# Whose tent are we in?
# The value of fundamental criticism in, of, and for sociology

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*Abstract*

This paper discusses a subject that has become increasingly delicate in recent years in sociology and related fields and subfields, including ethnomethodology and conversation analysis. The subject is criticism, particularly criticism of academic tendencies and trends that are uncomfortably close to home. A portion of Wes Sharrock's voluminous body of writings is critical of attempts to turn vernacular expressions into stable scientific concepts. Following Peter Winch, such conceptual criticism extends to 'the very idea' of a social science, and following ethnomethodology it re-examines the project of converting indexical expressions to formal analytical instruments and objects. Not surprisingly, such criticism sometimes stirs strong and indignant reactions and is shunned for being counterproductive. This paper discusses rationales for this mode of criticism and presents two examples of conceptual confusions that arise from ignoring the point of such criticism: one has to do with efforts to treat *motives* as an explanatory factor in social research, and the other has to do with efforts to use quantitative analysis to 'test' a 'hypothesis' about the systematic uses of *apologies* in conversation.

INTRODUCTION

Approximately thirty years ago, a graduate student with whom I was working at the time told me about a piece of advice that he had recently been given at an international sociology conference. After he had presented a paper critical of some of the trends in the field, a prominent sociologist took him aside and advised him to be careful not to 'piss in your own tent.' I did not have the benefit of attending the conference or hearing the remark, but whether or not my second-hand account accurately reports what was said at the time, the analogy contained in the advice vividly expresses a view that has become increasingly common in the social

sciences, particularly in sociology and related fields that have had to put up with criticisms from insiders as well as outsiders throughout their history.

For some decades now, fundamental criticisms in and of the human sciences have tended to be framed as partisan 'attacks,' with the debates in which they situate themselves characterized as 'wars,' In such a polarized universe, writers and speakers who direct fundamental criticism to others, in what is presumed to be the same rather than the opposition camp, risk being accused of disloyalty or worse.[1] A presumption expressed by the 'pissing in the tent' analogy is (to mix metaphors) that we are all in the same boat, and that we need to pull together in rough waters. However, such a presumption of professional solidarity is exactly what is placed into question in arguments that refuse to gloss over differences.

When they first burst on the scene in the 1960s, ethnomethodologists were among the more vociferous critics of 'conventional' sociology, and they made clear that they did not aim to comfortably situate themselves under the big tent of the social sciences.[2] Instead, they seemed more inclined to pitch their tents at a frontier that disciplinary sociology had barely recognized as being part of its domain. At the time, some of the leading figures in ethnomethodology and what later came to be called conversation analysis acquired reputations for taking a combative stance toward sociology, and also for being contentious in their relations with one another. The founders and some of the earliest proponents of ethnomethodology and conversation analysis in the United States—particularly, Harold Garfinkel, Harvey Sacks, Gail Jefferson, and David Sudnow[3]—would never have been accused of being overly concerned with civility in their professional conduct, and they also

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[1] A relatively recent example of a critical essay advocating an ethnomethodological approach that stirred angry and indignant responses from others in the 'qualitative' neighborhoods of the Human-Computer Interaction (HCI) field is Crabtree et al. (2009).

[2] In the 1960s, when Harvey Sacks, Manny Schegloff, David Sudnow, and Roy Turner were students of Goffman's at U.C., Berkeley, they embraced ethnomethodology and, according to recollections of others who were on the scene at the time, they were conspicuously aggressive in their rebellion against conventional sociology. One such recollection, in an interview about Goffman, is presented by Kurt Lang, who recollects having come to Berkeley at 'the time when ethnomethodology thrived, which was antiestablishment.' He adds that he viewed them as 'left Goffmanites,' with Harvey Sacks as the core member, as compared with the 'right Goffmanites' who 'were more or less rooted in symbolic interactionism.' The former, according to Lang, 'would go as far to the extreme as Harold Garfinkel, if not more so … . It was a little bit like deconstruction' (Lang and Lang 2009). The rhetorical assignment of ethnomethodology to left- or right-wings of an academic lineage is sometimes reversed, as in David Bloor's (1992) characterization of ethnomethodologists as right-Wittgensteineans in contrast to his own left-Wittgensteinian approach to the sociology of scientific knowledge.

[3] I did not place Manny Schegloff or Melvin Pollner in this list, even though both in different ways were not at all reluctant to formulate and publish critical remarks about near relatives as well as distant clans, because they managed to do so without courting accusations of personal animus and incivility. Schegloff occasionally would recount an inside joke made by Harvey Sacks that took the form of the announcement that Schegloff was a living exception to the 'preference for agreement.'

could be quite disagreeable in their relations with one another and (as I can attest) with their students. And, across the pond, Wes Sharrock, after he became the leading proponent of ethnomethodology in the UK, acquired a reputation for carrying on the campaign for ethnomethodology. According to his own recounting, as well as that of others who were around at the time, Sharrock took part in some fierce disputes with colleagues in the Department of Anthropology and Sociology at the University of Manchester in the 1960s and '70s.

In Sharrock's case, the reputed disputatiousness has little to do with overriding professional ambition or a nasty disposition. The Wes Sharrock I have known for decades—and I believe the overwhelming majority of the dozens of students and colleagues with whom he has worked over his long career would agree—has been an assiduous scholar, a caring mentor, and a gentle and humble person. However, I also have witnessed how formidable he can be in academic debates and published critical exchanges, where he presents his positions directly and forcefully. Many of his books and articles, including some that I value most highly, are fundamentally critical of specific social and cognitive science texts and trends. Even the titles of some of these items, such as 'Blunderbuss and Scattershot: A Response to Professor Wetherick' (Sharrock and Coulter 2000), explicitly express relish for an academic donnybrook. Such vigorous criticism is not only directed at sociologists and philosophers who are on the other side of debates about ethnomethodology and its philosophical antecedents; often enough, Sharrock directs his incisive commentaries at colleagues in and around ethnomethodology and conversation analysis (EMCA).[4]

Academic disputes are often dismissed with pithy sayings such as 'academic politics is the most vicious and bitter form of politics, because the stakes are so low.'[5] Such a dismissal might seem to apply particularly well to disputes among human scientists, and especially to disputes in a small field such as ethnomethodology, which is sometimes regarded as an obscure and narrow sub-specialty in which small differences are blown out of all proportion. However, such dismissals of academic arguments as petty and ineffectual political disputes, or as mere posturing, hair-splitting, and spleen-venting, deter further interest in what such disputes might possibly be *about*.

As other contributions to this *Festschrift* make clear, Sharrock's voluminous body of writings includes a substantial amount of empirical and analytical work, but in this essay I intend to make the case for the sense and value of Sharrock's explicitly critical writings. In my view, far from being empty exercises in academic politics, they serve to remind us of a question that sociological empiricism consigns to its prehistory: Does it make sense to pursue the project of a social *science*?

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[4] A recent example of such a provocation is Anderson and Sharrock (2017).

[5] Otten (1973: 14) attributes this saying to Wallace Sayre, a political scientist at Columbia University in the mid-20th century, and refers to it as Sayre's Third Law of Politics, adding that 'no one seems to know the first two, or whether there even were a first two.'

This is far from a trivially obscure or low-stakes question, but in sociology it is too often ignored, and when it is addressed explicitly it tends to be overwhelmed by a conflict of interest that transforms it to a tactical question that presupposes an actual, nascent, or potential scientific status: Just how can sociology secure its status as a bona fide science?

In what follows, this essay discusses a peculiar feature of what I have thus far called 'criticism': it is not a familiar kind of academic, political, or normative critique. In some contexts it is not criticism at all, or even critique in the philosophical sense of elucidating and questioning deep assumptions. In present-day academia, however, such criticism (if it is criticism) tends to provoke anger and protestation, and to be taken as an expression of personal or political animus. My aim with this essay in honor of Wes Sharrock, then, is to show how his many (often co-authored) writings provide reminders of how sociology's concepts and methods derive from and remain beholden to everyday activities and natural language use.

## THE MANCHESTER 'SCHOOL'

George Psathas (2008), in an essay on the history of ethnomethodology, acknowledges Sharrock as the central figure in the Manchester School. Psathas uses the word 'school' with care, recognizing that it tends to give the impression of a formal institution rather than a relatively small, loose, and unstable network.[6] The term is apt enough in this instance, however, since for more than a half-century, Sharrock has been the central node of a network of colleagues, students, and former students at the University of Manchester and Manchester Metropolitan University, among other institutions. Moreover, this 'school' developed a distinctive intellectual profile. As Psathas describes, Sharrock's interest in sociological theories of action, and his critical attention to the pivotal conception of the 'actor's understanding' in descriptions of social actions, initially led him to the social phenomenology of Alfred Schutz. Later (after being prompted by his colleague John Lee) he was drawn to Wittgenstein's later philosophy, as well as to the critical writings on social science by Winch, Ryle and other philosophers of ordinary language. Sharrock then discovered the writings of Garfinkel, and soon afterwards those of Sacks, and found in their studies a way to move from the philosophy of language to sociological research on observable actions and interactions in everyday and specialized organizational circumstances. Recently, Sharrock and his long-standing collaborator Bob Anderson characterized ethnomethodology's approach to action-in-its-course as 'third-person phenomenology' (Anderson and Sharrock

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[6] Psathas (2008) also discusses the 'Boston school' of ethnomethodology, which became connected to the 'Manchester School' when a former student of Sharrock's, Jeff Coulter, began his long career in the Sociology Department at Boston University in the 1970s. Sadly, that 'school' did not survive the retirements of Psathas and Coulter, and the Manchester School has diminished with Sharrock's and other key members' retirements.

2018: 4)—an orientation to the intelligibility and analyzability of 'the actor's point of view' in and as actions unfolding in a public domain—in contrast to a first-person explication of the perspective of an aware and acting ideal-typical subject.

Within the various lines of work in ethnomethodology and conversation analysis, the Manchester School acquired a distinctive profile by delving explicitly and extensively into ordinary language philosophy and developing an approach that emphasized the conceptual as well as sequential organization of social action and interaction. In line with the ordinary language philosophers, particularly Peter Winch (1958) and Gilbert Ryle (1949), the Manchester ethnomethodologists linked ordinary language concepts to philosophical and social science usage, and questioned attempts to stabilize ordinary linguistic meaning and grammar in order to operationalize logical, theoretical, and mathematical constructs and models.

Critical arguments by Sharrock and his collaborators (and it is important to keep in mind that a large number of Sharrock's books and articles are co-authored) are intimately bound up with their substantive approach to social actions. It was not unusual in British sociology in the 1970s and '80s to pursue an interest in Wittgenstein's later writings, and to take up Peter Winch's pointed critique of the way social scientists attempt to transform ordinary words into explanatory concepts.[7] However, unlike others who attempted to turn Wittgenstein and Winch into predecessors for new social science programs that would be 'strong,' 'empirical,' and 'scientific,' and yet 'relativist,' in their empirical treatments of knowledge, discourse, and action (Bloor 1976; Collins 1983; Mulkay, et al. 1983), Sharrock and his colleagues resisted such attempts to deconstruct the proverbial cake while eating it too. They also continue to resist ongoing efforts to integrate ethnomethodology and conversation analysis with established quantitative, experimental, and analytical methods in sociology, social psychology, and linguistics, but they also resist the anti-positivist, constructivist, and so-called 'postmodern' turns in social and cultural studies (Anderson and Sharrock 2013).

The difficulty with coming to terms with Sharrock's approach is similar to what he says about the difficulties presented by Wittgenstein's and Garfinkel's writings: without embracing eclecticism, they deliberately avoid affiliating with any of the 'isms' of the day, or of any day—relativism, constructivism, idealism, realism, positivism, empiricism, materialism, skepticism, solipsism, or any other familiar or obscure metaphysical position. This is not a matter of walking a tightrope to avoid falling off on either side of the cartesian dichotomy in any of its many guises. It is more a matter of keeping eyes wide open in the midst of the world while resisting the conceptual resolutions provided by ready-to-hand dichotomies between subjective and objective interpretations, social constructions and material realities,

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[7] Indeed, Colin Campbell (1996) treats post-Wittgensteinian approaches as having taken over the theory of social action, and he proposes a 'critique of a critique' that would reinstate the classical Weberian treatment of social action as 'behaviour possessed of a subjective meaning' (p. 1).

actors' categories and analysts' categories, and so on *ad nauseum*. What is demanded is a kind of wakefulness; a resilient attunement to the mundane here-and-now, rather than a delicate balancing act involving heroic skill.

## ETHNOMETHODOLOGY AND SOCIAL SCIENCE

Unlike many others in ethnomethodology and conversation analysis, Sharrock, together with many of his colleagues and students, has engaged extensively and deeply with philosophical issues. This does not mean that they fancy themselves as philosophers (although some of Sharrock's colleagues and co-authors, such as Rupert Read and Phil Hutchinson, are philosophers by training and departmental affiliation), nor does it mean that they avoid engagement with empirical research. Instead, unlike the vast majority of social scientists who pay any attention at all to philosophy, they do not simply mention philosophical 'speculations' in a cursory way to set up and/or justify their empirical studies and findings. And, unlike the overwhelming majority of social scientists (including many in EMCA), they take seriously the possibility that sociology and psychology will never convincingly attain standing as a full-fledged science (Hutchinson et al. 2008). While serious attention to that possibility might seem to hearken back to the polarities of the *Methodenstreit* of the late 19th century, the terms of the argument are quite different. The aim is not to displace all notions of 'objective' social structure, but to recognize the *ordinary* conceptual and methodological basis for the constitution of social order. By itself, this is a fundamental insight of sociology that is far from original to ethnomethodology, but instead of abstracting from such ordinary constitutive actions in the interest of producing and 'testing' theoretical models, ethnomethodology investigates how that work is done in detail, at every point where 'society' is enacted. Accordingly, deferring the aim to construct a social *science* does not spell the end of meaningful empirical research.

Sharrock and Anderson (1980: 2ff.) liken ethnomethodology's relation to sociology to be akin, respectively, to that of theater production – or, various modes of production involved in constructing the set, directing the play, performing as characters, and so on – and that of drama criticism. The alignment with production in ethnomethodology's case is not an instrumental aim to produce or reproduce social order, but instead is a descriptive orientation to its unremitting production on particular occasions. Sharrock and Anderson add that ethnomethodologists are not doing criticism of the critics, but instead are pursuing a completely different approach. According to the theater analogy, an ethnomethodologist does not assume the critic's vantage point as an articulate audience member and evaluator of a play's performance: there is no attempt to find fault with, or suggest how to improve upon, any of the instructional texts, scripts or other production documents that participants consult. Instead, the aim is to describe the production as it unfolds contingently in time and space.

Along the lines of their analogy, Sharrock and Anderson argue that ethnomethodology neither competes with sociology on its own turf, nor supports its theories with 'fine detail'. However, when they, as well as many other ethnomethodologists, turn attention to research practices in sociology, cognitive science, and psychology, among other social and behavioral sciences, they are not simply describing a practical production.[8] In the case of sociology, for example, the methodic practices involved in doing sociology are available for ethnomethodological examination—and, as a practical matter, they are readily accessible for ethnomethodologists who are trained in sociology and work in sociology departments. According to Garfinkel and Sacks' (1970) conception of ethnomethodological indifference, no special interest or privilege should be accorded to sociology's methods. However, when those methods *do* provide subject matter for ethnomethodological research, it is difficult *not* to recognize critical implications. A question such as 'How is the investigator *doing* it when he is making out the member of a society to be a judgmental dope?' (Garfinkel 1967: 68) simply cannot be taken as a 'how' question akin to, 'How is a musician doing it when she plays a sequence of notes on the piano as a recognizable jazz improvisation?' In the first case, the formulation of the investigator's practical 'achievement'—making members out to be judgmental dopes who unwittingly comply with social norms—presents the sociologist's 'achievement' as dubious from the outset. Similarly, when Garfinkel (1967: 19ff.) turns from treating coding as an instrumental method for reducing clinical file contents to statistically manageable categories, and instead focuses on the 'ad hoc procedures' the coders use to get their tasks done, the very idea that the data are assembled in an 'ad hoc' way makes it difficult to ignore the implication that those methods are less rigorous than advertised in the textbooks. And, further, when Garfinkel (1967: 79ff.) uses the 'counselor' exercise to explicate Mannheim's (1952) 'documentary method of interpretation,' it is difficult not to recognize the critical implications for artificial intelligence as well as for interpretive sociology.[9] Far from recommending the documentary method, as a scholarly procedure for identifying '… an identical homologous pattern underlying a vast variety of totally different realizations of meaning' (Mannheim 1952: 57, quoted in Garfinkel 1967: 78), Garfinkel's exercise demonstrates the extreme flexibility with which such patterns can be composed.

The relationship between critics and artists often is fraught with tension, but the relationship between ethnomethodology and social science involves a somewhat different tension. This tension is expressed with not-so-subtle irony in Garfinkel and Sacks' characterization of 'constructive analysis':

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[8] See Coulter and Sharrock (2007) for one of the many criticisms of cognitive science (or 'cognitive neurophilosophy') they and other ethnomethodologists have made.

[9] See Suchman (1990: 306) for linking the 'counselor' exercise and the theme of documentary method of interpretation to criticisms of artificial intelligence.

> In a search for rigor the ingenious practice is followed whereby [indexical] expressions are first transformed into ideal expressions. Structures are then analysed as the properties of the ideals, and the results are assigned to actual expressions as their properties, though with disclaimers of 'appropriate scientific modesty'. (Garfinkel and Sacks 1970: 339)

Indexical expressions are words, phrases, tokens, or other signs (such as the sounding of an automobile horn as a warning, greeting, or objection on different occasions) that take on a localized sense from their placement in an immediate context of action and interaction. For Garfinkel and Sacks, indexical expressions and indexical actions do not make up a clearly bounded domain, but point to a ubiquitous feature of actions-in-context. In sociology and other social sciences, indexical expressions present a methodological challenge to discern, define, and discipline what those expressions 'index' from one occasion to another. Familiar procedures for taking up the challenge are experimental protocols in which the same 'stimulus' is administered to subjects in a standardized setting, and standardized questionnaire designs solicit fixed scalar responses that enable the construction of indices. As, for example, Macbeth et al. (2016) point out, even in some studies of 'naturally occurring' interaction, indexicals are converted into stabilized indexes through the collection of similar tokens used on different occasions, naming and defining what the tokens naturally 'index,' and then using repeated instances of the 'same' token in the 'same' position to abstract a structurally stable referential function from the local context of interaction.

To return to Sharrock and Anderson's theater/drama critic analogy, a description of the work of constructing a stage set is likely to reveal various ways in which the designers and construction crew use ingenious practices to compose and recompose illusory settings and backgrounds that support an audience's suspension of disbelief. Similarly, a description of how a stage magician performs a magic trick exposes the sleight of hand, and for that reason the magicians' guild attempts to control the dissemination of such descriptions. However, in these cases, the audience is expected to know and appreciate—and even recognize with appreciation—that an illusion is being perpetrated on them. In the case of an empirical social science, Garfinkel and Sacks' mention of the 'ingenious practice' of turning indexical expressions into stable indices challenges the naturalistic auspices of empirical sociological research.

MOTIVES

The topic of 'motives' has had a long run as a featured concept in sociological and psychological explanations, and it has had no less of a run in philosophical and theoretical critiques of such explanations. In an early paper, C. Wright Mills (1940) put the topic into stark relief by criticizing efforts to treat motives as

substantive causes of human actions ('springs of action'), and suggesting instead that motives can be analyzed as ordinary 'vocabularies' used in common social circumstances to cast blame or suspicion, or give excuses for problematic (or otherwise notable) conduct. Consistent with his early interest in sociological pragmatism, Mills recommended that, instead of adopting motives as an explanatory variable, sociologists should treat motive explanations as practically and rhetorically integrated with socially institutionalized ways of life, and thus as part of the organized social affairs that sociologists investigate. For Peter Winch (1958: 71ff.), attempts by social psychologists to give motive explanations were a prime example of a misbegotten effort to turn ordinary language into stable explanatory concepts. The issue for Winch was *not* that motives are private, subject to strategic concealment, and thus difficult to discern with any certainty 'from outside,' but that it is fundamentally mistaken to suppose that, by using rigorous experimental procedures, social psychologists should be able to develop and test the linkages between motives and behavioral expressions and outcomes. Winch was not denying the salience of motives for explaining problematic actions, whether in a courtroom or informal conversation; what he was denying was the analogy between vernacular reasons and mechanical causes, and insisting that explaining the meaning of human actions should not be confused with explaining the 'actions' of a timepiece.

Motives also featured in a critical exchange between Bruce and Wallis (1983, 1985) and Sharrock and Watson (1984, 1986). After briefly presenting the arguments that Mills and many successors make for shifting analytical attention from motives as substantive 'springs of action' to investigating motives as dependent contents of discursive accounts and interactions, Bruce and Wallis (1983: 62) summarize the position by saying that this shift implies that '[t]he import of all this is clear: we can learn nothing about the reasons, motives or intentions behind some previous action from an actor's present account of that action.' Aside from overstating the argument they aim to refute, Bruce and Wallis endow reasons, motives and intentions with a substantive, albeit hidden, existence 'behind' the vernacular actions that express and profess them, or accept or refuse attributions of them.

Read charitably, Bruce and Wallis have a point when questioning the position that motives, reasons, and intentions cannot be intuited or discerned by outside observers of talk and conduct and even in some cases of talk that denies the salience of particular motives, reasons, and intentions. According to the position Bruce and Wallis criticize, motives are individual, subjective configurations that can be hidden, masked, and falsely projected in strategic interaction, so that any social science effort to interpret professed or expressed motives as evidence of 'real' motives for actions is bound to go wrong. According to Sharrock and Watson, Bruce and Wallis mistakenly attribute such a position to ethnomethodology and conversation analysis. Two key points in Bruce and Wallis' characterization of EMCA are, first, that EMCA aims to be more scientific than 'conventional' sociology and, second, that, while denying the objective availability of motives,

ethnomethodologists freely attribute motives and motivations to the actions and actors they study.

On the question of ethnomethodology's scientific aspirations, Bruce and Wallis quote Garfinkel's (1963: 160) line that 'there is no reason to look under the skull since nothing of interest is to be found there but brains.' They then associate Garfinkel's position with that of radical behaviourism, and with an aspiration to attain 'a more objective and scientific status' than 'conventional' sociology (Bruce and Wallis 1983: 64–65). For support, they quote a line from the introduction of an anthology of conversation analytic studies which, in their view, suggests a position of inductive empiricism.[10]

To support their second point, that ethnomethodologists freely impute motives while purporting not to do so, Bruce and Wallis cite a series of studies that question professed reasons and motives while imputing other motives and interests, such as 'a desire to appear rational, decent, sensible and honest and a desire to win some interactional advantage' (1983: 65). Focusing specifically on conversation analysis, they argue that published work in CA freely trades in inferences about intentions, motivations, and motives, even while denying the need to make inferences about what is 'under the skull' of the actor. And, at a general level, they argue, CA's descriptions of sequential structures presuppose that conversationalists are motivated to *conform* to conventional sequential rules, such as that a question should be answered or an invitation accepted or refused.

When Sharrock and Watson engage with Bruce and Wallis's argument, they do not defend the position that motives and intentions cannot be inferred with any certainty from conduct, or that sociologists should purge all mention of motives, intentions, and reasons from their descriptions. Sharrock and Watson point out instead that it is commonplace in formal and informal social affairs to inquire about and attribute motives, and on occasion to suspect or conclude that an agent's 'real' motives differ from those they profess. They *do* attempt to disabuse their readers of the view that real motives and intentions, as opposed to professed motives and intentions, are in the proverbial 'heads' of actors. On this point, they take Wittgenstein's (1958) view that natural language-use in recurrent contexts of action provides for the intelligibility of vernacular talk of intentions, reasons, and

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[10] Bruce and Wallis (1983: 65) present a quotation claiming that analytic findings in CA 'can be generated out of the matters which are in various ways evidenced in the data of interaction.' In a footnote, Bruce and Wallis (p. 71, n. 12) attribute the quote to Heritage and Atkinson's introduction to a book that was in press at the time, and do not give a page reference. The quotation apparently is from the first page of the introduction, though the published version is slightly different, reading that CA's analysis 'can be generated out of matters observable in the data of interaction' (Heritage and Atkinson 1984: 1). The quote and the sentences that follow may support the attribution of a position of inductive empiricism to the authors, though both versions of the quotation include the verb phrase 'can be generated,' which suggests a possible way to proceed analytically, rather than the only way to do so.

motives. Rather than defend the position that Bruce and Wallace impute to ethno-methodology and CA, Sharrock and Watson complain that Bruce and Wallace 'compress complicated, full and subtle lines of argument into bare and impoverished summary and are then surprised at the inadequacy of the account that results' (1984: 436).[11] Although it is correct to say that ethnomethodologists do not treat motives as 'special, general or specially sociological problems,' this does not preclude them from relying 'upon the analyst's "vulgar competence" (to borrow a phrase of Garfinkel's) … to make out what people are doing things for. Doing this does not, however, occupy any special place in the practice of the work nor is it anything which the analyst ought to refrain from doing' (p. 439). In other words, Sharrock and Watson propose that professions of motive, attributions of motive, denials of motive, scepticism about professed motives, and searches for 'real' motives, are vernacular goings-on, and in circumstances such as a trial court they are thematic to formally organized activities. Like other features of natural language use, conceptions of motives, intentions, and so forth, cannot be purged, wholesale, from sociologists' descriptive accounts, but this does not mean that motives lend themselves for use as stable 'factors' in generalized 'scientific' explanations.

## APOLOGIES

Sharrock and Watson defend CA in their exchange with Bruce and Wallis, but confusions engendered by abstracting vernacular expressions, including words, phrases, utterances, and conversational fragments, from their contexts of use and subjecting them to constructive analysis also can be found in a particular style of present-day CA. Indeed, Graham Button and Sharrock (2016) recently wrote in support of CA when criticizing that style. An especially clear example of constructive analysis is exemplified by a recent journal article co-authored by John Heritage and Chase Raymond (2016) which takes up the question of whether *apologies* in conversations are elaborated in a way that is proportional to the magnitude of the correlative offenses. Erving Goffman (1971: 108–109) formulated a principle of proportionality and linked it to the broader principle of distributive justice, and Heritage and Raymond set out to 'test' the principle with a quantitative analysis of a collection of fragments of recorded conversations.

As Heritage and Raymond (2016) elaborate, Goffman proposed that the discursive formats through which apologies are expressed range from perfunctory to

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[11] Sharrock and Watson use the term 'conversational analysis,' in their response to Bruce and Wallis, who prefer 'conversation analysis,' which by then was becoming the standard name for the field. In a footnote, Bruce and Wallis explain their preference as follows: 'In the early works the term 'conversational analysis' was used but we have chosen to follow English proponents in using 'conversation analysis' to emphasize that it is analysis of, rather than analysis by, conversation' (p. 71 n. 19). Sharrock and Watson's preferential use of the older form may have been a minor protest against the trend by some of their contemporaries to take ownership of 'analysis.'

highly elaborate, depending upon the 'virtual offense' involved. Goffman defined 'virtual offenses' as 'worst possible readings of an action that maximize either its offensiveness to others or its defaming implications for the actor himself' (Goffman 1971: 108). He introduced this term in connection with 'remedial work' in social interaction, and added that he selected the term 'because the remedial activity that follows a possibly offensive act very often can be understood best by assuming that the actor has the worst possible readings in mind as that which he must respond to and manage' (p. 109). He elaborated on this virtual scenario by noting that the imagined act implicates a 'virtual offender,' who is most liable to be blamed for the possible offense, and a 'virtual claimant' who might be, or might have been, offended. Goffman added that the 'function of remedial work is to change the meaning that otherwise might be given to the act, transforming what can be seen as offensive into what can be seen as acceptable' (p. 109).

Goffman related the function of such remedial work to the classic sociological theme of solidarity: the encounter is sustained harmoniously as one party voices an apology for a possible offense and the other does more than excuse it by essentially negating the possibility *and* denying the need for an apology. A compact example of such remedial work is provided in a later paper on apologies by Heritage, Drew and Raymond (2019). The extract is from a phone call, in which Vera thanks Jenny for having hosted her grandchildren, who arrived to visit Vera at a time when she was not at home:

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From Heritage et al. (2019: 186)
1 Vera:   I'm sorry yih had th'm all o[n you [ J e n n y] like that]
2 Jenny:                        [.hhh  [↑ Oh don't] be s i l ]ly=
3 Jenny: =No: that w'z lovely it w'z a nice surpri[:se
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Read in terms of Goffman's analysis, in the first line Vera apologizes by articulating a 'virtual offense' —the possibility that she burdened Jenny with hosting her own grandchildren—and Jenny responds by discursively 'transforming' that possibility from being a burden to being not only acceptable but 'lovely' and a 'nice surprise.'

Heritage and Raymond deploy the familiar idioms of quantitative social science—hypothesis, operational definitions, taxonomies, and correlation measures—in an effort to 'test' Goffman's principle with an analysis of a collection of 102 apologies and 'apologizables' extracted from tape recordings of telephone calls.[12] To set up their analysis, they construct a spectrum of apology formats, ranging from a simple 'sorry,' through slightly more elaborate forms such as 'I'm

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[12] The collection of 102 instances was drawn from a larger collection of 'apologies' and 'apologizables' attributed to Gail Jefferson. See Drew et al. (2016: 3) for an account of the origins of the collection, and Drew and Hepburn (2016: 114–115) for further elaboration on the composition of the collection.

sorry' (explicitly mentioning the speaker in connection with the apology) and 'I'm terribly sorry' (upgrading with a modifier), and then to more expanded forms that include named offenses and/or accounts. They also deploy a classification of virtual offenses as either 'local' (internal to the immediate exchange, such as in a self-correction) and 'distal' (referenced to an event beyond the immediate exchange, such as in the above extract where Vera refers to an event in the recent past). The results, according to the authors, broadly confirm Goffman's principle with statistically significant correlations between (1) the elaborateness of apology formats and (2) the magnitude (measured in terms of the local/distal locus) of the respective virtual offense. However, they note that they did not find significant correlations for apology sequences that were expanded across numerous conversational turns. Such sequences apparently were complicated by local interactional contingencies and negotiations, a possibility that Heritage et al. (2019) explore in a later paper. In what follows, I will briefly indicate some of the conceptual problems and confusions that are evident in the 2016 paper, though both papers exhibit conceptual and interpretive problems.

An initial problem has to do with Heritage and Raymond's conception of apologies. *Apology* is a vernacular term, and apologies are widely familiar actions. Producing and recognizing apologies should require no special credential or competency for speakers of the English language and other languages in which they are commonplace. However, the collection that Heritage and Raymond analyse includes doubtful instances. Each of the instances presented in their paper includes the word 'sorry'. In an article in the same journal issue, Drew and Hepburn (2016) acknowledge that apologies sometimes do not use that word, but they also argue that it is 'characteristic of apologizing that, in contrast to most (all?) other 'speech acts,' apologies need to be made explicitly.' Accordingly, an 'apology requires a speaker to say 'I'm sorry …,' 'I apologize …,' or 'My apologies …,' or 'Many apologies,' or something from this pretty restricted class, using the words 'sorry' or 'apology' …' (Drew and Hepburn 2016: 117).[13] The latter possibility is especially pertinent to the simplest instances of 'apology', such as in the following instance where Giles prefaces a self-correction in lines 4 and 5 with 'Sorry':

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[13] Support of a kind for the essential role of these words in apologies is offered in a website (MindTools, n.d.) that provides a four-step instruction on 'How to apologize': Step 1: express remorse; Step 2: admit responsibility; Step 3: make amends; Step 4: promise it won't happen again. The MindTools.com website came up at the top of the first page in a Google search under 'Constructing apologies' (at the time, I was searching for reference information for the Heritage et al. [2019] article by that title). The website's elaboration of Step 1 ('express remorse') begins with the advice: 'Every apology needs to start with two magic words: 'I'm sorry,' or 'I apologize.' This is essential because these words express remorse over your actions.' This advice is linked to a psycholinguistics study by Scher and Darley (1997), which also relies upon Goffman (1971) and some of Heritage and Raymond's other sources. The relations between such formulaic instructions to naturalistic accounts of apologies is an intriguing topic, though one that I will not go into here.

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From Heritage and Raymond (2016: 7):
1  Henry:      But whether that would (0.6) Whether that w- (0.3)
2              that bra::nd, (0.9) would fit in to (0.6)
3              East Midlands's got to be highly debatable.
4  Giles:  -> .t.hhhh I think, of any ↓si:te, it would fit in.
5              (0.3) <eh- eh-Sorry. Of all sites at East Midlands
6              it would fit in. It's a well known, local ↓product.
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Heritage and Raymond (2016: 7) add that 'a bare "sorry"' also is used in 'other-initiated repair,' instead of, for example, 'What?,' and they argue that when used this way it 'accepts responsibility (Robinson 2006) for the problem it raises.' They illustrate this format with the following example:

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Heritage and Raymond 2016: 8):
1  Les:     Hello:,
2  Mum:     ↑Hello:::::, Christi:ne?
3          (.)
4  Les:  -> Sorry?
5          (0.2)
6  Mum:     Christi:ne?
7          (0.3)
8  Les:     Oh yeh. Sorry I couldn'hear you very [well Je]:m's m-
9  Mum:                                          [O h :]
10 Les:     m-[Jem's
11 Mum:      [Are the family o:ff?
12          (0.5)
13 Les:  -> SORRY?
14 Mum:     'Av your family gone o:ff?
```

Heritage and Raymond provide no further commentary on this instance, but their transcript highlights two 'bare "sorry"' examples in lines 4 and 13 to illustrate the point they had made. Both instances of 'bare "sorry"' in the transcript raise doubts for me about (1) why Heritage and Raymond count these utterances as apologies, (2) how they identify the corresponding virtual offenses, and (3) how they decide who is responsible for, or who accepts responsibility for, the offense. The lack of any commentary or background information about the relationship among 'Mum,' 'Les,' 'Christine,' and 'Jem' compounds the confusion about what sort of trouble 'Les' is flagging with the prompt in line 4. Even with the account in line 8, it seems unclear that Les is taking responsibility for the problem with

not 'hearing' what Mum was saying.[14] The second instance of a 'bare "sorry"') in line 13, solicits a repeat of line 11, with some changes such as the replacement of 'the' with the stressed 'your' and the insertion of 'gone.' If the repeat is also a repair, this begs the question of why 'SO<u>RR</u>Y?' is marking a 'virtual offense' and taking responsibility for it. The increased amplitude and stress on the word also invites commentary about what *that* might be about. Robinson (2006) provides a micro-functionalist treatment of a collection of sequences in which a party uses the expressions 'Sorry?,' 'I'm sorry?,' and 'I'm sorry. What?' to elicit repairs of the recipient's prior utterance. Robinson (2006: 145), on the basis of his own prior research on the topic, concludes that the word 'sorry' in such a position not only functions as a conventional token in an other-initiated repair sequence, but also as an apology that *indexes* a 'possible offense.' As such, it brings into play a search for an actual or virtual offense and expressions that assign or take responsibility for it, thus constituting an apparatus that operates in parallel with the techniques of repair.

Rieger (2017) criticizes the procedure of including such instances of 'bare "sorry"' in a collection of apologies, and objects that in many such cases the recipient does not respond in a way that exhibits an orientation to an apology or offense: 'I would contend that the absence of a response, the absence of an orientation to the action as an apology suggests that sorry does not function as an apology' (Rieger 2017: 562). In their subsequent paper, Heritage et al. (2019: 188) dismiss Rieger's argument by invoking Goffman's notion of 'virtual offense' as 'worst possible reading' of an action, but this takes us to an even more onerous conceptual problem.

The word 'offense', like the word 'apology', is a vernacular usage, and we might suppose that intuitive familiarity with what the parties express or display as offensive in the circumstances of these ordinary phone calls would be sufficient for identifying the relationship between apologies in transcripts and the offenses they implicate. However, confusion arises from the way Heritage and Raymond compose and sustain a 'corpus' with a lumping strategy through which a variety of 'troubles' associated with repairs (hearing, voice recognition, understanding,

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[14] Robinson (2006) focuses on the word 'sorry' in the context of an 'open-class' other-initiated repair (a next-turn-repair initiator that does not specify the trouble-source in the preceding turn), and claims that 'Sorry?' differs from alternative (or accompanying) terms such as 'Huh?' or 'What?' by *indexing* an apology and taking responsibility for the trouble, in contrast to sequences in which a recipient's trouble with hearing or understanding would be attributed to the speaker. However, Robinson's functional analysis appears to rest on a literal reading of the word 'sorry.' The account following 'sorry' in line 8 might be taken as an apology and account of the trouble with hearing that occasioned 'Sorry?' in line 4, but this is far less clear with the more emphatic 'SO<u>RR</u>Y?' In line 13, which (especially given the lack of background commentary) seems open to quite a range of possible readings. In brief, the relation between evidence and what it is evidence for appears to be tautological.

enunciating etc.) and 'apologizables' are grouped together under the category of 'virtual offenses' which implicate the responsibility of an offender.

In addition, a *virtual* offense is a different sort of animal than an action that provokes offense in others. A virtual offense is not an overt act that gives offense, or is taken as offensive by a recipient of, or witness to, the act. It is not the sort of action that can be read off the transcript of a phone call; at best, it can be inferred from an apology and the response to the apology in relation to the local context. As noted earlier, when Goffman uses the term, he imaginatively assumes the vantage point of an ideal-typical person making an apology, who 'has the worst possible readings in mind as that which he must respond to and manage' (1971: 109). The *possibly* offensive 'act' is imagined by Goffman on behalf of the individual apologizer, and the recipient of the apology does not necessarily have a reciprocal vantage point on the 'same' virtual offense. The 'virtual' offense shadows, and runs ahead of and behind, the actual interaction, and is subject to erasure without ever materializing.[15] As Goffman elaborates, the 'virtual claimant' (who may or may not have been offended by, or even alert to, the virtual offense the apologizer has 'in mind') responds by 'changing the meaning' of the virtual offense so that it is not, or is no longer if it ever was, to be seen as offensive, but instead is to be treated as acceptable or even laudable (such as in the exchange between Vera and Jenny in (1)). Accordingly, the 'offense' has a paradoxical empirical status: an offense *might* be implicated by the apology, but the response changes or entirely erases the counterfactual meaning that 'otherwise might be given to the act.' Moreover, when a self-correction prefaced by 'sorry' (such as in instance (2) above) receives no response that treats it as an apology, there is no empirical warrant for classifying 'sorry' as an apology or correlating it with an offense. In such a case, an *act* correlated with the apology and normatively accountable as an *offense* never occurs.

The conceptual confusions attendant to the way apologies and virtual offenses are defined and operationalized in Heritage and Raymond's study also confound the correlations the study produces. In a provisional summary of their results, Heritage and Raymond (2016: 18) say that 'the results so far support the notion that local and intersubjectively available virtual offenses are treated minimally, whereas distal virtual offenses, addressing matters outside the here-and-now of the interaction, accrue more substantial apologies.' However, in a two-party phone call, a virtual offense would be made 'intersubjectively available' by means of an agreement between the parties that it took place. But, as I understand what Goffman is saying, an apology is motivated by the speaker's cognizance of a *possible* offense, which the apology presents in an overbuilt way that regularly solicits

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[15] Robinson (2006: 143) reads Goffman to be proposing that 'persons constantly monitor acts, retrospectively and prospectively, for their virtual (or possible) offensiveness.' This virtual object is thus always available to 'emerge' or 'surface' in the interaction.

the virtual claimant's denial that an offense (or *that* offense) took place.[16] This begs the question of *just what* is 'intersubjectively available' in such sequences. Moreover, the correlations between (virtual) offenses and forms of apology presented in Heritage and Raymond's study do not seem surprising, given the lack of independence between the 'variables': for the most part, a non-participant's ability to infer a virtual offense through the analysis of a transcript depends upon the form of an apology.

## PROFESSIONAL ACTION CATEGORIZATION ANALYSIS (PACA)

The version of criticism implied in the characterization of constructive analysis by Garfinkel and Sacks and the challenges to social and cognitive science raised by Sharrock and his colleagues do not simply *deny* social scientific aspirations and claims. Instead, they take the form of inquiries, such as, 'How are social scientists *doing it* when they make out their studies to be scientific?' A more specific variant of such an inquiry is, 'How are social scientists *doing it* when they turn contextually used vernacular expressions into structurally regular, classifiable, and countable types of action in ordered collections of data?' Garfinkel and Sacks (1970) express the insight that indexical expressions are produced *in situ* as intelligible actions for masters of the natural language ('members'). However, they pair that insight with a critique of long-standing efforts in logic, philosophy, and social science to wrest indexical expressions from their locally accountable uses, and to turn them into formal operators and variables. In the past half-century, that insight and critique have been subject to a tragic history in which the very effort to pursue the project of investigating 'the rational properties of indexical expressions and indexical actions' has gradually degenerated into a variation on the theme of constructive analysis.

Membership Categorization Analysis (MCA), originated by Sacks (1972), and developed by affiliates of the 'Manchester School' (for example, Hester and Eglin 1997), explicates the way alternative characterizations of persons in ordinary language bring into play coherently associated predicates and inferences. Investigating the associations of membership categories and predicated actions is a way to cut into 'culture'. The conversation analytic research on apologies described in the present paper might be considered as an example of Professional Action Categorization Analysis (PACA) applied to fragments of recorded phone calls. This variant of constructive analysis involves a mode of 'counting as' (Martin and Lynch 2009): a classification exercise that establishes *what counts as* an instance of a collection, the members of which can then be enumerated as equivalent cases and

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[16] Drew and Hepburn (2016) do not explicitly deploy Goffman's concept of virtual offense, and they focus on instances in which the accounts accompanying expressions of apology tend to minimize blame for an offense or mistake.

further refined into subcategories. This work is facilitated by treating selected indexical expressions as *indexes* that facilitate the assignment of local actions to general categories. Garfinkel's *praxiological* indexicals, whose referential uses (which are by no means their only uses) are autochthonous to the occasion, are rendered into C.S. Peirce's *semiotic* indexicals, which lend themselves for use as generic indicators that enable specification of a referent *of some kind*, including in some cases referents that are implicit or absent (Drew and Hepburn 2016).

Counting is an elementary procedure in quantitative research, but it is not only a matter of assigning numbers to things. It is also a matter of categorizing things as relevant and equivalent objects: deciding what *counts*, for example as an 'apology' as part of building a collection and using the collection to identify and analyse further instances. In accordance with the operations of PACA, vernacular expressions 'sorry,' 'I'm sorry,' 'I apologize,' and elaborated forms expressed in particular structural slots *index* forms of apology that can be ordered along a gradient. An apology, in turn, *indexes* an offense, though not always clearly or pointedly, and takes responsibility for that offense (Robinson 2006: 145). The contexture of associations built around the vernacular concept of 'apology' ramifies to imply further acts of blaming, excusing, or absolving. The key to putting this contexture in place is counting a range of actions *as* apologies. The nominal domain of apology is expanded by including in that domain selected self-repairs and other-initiated repairs that include the word 'sorry,' each of which in turn is correlated with an 'offense' *of some kind*. 'Offenses' that otherwise might be deemed momentary errors, mistakes, malaprops, lapses, cut-offs, or disruptions and interruptions from background events such as a noisy child, are treated as structurally synonymous, with each case violating a norm, such as the norm of progressivity (Robinson 2006: 139), and bringing into play associations with possible offenders, responsibilities, exculpatory reasons and acts of absolution. The final, and most crucial, stage in PACA is, to recall Garfinkel and Sacks (1970: 339) on constructive analysis, to assign the results to 'actual expressions as their properties.'

## CONCLUSION: WHAT'S THE POINT?

Sharrock and Watson (1984) title their paper 'What is the point of "Rescuing Motives"?'[17] It is a question that has no answer: there is no point, because the vernacular concept of 'motives' does not require rescue. Similarly, we could ask, in light of what is argued in the present paper, 'What is the point of *criticizing* social science treatments of vernacular concepts such as motives and apologies?' In this case, however, I would argue that the question *can* be answered.

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[17] When Sharrock and Watson question the point of 'Rescuing Motives' they are questioning the point of the article by Bruce and Wallis (1983) with that title; here I am following up on the substantive (lack of) point they question.

The point of such criticism is not to suggest that tighter definitions, more refined collections of instances, or more sophisticated correlational measures would solve the conceptual and methodological problems with efforts to treat 'motives' or 'apologies' as objects of empirical scientific research. Although the arguments in this paper suggest that the quantitative approach taken in the Heritage and Raymond (2016) study is ill suited for investigations of recorded and transcribed conversational exchanges, the problems with conceptualization and operationalization can also be found in studies that eschew the 'positivistic' approach. Indeed, the follow-up study of apologies by Heritage et al. (2019: 185) proposes a 'constitutive' and 'reflexive' approach to the principle of proportionality, in contrast to the 'largely positivistic analysis' performed in the earlier paper, and yet the more recent study also relies upon the same collection, as well as the taxonomies and typologies used in the quantitative study. It retains the functional 'meaning' of 'bare "sorry"' utterances as apologies, and the deployment of the notion of 'virtual offense,' in an interpretive exercise that treats the principle of proportionality as a teleological operation for members and professional analysts alike.

At least for now, I will leave aside further discussion of the 'constitutive' and 'reflexive' approach announced in the more recent paper. The quantitative study by Heritage and Raymond (2016) should be sufficient for documenting conceptual confusions akin to those identified many years ago by Winch and others, and elucidated and elaborated during the past half-century in critical research by Sharrock and his collaborators from the 'Manchester School'. I have argued that these confusions continue to infect and confound research in sociology, so much so that even some of the most prominent figures in ethnomethodology and CA are not immunized from them. The point, then, is that complaints about conceptual confusion are more than high-flown arguments associated with a bygone era of Oxbridge philosophy, they continue to apply to the detailed methods and results of empirical study in sociology. There is persistent and fierce resistance to efforts to argue and demonstrate this point, as well as many incentives for abandoning it in favor of eclecticism and professional citizenship. In the face of denunciations for opposing 'science' and for stirring up discord, Wes Sharrock's work and life exemplify the courage to press the case against efforts to trade conceptual confusion for the imagery of a 'science'.

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