



Silence in Context: Ethnomethodology and Social Theory

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Abstract. Ethnomethodologists (or at least many of them) have been reticent about their theoretical sources and methodological principles. It frequently falls to others to make such matters explicit. In this paper I discuss this silence about theory, but rather than entering the breach by specifying a set of implicit assumptions and principles, I suggest that the reticence is consistent with ethnomethodology's distinctive research 'program'. The main part of the paper describes the pedagogical exercises and forms of apprenticeship through which Garfinkel and Sacks aimed to develop ethnomethodology as a practice. These efforts were not entirely successful, partly because ethnomethodological 'practice' required an engagement with other fully-fledged practices. Aside from the difficulties of mastering such practices, it was unclear what an ethnomethodological study would add to, or take from, them. Whether successful or not, ethnomethodological research points to the specificity of discourse and action in any given practice which a general theory is bound to misconstrue. Current disputes about cultural constructivist versions of natural science illustrate the problems that arise when the terms of a general theory are used to describe and evaluate specific domains of practice. The paper concludes by recommending ethnomethodology as a way to dissolve an unbridgeable gap between cultural theories and socially located practices.

During a colloquium in Summer 1975, Harvey Sacks presented a public lecture on the organization of a type of question-answer sequence.¹ Afterwards, a gentleman in the audience stood up and asked a question that went something like this: "If I put a gun to your head, and asked you to name the theorist who had the most influence on your work, who would you mention?"² Sacks was smoking a cigarette (which was permissible in the US at the time). He paused. With head down, and his cigarette at the lip of an ashtray, he held the pause while periodically flicking the ashes. This went on for a minute or two. The pause seemed endless at the time. To say it was a pregnant pause was not enough. This pause had time to give birth and raise a family. At long last, Sacks looked up and quietly declined to answer the question.

Sacks certainly succeeded in focusing his audience on the here-and-now. He also drew attention to a peculiar reticence which was anticipated by the question's fictive threat ('If I put a gun to your head. . .'). The question tried to force an answer, but, as Garfinkel (1967) has taught us, such interactional demands do not *make* us comply with them, even if they persuade us to think

that we cannot do otherwise. And, as Sacks himself has taught us, silence is an *accountable* mode of communicative action (Sacks 1992, Vol. 1: 101), and this silence surely was a vivid instance of such action. Sacks did not simply fail to answer the question. His long pause was not merely the absence of an answer. It was a *studied*, and instructive, silence.

What can we learn from such a silence? What is the point of a stubborn refusal, even in the face of an imaginary death threat, to honor a conventional scholarly question? Sacks died a few months later, and I do not know if he ever discussed this incident. I have no idea what Sacks actually was thinking at the time, but for the present purpose I see no need to explore his personal intentions or motives.³ Sacks's silence was a contextual move. It was intelligible only in relation to the demand that preceded it. It was part of a dialogue. Moreover, it was part of a dialogue in the very way it deferred compliance to the demand imposed by the interlocutor. The silence was not a positive speaking turn. Instead, it created a delay that exposed the unilaterally stipulated terms of the question. The utterance with which Sacks broke his silence demonstrated that the option to refuse remained relevant, despite the interlocutor's effort to impose a more restrictive order. Sacks did not simply fail to come up with an answer. His evidently studied silence and eventual refusal were courageously unconventional: they constituted an act of civil disobedience that exposed the impertinence of his questioner's demand.

From Theory to Apprenticeship

This incident is emblematic of ethnomethodology's distinctive, some would say insolent, attitude toward theorizing. By "theorizing" in this context I mean the work of constructing intellectual genealogies that commemorate notable authors and foundational writings. It is part of a broader effort to index empirical investigations to bodies of literature. The work of indexing is facilitated by scholarly efforts to identify abstract themes and topics, formulate propositions and postulates, articulate common problems, and ascribe assumptions and presuppositions to authors and schools. This work is more than a matter of encoding and decoding a literature. It also has to do with methodology: the use of criteria, decision rules, and models which tie research designs to scholarly traditions. The point of such endeavors is to isolate fundamental precepts and to construct intellectual histories for one or another literary tradition of social thought. There may be many examples of theories in the natural and social sciences that do not fit this summary definition, but I believe that it applies to prevalent styles of social theory and philosophy of social science. Ethnomethodologists do not always refuse the summons to theorize. There are times when virtually all of us find it necessary, and even interesting, to reconstruct literary genealogies and discuss how ethno-

methodological research bears on general sociological and philosophical topics (Button, 1991; Lynch, 1993). Sacks occasionally did so himself.⁴ However, I believe that no effort to trace ethnomethodology's research program back to philosophical, theoretical, and empirical antecedents can reveal what Sacks's refusal made perspicuous. He did not simply fail to reveal his sources. Instead, he refused to satisfy his interlocutor's presumption that such sources must reside in a scholarly lineage.

Ethnomethodology's silence in the face of the summons to speak theoretically – to identify itself with a literary tradition of social thought – all too easily encourages the view that it is atheoretical. I will argue that the rude refusal to articulate a set of assumptions or presuppositions, or to lay out a literary ancestry, is not motivated by a naive attempt to proceed without presuppositions. Nor does this silence represent an intellectually dishonest suggestion that ethnomethodologists somehow investigate the world without any preconceptions or prejudices about that world. Instead, in my view, the refusal is connected with an unusual, and increasingly unpopular, conception of literary knowledge. Far from being presuppositionless, the refusal to take up the summons to speak theoretically has to do with an uneasiness about the summons: "Tell us where your ideas come from; or, short of that, give us a hint so we can tell you where your ideas come from." The summons demands that "ideas" should be expressed in one or a few succinct sentences, and it presumes that current ideas come from related ideas expressed by notable writers of scholarly texts. For as long as the demand holds sway in the human sciences, ethnomethodology's refusal will simply create the opportunity for others to construct literary traditions on its behalf.

There is no shortage of attempts to link ethnomethodology to such traditions. Different scholars have linked ethnomethodology to virtually all of the major theorists in the sociological canon (Marx, Weber, Durkheim, Mead, Simmel, Schutz, and of course Parsons). Various expositors have traced the ideas, assumptions, and presuppositions in ethnomethodological writings to radical individualism, subjective irrationalism, behaviorism, operationalism, relativism, social constructivism, pragmatism, inductive realism, positivism, phenomenology, and analytic philosophy.⁵ It is difficult to imagine how ethnomethodology could be all of these at once!

Despite the confusion that has resulted from efforts to formulate ethnomethodology's theoretical position, it may seem reasonable to suppose that ethnomethodology *must* have some sort of coherent theory behind it. Garfinkel and Sacks explicitly reject general theory and turn to 'naturally occurring', 'actual', 'real-worldly' sources of insight and inference, but their writings and transcribed lectures exhibit a deep, wide-ranging, and thorough mastery of the very literary enterprises they counsel their readers to abandon. Garfinkel's best known publications are often sparing in their citations, but there is no mistaking his rich understanding of existential phenomenology and classic

traditions of social theory.⁶ Similarly, Sacks's (1992) transcribed lectures reveal a broad knowledge of analytic philosophy, law, literary theory, anthropology, linguistics, and social theory that is less explicit in many of the papers he prepared for publication (e.g., Sacks 1972). Garfinkel's and Sacks's tendency to de-emphasize theory has had the paradoxical effect of inviting others to expound upon their evident, but unsystematically reported, literary sources. But, while careful exposition of those sources may help clear up some of the confusion, it is likely not to answer the question of why Garfinkel, Sacks, and many other ethnomethodologists turned away from theory.

All too often, the ethnomethodological turn away from theory is treated as a classic empiricist gesture, expressing a desire to find out about particular matters of fact without being prejudiced by theoretical speculations. Ethnomethodologists as well as their critics often suggest that the research is a straightforward inductive endeavor: a matter of going out and looking before deciding what the real world might be like. For example, during a transcribed discussion among a group of ethnomethodologists and sociologists, David Sudnow asserts that an ethnomethodological "finding" has to do with "the nature of the world" and not "the nature of the procedure" through which the world is observed. He goes on to say, "[i]f the world, in fact, is general, it is general whether you get it by induction or however else you might get it. It is the world that provides the adequacy for the generalizations you can make about it and nothing else" (Hill and Crittenden, 1968: 86). Some of the general remarks in Sacks's lectures and writings also seem to advocate an inductive procedure that begins with "unmotivated" examinations of data (Sacks, 1984; 1992, Vol. 1: 802–05; see Lynch and Bogen, 1994). Taken in isolation, such remarks may suggest an unquestioned faith in the world's capacity to speak for itself. (In this case the idea of the world speaking for itself is no metaphor: the "data" are recorded conversations.) Induction has fallen out of favor in 20th century philosophy of science. Much-abused slogans from philosophy of science about the "theory ladenness" of observation and the "under-determination" of theories by facts are frequently cited to suggest that any attempt to assign priority to an independent "real world" is a naive and disingenuous expression of "the view from nowhere". Mention of the problems of induction can licence equally naive and disingenuous views, but these days anyone who responds to the summons to speak theoretically by proposing that the real world, and not a literary tradition, is the primary source of ideas is liable to provoke knowing winks and muffled laughter among the cognoscenti. It is no less risky to assert, for example in defense of conversation analysis, that the raw data are extra-textual conversations, and that transcriptions, collections of transcribed excerpts, and written analytic descriptions are transparent media through which naturally occurring interactions are represented. It can be difficult indeed to express uneasiness about the dominant literary mode of theorising, or to express a preference for deriving ideas from

the world, without seeming naive or evasive. Note, however, that Sudnow's remark "whether you get it by induction or however else you might get it" suggests a casual, uncommitted attitude toward inductive procedure. I would say that this indifference extends to deductive, abductive, transductive, and seductive procedures as well. (Perhaps an exception would be made of the last alternative.) Practical experience of the "real world" has a leading role in ethnomethodology, but what Sudnow's remark leaves unspecified is that such "experience" should not be equated with the more restricted methodological concepts of observation and experiment, and the "real world" is the everyday life-world and not the analytical world of a science. It is exceedingly difficult to support the claim that ethnomethodology is an original kind of experiential investigation, in the face of incessant efforts to force its program into theoretical and methodological pigeonholes.

Ethnomethodology has become increasingly fragmented over the past four decades (Maynard and Clayman, 1991), and it is doubtful that it ever was a tightly unified research program. Perhaps it is appropriate for Garfinkel to speak on behalf of ethnomethodology's program (Garfinkel, 1996). It is, after all, his baby. But, since few people can fathom Garfinkel's writings, and even fewer have consistently followed his teachings, it is only fitting that the rest of us should qualify the way we represent ethnomethodology. Accordingly, I will discuss ethnomethodology's turn away from theory by recounting how I came to understand ethnomethodology's program. I am sure that others understand it differently.

In the early 1970s when I started studying ethnomethodology at University of California, Irvine and UCLA, Garfinkel and Sacks had distinct research programs, but their pedagogies shared at least one feature: they both instructed novices by requiring them to perform empirical, analytical exercises. The exercises differed. Sacks's tended to be more focused on collecting and analysing specific types of conversational sequence and addressing technical issues like why recipients of compliments tend to refuse them.⁷ Garfinkel's exercises were more wide ranging. In one seminar he asked students to take a part time job (any kind of job would do) as a prerequisite for addressing issues in the study of work and occupations⁸; and several years later he asked another group of students to volunteer as helpers or guides for disabled persons, as a prerequisite for gaining insight into the organisation of embodied action.⁹ Another exercise consisted of tape recording and attempting to transcribe a noisy assemblage (e.g., a party in which many conversations go on at once). Other exercises which he assigned over the years can be grouped into families. One family of exercises required students to describe, diagram, and take photographs of service lines (queues), automobile traffic, or pedestrians at a busy street crossing. Another involved descriptions of the experiences and troubles that arise during efforts to follow formal instructions: "occasion maps" (sketch maps showing how to drive to a particular destination); manuals of

all kinds; and rules for playing games.¹⁰ Some exercises were done in the seminar itself. For example, Garfinkel sometimes dragged out a battered old recording device with headphones. This was an audio feedback delay machine. A person using the device would wear the headphones and speak into a microphone. The device delayed the auditory feedback of one's own voice, so that as you were speaking, you heard through the headphones what you had just said a moment ago. The disruptive effects on the fluency of speaking were dramatic and sometimes hilarious. Another device he often used consisted of a welder's mask outfitted with a tank prism purchased through an army surplus supplier. This acted as an inverting lens, and Garfinkel invited his students to wear the device while trying to walk and write on the blackboard. Again, the results were often very amusing, as students struggled with the now-disrupted spatial contingencies of embodied action.

Garfinkel lectured at length about topics associated with the exercises (traffic, queues, perceptual disruptions, etc.), and he alluded to a background of classic social theory, existential philosophy, gestalt psychology, experimental social psychology, and many other fields. In connection with traffic and queues, he mentioned ethnomethodology's radicalization of Durkheim's (1964) conception of social fact; in connection with the inverting lens and some of the other exercises, he advised students to "misread" the phenomenological writings of Merleau-Ponty (1962) on the "intertwining" of world and body, and of Heidegger (1967) on the "equipmentality" of the world. Such "misreadings" were to be performed by imagining that the philosophical texts provided practical instructions for doing, and making sense of, particular ethnomethodological exercises.¹¹ Rarely, however, did Garfinkel go into detail about just what his students should read and how they should interpret the relevant texts. Students had to work out for themselves what the point was to the exercises they were currently doing. Some of the students who persisted eventually found their own way, and some even devised their own exercises, but others remained terminally puzzled.

I cannot speak very authoritatively about how Sacks's students responded to his pedagogy, though it is clear that they were very devoted and that several of them went on to do original, interesting work.¹² One aspect of his pedagogy was very clear, however: technical action came first, scholarly understanding afterwards. Although he expected his students to educate themselves, the relevance of what they read was established by the technical phenomena under investigation. This order of relevance is evident in the organization of Sacks's (1992) transcribed lectures. Most of the lectures are about specific interactional and rhetorical phenomena. He brings in themes from sociology, linguistics, anthropology, ethology, social psychology, literary studies, and other fields in a sporadic and often surprising way, but he devotes comparatively little time to systematic reviews of such literature and general discussions of theoretical and methodological issues.

Garfinkel's pedagogy in its own way was designed as an apprenticeship system in which novices would be given courses of training. Garfinkel lectured at length, and at a level of abstraction that left many students bewildered, but he repeatedly alluded to a mastery that could only be gained by working with materials. Relevant 'materials' were discoverable: they included, but were not limited to, tape recordings and transcripts, written texts, collections of photographs, field observations, and demonstrable performances. Garfinkel's allusions to a material practice were partial, suggestive, and never definitive. In the immediate situation of Garfinkel's seminars students were challenged to master a more discursive practice: how to talk and when to remain silent. They were nudged, cajoled, berated, admonished, ignored, insulted, encouraged to say more, and effusively praised, and often they had little idea about why their remarks provoked the reactions they did at any given time. In many respects they were trained and not taught, and it was not always clear what they were being trained to do or say.

Practice

Garfinkel and Sacks, in different ways and with differing success, undertook to initiate a practice that was fundamentally different from existing social science methods. They de-emphasized abstract theory and scholarship, and stressed the necessity to *do* studies. Their notions of practice differed from the currently fashionable interest in the social sciences with devising theories of practice,¹³ because *practice* was not just a topic of explanatory interest, it was the primary basis for attaining an ethnomethodological mastery. Largely due to the efforts of Gail Jefferson, Emanuel Schegloff, and others of Sacks's colleagues and students, conversation analysis has become a sustained and relatively coherent practice in its own right. The same cannot be said for Garfinkel's practice. Garfinkel's research practice (as well as his interest in practices) required the mastery of other practices; many of which could not be learned by studying sociology or any other academic subject.¹⁴ Especially when the practice under study required extended training in its own right, such mastery was not easily reconciled with the demands of a departmentally based social science career.

The coherence of Garfinkel's approach did not arise from his own theorising, or from his students' use of a common method described in Garfinkel's "manual" for the study of naturally organized ordinary activities.¹⁵ Instead, the coherence arose from an injunction to treat methods as "discoverable topics of order". This injunction is made explicit in Garfinkel and Wieder's account of ethnomethodology's (EM's) methods:

Since EM's methods are discoverable phenomena of order* in their own right, they are not methods as methods are "straightforwardly" understood.

The fact that EM's methods are discoverable phenomena of order* in their own right is not, however, a mere nicety or convenience, or simply an automatic EM response to any topic, problem, or issue (a sort of standard EM turn or twist). The fact that EM's methods are discoverable phenomena of order* in their own right is central to EM's treatment of methodogenesis, the relation of EM methods to EM knowledge, its position on phenomena of order* as prior to and independent of their EM study, the place of competence as a reflexive constituent of the "work" to which that competence is addressed, and more. (Garfinkel and Wieder, 1992: 181)

The asterisked "order*" is a way of glossing a programmatic aim to "respecify" an open-ended list of "classic topics of order" (for examples: structure, meaning, action, logic, and method itself) so that the topics are regarded as singular accomplishments in local scenes of action. The phrase that Garfinkel repeats like an incantation – "EM's methods are discoverable phenomena of order* in their own right" – alludes to an extreme, and rather convoluted, participant-observation requirement that he terms "unique adequacy". A conceptually simple, though often practically onerous, aspect of this requirement was that Garfinkel's students would have to become "vulgarly competent" with the methods they were studying.¹⁶ Not only would they have to learn by doing, but their mastery itself would provide the basis for, and subject of, their investigations. This requirement presented no initial difficulty for studies of competencies the students had already mastered, but it did create a demanding prerequisite for those of us who aimed to investigate specialized practices in mathematics, law, and the natural sciences (Livingston, 1986; Burns, 1986; Baccus, 1996; Lynch, 1985). In a different way it was a problem for those who investigated the competencies of persons with severe disabilities (Goode, 1994) and members of exotic cultures (Liberman, 1986).¹⁷

The technical difficulty of the methods that Garfinkel's students investigated was not just a challenge for individual students. Given the diversity of the methods and the difficulty of explaining them to non-practitioners, Garfinkel and his students collectively faced the problem of articulating what the different studies had to do with one another. Presumably, they were all "ethnomethodological" studies. But what did *that* mean? Unlike other ethnographic programs in sociology and anthropology, *by design* ethnomethodological research was not unified by formal descriptive models or methodological protocols. Although Garfinkel's students aimed to attain "ethnomethodological" mastery, the different practices they set out to master had their own established pedagogies. The students were left to wonder (and many of them did wonder) if there was a common practice they were being trained to master *together*. As he does in his more recent writings (Garfinkel, 1991: 14; Garfinkel and Wieder, 1992; Garfinkel, 1996), Garfinkel would allude to an extensive "corpus" of studies developed from "the work of an international and interdisciplinary company". But, as many of Garfinkel's students

came to learn (sometimes with amusement, at other times not), the volatility of Garfinkel's relationships and the reach of his fertile imagination had a great deal to do with the ever-changing composition of the company and its corpus. Garfinkel's allusions to technical apprenticeship were largely the expression of hopes running well ahead of the founding of a common practice, and it was never fully clear that there ever could be such a practice.

When viewed with hindsight, it is not surprising that Garfinkel's company was difficult to keep intact. Some of Garfinkel's students disappeared into the worlds they studied, while others struggled to sustain academic careers as specialists on the ethnomethodology of science, law, education, medicine, truck driving, Tibetan argumentation, and other substantive areas. I cannot speak for those who disappeared, but for the others Garfinkel's teachings provided an uncertain, never quite clear, and yet illuminating resource for critically engaging established social science fields concerned with science, law, medicine, occupations, language, religion, etc. But ethnomethodology was not a stable platform for developing social science careers. Garfinkel's ambivalence about the merits of the academic career as a basis for disseminating his practice was the kiss of death for some of his students, and those of us who survived in the academy had to find help from other quarters.

Ethnomethodology Ground Zero

Ethnomethodology's turn away from theory and its divestiture of method leaves us with a rather empty discipline. Taken to a radical limit, the unique adequacy requirement empties ethnomethodology of every possible general methodological rule, analytic procedure, or evaluative criterion, because all of these become discoverable as endogenous properties of the substantive methods ethnomethodologists study. Moreover, the community of ethnomethodologists disperses, as its members become integrated with other epistemic communities. A simple answer to Pollner's (1991) question "What's left of ethnomethodology?" would be "Nothing's left! Everybody left the field and went somewhere else." We have reached ethnomethodology ground-zero. But what kind of nullity is this? Clearly, it is not a Cartesian point of certainty. Nor is it a nihilistic denial of the possibility of knowing anything at all. Instead, the emptiness is a logical consequence of a deep, vivid, and self-consistent conception of social order. Despite his disclaimers about theorizing, Garfinkel again and again enunciates a comprehensive vision of how "the ordinary society" organizes itself. It organizes itself through its members' use of methods of all kinds, and the stress is on *all* kinds: formal and informal, tacit and explicit, expert and ordinary, efficient and inefficient, rational and non-rational, methods for analyzing other methods, etc. and etc. It is a paradoxical vision, because the society's methodic operations include all of the practical

and intellectual resources that sociology might want to claim for its own. The vision projects a picture, but it does not deliver a foundational theory that sets up a coherent program of academic research. Ethnomethodology's "program" is everywhere else. Ethnomethodology finds its themes in the classic traditions of social science, but relocates them where they always already belonged: the practical activities and investigations that make up the ordinary society. Nothing is left for ethnomethodology, because nothing is left over from the ordinary society's incessant operations. Any uniquely adequate study will already be incorporated into the methodological program in which it is situated, and it is doubtful that any proceeds will accrue to ethnomethodology.

Ethnomethodology ground zero is an imaginary point. It is a pure extrapolation. Given the fact that ethnomethodology has a name, a history, a literature, and a loosely affiliated gang of people more or less identified with it, it should seem obvious that there is something more to it than I have let on. Short of articulating a full-blown theory or a pure methodology, perhaps it would be possible to take an inventory of the various themes, exemplary cases, research maxims, historical landmarks, rules of thumb, characteristic slogans and tag lines, buzzwords, disputes, and other quirks and stigmata that contribute to ethnomethodology's identity. I will not undertake the task here, but I believe that an inventory of this rag-tag assemblage would give a more realistic account of the practice than would any account of its program. Whether by accident or design, ethnomethodology exists as an academic "thing" as well as a word that refers to the countless methods of the ordinary society. Its existence does not imply a unifying theory or method, nor does it call for an effort to construct one.

So what can we say *about* this academic "thing"? Perhaps we can begin by speaking of an *attitude*. The notion of attitude will be inadequate for this purpose if it is understood to refer to a general cast of mind, but it may help us gain an initial appreciation of how ethnomethodology is other than a method or theory. Recall that Alfred Schutz (1964) contrasts the attitude of daily life with the attitude of scientific theorising. He portrays the latter attitude in classic terms: a detachment from everyday, practical concerns, and a suspension of everyday beliefs unless and until they are compatible with a corpus of disciplinary knowledge. The canons of social science method promote a similar attitude: value freedom, open-mindedness, analytic detachment from ordinary interests and inhibitions, and so forth. This attitude is promoted in naturalistic as well as hermeneutic approaches in the human sciences. It is rejected by many social scientists today who advocate critical theories, and situate their "epistemologies" with one or another socially located "standpoint" (Smith, 1992; Harding, 1996). Ethnomethodology is not a critical theory, and so it might seem by default to be another objectivist program; just as ethnomethodology's turn away from theory may seem by default to imply that it is an empiricist program. I would like to claim otherwise – that ethno-

methodology is none of the above – but it remains difficult to establish this as a recognizable option.

If there is a characteristic attitude associated with ethnomethodology's practice it is not encompassed by the familiar analogies of an attitude toward a thing (objectivism), a machine (mechanism), a text (understanding), or the status quo (critical theory). Ethnomethodology's attitude is prescribed under the rubric of "ethnomethodological indifference". The term "indifference" may seem to suggest a detachment from everyday pragmatic concerns, and to some extent this is a correct understanding. When studying, for example, the orderly production of automobile traffic, an ethnomethodologist examines how traffic patterns are "achieved" by local cohorts of drivers. This differs from trying to determine if specific orders of traffic are safe, efficient, rational, or democratic. Similarly, when studying conversation, ethnomethodologists investigate the production of routine sequential orders; they do not as a matter of policy set out to identify mistakes, offences, or imbalances, except in so far as they are locally accountable as mistakes, offences, or imbalances. The attitude of indifference is not the same as a value-free or value-neutral posture. Contrary to the classic Germanic notion of value freedom, ethnomethodological indifference extends to the conceptions of scientific rationality that social scientists customarily claim as neutral grounds for describing and (re)evaluating the actions observed in a field of conduct. There is nothing heroic about indifference. It does not require an effort to purge the soul of all prejudice, or the performance of a technique that controls or rules out sources of bias. It is not a matter of freeing oneself of mentalities that are inherent in an ordinary situation; instead, it is a matter of explicating such situations with a full attention to their ordinary accountability. In other words, ethnomethodological indifference is not a matter of *taking something away*, but of *not taking up* a gratuitous "scientific" instrument: a social science model, method, or scheme of rationality for observing, analyzing, and evaluating what members already can see and describe as a matter of course. The main difficulty associated with ethnomethodological indifference is convincing sociologists that the questions and topics ethnomethodologists take up are worthy of attention. Indifference is a kind of objectivistic attitude, but it is misleading to compare it with the more familiar versions of objectivism. The idea is not to describe social objects *as though* they were subject to physical laws or governed by mechanisms, but to come to terms with *just the sorts of thing they are* for those who routinely produce and recognize them. There is no reason not to treat an embodied gesture, a greeting sequence, a traffic jam or a service line as an object, but the difficult task that lies ahead is to discover and describe how this object is produced. The "how" is an *achievement* in action, of action, and as action.

Ethnomethodological and Cultural Studies of Science

Unique adequacy and indifference together constitute a strong version of the ethnographic requirement to understand local languages and ways of life from a native point of view. However, ethnomethodologists do not elicit this “native point of view” by making use of an independent matrix of social science concepts and methods. The “native point of view” is not *represented* with a cognitive map or other social science construct. Instead, all that counts as investigations – methods and analyses, *and* judgements of their adequacy – become endogenous cultural accomplishments. This is particularly evident in ethnomethodological studies of science and mathematics (Garfinkel et al., 1981; Lynch, 1985; Livingston, 1986), because in these cases the objects of research are themselves research activities. The sciences already attend to their methods and develop criteria of adequacy, which, to a considerable extent, provide models for the social sciences to emulate.

Earlier, I discussed the hazards associated with ethnomethodology’s apparent lack of a normative or epistemological standpoint independent of the practices investigated. Few participants in the currently popular field(s) of cultural studies suffer such hazards. However, they suffer reciprocal hazards that stem from efforts to establish critical and skeptical vantage points from which to expose the “construction” of scientific facts and laws. By juxtaposing these hazards with the hazards I attributed to Garfinkel’s unique adequacy requirement, we can perhaps gain some further insight about the peculiar condition of the social sciences.

Especially when investigating “privileged” or “authoritative” modes of knowledge production, proponents of cultural studies insist that it is necessary to assume an extrinsic or marginal point of view from which to make visible the construction of “nature”. Members embedded in the privileged communities presumably fail to recognize the extent to which “nature” is their own product. Sometimes, programmatic statements to this effect modify familiar Marxist lines, so that relations of construction become “the real relations of humans with each other and the natural world” which only become visible by means of a critical intervention (Hartsock, 1983: 159, quoted in Harding 1996: 148). In the more explicitly critical studies, feminist historians and other cultural analysts confidently expose ideological bias (Keller, 1992) and limited rationality (Fuller, 1993) among scientists. Other studies (generally associated with the sociology of scientific knowledge (SSK), rather than the more politicized field of cultural studies) avoid imputations of error and blame in favor of “symmetrical” accounts of alternative possibilities (Bloor, 1976). Critics of SSK tend to link “symmetry” to philosophical relativism (Laudan, 1981), while proponents emphasize the way it sets up empirical case studies (Bloor, 1981; Collins, 1983). In virtually all social and cultural studies of science, philosophical arguments are used to create space for social or cultural

explanations. So, for example, it is commonplace to read that empirical data do not compel the acceptance or rejection of particular theories; that all scientific observations are “theory laden”; and that scientific research is “messy” and “contingent”, contrary to purified textbook versions of methods and findings (Latour and Woolgar, 1979; Knorr-Cetina, 1981; Pickering, 1984; Pinch, 1985). Such general claims are not meant to suggest that all scientific research is erroneous; instead, they suggest that alternative theories and interpretations are always possible, and that social circumstances provide non-rational conditions for accepting some theoretical alternatives and ignoring or dismissing others.

Such social and cultural explanations of science have been controversial for decades, but until recently the main critics were philosophers. In the past few years, a number of prominent natural scientists have denounced social and cultural constructivist studies of science (Gross and Levitt, 1994; Sokal, 1996; Weinberg, 1996). The controversy is sometimes given the label “the science wars”. Social and cultural studies which claim that practicing scientists are incapable of comprehending the limits of their own rationality or gaining insight about their “real” social and natural relations, are the prime targets of indignation. The more prominent criticisms by Paul Gross and Norman Levitt (1994), Steven Weinberg (1996), and famous hoax perpetrator Alan Sokal criticise cultural constructivists for suggesting that the laws of gravity, the structure of the DNA molecule, measurements of the speed of light, and so forth are arbitrary, uncertain, or insignificant. Richard Dawkins (1994), for example, says “show me a constructivist in an airplane at 10,000 feet, and I’ll show you a hypocrite,” suggesting that constructivism implies a disbelief in the reality of the various physical laws, constants, and empirical measurements that go into the design and operation of aircraft. According to the accusation, cultural constructivists deny the significance of nature and rationality in science, and insist that all facts and laws are “cultural” in origin. Gross, Levitt, Dawkins, Sokal, and Weinberg especially are bothered by claims to the effect that scientific knowledge is a product of a privileged collective of white male Europeans. While these scientists acknowledge that constructivist claims are not always consistent from one case to another, they object to the insinuation that scientists are (to borrow Garfinkel’s language) “cultural dopes” and that nature itself is a docile cultural projection. Defenders of cultural studies have objected to such one-sided interpretations of their subtle ideas (see several of the articles in Ross, 1996), but it is fair to say that the debate is a struggle over the use of cultural categories. It may seem natural for the professional anthropologists and other scholars who identify with the cultural studies movement to claim special expertise in the arts of cultural analysis. After all, “culture” is the explicit subject of their specialized field. Consequently, the physicists, biologists, and mathematicians who object to cultural studies may come across as amateurs in the game of cultural critique. Indeed, Gross and

Levitt's (1994: Chapter 3) sarcastic "cultural construction of cultural constructivism" is an explicit mock up of a cultural studies critique (Lynch, 1996). But, from a different point of view, while they may not write excellent contributions to the cultural studies literature, practicing scientists *are* expert users of cultural categories. Moreover, they are experienced investigators of the complex relationships between "cultural" and "natural" orders. To establish how this might be so, permit me to refer to some of my research on the subject of laboratory artifacts (Lynch, 1985). When conducting research on day to day practices in a neurosciences laboratory in the 1970s, I noticed that practitioners often discussed, openly worried about, and practically investigated what they called "artifacts". At the time, these phenomena struck me as interesting. The concept of artifact seemed to resonate with the themes of "archaeology of knowledge" and "social construction" which, then as now, were subjects of intense interest in the human sciences. There seemed to be more than just a pun relationship between the research artifacts of interest to biologists and the historical artifacts unearthed by archaeologists. In both cases artifacts are material residues that bear the traces of a hidden culture. There also are obvious differences: for archaeologists, artifacts are positive discoveries of the traces of ancient cultures, whereas for biologists, artifacts tend to be negative discoveries of traces of more immediate origin (they are more like scratches on the surface of a clay tablet engendered by a careless archaeological assistant than the traces of an original inscription). Laboratory work routinely included inquiries into the real and constructed properties of scientific data displays, and artifacts were frequently identified during such inquiries. Research artifacts differed from the various tools, instruments, scales, texts, and other objects lying around a lab, all of which would be called artifacts under a general definition of the term. Research artifacts were specific – sometimes unnoticed, contentious, or equivocal – properties of research materials that were deemed to originate from technical mistakes, instrumental defects, interference, noisy backgrounds and irrelevant sources of signal. Such artifacts were inadvertent products of work in an instrumental complex. Some were annoying but relatively unproblematic cosmetic defects (for example, opaque blotches in an electron micrographic field resulting from a stain deposit), whereas others were more difficult to distinguish from the visible features of possible biological phenomena.

My study of artifacts often took the form of tutorials in which laboratory staff members showed me how to recognise common types of artifact, explained possible sources of them, and outlined precautions for avoiding them. I learned that a common way to challenge a colleague was to suggest that a purported biological feature might be an artifact of the preparation. (I also learned that such challenges sometimes were countered by acknowledging that the researches already had taken the particular possibility into account.) Some challenges took the form of general sceptical arguments ("How do you

know that this evidence was not due to something other than you say it is?”) and responses were sometimes equally general (“Well, everything we see with the electron microscope is an artifact. What is the point of your question?”). Such exchanges occasionally resembled general epistemological debates about reality but they were situated in a very different context of inquiry.

I was tempted to treat practical inquiries about artifacts as situated expressions of a global metaphysical distinction between objective and subjective conditions of observation. I also was tempted to think that the possibility that artifacts remained undetected in the data made every substantive biological claim *problematic*. And, finally, I was tempted to say that the closure or prevention of controversy depended upon rhetorical efforts to hide or delete any reference to the potentially “messy” origins of apparent results. In other words, I was tempted to develop a social constructivist argument. But, since such an argument would neither be original (cf., Latour & Woolgar, 1979; Knorr-Cetina, 1981), nor correct from my point of view, I tried a different tack.

The problem with opting for a social constructivist argument was that a general distinction between constructed artifacts and unconstructed nature would not recover the specificity of the laboratory work I witnessed. In order to understand what electron microscopists were calling “artifacts”, I subjected myself to the laboratory’s pedagogy. I read research articles and practical microscopy texts, and I watched practitioners at work while questioning them about what they were doing. Such reading and conversing were part of an ethnography, but for the most part they involved ad hoc and idiosyncratic tutorials in practical biology. I was not instructed on the difference between natural and constructed objects in general. Even if one supposes that the lab practitioners were negotiating a boundary between global ontological domains of nature and culture, the actions I witnessed and the instructions I received were not about the nature and meaning of “reality” as such. Instead, they were about singular features of electron micrographs and the possibility that such features arose from hidden practical and material sources. The ontological possibilities were circumscribed, complicated, and inseparable from familiar materials, instruments, persons and practical histories.

So how were the tutorials I received in the lab culturally significant, if not as support for general constructivist interpretations? One way they were interesting was as a source of insight about how constructivists get themselves into trouble by using general themes from sceptical philosophy to interpret situated usage in laboratories. An analogy suggested by Sharrock and Anderson (1991: 51) can help to clarify the difference between such circumscribed questions about reality and a more general skeptical inquiry: “The epistemological sceptic, who denies that we can ever really know anything, has no interest in getting into dispute with someone who, say, claims to know where to find a good Chinese restaurant in a strange town.” It also

might be said that general philosophical arguments are not likely to be very useful for persons looking for restaurants (with the possible exception of the groups who gather for dinner after academic conference sessions).

Tutorials in the laboratory did not lead me to develop a new and improved theory of scientific practice and culture (cf. Pickering, 1995). Instead, studying the nuances of talk and action in a laboratory led me to appreciate the confusion generated by proposals about “construction” in science (Lynch, 1982; Button and Sharrock, 1993). Not only did general claims about the construction of facts strike me as imprecise, they seemed misleading and even absurd. When used in contexts of argument in which talk of “artifacts” signalled an effort to get rid of the bugs and glitches that mess up the data, claims to the effect that all facts are artifacts seemed pretentious: “Are you saying that the bugs and glitches have taken over the universe?”

In cultural studies of science, the conduct of scientists and the relations between the contents and cultural contexts of science are *subjects* of study. The researcher deploys interpretative resources which are designed to expose cultural “meanings” and “practices” which practitioners often fail to acknowledge or recognize. Not surprisingly, practitioners have been known to object that the cultural critics are attempting to dispossess them of their expertise. In ethnomethodological studies of science, scientific practices are more than an object: they are a potential *source* of tutorials about *relevant* contexts of scientific work.¹⁸ Instead of treating the elements of a social scientific method (e.g., a model of rationality, an investigative procedure, and a set of definitions and evaluative criteria) as resources for critically examining historical or contemporary usage, ethnomethodologists treat a given community’s practices as discoverable sources of theoretical and methodological instruction. This possibility arises from the ordinary fact that the social world consists not only of practices, but of associated pedagogies and tutorials. Or, to put this another way, practitioners and their activities not only furnish information for social scientists to interpret, they provide tutorials about relevant topics, distinctions, reflections, analytic procedures, and contextual interpretations.

Conclusion

I started this paper with an anecdote about Harvey Sacks’s conspicuous silence in response to a theoretical question. I construed his silence as an act of civil disobedience: a refusal to honor theoretical ancestors. His refusal was consistent with a more general effort to fashion ethnomethodology as a practice. This practice involves a kind of empiricism, but not the familiar kind. Like other empiricist endeavors, ethnomethodology tends to be anti-scholastic: it treats the world it studies as the primary source of its theoretical insight.

However, contrary to a more familiar empiricist view of the world as an independent repository of facts, the world for ethnomethodology is a source of theoretical tutorials. The world is not a barren landscape of forms and mechanisms, but a life-world inhabited by competent masters of the vernacular distinctions and themes that social theory turns into topics of learned discourse. Ethnomethodology's turn to worldly sources is not a turn away from ideas in favor of things. Instead, it is a turn toward worldly sources of instruction on the recurrent topics of social theory and philosophy. To exemplify my argument, I contrasted two different orientations to scientific culture: a cultural studies approach which investigates the hidden cultural origins of scientists' objective claims, and an archaeology of artifacts which describes scientists' investigations of the local cultural origins of specific research objects.

When presented in stark opposition to each other, cultural studies and ethnomethodology are mutually discrediting. Where ethnomethodology refuses to claim an independent expertise or normative grounding from which to review and evaluate other modes of investigation, cultural studies antagonize practitioners with rival analyses and criticisms launched from analytic, normative, and/or political vantage points. At the present time, cultural studies is a growing movement with hopes of becoming a vibrant academic field and source of social intervention. By contrast, ethnomethodology is in dismal shape. It hangs on in the academy, and has a small foothold in a few corporate research centers, but it is not the source of interest and controversy that it once was. Presidents of the American Sociological Association no longer take the trouble to denounce it. However, it is premature to consign ethnomethodology to "ground zero". Ethnomethodology originally secured its place in the social sciences as a counterpoint to modern studies of language, culture, and society. Ethnomethodologists stood apart from ambitious disciplines that sought to evaluate other disciplines. They remained indifferent while others attempted to transcend particular contexts, create metalanguages, formulate tacit rules, expose "real relations" beneath apparent rationales, and unearth deep desires masked by explicit denials. Ethnomethodology's indifference to all of these auto-authorizing moves may seem to leave it with nothing to say. But as long as others keep persisting with such moves, ethnomethodology's silence will speak louder than words.

In the context of the "science wars" ethnomethodology has more to offer than silence. It might even have some therapy to offer for both sides. From what I have said thus far about the unique adequacy requirement and ethnomethodological tutorials, it might seem to follow that ethnomethodologists would be on the side of scientists like Gross and Levitt in their debates with constructivists. This is not necessarily so. The laboratory tutorials I discussed earlier were not sessions in which I sat at the feet of the masters and asked them to explain "science" to me. Instead, within the limits of my technical understanding and the practitioners' tolerance of my presence, I

attempted to get access to vernacular exchanges and commonplace scenes of laboratory practice. Practitioners sometimes did speak in general terms about observation, experimental design, sources of uncertainty, and questions about reality. They also expressed strong opinions about a whole range of scientific, political, and cultural topics. But much of what they said and did was related to singular steps in a preparation, specific configurations of data, and other immediate communicative and practical contexts. Instead of talking *about* experimentation, observation, and measurement, they *performed* experiments, *made* observations, and *took* measurements. Many of the details of talk, technique, and investigative strategy were organized around specific tasks and did not obviously “reflect” a general philosophy, political viewpoint, or cultural bias. It is possible, of course, that I simply lacked the theoretical insight to “uncover” the way the details I witnessed traced back to a coherent metaphysic, ideology or cultural standpoint. I doubt this, partly because it often seemed *all too easy* to trace such details back to different, sometimes incompatible, ideological and cultural possibilities.

Gathering from the level of debate in the “science wars”, it seems clear that the lessons from ethnomethodological tutorials may be instructive to scientists as well as their cultural critics. I believe that such tutorials can provide a link between the lab and the podium. Gross, Levitt, and other critics of cultural constructivism speak almost exclusively from the podium. They identify themselves as scientists, and they certainly are entitled to do so, but their arguments against constructivism rarely mention anything about their own research. Gross and Levitt’s (1994) arguments are cast in the most general terms about “science” and “natural reality”; and they freely assign a coherent ideology to the elements of the “academic left” they criticize. Sometimes they focus on specific historical examples, and they occasionally make use of their knowledge of mathematics and physics when discussing such examples, but they do not address (or, apparently, see the necessity to address) the workaday science that they and their research assistants do. This is an odd situation, given the fact that the debate with the constructivists supposedly is about the “contents” and the “actual practices” of the sciences. I presume that Gross and Levitt do not need to be tutored in the specific experimental and mathematical techniques they employ in their research, and I am not proposing that ethnomethodologists should instruct them about tacit, unconscious, or contradictory aspects of their own practices. The purpose would not be to impart new information to scientists, or to expose their hypocrisy, but simply to present them with reminders of how they go about their work (Wittgenstein, 1958: §127). I doubt that such inquiry would end the realist-constructivist debate, but it might help dissolve some of the confusions that inhabit it. Based on what I learned from my tutorials about artifacts, I think I understand a pertinent source of confusion. Scientists accustomed to hearing the word “artifact” used in connection with specific properties of data that arise from

electronic noise, optical distortions, and the like, hear general claims to the effect that scientific discoveries are constructed or manufactured as suggesting that the discovered properties are *mistakenly* assigned to natural objects. Concisely stated, the lesson for antagonists in the realist-constructivist debate would be that general constructivist arguments are not equivalent to expressions of disbelief in the reality of *particular* facts and laws. Indeed, it is very unclear what general constructivist arguments imply about *particular* scientific practices and results. The gap between constructivist theory and practical laboratory actions is not the usual kind of gap between a theory and its practical implementation, since the relevance of general constructivist theories to laboratory practices remains to be demonstrated. By delving into the production of one or another practice, ethnomethodological research can convert the theorist's problem of relevance into a substantive, circumstantially specific, topic. This research strategy is unlikely to solve the theorist's problem, and it may never bridge the gap between general theories and local practices. When such research proves persuasive, it is more likely to encourage others to abandon the transcendental postures that create such "gaps" in the first place. And so, for as long as theorists continue to captivate their readers with grand theories which purport to describe and critique what the rest of us do in our daily lives, there will be a gap in the literature for ethnomethodologists to address and dissolve. Current trends in social and cultural "theory" suggest that ethnomethodologists will continue to have plenty of work to do.

Notes

1. The colloquium was held at Boston University in the Summer, 1975. The lecture focused on a collection of embedded question-answer sequences in which the recipient returns a question to the questioner. An earlier discussion of such Q-Q-A-A sequences is in Sacks (1992), Vol. 2: 344ff.
2. I do not have a tape recording or notes from the colloquium. This is my best recollection, but even good recollections can produce bad reconstructions. The quoted utterance and descriptions of how Sacks responded to it should be read as scenic elements of an illustrative story, and not as records of actual events.
3. George Psathas (personal communication, August 1997) reminded me that Harold Garfinkel was in the audience when Sacks was asked the question. Psathas pointed out that Sacks may have been reluctant to credit Garfinkel for being the prime influence on his work, while at the same time being reluctant to slight Garfinkel by naming anyone else.
4. See, for example, Sacks's transcribed lecture: 'An impromptu survey of the literature' in Sacks (1992), Vol. 1: 26–31.
5. Hilbert (1995) and Rawls (1996), for example, attempt to correct contemporary readings of Weber and Durkheim, by arguing that Garfinkel and ethnomethodology recover thematic continuities that were obscured by Parsonian interpretations of the original writings. There are many other examples of efforts to locate ethnomethodology in the

- literary traditions of philosophy and sociology. A small sample includes Alexander & Giesen (1987); Cicourel (1973); Denzin (1969); Giddens (1976); Habermas (1985); Handel (1982); Heap and Roth (1973); Heritage (1984); McHoul (1981); O'Neill (1980); and Wilson (1971).
6. Garfinkel's (1960) unpublished, and proto-ethnomethodological 'Parsons Primer' is a scholarly discussion of the problem of order.
 7. Anita Pomerantz (1978) later developed this topic in an outstanding piece of conversation analytic research.
 8. This seminar was taught in the School of Social Sciences at UC, Irvine, in 1972–73.
 9. This seminar was taught in the Department of Sociology, UCLA in approximately 1979–80.
 10. Most of these exercises are described in Garfinkel's large corpus of unpublished writings. Garfinkel (1996: 15) mentions his and his students' investigations with maps and manuals, and Garfinkel & Wieder (1992), discuss a demonic exercise on summoning phones.
 11. See Bjelic (1995) for an original demonstration of how phenomenological themes (in this case derived from Husserl) can be misread as praxiological descriptions.
 12. I attended Sacks's graduate seminars in Fall 1972 and Winter 1975, but most of what I learned about his style of teaching came from conversations with Alene Terasaki, one of Sacks's PhD students.
 13. See Turner (1994) for criticisms of contemporary social theories of practice. Turner's book is the subject of a review symposium in *Human Studies*, Vol. 20, 1997: 315–356.
 14. A mastery of the non-academic arts of conversation is a prerequisite for doing conversation analysis, but for the most part it is not a problematic feature of the pedagogy. Sacks (1974) explicitly invokes his readers' native understanding of the materials he presents as a condition for proceeding further with an explication. According to Schegloff (1992: xliii), Sacks eventually moved away from this explicative style of analysis in favor of a more analytic focus on conversational participants' sequentially displayed understandings. Consequently, the analyst's native competency with the practices under analysis had a less explicit part in the research.
 15. For several years, starting in the 1970s, Garfinkel used the title *A Manual for the Study of Naturally Organised Ordinary Activities*, when citing a collection of unpublished, not-yet-completed, and imagined studies by himself and his students. Garfinkel was consistently generous in assigning co-authorship to his students.
 16. As I understand it, the expression "vulgarly competent" alludes to the fluency that comes with being able to take one's mastery for granted. The convoluted aspect – what Garfinkel & Wieder (1992: 182) call the "strong use of the unique adequacy requirement of methods" – is expressed in the following (appropriately convoluted) sentence, which is intended to be "administered and used locally as an instruction":
 . . . a phenomenon of order* already possesses whatever as methods methods could be of [observing], of [recognizing], of [counting], of [collecting], of [topicalizing], of [describing] it, and so on, if, and as of the in vivo lived local production and natural accountability of the phenomenon, [observing], [recognizing], [counting], [collecting], [topicalizing], or [describing] it is at issue. (Garfinkel and Wieder, 1992: 182)
 17. Garfinkel's graduate and post-graduate students in the 1970s and early 80s also included: David Weinstein who studied truck driver's practices; Britt Robillard and Chris Pack who studied medical practices and encounters; Richard Fauman who studied video production; Doug Macbeth who studied classroom work; and George Girtton who examined and practiced martial arts; and Ken Liberman who studied methods of Tibetan philosophical argumentation.
 18. Although Bruno Latour seems to have little use for unique adequacy, and he otherwise views ethnomethodology as far too restrictive for his own ambitious projects, he aligns

with ethnomethodology on one key point: “context” is not a sociologist’s tool, or a general explanatory resource; social, economic, technical, and all other elements of “context” are uniquely specified and incorporated into the local histories of singular innovations. See Latour (1996: 133). Also see Cambrosio and Keating (1995) for a compatible treatment.

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