Methodological tokenism, or Are good intentions enough?

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Introduction

One persistent theme in considerations of the state of the cultural and social sciences is the apparent lack of connection between the work going on in the various disciplines. Often, this lack of connection is offered as reason for promulgating a generalized schema within which most, if not all, of the different kinds of study can find a place. The most recent of these schemas is based upon the suggestion that human cultural activities can be viewed as *communicative behavior* and that, under the aegis of a general theory or science of communication and signification, namely semiotics, ways will be found to relate the presently dispersed and discontinuous social and cultural sciences. Umberto Eco, for one, has given voice to this view in the following way:

> ... many disciplines other than semiotics have already undertaken or are at present undertaking research on subjects that a semiotician cannot but recognize as his own concern; for instance formal logic, philosophical semantics and the logic of natural languages deal with the problem of the truth value of a sentence and with various sorts of so-called 'speech acts' while many currents in cultural anthropology (for instance 'ethnomethodology') are concerned with the same problems seen from a different angle; the semiotician may express the wish that one of these days there will be a general semiotic discipline of which all of these researches and sciences will be recognized as particular branches; in the meantime a tentative semiotic approach may try to incorporate the results of these disciplines and to redefine them within its own theoretical framework. (Eco 1976: 5–6)

In this paper we will not concern ourselves with the general arguments over the propriety of unification. Nor will we consider the disputes about whose umbrella the unification ought to proceed under. What interests us are the practical possibilities of incorporation. We do not believe that it is sufficient to live in hope of transcending disciplinary boundaries. We want it to be shown that such a goal is, in fact, attainable in the foreseeable

Semiotica 58–1/2 (1986), 1–27.
0037–1998/86/0058–0001 $2.00

future; something which can only be done by considering actual cases. The bulk of our paper is given over to an examination of two sets of proposals which claim to demonstrate that for sociology and social psychology this can indeed be done (Butterworth 1978 and McDermott et al. 1978). Both may be thought of as contributions to what is loosely termed 'the ethnography of communication', a pursuit which, we would think, lies right at the heart of any attempt to forge 'a general semiotic discipline'. Our task will be to try to extract from these proposals, and from evidence offered in their support, some of the difficulties that lie in the way of attempts to intermesh sociological studies of conversation and face-to-face interaction with other broadly semiotic approaches. Our argument is that these two proposals demonstrate what we term 'methodological tokensim' and hence do not offer any evidence of either real innovation or practical unification.

Both sets of proposals under consideration are reactions to the failure of the ethnography of communication to facilitate rigorous, systematic, naturalistic descriptions of human communication. Both are designed to provide a *method* for achieving just this end. It is only descriptions of this sort which will be 'adequate' (to use McDermott et al.'s phrase).

By way of an introduction to our review of what they have to say, therefore, let us begin with what might be meant by the notion of 'adequacy'. In the first place, adequacy is to be taken as the criterion for successful *descriptions* and, in Butterworth's case, *theories* as well. Before allowing this criterial use, it would be as well for us to notice that in so doing, such a use transforms adequacy from a provisional into an absolute evaluation of descriptions. Rather than allowing investigators to say that their findings allowed them to propose at *least* this or that, adequacy is turned into an end-state, into a pseudonym for validity. Yet, this requirement of adequacy starts out from a denial of the possibility of a one-to-one correspondence between the description of some object or activity and selected or specified features of that object. It proposes that what we are given in descriptions are more or less adequate ways of recognizing objects from their descriptions and not representations which are identical to them. As Wittgenstein (1979, and elsewhere) points out repeatedly, if we can manage to prise descriptions away from such a correspondence notion, we are then faced with the task of specifying some other criterion for the acceptability of any particular description on any occasion. Both sets of proposals we examine seek to provide lists of canonical procedures which, if followed, are supposed to guarantee the adequacy of the descriptions, although neither grounds their methodological maxims in Wittgenstein's philosophical position. In addition to the centrality of the question of the grounding descriptions, these proposals merit attention because they make claims about *standards* for progress and recommendations — in one case explicitly, in the other implicity — for how semiotic descriptions might progressively be improved. Both sets of proposals intend, therefore, to make significant differences to the ways that such descriptions can be given. And yet, both sets of proposals evince 'methodological tokenism'. Were they to be adopted, nothing much would be affected. The real differences that either would make are only marginal. This is a crucial point to make since both proposals rely on research reports to illustrate and justify the extent of the methodological innovations they suggest. And yet, on examination, no gains are made at all. The programs that are offered turn out to be *post hoc* rationalized reconstructions of research experience. For ease of presentation, we will take the two sets of proposals in turn.

Butterworth's maxims

Butterworth is an experimental social psychologist with a research interest in linguistics, language acquisition, and language use (as an inspection of Butterworth et al. 1977 will show), who wants to provide (or rather he wants sociolinguistics to provide) a firmer grounding for the generation of *theories of conversation*. Butterworth's proposals (his maxims) are the result, he suggests, of reflections that he has given to the serious problems of conceptualization, data collection, and analysis with regard to face-to-face interaction.

A summary of Butterworth's maxims are:

1. Make your methods public.
2. Theories are better than stories.
3. Remember that conversationalists talk.
4. Remember that conversationalists are human.
5. Let the theory do the work.
6. Let the phenomena guide the theory.

Two immediate observations can be made about this collection of proposals. First of all, (2) is not a maxim at all but a proposition. To fit within the schema, it would have to be reformulated as something like: (2a) Prefer theories to stories.

The second observation to be made about the maxims concerns Butterworth's claim that an indication of their potential can be gained by reference to a research report by himself and two colleagues (cf. Butterworth et al. 1977). This paper concerns the range of resources used to regulate and manage speech interchange. In particular, it sets out to *test* Argyle's hypothesis that in speech-only exchanges (e.g. on telephones), audible clues replace visual ones (Argyle 1972). That study is cast in the classic hypothesis–prediction–test–conclusion framework, with the bulk being given over to presenting theoretical background-research design–results–conclusions. If the maxims are related in any way to that paper, they are revisionary versions of what went on to produce the finished article. The significance of this is to be found in the fact that the maxims paper was a presentation at a symposium where the constraints on developing an argument and making it stick, and finding examples to make oneself clearly understood, are very different from those in operation in more formal contexts. Some of the difficulties that we have with Butterworth's maxims may very well result from their nature as revisionary versions of his own research presented for an informal gathering. This may also account for the somewhat epigrammatic style adopted. It could be that we are pressing too hard; that we are demanding too much in asking for its arguments to be clear, coherent and systematic. However, we would like to think not. Allowing that an informal presentation, if such it be, does not set for itself the highest standards for the appraisal of arguments, does not mean that no standards at all are necessary. It may be that lapses from sense, coherence, and the rest, may be perfectly understandable; but that is not a justification of them.

Butterworth introduces his maxims by setting them against a particular methodological background. This provides a set of goals to which Butterworth wants the analysis of conversation to aspire. Most likely, these goals are a set of caveats analysts should pay heed to. Such methodology is provided by Butterworth's accommodation of his investigative attitude to his version of Feyerabend's philosophy of science (Feyerabend 1975, 1978). Not surprisingly, Feyerabend's strategy is a philosophic one. He wants to show that (a) methodological monism is not *necessary* for scientific progress — the rules supposedly defining 'good science' have been broken on repeated occasions by good scientists; and (b) that even where it is advancing, science is not *necessarily* the guardian of the supposed canons of rationality, scepticism, primacy of argument, evidence, and so forth. To achieve these twin objectives, it is enough for Feyerabend to demonstrate that in some crucial cases (his leading examples are the Galilean and Copernician 'revolutions'), scientific 'progress' was a result of a free-ranging investigative attitude, contingency, serendipity, and ideological domination. Feyerabend suggests that rationalists who wanted to make this attitude part of the program of science, would have to turn it into an apothegm such as 'anything goes'. As such, the import of the slogan has to be totally at variance with any kind of methodological monism. Feyerabend, then, has to show that, in particular cases (again, those of Galileo and Copernicus are his favorites), the extent of the domination of the 'progressive' views over 'established' ones is a result not of their 'scientificity', but of the promotional and propagandizing talents of their adherents. Since Feyerabend's intention is to show the fallacy of the view that any one style of scientific investigation can be laid down as a prescription for science, his limited examples suffice. What they do not amount to, however, is anything approaching a reconstruction of the intellectual history of science. They are wholly negative rather than positive in their implications. The irony is that where the whole point of Feyerabend's anarchic slogan was to put an end to dogmatizing over scientific methods, Butterworth turns it into a dogma — that of methodological pluralism. Where Feyerabend says that science keeps rewriting the requirements for rationality, scientificity, logicality, measurability, observability, and so on, to fit in with its dominant theorizations (hence there can be no eternal canons of rationality, logicality, etc.), Butterworth wants to justify his 'Feyerabendian' methodology on the grounds that pluralism is the most rational and most ideally suited method for the study of conversation.

> The study of conversation should provide a beautiful case for a Feyerabendian analysis. Conversation is approached from many different viewpoints, which differ in details, and more interestingly, in the very metaphysics the various practitioners bring to it. (Butterworth 1978: 323)

Butterworth seems to have totally misunderstood Feyerabend. What Feyerabend is arguing is something like this: out of the confusion of contemporary ideas that make up scientific theory at any one time, some come to dominate. *Post facto*, these are then enshrined as scientific advances. Any particular cluster of theories comes to dominate because of the efficiency with which they are promulgated and because of the socio-intellectual conditions of the time. It is only once they have come to dominate that they take on the aura of incontrovertibility. The different theories, opinions, and viewpoints, are all intermeshed with different conceptions of the nature of the investigable world. Once one is dominant, its conception is universalized in textbooks, exemplary experiments, and so forth. There is absolutely no point in trying to legislate in advance, which cluster of theories will come to be dominant. It certainly cannot be done by measuring the contenders against some idealization of 'scientific method'. Butterworth turns this putative *fact of the philosophical history* of science into a *methodological requirement* for good science. Where Feyerabend observes no universal methodology, Butterworth wants to insert a pluralistic one: '... conversations are a kind of microcosm of the human condition and the student needs to appeal to a wide variety of disciplines to justify his interpretation of a particular piece of conversational behaviour' (1978: 321).

Even if we are prepared to countenance this cannibalizing of Feyerabend's argument, the important question is how this pluralist methodology is supposed to work. The fact that the various disciplines which study conversation bring different metaphysics — the term which Butterworth appears to have in mind is that slippery melange of ideas designated as a 'paradigm' by Kuhn (1962) — is not taken as having any significance at all. Butterworth does not appear to believe that having differing metaphysics has implications for cross-disciplinary comparisons *or* testing. Thus, a problem of commensurability, which is central to both Kuhn and Feyerabend's arguments, is totally ignored by Butterworth. Both Feyerabend and Kuhn (at least in his early versions) claim that cross-disciplinary testing is both invidious and impossible. Butterworth undermines this position by taking up what he sees as two undeveloped themes in Feyerabend's work:

a. Science is a public and cooperative activity: the implications of this are that studies and their results are both communicable and replicable. Yet, for Feyerabend, this is exactly what all the fuss is about. Feyerabend argues that communication is promulgation; it is the attempt to impose one scientific ideology on others. Hence replication can only take place once the colonizing ideology has assumed control. Using Kuhn's original terms, replication has to be intra-paradigmatic. For Feyerabend then, scientists are not just communicating, they are attempting to convert each other. The availability and public character of science is precisely what Feberabend does not argue for; such a notion is a rationalist illusion, he says. Butterworth is free to find these conclusions unacceptable; he is at liberty to feel that there is something amiss in Feyerabend's arguments, examples, and inferences. However, he cannot simply disregard them when he takes over elements of Feyerabend's critique of scientific methodology without indicating why it is that Feyerabend should not draw the conclusions he does. Simply saying that scientific methods are public does not demonstrate why, in Feyerabend's own arguments, they *must* be so.

b. Science can be tested against the facts to determine preferential theories. It is not surprising that this is an 'undeveloped' idea in Feyerarbend's philosophy, since it runs counter to the whole tenor of what he says. For Feyerabend, getting some version or set of facts accepted as indisputable is part of a propagandizing exercise. What the facts are is by no means indisputably given. Certainly the idea that they are simply the test of theories is naïve. Such a view is not so much undeveloped in Feyerabend as it is totally absent, *except as a position which he consistently campaigns against*. The problem of determining what the relevant facts are is to be found within disciplines as well as between them. Different schools of linguists use different linguistic facts that constitute language in differing ways. They differ among themselves as well as from sociologists, psychologists, speech therapists, and so forth. If Butterworth wants to be open to the approaches of different disciplines and to assume the theories drawn from those disciplines which are better than our own at explaining the facts, he had better tell us how to do it. Or rather, he had better tell us what it is that we would be able to do *if* we could do it.

Both (a) and (b) contain a problem to which it is well worth devoting a little more time. To suggest that the investigation of conversation can be organized around the twin themes of methodological pluralism and the testability of theories against facts, presupposes *agreements* concerning what count as facts and what are allowed as *bona fide* alternative methods. But even within disciplines, alternative matrices may cast methods and facts in ways that are at odds with one another. To take one familiar case drawn from sociology, Hegel and Marx both make observations about the implications of the distinction between civil society and the state; whereas the state represents a manifestation of *der Geist* for Hegel and is the guarantee of moral autonomy and hence freedom, for Marx the state is an instrument of class oppression and domination. Marx has not simply inverted Hegel's inversion of the subject and predicate of history, he has cast the state as an entirely different theoretical object. What he means by the state and the distinction between it and civil society does not have the same theoretical reference as when Hegel talks of these matters. Now if this is true within a discipline, even one so chronically disorganized and disputational as sociology, how much more is it likely to be so between disciplines? Yet Butterworth blithely proposes that:

> ... [the] student of conversation is in an ideal position to avoid operating with what Feyerabend terms 'a closed metaphysical system' and to exploit instead the 'pluralistic methodology' [1975: 30] which becomes available due to the convergence of so many disciplines on conversational data. (1978: 325)

The difficulties facing Butterworth's suggested pluralistic methodology are formidable, yet his paper does not even consider them. Instead, his strategy seems to be one of assuming that the end will not only justify the means, but that the obvious goodness of the end will provide an adequate demonstration that pluralism *must* give better theories than monism. Even if we were to disregard this disingenuineness about serious epistemological difficulties, does the end really justify the means? What exactly is

pluralism a means to attain? It appears that the notion of pluralism has been devised to meet two central difficulties that have been encountered in the analysis of data drawn from conversations:
a. the wide range of possible sources of any particular piece of behavior; and
b. the volume of coincidental behavior and media which is available at any one time.
In (a) the term 'sources' is interestingly vague. Is Butterworth referring to different causes of some piece of behavior? Or perhaps he simply means that there are many different ways of looking at that behavior? For example, Butterworth talks of the *social* and *cognitive* sources of pauses in conversation. These different sources (or perhaps it is explanations now?) ought in some way to be combined.

> Thus even if one's purpose is to model some one aspect of conversational behavior, the employment of conversational data to support the account *requires* taking into account models of other aspects which might also concern themselves with describing the same behavior.

He continues:

> It should go without saying a conversation is an extremely intricate phenomenon in which cognitive and neuromuscular skills are put at the disposal of a wide range of persons and social purposes, and the whole embedded in interlocking systems of social and linguistic conventions. (1978: 318; emphasis added)

Given the muddle concerning combining and coordinating disciplines discussed above, what this 'taking into account' is supposed to be remains thoroughly and unremittingly opaque. If we follow the leading example of speech production, things get more muddled rather than clearer. In general, sociolinguists seek to build (some kind of) predictive models for token selection and application. Usually these models are explications of metatheoretical models of language acquisition. Each of the models postulates the probability of a particular token being selected under given circumstances. Since the models contain the postulate of uncertainty, each has built into it a degree of risk. Consequently, to make the model work to produce linguistic output, selected tokens, secondary elaborations concerning things such as context, setting, persons present and their relationships, individuals' perceptions and interpretations, and so forth, have to be given. This secondary elaboration shifts explanations, causes, and accounts away from token selection to speech interpretation and its class, power, interactional determinants and the like. Such a view of language production (i.e. the model being developed) contrasts starkly

with that used by, say, transformational grammarians. Transformational grammar does not have the interpretive actor as its focal point. The speaker in transformational grammar is little more than an animated speak-your-weight machine, programmed by the rule-governed nature of his semantics and grammar. To propose that differing models of the nature of language and conversation have 'to take account' of one another, means having to solve the prior problem of how these two conceptions of the speaker are to be reconciled. It is simply not enough to brush the difficult questions aside with: Separate entities like oil and vinegar can be combined to give a continuous result: vinaigrette. A discrete model of vinaigrette is necessary, which says that it is composed of two separate things (Butterworth 1978: 321). This is a beautiful example of how theoretical reflections can become entranced by a way of representing something, in this instance, a methodological step. Once we all agree that a new discrete model is what we need, then an end can be put to meta-theoretical reflection. We no longer have to ask ourselves what it is we are demanding. Nor do we have to bother deciding whether we really need it or not. We can plow on regardless, munching our way through mounds and mounds of data, trying to find a new theory.

There are, then, profound difficulties with the general goals that Butterworth has set for himself. In his methodological discussions he seems to give no indication that he appreciates their extent or significance. Might his elaboration of his maxims display the sensitivity that we are looking for?

1. Make your methods public

This maxim is supposed to follow from the Feyerabendian arguments outlined above. Its aim is to facilitate replication and testing. As it stands, the maxim can be no more than a plea for the publication of information concerning techniques of data selection, collation, and retrieval. However, methods, in this sense of techniques, are exactly what are made public, mulled over, disputed, and hence replicated and so on. What are never made public are the analytical procedures for describing data, finding adequate descriptive categories, and relating phenomena. The standard techniques for the analysis of data are publicly available. What are not, are the analytical procedures by which that data is generated as data in the first place. Butterworth cites his own paper as an example of the publicizing of methods (Butterworth et al. 1977), but does not make the process of developing his analytic categories available. If methods are understood to be the techniques of data collection, collation, and retrieval, then the maxim is an exhortation to continue what we are already doing, only more so. If it is something else, then it is not clear what is being referred to, let alone whether having it would make any difference to what is done in research.

2. *Prefer theories to stories*

The distinction between theories and stories is rooted in a contrast between interpretation and prediction, and hence, in different *styles* of analytical work. Butterworth proposes that the work of Goffman (1959) and Scheflen (1964) should both be considered simply as stories since they do not allow for the testing of their accounts. The preferability of theories to stories is adduced in two ways:

a. Theories are not subject to a strategy of preservation in the face of counterfactual cases. Leaving aside the general truth of this assertion, it appears that, in essence, what Butterworth is saying is that theories are more formalized and articulated than are stories. This ignores the fact that it is the very informality of the story that gives it its analytical power. To take one of the cases Butterworth mentions, Goffman is held to organize his descriptions so that replication is impossible. But who would want to replicate Goffman's work? What would that accomplish, other than more Goffmanesque accounts? Butterworth has missed the point of Goffman's description, namely, imaginative observation, and has imposed upon it crude canons of the most simplistic kind of empiricism and experimentalism. The very last thing that anyone could accuse Goffman of doing is experimental social psychology. What he is doing is pillaging analytical metaphors to find ways of presenting the detail of the forms of interactional processes.

b. Theories lay down the limitations of their own application by excluding classes of variables. In defining the points at which they will no longer apply, they set out their own test conditions. The trouble with this kind of Popperian demarcation is that while some (perhaps even the best) theories in fact do this, many thoroughly acceptable and useful ones do not. It might be, that if one excluded Darwin from biology, not much would be missed (though we doubt it); If we were to remove the theories of Keynes, Marx, Chomsky, Parsons, Lévi-Strauss, and Freud, from the social sciences, little would be left. All these are reasonably good theories, at least by the standards current in the social sciences, but not one of them is testable in the Popperian sense that Butterworth intends. Once these become classified as stories, we might as well extend the term to Skinner, Piaget, and Newton and Einstein as well. The theory/story dichotomy

simply breaks down if, instead of a description of how good science gets done, it is turned into a prescription of how it must be done.

The case for the obvious attractiveness of stories over theories is made by reference to a particular account of meaning and speech production and its relation to speech-gesture asynchrony. At its simplest, the argument is that the verbal token and the gesture convey or represent the meaning intended. Asynchrony is, therefore, to be interpreted as a result of selectional malfunctioning, physiological process disruption, interference, or the like. Behind this definition of what is involved in asynchrony is a mentalistic conception of meaning which is never explicated, or argued, despite its contentious nature. With this view, both token and gesture are to be treated as alternative *channels* for the conveyance of meaning. The form of channel — the token or the gesture — is selected to fit the meaning intended. Hence meaning intention takes place prior to token or gesture selection and asynchrony results from pathway 'troubles' of some kind. We are given no indication that this storehouse conception of meaning interpretation is just a representation of mental activity (and a dubious one at that) nor what the selectional process refers to, where it takes place, and so forth. It would be tedious to rehearse all of the arguments that have been summoned against this kind of mentalism. Suffice it to say that *if* thinking is conceived only as a mental process of selection and organization, and *if* speech production is defined as a necessary consequence of that process, *then* it becomes possible to ask for a production-process theory that makes predictions about speech-gesture asynchrony. If, however, one were to depart from either of these presuppositions, then the case for demanding such a theory is less than compelling. That is to say, this conception of the preferability of theories rests, in the end, upon a very limited conception of what it is to do sociolinguistics, psychology and so on, and a very un-Feyerabendian one at that!

3. *Remember that conversationalists talk*

At first sight this maxim is reassuring. It looks as if Butterworth is only too willing to acknowledge the tendency to over-abstraction and artificiality in theories. He reminds us that *people* use conversation to achieve all sorts of things, that they, too, have ideas about what conversation is used for and can monitor talk to distinguish between what is said and what is meant, how others understand what is going on, and so forth. This is refreshing because of the apparent unwillingness of some language scientists even to countenance such facts. However, good intentions do not make good research programs. Butterworth does not tell us how h intends to include this kind of orientation in what appears to be a excessively logico-experimentalist investigative strategy. 'Remember tha conversationalists talk' remains merely a slogan. It tells us nothing of ho we are to build that fact into our analyses.

4. Remember that conversationalists are human

Trite though this maxim may look *prima facie*, it is wrong. Conversatior alists are *not* human. As we pointed out earlier in the discussion c different kinds of models of speech production in linguistics and els where, conversationalists are speech producers and interpreters/decode exclusively. What Butterworth means by this maxim is obvious enoug We should not attribute to conversationalists qualities or properties th are ruled out of court as impossible by adjacent disciplines. The obvio example is the one he chooses, the procedural monitoring conception c speech production and understanding. Once understanding, for exampl is defined in procedural terms, then what we can say about conversatior alists' abilities has to be delimited by neurophysiology, cybernetics, an micro-biochemistry. The trouble is that *unless* all of the disciplin concerned share a common view on how to constitute the thinkin subject, the coparticipant in conversation, the nature of the mental, an so on, then they will delimit their objects in entirely different ways. The will certainly *not*, for instance, be likely to find conversation analyst views of the speaker/hearer as a course of action and treatment, as a operator of the turn-taking machinery, of much value in studying th problems which they wish to take up; and *vice versa*. In large measur it is the difference between theoretical objects which makes disciplin different.

As Butterworth himself admits,

5. Let the theory do the work; and
6. Let the phenomena guide the theory

are contradictory statements. On the one hand, theories should lead, b ambitious, speculative, and generative. On the other hand, theori should be responsive to data and confirmation by experience. Theori stand in both an *a priori* and *a posteriori* position *vis-à-vis* data. Far fro offering any resolution of this contradiction, Butterworth seems to delig in it. The hollowness of its pseudo-profundity catches the spirit of h proposals.

In sum then, Butterworth's maxims turn out to be (a) based on garbled epistemology, (b) designed entirely for an empiricism rooted i experimentalism, and (c) utterly unhelpful concerning the ways that w

iight seek to implement them to formulate better and better theoretical escriptions of conversational activities.

lcDermott's criteria

ıttempting to apply Butterworth's maxims to any particular research roblem could only result in chaos. As we have shown, just a little hought is required to see that all they will lead to is confusion. Such is not he case with the recommendations made by McDermott and his colleag-es (1978) (hereafter McDermott). To begin with, McDermott is remark-bly clear about his own analytic goals. There is no doubt in his mind that vhat is needed in social science in general, and especially in his particular ield of sociology, is a systematic framework for formulating and assess-ng descriptions. The search for a systematic ground for what are, in this ase, sociological descriptions, is a commendable one.

We do not have (as with Butterworth) an initial problem of trying to vork out what McDermott is saying. Rather, our difficulties with McDermott's recommendations begin when we try to envisage his mplications for investigative work. It is only then that we come to the onclusion that McDermott formulated the task to be completed in a nisleading way, and hence his attempts to find a solution are not ecessary. McDermott is worrying about the *wrong* problems. Our rgument will be, then, that McDermott has misformulated false prob-ems. When we get the formulations straightend out, the extent of the rror becomes clear.

Some appreciation of what is going on can be gained by looking at McDermott's initial and rather novel approach. According to him, the oal of ethnography is to ensure the 'believability' of the descriptions iven. Such a statement has the interesting consequence of allowing McDermott to consider ethnographic accounts as, in some sense, argu-nents designed to convince their readers/hearers. Having set ethnogra-hic descriptions up as arguments it is but a small step to propose that here ought to be criteria available for their evaluation. After all, the anons of formal logic provide criteria such as consistency, noncontra-iction, universality, parsimony and so on. The parallel criteria to be mployed in the evaluation of ethnographic descriptions are *descriptive larity* and *conceptual and methodological rigor*. In combination, it is nvisaged that these two will provide for the formal acceptability of thnographic descriptions.

The idea that ethnographic descriptions are somehow made believable r convincing seems to be the result of a muddle between the aesthetic and logical properties of the 'good' in 'good arguments'. Rambling, ramsha kled and poorly articulated arguments may be good ones in the sense th they are logical, consistent, and convincing. Clear, precise, and coord nated ones may be fallacious. In this sense, then, Occam's razor and oth devices turn out to be aesthetic rather than logical in their import. Th conflation of the logical and the aesthetic (a tendency by no mea confined to McDermott nor sociologists) will emerge more clearly and i effects be appreciated more fully if we tug a little harder on the moorin of the two criteria. With a little effort, it should be possible to pull the loose and so get a view of what lies behind this conception of ethnogra phic descriptions.

One way to think about what McDermott means when he talks of th believability of a description, is to think of it in phenomenological term To read or hear an ethnographic account *and* to find it meaningful, w have to assume that it possesses a degree of plausibility. Giving a ethnographic/anthropological reading/hearing is taking some things fo granted, suspending judgment about the dubitability of aspects of th social world, and so on. As we have demonstrated elswehere in discussion of the work of Clifford Geertz, this plausibility is achieved b providing a situated logic to actions in which those actions can be seen t be sensible, rational, and expectable (Anderson and Sharrock unpubl. The rationality of activities allows us to consider them as relevant an related aspects of the culture, organization, or way of life, under stud Ethnography, therefore, is the work of writing up those activities so tha they evidence the rationality ascribed to them and in such a way that th descriptions are deemed plausible. The idea of rigor can be introduced a this point to guarantee the description which accomplishes plausibility. not only *works* as a description but it is a *good* description as well. There an ambivalence in McDermott's use of rigor here. Rigorous and form descriptions are not necessarily the same thing, although some form descriptions may be rigorous. For Garfinkel and Sacks (1970), form descriptions are descriptions of the form that activities take, a form whic is recognizable, patterned and repetitive, and so on. Carnap beautifull captures the general sense of formal that we are proposing here:

> A formal investigation of a certain sentence ... does not concern the sense of th sentence or the meaning of the single word, but only with the kinds of words an the order in which they follow one another. (Carnap 1935: 39)

Such descriptions may be given in a formalized manner with the for being described by universally applied categories axiomatically define and manipulated according to general procedural rules. Examples of suc

formalized formal descriptions are to be found in logic (as with Carnap) and in the uses of mathematics in various sciences. The ambivalence occurs because McDermott seems to have mistaken formal descriptions for a subset of formalized descriptions, rather than *vice versa*, and to have equated them with rigor. The use of predefined categories, general rules of use, and so on, give formalized descriptions something of a developmental momentum which might be what 'believability' in this context is an attempt to capture. But formalized descriptions are just some of the ways that descriptions of the structures of activities may be given, and all such descriptions are themselves practically organized. Their organization, its form and its structure, can be examined in its own right. By failing to notice this point, McDermott endangers the whole of his methodological program. He takes one set of practical descriptive procedures and makes them stand for all kinds of ethnographic descriptions.

Conviction by, or belief in, ethnographic accounts is an attempt to patch over the methodological cracks brought about by stressing what is essentially the narrative nature of ethnographies. Ethnographies are attempts to tell convincing stories. While ethnographers are allowed some imaginative rein, such an allowance does not justify a lack of criteria for discrimination. Not just any version of events will do; as McDermott puts it:

> Adequate description depends heavily on the practical knowledge and deep feelings of the ethnographer. But, in celebrating the intuitive flash of the fieldworker, we should not lose sight of descriptive clarity and rigor as goals. (McDermott et al. 1978: 267, n. 1)

What McDermott wants to call descriptively clear and rigorous ethnography, is a description that creates and demonstrates its own plausibility.

The important point about this conception of McDermott's is that it posits ethnography as a practical activity. The implication is that ethnographic practices can be studied in their own right. Unfortunately, such an advance is then lost by McDermott's misleading formulation of what it implies. Proposing that ethnographers are engaged in methodical practices does not imply that a belief in their descriptions is subsequently secured. Hearers/readers are not, *ab initio*, suspicious of the accounts they hear or read. Rather they take on trust the believability and rational recognizability of descriptions, until such time that the nature of the descriptions can no longer be normalized within a plausible framework. It is not, then, a question of ethnographers securing our belief but rather, of ensuring the continued cooperation of their readers/hearers that things are as usual. The ethnographic task is not the grounding, in some formal sense, of descriptions (that is, in making them rigorous, acceptable or believable), but in making them recognizably, normally, ethnographic. This, then, is achieved by the use of ethnography's own logical practices. The most apt illustrative example here is that of Garfinkel's (1962) experiments on trust. In those experiments, it becomes clear that we cannot treat trust as if it were a set of background expectancies, a safety net to normalize experiences. Rather, trust serves as a member's methodological maxim and not as a resource, that is, until further notice, trust what people do, and that what is said is relevant, and so forth. Members do not begin with doubt and then use 'trust' to reassure themselves. They simply trust one another. By trusting one another, they produce the cooperative nature of their coordinated activities. In telling the story of a set of activities, the ethnographer gives them a logic. It is at this point that the confusion often found over the nature of 'ethnographic understanding' enters. The logic that the ethnographer gives to a set of activities cannot be exactly the same as that which the participants themselves would give. Sociological recognizability, constituted under the theoretical relevances of sociology, is not the same as that of common sense which is constituted under the natural attitude. To be sure, there must be some kind of relationship between sociological descriptions and naturalistic descriptions, but that relationship need not be a strict symmetry. It need not be a one-to-one correspondence. The question we can ask of McDermott's criteria is, then, a simple and straightforward one. Do they enable us to discriminate good ethnographic descriptions of communicative activities from bad ones? Can we do this without assuming that the participants to the activity should both recognize the description and endorse it? In the end, our answers will have to be negative. But in seeing why this is the case, we can learn a great deal about the difficulties of describing descriptive practices (see also Sharrock and Anderson 1981), and hence the pervasiveness of 'methodological tokenism'.

One might have to be forgiven for thinking that since McDermott is so concerned with descriptive clarity and rigor, their explication ought to feature prominently in his paper. Such an expectation would be in vain. Although descriptive clarity is mentioned a great deal, it is never actually examined at any length; yet we need to know exactly to what it refers and what properties or characteristics it exhibits. In the absence of a clear discussion, we are left with the task of trying to understand what descriptive clarity means from what McDermott says. It does not seem unfair to assume that descriptive clarity probably refers to one of the following sets of ideas:

a. A direct correlation between the categories employed in the description and some putative properties of the object under discussion.

The test of adequacy under this rubric would have to consist in a direct comparison of the description with the object, and would involve looking for and seeing similarities, measuring them, and so forth. To do this, however, requires either some prior description of the object under discussion so that the investigator will know what to look for, or the incorporation into his comparisons and measurement of common sense conceptions of the object. The search for direct correlations dissolves, then, into a search for some protocols for validating the prior descriptions upon which the subsequent observations are to be made. One route that might be chosen to work through this problem would be to assume the unproblematic nature of assimilating subsequent descriptions to prior ones ('they are just like that aren't they?') and then attempt to demonstrate validity under control conditions. The model for this kind of assimilation is probably that of classical electro-magnetics and optics where wave equations for electro-magnetic radiation become assimilated into the Maxwell field equation. The grounding that this route provides has two implications: Exercizing control to achieve validation can only take place within some approximation to the logico-experimental method. Also, transformations across prior and subsequent descriptions have to presume a constancy of object (e.g. the phenomenon of electro-magnetic radiation is the *same* as a field of electro-magnetic forces). McDermott explicitly rejects the former and the whole tenor of his paper is antipathetic to the latter. It would appear, therefore, that this first notion of descriptive clarity and how to obtain it will not do.

b. The presentation of an internally coherent description where activities are characterized within some predefined framework or rationale.

The problem with using internal logical consistency as a criterion is precisely that it rules nothing out. Any logical reconstruction of events must be allowed to stand, for we have no way of discriminating good from bad except in terms of consistency. To take a much over-used case, consider that of memory slips, speech errors, and so on. We have no way of determining which is a better (more adequate) description of these phenomena: Freud's (1975) relation of them to the dynamics of the unconscious, or Jefferson's (1975) account of them as merely verbalized speech production errors. The question is not *whether* Freud provides a better explanation of these things than Jefferson but that we have no way of telling *if* one is better than the other. If this is the case, are we forced to embrace both as somehow partial and, at least in principle, reconcilable? (It will be remembered that this was Butterworth's solution.) If some wider account can reconcile say Freud and Jefferson, do we not return to the problems associated with (a) laying out that account and (b) warranting the constancy?

Both (a) and (b) are, in one guise or another, conventionalized positions within the social sciences. McDermott does not accept either of them. Indeed, the point of his paper is to try to supersede such positions by laying out a new way of doing ethnographic descriptions. If McDermott's conception of adequacy is not (a) or (b), what is it? It appears to be something like:

c. the reproduction in sociological descriptions of the features of activities discernable under the natural attitude.

Let us try to unravel this a little. The incorporation of features from the natural attitude into sociological descriptions cannot mean simply measuring members' accounts against sociological ones and granting the former primacy. Nor can it mean that any discrepancy between the two kinds of descriptions should be resolved by reshaping sociological descriptions so that they fit common sense ones. Such a strategy would be, in effect, the reduction of sociology to common sense. Sociological description under the theoretical attitude of sociology involves work whose outcome is distinct from that of common sense. This does not imply that the methods used by common sense and sociology are necessarily different; but it does imply that their outcomes are. There must be some kind of relationship between sociological descriptions and common sense, otherwise we would fall prey to the problems faced in (b) above. However, the relationship does not have to be one of identity. McDermott attempts to deal with the problem in the following way:

> The ethnographer's adequate account of what natives do together must follow from the way in which the natives structure a situation to allow their participation with each other from one moment to the next. The ethnographer must articulate the same hesitant and momentary context that the natives are displaying to each other and using to organize their concerted behavior. (McDermott et al. 1978: 246)

He continues, proposing that

> The warrant for setting such a difficult goal for ethnographic description is that people manage concerted activity only by constantly informing and conforming each other (sic) to what it is that has to happen next. (1978: 246)

The crucial term is the phrase 'must follow from' in the first quotation. Yet it is left exasperatingly vague, despite the fact that what 'following from' in this context means is precisely what the whole argument is about. 'Following from' has to be cast in strict methodological terms as a set of instructions for making descriptions *before* we can see how rigor and clarity are to be achieved. Since this does not happen, McDermott's proposals come to no more than a rhetorical plea for the alignment of ethnographic accounts with common sense ones. Lay ethnography is, therefore, treated as wholly unproblematic (as *not* a topic for investigation) and as the proper measure for sociology. Apart from this failure, McDermott seems also to depend on an unargued justification for naturalism. This accounts for the warrant mentioned above. This results in an assertion of the primacy of common sense procedures in *all* methodological matters because:

> We can use the ways that members have of making clear to each other and to themselves what is going on to locate to our own satisfaction an account of what it is they are doing with each other. In fact, the ways they have of making clear to each other what they are doing are identical to the criteria which we use to locate ethnographically what they are doing. (McDermott et al. 1978: 247)

But, such a justification for naturalism and the vague outline of what it mean, depend upon a particular constitution of the natural attitude; a constitution which is not argued at all or, as we argue, understood. This constitution involves taking one methodological presupposition and holding it constant while varying other elements within the natural attitude. The presupposition which is held constant might be summarized in the following formula: treat social activities as *contingently* orderly. The extent that McDermott has embraced this proposition can be seen from his conclusion that social life displays a 'working consensus'. Such a consensus could only be the result of the practical activities that members use to achieve orderliness, and not the result of some built-in tendency to orderliness in social life itself. This postulate concerning the sociological treatment of daily life is taken over by McDermott from Garfinkel. In Garfinkel's work, it has been used as a facilitating device for the interrogation of activities for their achieved orderliness. Such sociology proceeds under what Garfinkel (1956) refers to as a praxeological rule. As such, this postulate has only *methodological* significance and not metaphysical or ontological ones. There are simply no grounds for supposing that members have to experience the world or social activities in exactly the same way that sociology conceives of those phenomena. The praxeological rule is a means of constructing social life for sociological purposes. It is crucial that the importance of this is understood. Nowhere in Garfinkel's work is the claim made that the sociological conception of social activities must be identical with common sense ones, or that common sense social theories are the measuring devices against which sociology has to be laid. Garfinkel does talk about sociologists treating social life as a means of providing solutions to sociology's problems and he does say that common sense accounts of social life, as they display nonprofessional sociological theories, are, in fact, lay ethnography. But he does not say that sociological descriptions have to conform to formulations derived from this lay ethnography. What McDermott seems to do is define social activities under the postulate of contingent orderliness, provide descriptions of those activities that can be derived from that postulate, and then claim that the descriptions are valid because of the consonance between them and the defined nature of members' activities. The net result of this circulatory argument is that McDermott's sociological descriptions appear naturalistically bizarre. They are almost totally at variance with how we normally conceive social life. By turning a methodological presupposition into a metaphysical one, McDermott has driven a wedge between sociology and common sense rather than uniting them. This wedge gradually widens the gap until we are faced with observations such as:

> People never know exactly how to make sense of each other. Rather, they must do interactional work to create the kinds of environments which members can recognize as suitable environments for displaying whatever it is they know how to do with each other. (McDermott et al. 1978: 269, n. 3)

There is a world of difference between a conception of members as social actors, as courses of action and treatment, that is, as a methodological principle, and promoting that view as definitive of how they *must* experience each other in their daily lives and against which tests of descriptive adequacy have to be formulated. Presupposing contingency as a methodological principle does not imply having to lead one's non-investigative life by investigative methods. What this peculiar (and theoretical) stance allows is the treatment of phenomena, such as mutual understanding, as essentially public and cooperative rather than as yet another puzzle to be stored in the closet of the mind. Treating mutual understanding as practical enables the sociological investigation of members' methods for investigating, managing, and finding mutual understanding. But all of this depends upon a theoretical attitude, a viewpoint, that suspends the primacy of common sense theorization. To take Garfinkel's construction of social life and to read that back into social life, thereby giving it primacy over common sense, is the oddest of inversions. It results in a paradox where, since he stresses the common sense and practical nature of theorizing, Garfinkel would have to argue that the professional and sociological attitude is found to be universalized in the natural attitude. This he plainly does not do.

As we suggested at the beginning of this discussion, once McDermott's formulations have been clarified and straightened out, the points he makes become understandable, if not uncontentious. However, they still stand in need of some reshaping. The confusion over the proper role of the methodological principle of contingent orderliness for ethnography does not, in fact, render his criteria ludicrous. As criteria for the presentation of ethnographic findings, they are eminently sensible. It is only when McDermott tries to apply them to his own research that the misconceptions we have outlined come to the fore and have a noticeable effect. As McDermot lists them, the criteria are:

a. members usually reference, or in some way formulate, the context of their actitivies.
b. members usually organize their posture to form a configuration or position which signals the context of their behavior.
c. members orient towards their concerted behavior and accordingly constitute and signal their contexts for each other.
d. members usually hold one another accountable for proceeding in ways consistent with the context of their concerted activities.

As these stand, they are not criteria at all. They might better be thought of as observed features of daily life; observations which have been made under the contingency presupposition. This is not intended to deny the use of these features within a particular sociological description. But it is to say that that is all they are. Reconstructed as criteria for the evaluation of sociological descriptions they might take the following form:

Where the presupposition of the contingency of orderliness is operational, ethnographic descriptions may be considered adequate if they incorporate:

a_1 descriptions of the methods members use to form the contexts of their behavior;
b_1 descriptions of members' orientations to the sequential ordering by which they constitute and signal context;
c_1 descriptions of members' practices for deducing and ascribing the accountability for courses of action.

(b) has disappeared from (a_1)–(c_1) since it has become redundant. Posture may be treated as just one way among many for formulating context. In summary form, McDermott's criteria can be set out as a collection of relatively straightforward requirements indicating the ways in which context utilization is achieved:

a_2 by methods of formulating,
b_2 by methods of displaying an orientation to sequential orderliness,
c_2 by methods for ensuring accountability.

Once the collection of criteria has become available in this form, several interrelated questions leap to mind. Is adequacy achieved only when all three criteria are met or will a partial collection satisfy? If a subset of the collection will do, are any members of the whole necessarily included? Is there a preferential ordering among any members of the set? Despite the vociferous concern with methodological rigor, McDermott gives us no help at all here. The best we can do is to consider these questions in light of the descriptions he gives, and hence, not as criteria at all, but as *ethnographic maxims*.

Treat contexts as formulated in activities

We have suggested several times now that the methodological injunction that a maxim like this can operate under is one that suspends the presupposition of mutual understanding. Members' activities can then be treated as courses of action designed to make their understandings available. This leads us to a conception of actions and activities designed for particular participants in particular circumstances; a feature described by Sacks as *recipient design*. Once the principle of recipient design becomes a noticeable feature of activities, it is possible to treat their orderliness as a product of participants' coordination, so that the contextuality of meanings can be treated as well as reproduced. To repeat, taking such a stance makes a great deal of difference as to how one goes about making sociological investigations of daily life. It makes no difference at all how daily life is accomplished and experienced under the natural attitude, except in so far as that life is investigated by sociology. To suggest, as McDermott (1978: 247) does in his discussion of this criterion, that 'we have to become sensitive to some of the less obvious formulations in terms of which people struggle to keep themselves informed of what they are doing together', is meaningless. A methodological presupposition about how to treat daily life under some theoretical attitude can never be allowed to go proxy for a philosophy of life. Garfinkel's ethnomethodology does not set out to be, nor is it amenable to, a Californian version of existentialism.

Treat activities as displaying an orientation to sequentiality

In the event, the orientation to sequentiality is explained and illustrated by reference to Sacks' work on conversation. This maxim is an instruction to treat orderliness as a normative order and this is certainly how Sacks views conversation. Sacks suggests that the organizational principles of the turn-taking procedures are both oriented to and produced by conversationalists. This has the effect of allowing certain kinds of treatment of transcripts. To use his own phrase, they can be interrogated

to see what features they display. It is clear, then, that Sacks' work embodies a sociological conception of conversation, not a generalizable definition of it. His suggestion is simply that *if* coparticipants are treated as orienting to the preservation of the orderly features of the turn-taking system, as observed in transcripts, *then* it becomes possible to describe some of the putative design features of, for example, dirty jokes (the example McDermott uses). As a sociological conception 'coparticipant' has all of the classic features of an ideal type (perhaps better, sociological idealization). Coparticipants are speaker turn-takers. They are not people as we experience them in our daily lives.

Treat members as displaying the accountability of activities

Given the nature of maxims (a) and (b), it should not be difficult to imagine what this maxim makes available. Members' conformity to normative order is taken as grounds for treating their activities as 'displays', recipiently designed 'displays'. Since members treat activities as occasioned, then activities can come to be treated over their courses as 'documents' of what they are, with each successive phase being treated for what it says about preceding and subsequent phases in the coordinated activities. It is this orientation to sequence that Garfinkel calls the *documentary method of interpretation*. That McDermott has completely scrambled this idea is seen in the example with which he chooses to illustrate this maxim. The example concerns a remark made by a teacher during a reading lesson. In talking about it, McDermott treats the accountability of the remark as only occasionally displayed: that the remark is made indicates that accountability is sometimes discernable; whereas Garfinkel's argument is that accountability is a pervasive feature *of* daily life that only occasionally becomes thematic *in* daily life. It is its pervasiveness that is central to Garfinkel's kind of ethnography.

We want to make a final point about these maxims. In our discussion of McDermott's proposals, we tried to draw a consistent line between the theoretical attitude of sociology and the natural attitude of common sense. We have done so in order to highlight the nature of McDermott's confusions. This does not mean that we would want to argue that there can be no relationship between sociology and common sense, or that sociology's descriptions can choose to ignore common sense ones. The question of the nature and degree of interpenetration — at what levels and for what purposes — is, for us, a central one in sociology generally, and in ethnomethodology in particular. It is one to which considerable and painstaking attention must be given if any solutions are to be found.

Such solutions will not be provided by a cavalier assumption that such problems are not wholly serious ones that can be circumvented by assimilating common sense to sociology, or *vice versa*.

This has been quite a lengthy discussion of what is only half of McDermott's paper. We have said almost nothing about his ethnography of classroom interaction. The reasons for this should, by now, be plain. Since McDermott's criteria were set up to provide ways of evaluating ethnographies, and since, at several crucial points, they were found to be vague, vacuous, and plainly mistaken, we should hardly be surprised to find that his description of the reading lesson, his ethnography, is disappointing. And yet, from that disappointment one or two conclusions might be drawn.

First, the descriptions given consist of loose commentary. The sets of categories, features, and observables, set out in Section III of his paper, all but disappear. What we are given instead is a commentary which tracks through, movement by movement, what is happening. For example, in discussing a particular cluster of movements we are told:

> ... the teacher has just oriented to the fact that Maria has stopped reading in the middle of her turn, and it can be seen that Perry is looking out elsewhere. It becomes apparent that Perry has kept a careful feeler out for developments in the reading group. On exactly the same frame both Perry and the teacher start to move their heads towards one another. (1978: 261)

As Thoreau once wondered, is it really worth travelling all the way around the world simply to count the cats in Zanzibar? Was all of the discussion of rigor, descriptive clarity, and criteria necessary merely to provide this kind of description? Whatever formality McDermott's criteria might provide for ethnographic descriptions, none seems to be observable here.

Second, what McDermott's description tells us most about is how teachers handle children, how they keep their attention focused, how they notice inattentiveness, and so on. That the lesson is a reading lesson is irrelevant. They might have been doing anything. For the participants, it was a recognizable reading lesson and that recognizability provides for the organized character of their activities. It was the reproduction of that recognizability which was the driving point behind the search for criteria in the first place. And yet, in this description, we are provided with no account of the *in situ* recognizability of this lesson as a reading lesson. It is precisely this description which the criteria turn on. It would seem, then, that McDermott fails his own tests. This is not to say that these tests are impossible to satisfy. Rather, McDermott goes about satisfying them in the most ill-conceived manner.

Conclusion

What, then, are the conclusions we would want to draw from all of this? First, and most importantly, we would want to point to the difficulty, and hence the comparative rarity of real methodological innovation. Second, we think that what we have said has implications for the ways that we should view the relationships between the various social science disciplines, in particular, the place of sociology. All of us are familiar by now with the truism that sociology's methodological troubles derive from an over-enthusiastic incorporation or rejection of canons of scientific procedure. None of the available schemes said to designate scientific procedures seem to fit what sociologists want to do. Usually this means that either the schemas or sociology will have to be altered. But perhaps it is the use of science as an imitative model that needs to be questioned. In light of what we have been saying about the methodological proposals made by Butterworth and McDermott, we feel that one might get a clearer view of what sociology is about if sociologists stop thinking about their work as a putative science and begin to treat it as an analogue of philosophy. Like philosophy, sociology can be set up as a cluster of ways of constituting problems, each of the elements in the cluster providing its own topic to be investigated and its own arguments on data, findings, and conclusions. Different sociologies provide different organizations to phenomena, different starting points for investigative strategies. One comfort from a view such as this is that we no longer have to be embarrassed by the fact that sociology is disputational and that there have been few radical innovations. Only the most rabid devotee of the fashionable in philosophy would deny that there have been merely a handful of genuine revolutions in philosophy since 1600. Those associated with Descartes, Hume, Kant, Frege, and Wittgenstein would be our choices. That sociology is still locked in debate over what are essentially nineteenth-century problems is irrelevant; most of philosophy is preoccupied with issues that are even older! Another implication would seem to be the pointlessness of searching for a formula by which to integrate sociology with other disciplines. If the differences within sociology are so significant, is it not equally so for the differences between sociology, psychology, linguistics, and the rest?

Even with this skepticism concerning innovation and integration, together with a certain sanguinity with regard to the extent of methodological tokenism, we do not think that there is cause for complacency or despair. This would be our third conclusion. As we suggested when we were discussing McDermott's proposals, we feel that, properly understood and applied, the maxims or criteria he enunciates could well become the basis of genuine innovation and provide the basis for the kind of communication ethnography he seeks. Notice, we said that they *could* become. They have potential and that is all. They offer promise, and they show a way forward — no more. As Garfinkel has painstakingly and repeatedly pointed out, his investigations are simply a beginning. They map out a field of possibilities, not a collection of conclusions. Only time will tell what, if anything, these possibilities will amount to. The only way to find out is to try them; to extend, elaborate, and, where necessary, amend them. The maxims which McDermott, along with others, has taken from Garfinkel's work are guidelines and resources, not dogmas of an epistemological and metaphysical kind. Nor are they merely convenient slogans to be jettisoned as soon as the methodological going gets tough. Both dogmatizing and sloganizing are evidence of what we have called methodological tokenism, and both are, sad to say, characteristics of much of the work that claims lineage with Garfinkel's ethnomethodology.

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