowledge)—the *effect* of the criticism is not sympathetic and friendly. The result, rather, is to further misunderstanding and marginalisation of EM/CA within the discipline. [10]

WATSON and SHARROCK then summarise what they take to be ATKINSON's main criticism: that ethnomethodology and conversation analysis "are insignificant without the supplementation of ethnographic context" (WATSON & SHARROCK, 1991, p.3). They go on to say that "in his recommendations of how this

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2 Much the same attitude persists today. LYNCH (2016, p.6) notes approvingly that: "Garfinkel, Sacks, Schegloff and many others battled against 'friendly' efforts to integrate ethnomethodology with social psychology, cognitive science, symbolic-interactionist sociology, and psycholinguistics, among other fields and subfields".

[supplementation] might be done, he re-imports presuppositions which ethnomethodology and conversation analysis were designed to abolish, and to which they must, as a principled matter, be unresponsive" (ibid.). The authors then outline some of his arguments: that there is a tension between conversation analysis and ethnomethodology, and that "principled and selective trade-offs are possible and desirable" (ATKINSON, 1988, p.118) in combining ethnography with conversation analysis; that ethnomethodologists' distinction between topic and resource has been exaggerated; and that there are examples of "ethnomethodologically-informed ethnography" and of "ethnographically-informed ethnomethodology" that show the value of combining the two. [11]

It is also noted that ATKINSON treats LYNCH's (1985) study of science "as proffering a hyper-realist profusion of detail which ends up by losing all significance" (WATSON & SHARROCK, 1991, p.7). In fact, this is part of a broader discussion by ATKINSON of the "studies-of-work" programme, one that is generally appreciative, but he goes on to argue that it involves "self-imposed limitations that are extremely radical in their consequences"; in that, for example, "the analyst seems bound merely to recapitulate the observed sequences of activities with little or no framework for selection [...]". And he claims that this "stance is sometimes justified by appeals to canons of natural science which are themselves problematic" (ATKINSON, 1988, p.446). [12]

WATSON and SHARROCK's central argument is that ATKINSON's approach in this article, like that of other "constructive-analytic" critiques of ethnomethodology, displays a commitment to "perspectivism", the position of MANNHEIM (1960 [1936]), or at least "a vulgarized version of it" (WATSON & SHARROCK, 1991, p.5). This treats perspectives as "restricted models of thought—thought conducted according to some limited set of terms, so that they can never reveal the 'full phenomenon' [...]"; in fact, that "in many ways, [they] comprise ways of *not* seeing" (ibid.). As evidence for this attribution of perspectivism to him, they note that ATKINSON refers to EM/CA as "an unduly restricted perspective" (1988, p.441), suggesting, for example, that "conversation analysis's focus on sequencing is a 'limited view' of the temporality of social life (1988:451)" (WATSON & SHARROCK, 1991, p.6). More generally, according to them, ATKINSON "treats conversation analysis as suppressing the 'hermeneutic-interpretive strand' of ethnomethodology in favour of a more 'narrowly empiricist, even behaviourist element' (1988:460), where the actor is reduced to a 'mere exponent of sequenced activities'" (ibid.). [13]

WATSON and SHARROCK examine the "anchorages in ordinary discourse" of terms like "perspective", which they portray in general as "a downranking procedure". They suggest that "the *ad hominem* quality of the perspectival view [...] can lead to the dismissal of a point of view without attending to its reasoning" (1991, p.7). The complaint here, then, is that ATKINSON has not looked carefully enough at the ethnomethodological arguments supporting what he takes to be its "restricted view". Instead, it is suggested, he relies upon "an assumed moralistic cast" or a "textually-generated moral climate", in terms of which "ethnomethodologists and conversation analysts are not only alleged to suffer

from 'tunnel vision' but are expected to feel guilty about it too" (WATSON & SHARROCK, 1991, p.8). WATSON and SHARROCK also argue that ATKINSON's "additive solution" (p.9) cannot overcome the problem, since (in terms of perspectivism) a broader perspective is still a perspective: it too will not comprehend the whole. [14]

As against this perspectivism, these authors deny the "'perception'-'object' division" built into it. They claim that for EM/CA "the object is not an ontological thing but a methodological thing, subject to methodological suspension and the like" (p.10). In other words, they suggest that "different methodological positions radically re-cast the object" (p.11). Thus, the authors present the kind of ethnography championed by ATKINSON, on the one hand, and EM/CA, on the other, as mutually exclusive. Here, they use the analogy of the sort of gestalt-switch characteristic of one type of visual illusion: for instance where the image presented could be a rabbit or a duck. They argue that, rather than conventional ethnography and EM/CA addressing the same phenomena, each *constitutes* the phenomena it investigates *quite differently*: one studies rabbits, if you like, while the other studies ducks. And they cite WIEDER's (1974) investigation of the "convict code" as illustrating this gestalt-switch, and as showing that "ethnomethodology [is] a radicalization of ethnography" (WATSON & SHARROCK, 1991, p.15). [15]

It is on this basis that WATSON and SHARROCK argue that these two approaches cannot be merged or combined in the way that they take ATKINSON to propose. And they complain that "sociologists who in other contexts would perhaps take a Kuhnian incommensurability for granted do not necessarily continue in their conviction" (WATSON & SHARROCK, 1991, p.11). The implication is that there are incommensurable paradigms here, but that ATKINSON is in denial about this.<sup>3</sup> [16]

At the same time, as noted earlier, WATSON and SHARROCK do not present these paradigms as of equivalent value. For instance, they write that

"if one wants to talk with fewer analytic presumptions about problems of 'identity' [a topic that Atkinson (1985) had used to illustrate his argument in a related article] then there just may be work in that area which does address the problem no less plausibly than Meadian social psychology [to which Atkinson appeals] [...]" (p.13). [17]

They continue:

"This work may be seen as coherent in that it systematically works through the moves in a particular scheme; this working out, when taken over a set of contributions, might be seen as potentiating another type of comprehensiveness, not Atkinson's additive one but an *endogenous* comprehensiveness" (ibid.). [18]

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3 The notion of incommensurability in the form of gestalt-switch continues to be used by some ethnomethodologists today to characterise the difference between their approach and conventional social science and philosophy, see for instance LYNCH (2016, p.11).

And they cite MAYNARD's research on "perspective display" (1989) as an illustration to demonstrate the relative superiority of EM/CA over the sort of ethnographic approach to identity that ATKINSON (1985) recommends. WATSON and SHARROCK argue that what is produced by this ethnomethodological research "is clearly more closely-fitted for at least one task in the analysis of identities in conversation than, say, G.H. Mead's notion of identity" (1991, p.14). Furthermore: "it is a generative apparatus which operates in a way that, try as one may, it is hard to see Mead's as being when applied to these data" (ibid.). They add that "of course, the point is that the notion of identity in the Meadian tradition is designed to do other things" (p.14). But they conclude that what MAYNARD offers, or the elaboration of this they propose via the use of membership categorisation analysis, "fulfils the prerequisite of theoretical economy rather than weighing into the data with a ponderous, complex, florid and internally inconsistent theoretical edifice" (WATSON & SHARROCK, 1991, p.15). [19]

Towards the end of their paper, WATSON and SHARROCK identify a number of issues that they regard as "pivotal" for ethnography:

"how do professional ethnographers match up what their observed subjects do with what they say they do (given that saying is also doing)? More generally, how do the subjects themselves perform that matching activity? [...] How, in other words, do they procedurally go about establishing their shared sense of social structure as a familiar, self-describing state of affairs?" (p.16) [20]

WATSON and SHARROCK insist that ATKINSON "fails to appreciate the radical nature of the distinction between topic and resource" (p.17). They write: "This is not, as he claims, a matter of hyperbole but one of reasoned argument and exemplification: Atkinson fails to adequately address either" (pp.17-18). They declare that

"In that Atkinson seeks to treat rules as a species of empirical generalization (by treating the ethnographic enterprise as purely inductive in nature), he utterly fails to understand Wieder's point that there is a logical gap which cannot be bridged" (p.17). [21]

What EM/CA offers, then, is "something analytically more generative than 'straight' ethnographies, and, indeed, such ethnographies are now cast as pursuits of commonsense status, to be treated as the objects of analytic [that is, ethnomethodological] attention" (ibid.). [22]

However, WATSON and SHARROCK conclude by insisting that they have been arguing only that the assumptions underpinning EM/CA "comprise a free-standing set" that is "autonomous". They continue:

"Even though they are ours, we do not, of course, regard them as mandatory or that they should have a monopoly in sociological analysis. Nor have we argued as such against anyone else's assumptions but have merely argued that their assumptions

are not mandatory either [...]. There are no privileged supervisory positions in sociological work" (p.18). [23]

### 3. Discussion

So, WATSON and SHARROCK's starting point is that, like other external commentators, ATKINSON has misunderstood the radical distinctiveness of ethnomethodology and the inseparability of conversation analysis from this. Where he finds overlap and scope for productive collaboration between EM/CA and the kind of ethnography he practises, they see an imperialistic strategy that must be resisted. At this meta-level, at least, it does seem that the dispute amounts to a clash of *gestalten*, as WATSON and SHARROCK claim. However, at other levels, this is not quite so clear. I will begin by looking at some of the specific points that ATKINSON makes and WATSON and SHARROCK's response to them. I will then examine their argument about perspectivism and consider whether this leaves any scope for comparing the two approaches that they regard as fundamentally at odds. [24]

#### 3.1 Specific criticisms

ATKINSON argues that there is a tension between much ethnomethodological work today and the original spirit of the movement, which he regards as having been more compatible with the sort of interactionist ethnography to which he is committed. For example, he contrasts the technical focus of most contemporary conversation analysis with the hermeneutic and interpretive concerns that shaped EM in its early days; though he also identifies problems in the early development of ethnomethodology, centring on the very sharp distinction drawn between scientific and practical rationality. He cites the work of COULTER (1979, 1983), drawing on ordinary language philosophy, as an important countervailing influence to the shift towards a technical approach concerned solely with investigating the sequential structure of activities. He also discusses McHOUL's (1978a, 1978b, 1980, 1982) analysis of reading, as illustrating the connections between ethnomethodology and literary approaches: such as reception theory and classical rhetoric. [25]

Another example of EM/CA work that displays what ATKINSON takes to be links to the broader ethnographic tradition is ATKINSON's (1984) analysis of political speeches, and he notes parallels here with anthropological work on ethnopoetics. In addition, he treats some ethnomethodological studies of work as similar in important respects to other kinds of sociological investigation, perhaps most obviously in the field of science studies. At the same time, as noted earlier, he criticises the work of LYNCH in this field on the grounds that there is no criterion of significance applied to shape what is reported—no frame regarding what aspects of the work studied would have relevance to sociological problems. [26]

It seems to me that at least one of his arguments cannot reasonably be disputed. For example, he writes that: "In particular a tension exists between the specific treatment of conversation's sequential order and more general interests in



mundane reasoning" (ATKINSON, 1988, p.443). As he points out, this tension has been noted by ethnomethodologists themselves (see BUTTON, 1977; HESTER, 1981). And subsequent disputes amongst ethnomethodologists and conversation analysts have confirmed this, with some commentators claiming that conversation analysis has developed in an empiricist direction, neglecting key early commitments of ethnomethodology (see LIBERMAN, 2013; LYNCH, 1993, 2018 [2016]; LYNCH & BOGEN, 1994).<sup>4</sup> Furthermore, his specific argument about the need for ethnographic context as a complement to conversation analysis has also been put forward from within ethnomethodology by MOERMAN (1988). [27]

However, the defensive orientation of WATSON and SHARROCK's paper means that, for the most part, they do not seek to engage with the specific criticisms of EM/CA that ATKINSON puts forward. Where mentioned, these tend simply to be dismissed, as when they complain that he "fails to appreciate the radical nature of the distinction between topic and resource" (WATSON & SHARROCK, 1991, p.17). This radical nature is not spelt out in their response. Similarly, they do not explain why ethnomethodologists adopt the "self-imposed limitations" that ATKINSON believes have undesirable consequences. Of course, the reasons for these commitments can be found in the ethnomethodological literature, but they are not unequivocal and in my view are by no means unproblematic (HAMMERSLEY, 2018a). And when ATKINSON uses a distinction that I drew—between competence and action approaches (HAMMERSLEY, 1986)—they dismiss this as "shaky to the point of utter incoherence", but provide no supporting argument, nor cite any other publication where this can be found.<sup>5</sup> This attitude towards ATKINSON's review, the purpose being rebuttal, undercuts any possibility of dialogue. It reflects in part, of course, the fact that their paper was aimed at fellow ethnomethodologists, and was concerned with how they might defend themselves against external criticism. [28]

There are also some weaknesses in WATSON and SHARROCK's criticisms of ATKINSON's review. First, there are places where their portrayal of his position is unconvincing. An example is the implied charge that, like other critics, he was seeking to marginalise EM/CA. Yet, if marginalisation of this approach had been his aim he would surely have adopted the majority sociological response of neglect—rather than writing a review of ethnomethodology in a major journal. In fact, in some ways, it would be more true to say that it is WATSON and SHARROCK who marginalise EM/CA, since they insist on its radical distinctiveness from the mainstream.<sup>6</sup> [29]

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4 Of course, ATKINSON's conception of those commitments may differ somewhat from that of these internal critics, but I do not believe the difference is substantial.

5 Later, they imply that this distinction is incompatible with the idea that "language is constitutive of social action" (WATSON & SHARROCK, 1991, p.4). But much here hangs on what 'constitutive' means, and they provide no clarification of this.

6 If their aim is not "regime-change", as they insist, then presumably what they want is a more pluralistic structure within the discipline; but they do not indicate why they believe this would be desirable, or why the present state of disarray is not already pluralistic enough.

Moreover, in their discussion they emphasise ATKINSON's *criticisms* of EM/CA, downplaying the extent to which he highlights its value. Unlike some other external evaluations of EM/CA, his review is highly appreciative; and this, too, is surely incompatible with an attempt to marginalise it. He begins by describing GARFINKEL's work as "pioneering" (ATKINSON, 1988, p.441) and declares ethnomethodology "a force to be reckoned with" (p.442), as well as outlining its contribution across a variety of fields. In his Conclusion he summarises part of his review as follows: "It has been argued that contemporary styles of ethnomethodology have made major contributions to the sociology of everyday life in a variety of mundane and organizational settings" (p.459). Finally, he does not seem to be recommending the *incorporation* of EM/CA within ethnography: the term he uses is "rapprochement" (p.461). And, as I noted earlier, he explicitly rejects the idea of a "grand synthesis" (ATKINSON 1985, p.117), suggesting instead "more modest" "principled, selective trade-offs". Of course, questions can be asked about the terms on which these trade-offs are to operate. While one of ATKINSON's primary concerns was to encourage interactionist ethnographers to make more use of the insights and techniques of conversation analysis and ethnomethodological studies of work, this clearly assumes that the elements to be combined are compatible. However, while such borrowing may count as abuse from WATSON and SHARROCK's point of view, it is not in any simple sense an attack or even an imperialistic strategy of incorporation. [30]

Somewhat more challenging is ATKINSON's argument that conversation analysis would benefit from the provision of ethnographic context. This touches on a fundamental issue about how the relevant context for analytic understanding of any pattern of action is to be determined: must this be solely what is displayed as context by participants in the course of their actions, or are there features that they take-for-granted, or are unaware of, whose recognition would facilitate sociological understanding? WATSON and SHARROCK are correct that, in his critical review, ATKINSON does not engage in any sustained way with why ethnomethodologists adopt the first of these positions. [31]

### **3.2 Perspectivism**

As I indicated, WATSON and SHARROCK accuse ATKINSON of adopting a form of perspectivism exemplified by Karl MANNHEIM's (1960 [1936]) "Ideology and Utopia". They suggest that this, "or a vulgarized version of it—has come to permeate the received corpus of orthodox sociologies in ways that are largely unappreciated by very many of its practitioners" (WATSON & SHARROCK, 1991, p.5). They treat this position as insisting that there are multiple perspectives, and that each one operates in such a way as to enable some things to be seen whilst others cannot be, suggesting a parallel with "the strong version of the Sapir-Whorf thesis, where language or concepts comprise a kind of perceptual prisonhouse" (p.6). In addition, they note that ATKINSON uses the term "paradigm", which they suggest "in its Kuhnian conception in some ways operates as a synonym for 'perspective'" (ibid.). [32]

WATSON and SHARROCK also appeal to the "grammar" of the use of "perspective" in ordinary language. They argue that, by suggesting that EM/CA amounts to "an unduly restricted perspective", ATKINSON (1988, p.441) is committing himself to the view that all perspectives are partial; and, as I indicated, they go on to point out that this would imply that even the broader one he is recommending would still be partial—that it too would not grasp "the whole". [33]

WATSON and SHARROCK are certainly correct to emphasise that most EM/CA work is guided by distinctive commitments that mark it off sharply from interactionist ethnography and the other kinds of work that ATKINSON mentions (see HAMMERSLEY, 2018a, Conclusion). It is these that lead to what ATKINSON regards as a "neglect" of context, a "failure" to examine surrounding aspects of the phenomena that are the main focus. Central to EM/CA, though not spelt out by WATSON and SHARROCK, is the idea that social interaction necessarily displays what is going on in a manner that is available to members; that this provides the possibility of a form of rigorous analysis that relies entirely on what is made observable in this way; so that the task of EM/CA is to document how social interaction constitutes itself and its environment, and thereby the world that we all experience as social participants. This is the basis for their argument that ethnomethodological analysis can achieve endogenous comprehensiveness. And this focus on social order as intelligibility is at odds with much ethnographic work—and with most conventional sociology. WATSON and SHARROCK are right, then, that there is a fundamental division here. [34]

However, the term "perspectivism" is ambiguous; and it is not clear from his article that ATKINSON is committed to any of the positions to which it can refer; or, if he is, to which one. As WATSON and SHARROCK indicate, an influential version of perspectivism holds that we can never grasp the whole, since all our understanding is partial: we always must operate from within a particular viewpoint. While each perspective reveals a different aspect of reality, perspectives cannot be combined to provide a panoramic view that would represent the whole. This is characteristic of some neo-Kantian philosophy, and it may have been the view of MANNHEIM as regards "total ideologies", though he seems to have harboured the hope of an ultimate synthesis (see KETTLER, MEJA & STEHR, 1984, p.35). This position contrasts with what we might call holism: the argument that a view of the whole (however defined) *is* immediately possible *and is desirable*. Now, interestingly, both of these views—perspectivism and holism—can support criticism of an account for not including all that is relevant. This is obvious in the case of the second, but even with the first a particular account could be criticised because it does not fully represent all relevant phenomena from within the perspective it adopts. [35]

ATKINSON certainly does argue that EM/CA is restricted in an undesirable way, omitting aspects of social interaction and its context that would be relevant to an understanding of it. But rather than this signalling his commitment to a specific philosophical position—perspectivism or holism—it seems instead to amount to a relatively mundane criticism, to the effect that relevant matters have not been given due consideration. Of course, his judgement here may be wrong or

misguided, but demonstrating this would require clarification of the sort of understanding being aimed at (as already indicated, there seems likely to be disagreement at this fundamental level between him and his critics) and what is necessary to achieve it. [36]

Interestingly, WATSON and SHARROCK appear to go on, themselves, to adopt a position that has sometimes attracted the label "perspectivism"—indeed, it is a more radical version of the position I have just outlined under this heading (and it too can be found in some neo-Kantian philosophy). This requires that even the assumption that there is a whole, or a single, reality—of which each perspective captures only an aspect—be abandoned (not just the idea that perspectives can be put together to provide a comprehensive grasp of the whole).<sup>7</sup> I take this to be implied by their denial of the perception-object distinction. [37]

WATSON and SHARROCK seem to argue that the two approaches in dispute here—EM/CA and interactionist ethnography—must be treated as each *constituting* its own, distinct, object of investigation. Thus, the idea of any "correspondence" between an account produced from some perspective and objects existing independently of it is rejected. The *gestalt*-switch analogy (a notion also employed by KUHN, 1970) implies that from this point of view there is simply no basis for comparative assessment in terms of which one approach could be regarded as more restricted than, or as superior to, another. Indeed, these authors would probably argue that the metaphor on which the term "perspective" relies is inappropriate. Even MANNHEIM's proposal that it is possible to gain insights by adopting one perspective and then another, drawing out the lessons offered by each, without being able to integrate them immediately into a single viewpoint, does not make sense in these terms. It would be more accurate to say, with KUHN, that once we have entered into one paradigm we cannot make sense of others; even those that occupy or have occupied the same disciplinary field (SHARROCK & READ, 2002, pp.144-145). [38]

While this radical perspectivism certainly underlines the fundamental differences between EM/CA and the mainstream, it carries some complications. In particular, it seems to be at odds with the more phenomenological orientation of much ethnomethodology.<sup>8</sup> Where the former insists that any understanding is always from some constitutive perspective, the latter claims that there is a kind of understanding that can capture phenomena in their own terms (see HAMMERSLEY, 2018a, Ch.4). As I outlined earlier, EM/CA insists that social phenomena are constituted publicly in and through processes of social interaction. This, it is argued, generates the world that we all experience within the natural attitude. But WATSON and SHARROCK's radical perspectivism implies that this view of social phenomena is simply one perspective amongst others, with a fundamental incommensurability reigning between them.<sup>9</sup> It is thus unclear on what principled basis we could choose one perspective rather than

7 It is quite close to what MACINTYRE (1988, p.352) refers to using this label. In some interpretations, KUHN (1970) also adopts this view: see SHARROCK and READ (2002, Ch.4).

8 See SHARROCK and ANDERSON (1986). GARFINKEL appears to conflate these two positions in his early writings (see GARFINKEL, 2006 [1948]).

another. While these authors appear to dismiss KUHN's perspectivism, along with that of MANNHEIM, their position shares similarities with his account of scientific paradigms. [39]

### 3.3 Evaluating "incommensurable" approaches

Thus, WATSON and SHARROCK present EM/CA as radically distinct from, and therefore incommensurable with, mainstream sociology, including interactionist ethnography. This would imply no prospect of any comparative assessment of the two approaches, and therefore no grounds for choosing one rather than the other. However, they do not entirely adhere to this position: as I have noted, in some places they claim EM/CA is superior, which implies that comparison *is* possible.<sup>10</sup> [40]

We can draw a parallel with KUHN's (1970, 2000) discussions of paradigms here. It is striking that despite his concept of incommensurability, and use of the notion of *gestalt-switch*, in practice he did not treat different paradigms as instituting *totally* different perceptual worlds. This is made clear by his argument that a successful new paradigm can both answer all of the questions answered by the previously dominant one *and* resolve the anomalies to which that paradigm gave rise. Furthermore, some of the reasons WATSON and SHARROCK adduce for believing that EM/CA is superior to conventional sociology are similar to the formal ones that KUHN mentions in his later discussions of paradigm change in the natural sciences: theoretical economy, or simplicity; and internal coherence, or consistency.<sup>11</sup> Do these provide a basis for evaluating the two approaches that WATSON and SHARROCK regard as at odds, given that most people would probably accept that economy and coherence are desirable features of any sociological approach? [41]

While these authors claim that EM/CA is more economical and coherent than the alternative ATKINSON recommends, they offer little evidence for either claim—they seem to take them as obviously true (perhaps because their audience would also do so). Yet there are questions about the application of both these criteria. [42]

One interpretation of "theoretical economy", perhaps the most usual one, concerns the number of variables that are employed to account for what is to be explained. But EM/CA is not a theory in this sense, nor for that matter is

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9 POLLNER (1978) took this position, arguing that ethnomethodology's superiority for analytical purposes lies solely in its being explicit about its own constitutive role. However, it is not clear that WATSON and SHARROCK would share this view (HAMMERSLEY, 2018b/Forthcoming).

10 At one point, they describe ethnomethodology as a "radicalisation" of ethnography. While they clearly regard this as a positive feature it is not immediately obvious just what this term implies, or why what it refers to is being evaluated in a positive way.

11 See, for example, KUHN's (1977, Ch.13) discussion of shared scientific values that guide paradigm change. There is a significant deviation here from his earlier account (KUHN, 1970), most notably in that he includes under the heading of 'consistency' not just internal coherence but also the consistency of a candidate theory/paradigm with other theories/paradigms (he seems to use these two terms synonymously in his later discussion). At the very least, this suggests a modification to his earlier notion of incommensurability. KUHN went on further to develop this concept (see CHEN, 1997; KUHN, 2000).

interactionist ethnography; and both tend to reject the validity of "variable analysis". We could, perhaps, interpret "economy", instead, as concerned with the relative degree of complexity (in the phenomena studied) that is assumed by the two approaches. That which involves a lower level of assumed complexity would be judged superior. However, I am not sure how we could go about evaluating the approaches in these terms. There is also some uncertainty about whether an assumption of less complexity would be the more desirable. For instance, a common complaint against orthodox economic theory is that the model of human behaviour it adopts is simplistic, and I suspect that WATSON and SHARROCK would agree with this (ANDERSON, HUGHES & SHARROCK, 1989). At the same time, taking account of the full complexity of reality is surely impossible and the attempt to do this is undesirable (HAMMERSLEY, 2008, Ch.2). So, it seems that we would have to determine what is a *necessary* level of complexity, or what would be *sufficient*. But I find it hard to know how this could be done in general terms—much would depend upon the particular questions about the social world being addressed. It is also not clear that the two approaches being examined here are comparable in these terms. This first criterion, then, does not seem to provide much basis for evaluation. [43]

There are also difficulties with the other criterion: consistency. This too is a matter of judgement, and judgements are likely to be affected by the background of the person making the assessment. Furthermore, what is involved here is judgement about *degree* of coherence. What we *could* say, though, is that EM/CA is far from fully coherent: as MAYNARD and CLAYMAN (1991) show, it is internally diverse in character and there are conflicts amongst the rationales for some of its forms—in fact, it is these that have generated the disputes amongst ethnomethodologists themselves. And in some cases the divisions are deep, as indicated by accusations that ethnomethodological principles have been abandoned (see, for example, LYNCH, 2016, 2018 [2016]).<sup>12</sup> Of course, some internal conflict is to be found within interactionist ethnography (for discussion of one relevant area of disagreement, see HAMMERSLEY, 2017a); and in the case of mainstream sociology the internal divisions are blindingly obvious. At face value, it seems likely that we would be faced with a "dead heat" as regards this second criterion. [44]

As I have indicated, KUHN did not see paradigm change as operating entirely on the basis of formal criteria like economy and coherence. He also suggested that the relative value of competing paradigms would become clear through the work they produce; and, in particular, the extent to which a new paradigm could answer all the questions addressed by the previous paradigm as well as solving the problems that had arisen within it. If we look at another paper by SHARROCK and WATSON (1988), published around the same time, we find them arguing that ethnomethodology avoids the dualisms that have plagued conventional sociology, for instance between agency and structure or between micro and macro foci of

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<sup>12</sup> To some extent, this seems to count against WATSON and SHARROCK's claim that the adoption of EM/CA produces a gestalt-switch. Rather than a coherent new perspective (it's a duck!), we seem to have a perspective that not only involves sharp internal disagreements but also ones that overlap with criticisms made by outsiders, such as ATKINSON.

analysis. So, could this provide a basis for some comparative assessment of the two paradigms they discuss? [45]

Once again, comparison in terms of this criterion is difficult. It is not just that KUHN's analysis was designed to deal with mature natural sciences, in which a single paradigm dominates most of the time (whereas sociology is "multi-paradigmatic"), but also that the paradigms he studied all aimed at the same goal, in terms of the kind of knowledge to be produced. This is patently not the case with EM/CA and its rivals, since ethnomethodologists have explicitly announced a re-specification of the purpose of inquiry away from the preoccupation of mainstream sociology with theoretical explanation, and towards an entirely descriptive discipline (BUTTON, 1991). Indeed, this was SHARROCK and WATSON's (1988) core argument in their earlier paper. In this respect, ethnomethodology proposes a much more fundamental re-specification of the field as compared to any of the new paradigms investigated by KUHN. In other words, the problem is not just whether we can obtain evidence with which to compare paradigms, the paradigms are likely to differ as regards the very terms in which they should be judged and therefore over what would count as evidence. [46]

While GARFINKEL (1967) presents ethnomethodology as concerned with showing how social order is achieved—and this has sometimes been treated as the main focus of sociology, notably by PARSONS—most sociological work has not been centrally concerned with this issue. Rather, it has focused on a host of more substantive problems: from the existence, scale, and causes of poverty or inequalities of various kinds to the nature and social function of religion; from the distinctive character of the professions to the role of the state in capitalist society. Moreover, where sociology *has* addressed the issue of social order, the meaning of that concept has usually been defined by contrasting it with the presence of conflict and violence—whereas GARFINKEL's focus is on how the *intelligibility* of social scenes is produced, this being treated as a prerequisite for the coordination of social actions. Clearly, coordination is not cooperation: as GARFINKEL (2006 [1948], p.114) has himself pointed out, riots are orderly. Similarly, the convict code that WIEDER (1974) studied formed part of a conflictual relationship between staff and inmates in the halfway house where he carried out his research. So, what GARFINKEL meant by "social order" is significantly different from what PARSONS and other sociologists have intended by that phrase. [47]

Given this fundamental difference in focus, it seems clear that KUHN's account of how one paradigm can be judged superior to another on the basis of the findings produced (according to whether anomalies have been resolved and previous knowledge preserved, albeit in a new form) does not apply in this case. Indeed, we seem to be thrown back on the other causes of paradigm-change he mentions, such as conversion experiences and the replacement of one generation of scientists by another. But these provide no basis for the claim that one paradigm is superior, they only indicate that it may be more successful in recruitment terms. Moreover, EM/CA does not seem to have out-performed other approaches in this respect, despite its longevity. [48]

There is one other criterion that WATSON and SHARROCK mention. At one point they suggest that an EM/CA approach to the study of identity is superior to an interactionist one because it provides a "generative apparatus" (WATSON & SHARROCK, 1991, p.14). And, in another place, they remark that ethnomethodology offers "something analytically more generative than 'straight' ethnographies [...]" (p.17). The authors do not explain what they mean by "generative" here, but the reference is to SACKS' conception of rigorous social scientific inquiry as specifying a "machinery" or an "apparatus" that produces the phenomenon to be understood (SACKS, 1984, p.26, 1992, p.169). He presents this in the context of his analysis of a child's story—"the baby cried, the mummy picked it up"—where what is to be explicated is why we hear the "mummy" as the mother of the baby. And conversation analytic work focusing on sequential structures can also be formulated in similar "generative" terms (SACKS et al., 1974). However, the status and character of this knowledge is in dispute within EM/CA, along with the issue of whether there are context-transcendent methods that would be generative in this sense (HERITAGE, 2016, 2018; LYNCH, 1993, 2016, 2018 [2016]; WATSON, 2008; WILSON, 2012). [49]

There is also a question about why this generative model should be regarded as superior to the sorts of explanation put forward by conventional sociologists. It could be argued that conversation analysis has demonstrated that it can meet its goals, whereas mainstream sociology has recurrently been recognised as failing to do this, even by its own practitioners. But, even if this is the case, it may also be true of other kinds of ethnomethodological investigation, such as studies of work (HAMMERSLEY, 2018a, Ch.3) Moreover, changing the focus of inquiry to one where recognisably sound knowledge can be achieved more easily is not automatically justified. To warrant this we would need to establish that the grounds for judgements about the failure of conventional social science are sound. Also required would be strong evidence that conventional social science is *incapable* of producing worthwhile knowledge about its traditional topics. That this is the case has certainly been asserted by some ethnomethodologists (see BUTTON, CRABTREE, ROUNCEFIELD & TOLMIE, 2015, p.137; HUTCHINSON, READ & SHARROCK, 2008). However, in doing so they rely upon a conception of rigour that, in my view, is unattainable and unnecessary; one that ethnomethodology probably cannot meet itself (HAMMERSLEY 2017b; 2018a, Ch.4 and Conclusion); and, indeed, one that seems to be challenged by some ethnomethodologists, since it relies upon a sharp distinction between analytic and common-sense investigations (for instance LYNCH, 1993). [50]



#### 4. Conclusion

Since the late 1960s and 70s there have been recurrent disputes about the relationship between EM/CA and what have been widely regarded as cognate approaches. Initially, there was a common preoccupation across all of these with challenging the then dominant forms of sociological work, which were based on structural functionalism and survey methodology. But significant differences *among* these new approaches soon came to be highlighted by some commentators, and particularly by ethnomethodologists; by contrast, adherents of the other traditions—such as interactionism and interpretive ethnography—often tended to adopt a more eclectic approach, and sometimes this included drawing on EM/CA. WATSON and SHARROCK, on the one hand, and ATKINSON, on the other, exemplify these contrasting attitudes: what we might crudely label purism versus eclecticism. [51]

ATKINSON partly anticipated the reception of his review by ethnomethodologists. He wrote: "Attempts to establish common ground frequently falter in the light of mutual misunderstandings and reaffirmations of competing dogmas" (1988, p.461). However, he does not treat this as indicating an underlying, unavoidable impasse; whereas WATSON and SHARROCK argue that the very assumption that there could be common ground is misplaced, and that the parallels and connections that ATKINSON perceives are an illusion. I have argued that they are undoubtedly correct that ethnomethodology was always, and remains, sharply at odds with other interpretive approaches, but that their responses to ATKINSON's more specific points are not convincing. Nor, of course, do they show (or attempt to show) that, contrary to what ATKINSON claims, conventional ethnographic work cannot be improved by drawing on EM/CA. The key issue, though, is whether there is any possible ground for dialogue across the divide. [52]

Ethnomethodology had distinctive roots in GARFINKEL's inheritance of PARSONS' concern with the fundamental problem of social order, albeit re-specified. By contrast, most of the other new approaches from the 1960s and 1970s rejected this focus on social order in favour of an emphasis on action, conflict and change. Furthermore, ethnomethodologists were distinctive in criticising mainstream sociology for being insufficiently rigorous, for trading on common-sense knowledge. What was unique about GARFINKEL's work, then, was that it promised a solution to the problem of social order that at the same time offered the prospect of a highly rigorous form of empirical inquiry. He argued that people necessarily display what they are doing in and through doing it, and at the same time display their understandings of what others are doing via their actions. It is in this ongoing process that social phenomena are constituted as recognisably what they are to all participants, and through which social actions are coordinated. Moreover, since this process is a public one, it is immediately available to analysts as objectively describable in a way that the sorts of meanings of interest to interactionist and interpretive ethnography frequently are not. [53]

It follows directly from this that ethnomethodology involves a radical re-specification of the focus of sociological inquiry. So, for example, in studying a

particular form of work, the primary concern of the ethnomethodologist is to document exactly what is involved in doing this work that makes it the distinctive kind of work that it is. By contrast, most sociologists would be interested in research questions that relate to such matters as: what factors shape the character and conditions of this work, how people define their work identities, their levels of job satisfaction, the political attitudes that are associated with those carrying out this type of work, the relationship between work of this kind and wider patterns in the division of labour and the exercise of power, and so on. [54]

It is ethnomethodology's focus on intelligible social order, commitment to rigour, and consequent re-specification of the sociological task that generates the severe difficulties facing the kind of dialogue between approaches sought by ATKINSON. The only way that such dialogue might be possible, I suggest, is through detailed appraisal of the fundamental assumptions of ethnomethodology as compared with other versions of sociology. In portraying social phenomena (in fact, all phenomena) as entirely constituted on particular occasions through processes of social interaction, ethnomethodology is sharply at odds with the assumptions that make up the "natural attitude", and the rest of social science (including interactionist ethnography), in much the same way as is HUSSERL's transcendental idealism (HAMMERSLEY, 2018a). The question that arises is: what grounds are there for treating this alternative ontology as true or useful—or, for that matter, as better than HUSSERL's? [55]

While this sort of appraisal is unlikely to offer definitive conclusions that will be accepted by all, it could clarify the nature of the disagreement and the strengths and weaknesses of the arguments on each side. MACINTYRE's (1988, Ch.18 and *passim*; see also TRENER, 2014) discussion of the rationality of traditions may offer a model here. He argues that even in the case of conflicting metaphysical or religious perspectives—his examples are Aristotelianism, Augustinian Christianity, AQUINAS's philosophy, and Enlightenment liberalism—there is scope for fruitful dialogue. He suggests that there can be significant overlaps of one tradition with another, in particular areas, and that this may stimulate a process of rational appraisal on one, or both, sides. [56]

Of course, for this to occur, representatives of each tradition must engage with one another's arguments, whereas the defensive strategy that WATSON and SHARROCK adopt in their paper rules this out by its reliance upon a radical perspectivism that treats the approaches as deeply incommensurable; their stance here is in some ways analogous to DERRIDA's rejection of GADAMER's proposal of dialogue (see MICHELFELDER & PALMER, 1989). Ironically, this seems destined to preserve the very marginalisation of ethnomethodology within sociology about which they complain. It also reinforces the "multi-paradigmatic" state of sociology as a discipline, and that of social science more generally. [57]

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## Citation

Hammersley, Martyn (2019). Understanding a Dispute About Ethnomethodology: Watson and Sharrock's Response to Atkinson's "Critical Review" [57 paragraphs]. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, 20(1), Art. 1, <http://dx.doi.org/10.17169/fqs-20.1.3048>.