Critical Discourse Analysis and Conversation Analysis: an exchange between Michael Billig and Emanuel A. Schegloff

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Whose terms? Whose ordinariness? Rhetoric and ideology in Conversation Analysis

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ABSTRACT. This article examines Schegloff's (1997) defence of Conversation Analysis (CA) and his attack on critical discourse analysis. The article focuses on Schegloff's claims that CA takes an empirical stance without a priori assumptions and that it examines participants' talk in 'their own terms'. It is suggested that these claims are problematic, and that CA, as depicted by Schegloff, contains an ideological view of the social world. This can be seen by examining CA's own rhetoric, which conversation analysts themselves tend to take for granted. First, CA uses a specialist rhetoric which is literally not the participants' own terms. Moreover, this specialist rhetoric enables conversation analysts to 'disattend' to the topics of conversation. Second, CA's 'foundational rhetoric' is examined. It is suggested that this foundational rhetoric, which includes terms such as 'conversation', 'member', etc., conveys a participatory view of the world, in which equal rights of speakership are often assumed. The assumptions of these rhetorical conventions are revealed if they are applied to talk in which direct power is exercised. In this respect, CA is not, as Schegloff suggests, ideologically neutral, but habitually deploys a rhetoric that conveys a contestable view of social order.

KEY WORDS: *conversation analysis, ideology, rhetoric*

Schegloff's (1997) article 'Whose Text? Whose Context?' provides an important defence of conversation analysis (CA). The article is significant not only because of its strong clear argument, but also because Schegloff is one of the most distinguished creators of CA. In his article – and again in his reply to Wetherell's (1998) considered response – Schegloff provides a powerful case for using the

procedures of CA (Schegloff, 1998). He contrasts the empirical stance of CA with that of critical discourse theorists, who, according to Schegloff, let their own assumptions dominate their analyses. Powerful though Schegloff's arguments might be, and great his personal contribution to the study of talk, it is necessary, however, to analyse some of the assumptions of his position.

Wetherell (1998) suggests that CA needs to be augmented by social theory in order to examine the ideological aspects of language. The present argument aims to complement her critique, but it takes a different line. It critically analyses Schegloff's depiction of CA as merely a detached investigation of conversational detail. CA contains its own sociological and ideological assumptions. As such, CA is always more than conversation analysis, and, by implication, it is not so different from the sort of critical analyses, that Schegloff takes to task.

First, some disclaimers should be made. Although this article will doubtless appear critical of Schegloff's position, some major points of agreement should be stressed. In no way do I wish to defend the sorts of loose 'critical' analyses, which Schegloff had in mind but which he was too tactful to name. I share Schegloff's unease about studies which pronounce on the nature of discourses, without getting down to the business of studying what is actually uttered or written (see Billig, 1997a, for a critical examination of cultural studies on this account). Like Wetherell, my academic background is in discursive or rhetorical psychology (Billig, 1996). Discursive psychologists have shown that much insight is to be gained by close-grained analyses of discourse, using most notably CA (Antaki, 1994; Billig et al., 1988; Edwards, 1997; Edwards and Potter, 1992; Potter, 1996; Potter and Wetherell, 1987; Wetherell and Potter, 1992). In my own work, I have stressed the contribution of CA: for example, the notion of turn-taking is vital for recasting the processes of Freudian repression in terms of discourse (Billig, 1997b, in press).

The present critique is not intended to be an overview of CA. It concentrates on Schegloff's portrayal of CA, because his dismissal of critical discourse analysis follows from this portrayal. At present, there is debate amongst conversation analysts about the directions that CA should take (see, for instance, Watson, 1997, and more generally Silverman, 1998, chapter eight). Not all adherents of CA would necessarily subscribe to Schegloff's construction of CA. I do not go into these debates as such, although some of the issues raised in them overlap with some of the issues I discuss here.

In this article, I do not follow Wetherell's (1988) example of introducing new conversational data; nor do I re-analyse Schegloff's data extracts. There is a case for stepping back from the sort of data which CA examines, in order to investigate CA's own rhetoric – or at least, the rhetoric which Schegloff uses to present CA. The present critique, thus, belongs to the tradition of the Rhetoric of Inquiry, which takes the writing of academic disciplines as its object of study (Bazerman, 1988; Billig, 1994; Gross and Keith, 1997; McCloskey, 1986; Myers, 1991; Nelson et al., 1987; Simons, 1989, 1990). If one wishes to talk of 'data', then Schegloff's own texts can be treated as data: their rhetoric can be treated as

objects for analysis. By so doing, it will be possible to argue, contra Schegloff, that CA's 'foundational rhetoric' is not neutral, but it conveys a particular and contestable image of social order.

Studying participants in their own terms

Schegloff constructs a number of contrasts between CA and (unspecified) critical discourse analysis. As might be expected, these contrasts are not rhetorically neutral but are designed to illustrate the strengths of the former and the weaknesses of the latter. Schegloff's prime complaint is that critical theorists claim to know how power is accomplished within talk but do not bother to study the mechanics of conversation. Schegloff's contrast between the a priori biases of critical analysis and the empiricism of CA is related to another claimed difference. Critical analysts supposedly impose their own terms on the object of analysis, while CA is based on the terms of the participants. CA follows the injunction of 'taking seriously the object of inquiry *in its own terms*' (Schegloff, 1997: 171, emphasis in original). CA privileges 'the orientations, meanings, interpretations, understandings etc of the *participants* in some sociocultural event' (p. 166, emphasis in original). By contrast, traditional sociologists and critical analysts 'deploy the terms which preoccupy *them* in describing, explaining, critiquing, etc. the events and texts to which they turn their attention' (p. 167, emphasis in original). Schegloff produces a nice ironic move, which returns the rhetoric of critical theory against its practitioners. By imposing categories on participants, critical analysts display a 'theoretical imperialism' or 'a kind of hegemony of the intellectuals' (p. 167). As shown later, critical analysis is not uniquely vulnerable to such irony. Schegloff's own rhetoric, or that of his version of CA, can likewise be turned against itself.

Schegloff proposes that CA should be methodologically primary. He writes of 'the mandate to first understand the target "text" in its own terms' and stresses that this mandate applies to 'talk-in-interaction' (Schegloff, 1997: 171). The analysis, thus, must be based on participants' hearings: 'If the parties are hearing that way and responding that way – that is, with an orientation to this level of turn design – we are virtually mandated to analyze it that way' (p. 175). Schegloff is concerned about analysts, who impose their own theoretical concerns. Feminist analysts might be predisposed to 'hear' the operation of unequal gender power in interchanges between men and women. Unless the participants themselves can be heard to 'orientate' to gender issues, then this hearing of gender will be illicit (or unmandated). This is the point of Schegloff's (1997) second example, in which males are heard to mention gender. There is no 'impossible hurdle' to analysing the concerns of gender, but analysts must not introduce these concerns if the participants have not done so. That would be an infraction of the principle to study participants in their own terms.

Realist tales

The first issue is to probe what exactly it means to claim to study participants' talk 'in its own terms'. This notion, which lies at the core of Schegloff's defence of CA, cannot be taken for granted. In outlining his thesis, Schegloff claims that analysts should directly observe the realities of social interaction. Analysts do not need 'readings in critical theory, but observations – noticings about people's conduct in the world' (1998: 414). As Schegloff realizes, this idea of direct observation may seem to indicate 'a methodological and epistemological naivety'. Nevertheless, he defends the idea that talk-in-interaction has its own 'internally grounded reality' (1998: 171). Heritage (1984) expressed the same idea when he claimed that CA provided a powerful microscope for the study of social life. Similarly, Boden (1994) has written that 'by giving back to social agents their knowledgeability of their own social actions, it was then possible to sit back and observe the structuring quality of the world as it happens' (p. 74).

Schegloff's highly sophisticated 'naive methodological and epistemological naivety' needs examining. As analysts have shown, the realist rhetoric of science is not straightforward (Edwards, et al., 1995; Potter, 1996). Scientists are making all sorts of extra-scientific claims when they claim that the facts merely speak for themselves (Gilbert and Mulkay, 1984). Such realist tales, including those told by the practitioners of CA, are themselves rhetorically examinable.

It is not the case, as Boden suggests, that the conversation analyst can just sit back and observe. Like all academic disciplines, CA must be written. For this, it requires its own practices of writing. There are certain words and phrases, which let readers know that they are reading a CA text and that the author is 'doing CA'. There is no doubt that CA uses a highly technical vocabulary. This creates a paradox. Although participants are ostensibly to be studied 'in their own terms', they are not to be written about in such terms. Instead, analysts use their own terms to accomplish this observation of participants' own terms.

The speakers, conventionally studied by CA, do not talk of 'adjacency pairs', 'preference structures', 'receipt designs', 'self-repairs', etc. These are categories which the analyst imposes. Schegloff several times makes a distinction between the sort of ordinary language, that the participants might use, and the specialized language of CA. He uses the word 'vernacular' to describe the sort of ordinary language that the analyst must get beyond in the analysis. In his first example, Schegloff (1997) demonstrates that one of the participants in the talk is not interrupting 'in the conventional vernacular sense' (p. 196). He writes that to call a participant's response 'an emotional response' is to give a 'vernacular gloss' (p. 196). More generally, he contrasts CA, which is applied to the world 'refracted through the prism of disciplined and molecular observation', with critical analyses which are 'refracted through the prism of "casual" vernacular observation' (p. 180). 'Vernacular', in these contexts, is not being used in a neutral manner: the analyst is being criticized for using (or being misled by) 'vernacular' terms, which are contrasted with the specialized vocabulary to be deployed in the business of analysis.

The question, then, is how the analyst gets from the participants' own words to the specialized non-vernacular vocabulary of analysis. The realist tale is a way of dismissing the problem. The 'naive methodology and epistemology' allow conversation analysts to claim that they are not 'imposing' categories: they are merely labelling what actually exists and can be observed to exist. Thus, it is asserted that the technical terms describe objective realities in an unproblematic way. The analyst can point to a transcribed text and say 'Look, there is a preference structure', as surely as a realist can kick the table as proof of that object's existence (Edwards et al., 1995).

The difference between the analyst's rhetoric and the vernacular of the participants is more than merely a difference in vocabulary. Analysts are attending to matters that the speakers do not. In Schegloff's (1997) first example, the speakers are discussing how their son's car has been vandalized. The topic, which preoccupies the speakers, is not of especial interest to the analyst. Schegloff writes of the use of 'second assessments', 'WH-questions' and so on – topics, which the speakers do not talk *about*. This difference between the topics of the analyst and the topics of speakers has been discussed by some conversation analysts. For example, Sharrock and Anderson (1987a) specifically discuss how analysts move away from attending to the features of talk that 'are readily observable' by the speakers. According to them, the 'result is that Conversation Analysis *necessarily* disattends to what actors may see as the business of their talk, in favour of the activities which actors engage in solely by virtue of their character as operators of a speech exchange system' (p. 246, emphasis added).

The reason why this is a necessity, according to Sharrock and Anderson, is that CA is based on the 'stock idea' that conversations are organized and orderly (1987a: 245). Analysts seek to uncover this underlying organization from different conversational incidents, extracting the general features of conversational organization from specific examples. Thus, it has been claimed that CA is 'content free' (Lee, 1987). Psathas (1995) points out that the turn-taking system, which has been so central to classic CA work, is assumed to be 'context free' and is 'independent of the contents or topics talked about' (pp. 35–6). Schegloff's (1997) treatment of his examples exemplifies this form of analysis, which Watson (1997) identifies as 'traditional conversation analysis'. Schegloff finds patterns of sequential organization, relating to assessments, turn completion and turn-taking. Such patterns are general features of conversation and are unrelated to the specific topic, which the participants are discussing. To accomplish this sort of analysis, Schegloff, to use Sharrock and Anderson's terms, 'disattends' to the particular topic, that the participants see themselves talking about.

Two problems can be raised. The first is to question whether the principle to study conversation in the participants' 'own terms' is necessarily breached, at least in part, by its own practice and by its programme to find general structures of orderliness. In order for CA to study, for example, the orientations to turn-taking, it 'disattends' to what the participants see as their main concerns. In a literal sense, analysts, in writing of the participants, impose their own terms.

The second problem arises if the analyst wishes to study those matters, to which traditional CA routinely 'disattends'. Can analysts do so, if they follow typical CA practices that direct analytic attention to issues which the participants do not overtly talk about? For instance, an analyst, such as a critical feminist, might wish to use Schegloff's first example to talk of patterns of child supervision, rather than of second assessments. The irony is that to follow Schegloff's recommendations – and ostensibly to observe the participants in 'their own terms' – the analyst would end up speaking about the things that the participants do not speak of, using a set of terms which the participants do not use. But to speak of the same things as the participants do, the analyst would run the risk of being accused of imposing her own categories on the analysis.

Foundational rhetoric of CA

One of the great strengths of CA is its insistence on working with openly available data. The analysts specifically relate their technical terms to aspects of extracts of talk. For the sake of argument, let us concede for the moment that there is no epistemological or rhetorical difficulty with the ways that CA translates the words of those it studies into its own technical vocabulary. However, not all the aspects of CA's technical vocabulary result from such exhaustive pointing to textual extracts. Like all theoretical perspectives, CA deploys terms which might be called 'foundational'. These are not terms which are linked to specific pieces of data, but terms which enable the pointing and the linkages to be made. The foundational rhetoric of CA is not justified in terms of specific features in particular transcripts, but is used in order to analyse that data.

Using Schegloff's texts, one might start to compile a list of foundational terms. The list might include 'conversation', 'mundane conversation', 'everyday conversation', 'vernacular', 'participants', 'members', 'talk-in-interaction', 'orientation'. Schegloff in his article does not point to specific features of his data in order to claim that 'those words in that line' provide an example of 'talk-in-interaction' or a 'member's orientation'. He takes their usage for granted. This usage marks these terms as rhetorically different from terms such as 'preference structures', which are linked to particular examples.

Sharrock and Anderson (1987a) claim that CA examines people's taken-for-granted habits. CA uncovers, they suggest, orderliness 'in the unnoticed, taken for granted, flotsam and jetsam of talk in all our ordinary, daily lives' (p. 247). Again the principle can be turned around. As Ashmore (1989) has so provocatively demonstrated, the rhetoric of ethnomethodology can be turned against ethnomethodology. In this case, one might ask, what does CA take for granted in its own discourse, as it examines the taken-for-granted habits of 'ordinary' speakers?

The question itself is not altogether innocent, if ideology is to be uncovered in the unnoticed habits of life (Billig, 1991; Bourdieu, 1990; Eagleton, 1991; Van Dijk, 1998). So ideology might stalk the unnoticed and the taken-for-granted

assumptions of intellectual inquiry, especially the sort of social inquiry that overtly claims to be ideology-free and merely empirical. Therefore in asking what assumptions the foundation rhetoric of CA conveys, one can be raising questions about its ideology.

The rhetoric of 'ordinary conversation'

As always, Schegloff urges analysts not to remain at the level of abstract theory, but to observe how discourse is used. Therefore, abstract remarks about CA's foundational rhetoric are insufficient. One needs to examine how specific terms are used. Two sets of terms are briefly considered. The first set refers to 'conversation' and the second to those who are observed to engage in conversation.

Schegloff in his critique of critical studies, like many practising conversation analysts, does not spend time discussing what a 'conversation' is. The term is deployed as if there were no difficulty in pointing to conversations. Schegloff conveys that his two data examples are extracts from 'conversations', without pointing to specific features in the data to justify that description. Like other conversation analysts he recognizes that not all talk might be conversational. Thus, Schegloff (1997) switches between the terms 'conversation' and 'talk-in-interaction', sometimes, but not always, conveying a distinction between the two. For example, he refers to 'work on conversation and other talk-in-interaction' (p. 168), implying that not all talk-in-interaction is conversation. CA, however, does not appear to have a technical term to denote talk-in-interaction which is not conversation (although, as shown later, 'institutional talk' sometimes fulfils this function). The omission is indicative. Schegloff implies that CA is not confined to studying 'conversation', but it can be applied to talk which may not be conversation. He does not discuss why the analysis is called '*conversation* analysis', when it can be applied to non-conversations. As Psathas (1995) writes, in briefly discussing this issue, the term 'conversation analysis' appears to be 'a misnomer', for its object of study is wider than conversation (p. 2). The point, however, is not that analysts *should* be more precise, but that an analyst like Schegloff can engage in CA without being so.

Sometimes CA texts employ the distinction between conversational talk-in-interaction and non-conversational talk-in-interaction in order to make argumentative points. For instance, the distinction can be deployed in order to criticize the conventional methodology of interviewing. Here the terms 'ordinary' and 'naturally occurring' will be mobilized in order to distinguish the sorts of conversations, studied by CA, from the sort of second-best talk (normally not characterized even as 'conversation') studied by sociologists. Schegloff (1998), in his reply to Wetherell (1998), distinguishes between '"ordinary" conversation' and Wetherell's interview data which is described as 'an exchange' and appears to be 'researcher-prompted' (p. 415). Heritage (1988), in the first paragraph of an introduction to CA, distinguishes between accounts which are '*naturally occurring* in conversation' rather than being elicited by interviewers (p. 127, emphasis in

original). 'Natural', 'ordinary' and 'conversation' are deployed to make this distinction. Heritage also offers another distinction in the next sentence: 'I will focus on the use of accounts in *ordinary conversation* rather than some more specific or specialized location in social space such as a hospital, a school or a courtroom' (p. 127, emphasis in original). Here, it would appear, the 'ordinariness' of 'ordinary conversation' does not depend upon it merely being 'natural', but on its lack of ties to a specific location. As in Schegloff's examples, the non-specific locations of 'ordinary conversations' are often private homes, or, as in the case of recorded telephone conversations, conversations between homes.

'Ordinary conversation' is sometimes distinguished from 'institutional talk' in terms of rights of participation. For example, Drew (1991) suggests that 'in conversation, turns are allocated equally between participants' for the 'rules for allocating turns ... do not favour any particular participant or category of participant' (p. 21). Thus, 'in principle at least, participants in conversation generally share equal rights of speakership' (p. 22). This is not so, he suggests, in institutional settings where 'there might be quite striking inequalities in the distribution of communicative resources'. For example, in classrooms, courts and news interviews, 'talk' may be restricted (p. 22). One might note how the term 'conversation' is deployed for the non-institutional setting, while Drew switches to 'talk' to describe the institutional setting. Similarly, Psathas (1995) distinguishes between 'free-flowing conversational interaction' and other talk, such as those in classrooms, religious ceremonies, etc., where there are restrictions on who might speak and when they might do so (p. 36; see also Nofsinger, 1991: 4 ff.).

Equal rights of speakership are frequently presumed to be a feature of 'conversations', rather than being specifically demonstrated as such. The presumption is based on assuming that the speakers are sharing the same system of turn-taking. Drew's point is to demonstrate how, in the course of a conversation, there can be specific, momentary asymmetries. In this respect, his analysis is a deviant case study (Heritage, 1984, 1988). In specific moments, when a speaker claims more knowledge on the topic to hand, equal rights of speakership are suspended. The deviant case highlights the general supposition of equal rights of participation in conversation.

If Schegloff's use of CA's foundational rhetoric possesses a vagueness, this should not be assumed to be a fault. It can be argued that academic vagueness in rhetoric can be a vital means of accomplishing a particular way of doing social sciences (Billig, 1994). The vagueness about what exactly is a 'conversation' and what it is not has not hampered the development of CA over the past 30 years. Analysts who promote CA and who imply that it can be successfully applied to non-conversational talk, are demonstrating in a practical way that such distinctions do not matter. They are practising the principle that theoretical distinctions take second place to close working with the details of transcripts. Yet, this does not mean that their rhetoric and methodological practices are free from assumptions about the nature of the wider social world.

If analysts had been vitally concerned to distinguish between conversation and non-conversation, they might have found themselves moving from analysing details of talk towards making the sorts of distinctions that mainstream sociologists make. Analysts, for instance, might need to distinguish between different types of institutional settings. In doing this, they would not be practising CA in its traditional form, as exemplified by Schegloff (but see Watson, 1997, for arguments about how such a move might be made by modifying the practices of 'traditional', sequential CA). However, the point is that analysts such as Schegloff do not dispense with conventional sociological distinctions: they often presume them. Thus, the key term 'conversation' routinely conveys a distinction between institutional social interaction, in which there is an asymmetry of rights, and 'ordinary' interaction, in which there is equality (except, as Drew implies, at particular, observable moments).

It is easy to detect in these undeveloped but present assumptions a distinction which feminist social theorists have disputed. This is the distinction between the public and the private world, or the institutional and the domestic. Feminists have most particularly disputed the notion that 'in principle' there is equality of rights in the private sphere (see, for instance, Fraser, 1989). Of course, any dispute on such matters should, as Schegloff insists, be conducted in the light of close examination of the evidence. However, one can question whether such an examination can be accomplished if the key analytic terms assume the very distinction in the first place. At the minimum, a modified foundational rhetoric might be required.

Participatory rhetoric

More can be said about the sort of social world implied by the foundational rhetoric. Analysts use a number of terms to indicate those whom they are studying. These terms are typically used without justification, as if the rhetoric were obvious and unproblematic. Sometimes the speakers are 'the speakers'; sometimes they are 'participants', 'co-participants', 'members'. Schegloff (1997) uses all these terms. A detailed study of his usage, and that of other conversation analysts, would be likely to reveal that the terms are frequently used interchangeably. Certainly, no overt accounts are given why one term rather than another is being deployed on each occasion.

The terms themselves contribute to what might be called a 'participatory' rhetoric. 'Ordinary conversations' have 'participants', or 'co-participants', who share the same organizational principles of talk, such as turn-taking systems. As such the participants are 'members'. Schegloff uses the term 'member' without specifying what the members are members of. Perhaps it is a 'culture' or a 'society' (Garfinkel, 1967). But this is left unelaborated. To elaborate exactly what the 'members' are members of and what the criteria of membership are, would take this sort of CA towards the sort of sociology that it disavows. 'Member' is, of course, an analyst's term: it can be used whether or not the speakers orientate to any common 'membership'. In Schegloff's main example, we do not hear Marcia

and Tony referring to themselves, or to each other, as 'members' or 'co-participants'.

The terminology conveys commonality and equality. Some conversation analysts take it as a methodological virtue that CA does not deploy sociological categories. For example, Sharrock and Anderson (1987b) claim that CA shows 'little acquaintance' with 'the way of life' from which the chosen data extracts are taken (p. 299). They claim that the mode of analysis is largely independent of background factors, unless the 'conversationalists' attend to such matters (p. 316). However, matters cannot be quite so straightforward if the foundational rhetoric, including the terms 'conversationalists', conveys a historically and culturally specific way of life. As Burke (1993) has shown, the very idea of 'conversation' developed in early modernity in Europe and was bound up with assumptions of equal rights of talk within the specific, semi-private contexts of 'conversation'. In late modernity, one would presume that the sort of domestic conversations studied by conversation analysts, including those presented by Schegloff (1997), take place in private living spaces, not overheard by domestic servants. This need not be specified, because the analysts and their readers presume such a sociological organization. The 'members', then, are presumed to be members of something sociologically and historically specifiable.

There is a further feature of orthodox CA's deployment of its foundational rhetoric. This is the methodological and theoretical primacy given to conversation over non-conversational forms of talk. This primacy is conveyed by the maintenance of the term 'conversation analysis'. Institutional talk is seen, as it were, as the deviant case, marked by restrictions. By contrast, conversation is seen as the point-zero, in which the basic systems of organization, such as turn-taking, can be most easily discovered. Thus, Heritage and Atkinson (1984), for instance, refer to conversation as having 'bedrock status' (p. 12). As some analysts have pointed out, the primacy given to conversation over institutional talk can itself be deconstructed, as can the notion that 'mundane talk' is free from institutional structures (see, particularly, Potter, 1996: 85 ff.; see also Watson, 1997).

What needs to be stressed is that the bedrock status given to conversation is not merely contestable; it carries wider rhetorical and sociological presumptions. Above all, it conveys an essentially non-critical view of the social world. The bedrock situation – or the default option – is implicitly depicted as a world of equality and participation, in which 'members' share systems of social order. Inequality is to be found in the exceptions – in institutional talk, interviews etc. Thus, traditional CA, far from being free of social presuppositions, carries them in the regular deployment of its foundational rhetoric. The warnings against being theoretical, and against using conventional sociological analyses, together with the prescription to keep to the data, can serve to protect these assumptions from analysis. If Schegloff claims that critical discourse analysts explicitly bring socially critical concepts to their study of conversation, so it can be argued that his form of CA is not ideologically neutral: it implicitly uses socially uncritical concepts in the regular conduct of its analyses.

Textual identification of speakers

The conventions for identifying speakers/participants in CA texts emphasize the assumptions about contemporary 'ordinary' conversation. Again, this is a matter of observing rhetorical practices which are performed as a matter of habit and, as such, are not typically justified (but see Watson, 1997, for a discussion which focuses on this issue). Conversation analysts often go to pains to outline their transcription practices, without drawing attention to the problem of how to label the speakers in the transcript. Some conversation analysts identify the individual speakers/participants by single, capitalized letters, normally the first letters of the alphabet (i.e. 'A', 'B' or 'C'). Sacks, for example, did this frequently (Sacks, 1987). The practice emphasizes the analyst's lack of interest in categorizing the speaker sociologically. It conveys that the speakers are interchangeable: the conversational devices produced by 'A' might just as easily have been produced by 'B'. Since one of the expressed aims of CA is to reveal the organization of talk, this practice of naming underlines how the structures are equally shared by the 'participants'. It 'disattends' to differences between 'participants' or 'members'.

Another convention much used by analysts is to identify the speakers/participants by first names (whether real or pseudonymous is not always clear). Schegloff (1997) adopts this convention, presenting the speakers in his first example as 'Marsha' and 'Tony', although in the extracts, neither speaker uses the first name of the other. The use of first names conveys informality. When examining talk in institutional settings, analysts will sometimes identify speakers by social role, such as 'doctor', 'police officer' or 'plaintiff' (Pomerantz, 1987). Sometimes a mixed, or unbalanced, code is used. Those officially employed in the institutional setting are identified by role ('counsellor' or 'therapist'), while 'clients' ('members of the public') are given first names (Buttny, 1993; Edwards, 1997).

Schegloff (1997) does not justify naming the speakers. The names, of course, convey the gender of the speakers, in the way that 'A' and 'B' do not. Why this background information, and not other information, should be given is unexplained. Certainly, the speakers are not 'doing gender', as Schegloff himself stresses. The speakers could have been identified in terms of the content of their talk: they could have been identified as 'mother' or 'father', or 'male child-carer' and 'female child-carer'. Instead, the first names convey an absence of role. The absence is also a presence. In this case, it is a presence of the contemporary norms of informality, as practised in contemporary Anglo-Saxon and American discourse, where 'first-name terms' are considered *de rigeur*. Thus, it is 'Marsha' and 'Tony', not 'Ms A' and 'Mr B'. Not only is a historically specific style of interacting conveyed, but it is taken for granted as a 'natural', 'ordinary' way of relating. Moreover, the naming practice helps to 'disattend' to the specific topic of the talk. The speakers are not referred to as 'primary caretaker' and 'secondary caretaker'. Had they been so identified, the unsuitability of the episode for examining contemporary gender relations might have been highlighted. 'Tony' and 'Marsha' are

that statistically less frequent couple where the father has primary care responsibility for the child. However, the labels 'Tony' and 'Marsha' disattend to role responsibilities in the domestic setting, in the way that 'therapist' or 'plaintiff' do the reverse in the 'institutional context'.

The use of first-names, or the use of interchangeable letter codes, brings its own rhetorical baggage. It conveys that social distinctions are irrelevant in informal situations, where democratic conversational participation is expected. The naming practice, thus, supports the assumption that the conversational situation can be considered as a sociologically neutral space. Feminist critics have argued that the surface of private equality conceals deeply practised and often unnoticed inequalities. If there are such inequalities, then they should be detectable in talk. CA, as conventionally practised and written, may need to be adapted if it is to be suited to revealing such inequalities. Three related points might be made in this regard:

(i) Although analysts claim not to study historical background, they may be taking such background for granted, even unwittingly accepting an uncritical (or participatory) version of that background.
(ii) The strategy of much CA has been to look for the commonalities between speakers and what they share in common. Hence it is reasonable to use the labels 'A' and 'B' for such purposes. But people are not interchangeable. There are differences in wealth, education, gender, age, etc. The exploration of these differences within conversation/talk may require a different foundational rhetoric.
(iii) The foundational rhetoric is not neutral, but it conveys 'ordinary' life as equal and participatory. Since some conversation analysts imply that conversation represents the crucial 'bedrock' for the reproduction of the 'big picture', as studied by conventional sociologists, then a highly controversial picture of 'society' is conveyed, but not overtly argued for. It is an image of sharing, participation, equal members and first-name informality. To take this image as a microcosm of the social world is to take a highly ideological step. It is doubly ideological when the step is taken as if it is itself non-ideological, to be contrasted with the ideological biases of other approaches.

The limitations of the participatory rhetoric

The theoretical implications, conveyed by the foundational rhetoric that Schegloff and others routinely use, can be highlighted by considering when it would be inappropriate to apply uncritically this sort of rhetoric of analysis. The fact that analysts adopt different naming practices for talk in institutional settings, where inequality of speakership is expected, is itself suggestive. It raises the possibility that Schegloff's orthodox CA might be problematic if straightforwardly applied to episodes in which power is directly, overtly and even brutally exercised. One might consider how analysts could describe speakers in situations of rape, bullying or racist abuse. One might imagine that the talk, in the course of a rape in a non-

institutional, private setting, had been recorded and transcribed. One can imagine the rapist threatening and verbally abusing the victim, who in return pleads. Two related questions arise: how should the speakers be identified and how should their talk be analysed?

Conventional practices of naming would seem inappropriate in the case of rape. To call the speakers 'A' and 'B' would suggest that their parts were interchangeable and that gender was unimportant. First-name terms, too, would convey an informality and equality that would be at variance with the situation. Perhaps they should be 'man' and 'woman'; or 'rapist' and 'victim'. In the latter case, the identification would be made on the basis of the content of the talk. If it is 'rapist' and 'victim' in the rape situation (or 'bully' and 'victim' / 'racist' and 'victim' etc), then why should it be 'Marsha' and Tony', not 'primary caretaker' and 'secondary caretaker'? In short, the conventional name-practices gloss over, and reproduce, a series of wider assumptions. These spill out when the 'deviant' case is seriously considered.

Then there is the question what the analyst should be studying in a dialogue of rape, bullying, etc. No doubt the typical organizational properties could be investigated. One might presume that, as the rapist threatens and the victim pleads, they would share the same organization system for alternating their turns. Perhaps, they might even show other features such as 'repairs', 'second assessments', 'WH questions' and so. The analyst could show how the two speakers orientate to each other. The analyst might describe them as 'co-participants' in the conversation, or even as 'members', sharing the same practices.

All this would indicate that something had gone seriously awry. The conventional terminology of the 'participatory rhetoric' would assume that victims participate in their suffering. In what sense are victims 'co-participants' in talk which abuses them? Attention to what abuser and victim share in common, in terms of the organization of talk, would seem to miss the point. The analyst would be 'disattending' to the very matters which upset the assumption of an ordered, participatory social world. To imply that CA *must* disattend to such a matter (or must do so as a first step) is to say something about the limitations of an orthodox CA and its implicitly uncritical theory of the social world.

Supporters of CA might respond by saying that these are unfair examples. Of course, no-one would dream of analysing them in such a way. But that is the point. If one were to analyse them, a different pattern of 'attending/disattending' would be required. Some other analytic attention, beyond the conventional analysis of sequencing, would be required, together with a different rhetoric. If a different pattern of attending/disattending is needed to examine imbalances of power in such situations, then who is to say that similar patterns might not reveal imbalances in the more 'normal' situations that Schegloff presents?

It might be argued, in response, that CA studies 'everyday conversation' or the 'mundane' aspects of the social world, where the conventional terminology is appropriate. Rape or bullying, it would be suggested, is not mundane or ordinary. But, to turn Schegloff's rhetorical question around, one might ask 'Whose every-

day life'? 'Whose mundane world?' Who determines what is to be classed as 'ordinary' or 'extraordinary'? Why would CA assume that in the 'ordinary world' rape, bullying, racist abuse and so on are not mundane occurrences? Where did this assumption come from?

Again, terminological matters cannot be left to the participants/speakers themselves, as if rape and bullying demand a different set of rhetorical terms, if and only if the 'participants' allude to the extraordinary nature of the episode. Schegloff's 'Marsha' and 'Tony' do not allude to the 'ordinary' nature of their talk: in fact, they are speaking about something that for them is an 'extraordinary' event – the theft of their son's car. The analyst does not have to wait until the speakers specifically mention that their talk is 'ordinary', before calling the speakers 'Tony' and 'Marsha' or before claiming the extracts to be examples of 'ordinary conversation'. By the same token, analysts need not wait until the rape victim declares the event exceptional before first-name terms are dropped in the analysis, or the words are attributed to 'rapist' and 'victim'. One should beware of deploying any implicit scale of mundaneness, which assumes that unpleasant, non-participatory features of the social world are non-mundane rarities. Certainly rapes occur every day. Some people may feel that they themselves are bullied every day in their private world. According to many feminist theorists, routine bullying or the gendered exercise of power mundanely occurs in domestic life. This would, of course, need to be demonstrated by close examination of data.

None of these arguments is intended to imply that power and its operations cannot be studied by examining talk-in-interaction. Quite the reverse, power should be examined in relation to the close examination of talk. However, the examination is not as straightforward as Schegloff implies. The contrast between non-ideological CA and ideological critical analysis is not clear-cut. CA, as depicted by Schegloff, has its own ideological baggage. The response should not be to seek to discard all sociological assumptions, as if a pure empiricism were possible. On the contrary, it should be to work with the assumptions. Indeed, Watson (1997), for example, has argued that conversation analysis needs to develop along lines which take into account both the participants' and analysts' assumptions about category-membership. Some projects in discursive psychology specifically aim to take a critical stance, in relation to the assumptions of the speakers who are being studied (e.g., Billig, 1992; Edwards and Mercer, 1987; Wetherell and Potter, 1992). There is no need to fight shy of such a critical approach if the alternative is not a pure empiricism, but an unexamined uncritical view of the social order.

One last terminological point can be made. CA might have more in common with critical discursive studies than Schegloff allows. Productive future developments might be possible if the aim of sociological neutrality is abandoned as unrealizable, and the uncritical assumptions are replaced by explicit critical awareness. Such developments might benefit from a new label, especially since the 'conversation' in the term 'conversation analysis' is recognized to be a misnomer. 'Discourse' is not such a bad term, especially if analysts wish to explore

the continuities and discontinuities between oral and written communication. What price, then, the future developments of CA coming from 'critical discourse analysis'?

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'Schegloff's texts' as 'Billig's data': A critical reply

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I

In the Spring of 1968, when I was teaching at Columbia University in New York, a student strike and occupation of the buildings settled over the campus, often affecting segments of the campus in disciplinarily distinctive ways. One example occurred in the philosophy department, largely 'analytic' in commitment. There

(at least so the rumor had it), students had written on the blackboards: 'Radicals point a finger at the world! Philosophers examine the finger!' The apparent political 'role reversal' aside, I feel myself similarly frustrated by Michael Billig's comments on my article. Rather than addressing directly the analyses I presented of events in interaction together with proposals on how to conduct such analyses, or my treatment of the relationship between political and formal considerations in discourse analysis, or between conversation analysis and critical discourse analysis – what I was 'pointing at', he has chosen to 'examine the finger'.

Still, whether advertently or not, Professor Billig may have done us a service. Over the years, I have been collecting specimens of text under the rubric 'The trouble with conversation analysis (CA) is . . .'. It is quite a substantial collection. Professor Billig has signed on to many of these texts, and has used my paper 'Whose Text? Whose Context?' (1997) as an occasion for re-issuing them, even when what they complain of has little to do with my own text, and requires him to go elsewhere to document the complaint – 'elsewhere' referring not to other things I have written and with which he might fairly charge me (none of which he cites), but to other authors, with their own projects, topics, and commitments altogether. The upside of this is that it provides me a reciprocal occasion to address some of these asserted 'troubles' – many of which are addressed to claimed features of CA which are in fact the product of misreading or misunderstanding. I address as many of these as the editor gives me space for.

I could probably find things to take issue with in 75 per cent of Billig's sentences – including, I must say, virtually every one in the Abstract, which begins by mis-characterizing the article to which it is a response, a bad omen indeed. My article 'Whose Text? Whose Context?' was not an 'attack' on critical discourse analysis as is claimed at the start of the Abstract; it was in fact written in response to an invitation to a symposium organized by Claire Kramsch and Ruth Wodak – not exactly strangers to Critical Discourse Analysis – to offer some reflections on the relationship between political and formal/aesthetic considerations in discourse analysis. Nor is it a defense of Conversation Analysis; although often attacked, I did not take the occasion to be an attack or to warrant a defense.[1]

Such oblique displacements of my article run through Billig's text, half-hidden in choices of diction, tucked away as tacit presuppositions of clauses, etc., cumulatively casting my article, and CA work more generally, into the mold to which Professor Billig prefers to address *his* 'attack'.[2] The strategy is familiar; you may be able to dodge one bullet or several, but not a hail of pellets. It is the principle of the shotgun. I will try to extract several broader gauge (pardon the pun) themes grounded in major – and often widely shared – misunderstandings or misreadings of CA, and hope to do some broader good by setting the record straight, at least as I see it. As for the pellets which I will end up not having deflected, of which there will be many, I leave it to readers to find them and assess their merits.

In the end, however, those readers who find the mode of discourse exemplified by Billig's piece attractive are, I think, unlikely to want to undertake serious con-

versation-analytic work. My aim is to set the record straight and to allow those who are more interested in the world than in the finger to examine it with the tools which CA provides, free of misconceptions about what those tools are, and why they have been fashioned and deployed as they have. Those who find the tools and enterprise unsatisfactory in their own terms will seek out undertakings they find more compelling.

II

Before addressing several substantive issues, something must be said about Billig's method. He proposes that his article 'belongs to the tradition of "Rhetoric of Inquiry", which takes the writing of academic disciplines as its object of study'. But this genre has its own presuppositions, practices and rhetoric – among them that it is permissible and cogent to disengage 'the writing of academic disciplines' from the materials which are claimed by its writers to occasion and warrant it, and to examine this denatured object as if its integrity had not been violated. For conversation analysts who are committed to putting their theorizing under the control of data and decline to write ungrounded papers (as I have for some 30 years – Schegloff, 1991, being I think the sole exception), his is an ill-suited undertaking at best.

However, rather than play one-upmanship in a contest of meta-analyses, I try in what follows just to set the record straight on a number of assertions which Billig makes, which may be shared by others, but which nonetheless have little warrant or grounding in CA work itself, as I understand it. In the space available I can address only some of these misconceptions. The reader should therefore be alerted that ones not contested are not on that account being treated as uncontestable.

There is, however, one practice employed by Billig which should be isolated, exposed and identified, so as to neutralize its tacit operation and allow the reader to interrogate it critically wherever it is employed. This is the practice of 'mere description'. On the whole, 'mere description' is not a viable action in ordinary discourse. To say of/to someone 'There you are, hands folded, sitting with a newspaper' is to be heard as offering a critical observation, not a neutral one. How this works cannot be taken up here in any detail, but the general thrust is this: because there is in principle an indefinitely extendable set of observables, and an indefinitely expandable set of noticings which can be formulated and articulated about each of them, no noticing can be warranted by its mere 'correctness'. Recipients apply to noticings the generic question 'why that now?' and ordinarily find in the noticing some assessment, either positive or negative, more commonly the latter, and with it some critique, complaint, or suggestion for change for which the ostensibly objective, neutral description is the vehicle.

Throughout his article, under the auspices of taking my 'texts as data: their rhetoric as objects for analysis', but doing so in a *polemical* context, Billig writes such sentences as 'He [Schegloff] uses the word "vernacular" to describe the sort of ordinary language that the analyst must get beyond in the analysis'. Ostensibly a mere neutral description, the observation conveys an implication of

impropriety, of misleadingness, of untowardness, without any warrant for it. A vague illegality is draped over things described in this manner in a polemical context which is not attendant to 'mere description' in the context of empirical inquiry. Readers should stay alert to the tacit and ungrounded critique imported into Billig's text by this practice, which issues in a blizzard of innuendo.

III

One article of Billig's indictment (and 'indictment' is surely the right term here, for the complaint goes to the moral and political sensitivity and stance of CA and conversation analysts) takes the following form. If we had an instance of rape or abuse of a woman or wife-battering it would be ludicrous and outrageous to examine it in terms of turn-taking, adjacency pairs, repair, and so forth. Not, he says, that any conversation analyst would do such a thing. A conversation analyst, in common with other right-thinking people, would not do so where gross evil and abuse and injustice were being perpetrated. When conversation analysts *do* focus on turn-taking, then, they surely seem to presuppose that no evil is being done – an impression reinforced by their use (i.e. *my* use) of terms such as 'party to', 'participant', and the like, with their implications of evenhanded and equalitarian status in the event. In the very topics they choose to focus on, then, conversation analysts stand *self*-indicted for a kind of naive at best, and malign at worst, blindness to the warts of an unjust and oppressive society.

At the risk of converting indictment to conviction, I beg to differ. It may well be that when thinking of *categories* like rape, wife-battering, etc., it seems silly and obfuscating to address oneself to turn-taking, sequence organization, and the like. But if, as in Billig's imagined scenario, we were confronting an *instance* of an interaction in which such conduct featured, it is far from obvious that such an approach would be irrelevant and distracting.

In fact, this scenario is for me *not* imagined. Several years ago, I spent several weeks with a seminar trying to use material available to us on families at risk for violence to begin to understand the interactional dynamic that sometimes culminates in violence against women. Serving at the time on the dissertation committee of a (non-conversation-analytic, statistically minded) graduate student who was trying to understand recidivism among wife-batterers from interview-type data and police records, it seemed to me cogent that we (conversation analysts, that is) should be able to make a contribution using conversation analytic resources. I learned from those working in the area that in many cases, an episode of wife-battering is not the culmination of an increasingly distempered interaction; the man simply comes home and strikes. However, in at least some instances, an ordinary interaction at home cultivates, from a seed we do not yet know about (because we have *not* examined such events with the tools for understanding ordinary talk in interaction) an increasingly hostile tenor of interaction which, it is conjectured, can culminate in violence.[3]

Rape, abuse, battering, etc., do not exist in some other world, or in some special sector of this world. They are intricated into the texture of everyday life for those

who live with them.[4] How else are we to understand their explosive emergence where they happen if not by examining ordinary interaction with tools appropriate to it, and seeing how they can lead to such outcomes. And here, as elsewhere, 'appropriate to it' means 'addressed to the units and resources and practices from and by which ordinary persons co-construct interaction'. If interaction is produced within a matrix of turns organized into sequences, etc., and if it is from these that motives and intensions are inferred, identities made relevant, stances embodied and interpreted, etc., how else – when confronted by the record of singular episodes – are we to understand their genesis and course, how else try to understand what unwilling participants can do to manage that course to safer outcomes, how else try to understand how others might intervene to detoxify those settings?

If this position is ideologically problematic for Billig or other Critical Discourse Analysts, I will simply have to live with that – without apologies, I might add.

IV

A number of the lines developed by Billig appear to turn on his understanding – apparently shared by others – that conversation analysts believe that conversation is egalitarian in nature, and that they have predicated their account of its constitutive practices on the premise that all are equal before those practices. This is simply incorrect. A few key points will have to suffice to indicate how and why, and to direct readers interested in how CA is done, rather than what it can be charged with, to the relevant sources.

First, a distinction needs to be made between the *organization of turn-taking* and the turn-taking character of a particular episode of interaction or some part of it. 'The organization of turn taking' refers to the resources which are brought to bear on the allocation of turns and their construction, and the practices for deploying those resources (what was called in Sacks et al., 1974 the 'rule set', a terminology which appears to have misled some readers). By 'the turn-taking character of a particular episode of interaction or some part of it' I mean the particular ways in which turns were co-constructed and transitions between speakers effectuated, the ways in which a conversation may have schismed into more than one conversation and then have re-formed into one (or not), etc. The contrast is roughly like – though not identical with – the rules of a game on the one hand, and the accomplished course of one playing of it on the other hand.

About the first of these, Sacks et al. (and its several authors – and others – separately) did *not* claim that the turn-taking organization is egalitarian or that it treats all participants as if they were equal. In fact, if anything, it underscored the opposite possibility. Referring to the array of speech-exchange systems – from ones which pre-allocate all turns to those which allocate turns locally, that is, one at a time, as in conversation – we wrote (Sacks et al., 1974: 729–30):

> Thus one pole (local allocation of turns) permits maximization of the size of the set of potential speakers to each next turn, but is not designed organizationally to permit the methodical achievement of an equalization of turns among potential speakers;

> whereas the other pole (pre-allocation of all turns) is designed to permit the equalization of turns (or can be – it can be designed for other ends), which it does by specifying next speaker, thereby minimizing the size of the set of potential next speakers.

In fact, most turn-taking organizations which could be designed to permit equalization of turns are instead designed for other ends. Conversation, on the other hand, appears to be so organized as to allow virtually any overall distribution of turns, from a wholly equalitarian one to a highly skewed and asymmetrical one. As we wrote (1974: 711):

> . . . the rule-set provides for the possibility of any over-all distribution of turns, and frees turn-distribution for manipulation by such interests as can be realized with the distribution of turns.

Two points which follow from this may be mentioned here, from out of the many lines of analysis which merit pursuit: First, rather than attributing a kind of rosy-eyed equalitarian optimism to CA, it might be noted that the text immediately following the above citation points to 'biases' internal to the turn-taking organization which tilt in the direction of a concentration of turns among a very few participants (indeed, to two).[5]

Second, those committed to analyzing forms of inequality and oppression in interaction might do better to harness this account of turn-taking organization as a *resource* for their undertaking than to complain of it as an ideological distraction. For if, except for certain internal biases, conversation's turn-taking organization 'frees turn-distribution for manipulation by such interests as can be realized with the distribution of turns', then actual turn-distributions which are skewed can be inspected for the 'interests' and practices which drive and enable such skewed distributions.

And that leads us directly to the second of the two senses of 'turn-taking' distinguished here – namely the particular trajectory of turn distribution achieved in a particular episode of talk, a trajectory achieved through the interaction at each transition space or possible transition space in that episode. Given that the turn-taking organization for conversation *as an organization* is not designed for asymmetry, then such asymmetries as characterize the talk are the products of local determination, and the sites of that determination can be analysed to determine what appears to underlie each next turn allocation, and thereby the cumulative pattern of distribution. Those who believe that there are categorical sources of oppression at work in this domain – whether by reference to gender or class or race or ethnicity or age or physical disadavantage, etc. – have a set of places to go to work on – the transition places – to try to depict the mechanisms or the interactional scenarios by which such categorial disadvantage is realized and reproduced, site by site.

Here, by the way, is one basis for discriminating conversation from other speech-exchange systems, ones often characterized by common institutional context terms. Because some turn-taking organizations *do* pre-allocate turns, and do so by category – of participant (Judge/Attorney/Witness/Observer;

Interviewer/Interviewee) or of contribution (Question/Answer; For/Against) – our understanding of what transpires at such transition spaces is not as open to unconditioned examination as *conversation* is in principle. As Heritage and Greatbatch (1991) have argued, some institutionally specific speech-exchange systems are undergirded by such preallocational turn-taking organizations, and the course of actual occasions of interaction are shaped by those constraints. As each next increment of the occasion is played out by analyzable reference to that turn-taking organization, the parties show themselves to be oriented to it and thereby ground the relevance of our understanding by reference to that turn-taking organization.[6]

Such non-conversationally organized talk aside, the point is that the Sacks et al. (1974) account of conversation does not *presume* an equalitarian society, it *allows* for one. It also allows any complement of parties on any occasion to embody progressively a local cadre of participation with its composition on that occasion.[7] It can thereby become a canvas on which the practices end up having painted a picture of inequality, or exclusion, or oppression. or asymmetry without a sense of oppression, etc. Here is open terrain for analysts. Those who take conversation or other talk-in-interaction to be basically an arena of oppression should undertake to *show* that; the available tools of analysis do not preclude that showing. However, analysts need to busy themselves more with honing the analytic skills of deploying these analytic tools on actual materials and less with belaboring the tools with ideological character assassination.

V

The issue of the language we use in characterizing what transpires in a strip of interaction, including how we characterize the parties to a strip of interaction in a transcript, is indeed a serious one.[8] Surely Billig knows that I think that, since the theme of which it is one specification featured in the article to which he is responding, and has been a major thread of my work for years (Schegloff, 1972, 1979, 1987, 1988, 1988/89, 1991, 1992a – especially pp. 195–98, 1996a, inter alia), as it was of Sacks' (Sacks, 1972a, 1972b, 1992 passim; Sacks and Schegloff, 1979). One form which this issue takes in his article concerns what he terms a 'participatory rhetoric'. The point of departure here is my use of terms such as 'participant', 'co-participant', 'member', and the like, and his concern is the aura of 'commonality and equality' which this terminology conveys, and its apparent treatment of the 'bedrock situation' of conversation being 'implicitly depicted as a world of equality and participation'. I have already urged that this view, to the degree that it is grounded in a reading of turn-taking as equalitarian, is mistaken. As for the terms themselves, I doubt that readers of this work will understand the usage of '(co-)participant in interaction' as conveying an image of an equalitarian world, or that (even worse) such an implication will have been sneaked in beneath their critical guard. Perhaps I am wrong in this; readers will have to judge for themselves whether they were 'taken in'.

The more serious issue is the identification of the participants in a transcript.

This is an area which well exemplifies the dictum that one cannot avoid complaints by avoiding complainables. Virtually any practice which one adopts for its virtues can be complained of for its (sometimes alleged, often real) shortcomings, as is attested by Billig's text, which in several instances spells out the attractions of some convention (e.g. the use of alphabetic characters to refer to participants) and then spells out what to him are its problems. My own practice has been sensitive to complaints from readers that letters treat the parties as anonymous, as robots, as depersonalized, etc. (not a substantive issue, but an unnecessarily distracting one), and I use them now largely when (a) I am presenting a generic schema, such as the basic trajectory of a sequence type (Schegloff, 1992b: 1327); or (b) when that is the form of person identification employed by the transcriber (e.g. when the material is not my own but has been shown to me by a colleague, or when I am taking it from a published article).

In many of these same environments, and ones ostensibly involving the participants' institutional roles in particular, I also have used category terms. Sometimes these are category terms specifically related to the interactional and/or sequential business at hand – what is sometimes called 'discourse identities' (such as 'caller/called or caller/answerer'; Schegloff, 1986: 122, 125), requester/requestee, etc. Sometimes these are category terms which are not only putative discourse identities but are also category terms from the occupational/professional inventory of the society, such as doctor/patient, therapist/patient, interviewer/interviewee, etc. There is an overarching concern about this identificatory practice, and that is that it insists into relevance these categories and the bodies of common-sense knowledge organized by reference to them, and in so doing it makes it more difficult for analysts to continually attend to the practices of talk and interaction by which the participants are continually 'doing being members of that category'. Not everything that happens in the examining room has one party doing being 'doctor' and the other doing being 'patient' or 'nurse' or 'doctor,' and so labelling each turn can reinforce a default orientation by investigators to accept the institutional mantle of the occasion (see for further discussion Clayman and Whalen, 1988/89; and Schegloff, 1988/89 for the broadcast news interview setting; see Schegloff, 1991, 1996a: 464–5 and n.36 more generally).

Given these concerns (with which Billig may or may not be familiar and with which he may or may not agree), I am not drawn to endorse and implement the suggestion that this practice be broadened so that, as he puts it, 'the identification would be made on the basis of the content of the talk . . . why should it be "Marsha" and "Tony", not "primary caretaker" and "secondary caretaker"?' On the 'why not' side, for at least this reason: it requires and presupposes an analysis of what is going on in that strip of interaction when that is not yet known (by the parties or by the analyst), and it requires that that analysis be derived 'on the basis of the content of the talk . . .' which is thus presumed to be transparent and not requiring analysis. For those who actually do this work, this is a hopeless prospect.[9]

But why name? Personal name is the identifier I prefer to use if I can. The main reason is that this is the form participants appear to use if they can (a product of the preference for recipient design in this domain, specified as: if you can use a recognitional, do so; Sacks and Schegloff, 1979; Schegloff, 1996a). This is how they address one another (and they do in the data I examined, even if not in the particular segment I analyzed) and how they generally refer to one another. With respect to address, furthermore, after the opening they generally refer to one another as 'I' and 'you' (which is why readers did not get to see them use first names to one another in the extract analyzed in my article), and in English, these pronouns are opaque with respect to all categories except (with a few exceptions, Schegloff, 1996a: 442–49) speaker/recipient identity, and first name is opaque, except (as Billig points out) with respect to gender and (as he does not) sometimes age, given fashions in naming practices. That is, this is the best way of neutralizing the 'category shadow' problem sketched here (aside from the use of alphabetic characters, treated earlier).

But what about the importation of gender which attends all but so-called unisex names (e.g. 'Pat', 'Les', and the like)? Several considerations may be mentioned here, but only mentioned because I have already 'way exceeded' the editor's page constraints: (a) First of all, elsewhere in his remarks Billig, on his own account and on behalf of feminists more generally, appears to wish to have gender made relevant to such interactional materials more generally. And this is in keeping with a position which has much to recommend it, though I do not believe it has yet been established, namely that gender is omni-relevant in interaction. One line of argument for this view might be grounded in the largely (though not entirely) distinctive pitch ranges of men's and women's voices, the very resonances of which might be argued to introduce gender identity into any interaction in which talk-by-articulation is being done. So if any category is going to be belied by the identification of speakers, gender is possibly the best one to have, or so it might be argued, because the conversation is bathed in its acoustic waves and the relevance they impart. (Of course, not everything that is physically present is on that account treated as relevant by the parties, which is one line of counterargument to the preceding.)

One other reservation which Billig has about using names to identify speakers in transcripts is this: 'It conveys that social distinctions are irrelevant in informal situations, where democratic conversational participation will be expected'. I am not sure to whom Billig thinks this is conveyed. I am assuming, of course, that readers do not read *around* the data, but actually examine the talk and other conduct being analyzed. Surely if social distinctions are relevant there, readers will find them, whether or not speakers have been referred to by name. More important, the chance is enhanced that they will find what *they* find, and not what the author has stacked the deck in favor of finding by analytically tendentious labelling of the speakers – which is all to the good. The whole point of including the data is to allow the reader to find grounds for challenging the author's analysis. Which is why I was so disappointed that Billig did not choose to exploit that possibility.

Curiously, in support of the reservation quoted at the start of the previous paragraph, Billig suggests the following grounds: 'Feminist critics have argued that the surface of private equality conceals deeply practised and often unnoticed inequalities. If there are such inequalities, then they should be detectable in talk'. My position precisely! He goes on to claim (whether on his own behalf or theirs or both is unclear) that CA 'may have to be adapted if it is to be suited to revealing such inequalities'. Of this I remain unconvinced, as I was in my previous reply, to Margaret Wetherell (Wetherell, 1998; Schegloff, 1998). There is no ideological veil in CA that precludes analysts finding in a strip of interaction what is going on there, and in a collection of strips of interaction a recurrent practice deployed by participants in interaction.

VI

In the end there may be a difference in basic stance underlying many of these and other differences between CA and Critical Discourse Analysis. I suspect that Billig and many who share his position believe that students of the social world know basically how things work, whereas I and many colleagues who work along conversation-analytic lines believe that basically we do *not*, and that we need to win that knowledge bit by bit from the social world we try to understand, by examining it bit by bit. Those who think they already know, and think that what they know is more or less in accord with Billig's position, will surely be impatient proceeding as most conversation analysts do; conversation analysts will as surely find it problematic to stipulate to and presuppose the takes on the world which Critical Discourse Analysis presumes. Surely each should walk down their preferred path, but those who have yet to decide may perhaps linger a bit in determining which path that should be.

NOTES

1. Before having read the article itself, I found in the Abstract eight contestable assertions, claims, or innuendos. I list them here as an alert, even though I have the space to address only a few of them properly in this reply. Here they are: (1) As noted, my article was neither a defense of conversation analysis (which was not in need of one) nor an attack on critical discourse analysis (it would have been sharper and more effective had it been designed to be that); (2) My article did not make the claim that CA has no a priori assumptions, nor is that my position; (3) Whether some view of the social world is 'an ideological view' is itself a function of the characterizer's position. Labelling something 'ideological' is a form of rhetoric alternative to assessing its merits as argument or analysis; it is not intrinsic to what is being characterized; (4) The assertion that 'CA uses a specialist rhetoric which is literally not the participants' own terms' conveys the impression that conversation-analysts propose otherwise, as if they believed analysis was equivalent to the thing analyzed. This is simply not the case. The lines at the end of this note, taken from Wallace Stevens' poem 'Description Without Place' (Stevens, 1982: 334) will have to do as my response here, for lack of space; (5) Although some CA work disattends the topic of the talk being examined, this is demonstrably neither endemic nor generic, as there is ample CA work that is addressed to topic; (6) CA's rhetoric is one 'in which equal rights of speakership are often

assumed'. This is sometimes claimed about CA's depiction of turn-taking, where it is simply incorrect. Otherwise it is unclear what is intended, where this assumption is found, and what 'often' means in this context; (7) The claim that 'the assumptions of these rhetorical conventions are revealed' (and presumably are belied) 'if they are applied to talk in which direct power is exercised' is grounded in entirely hypothetical data and putative analyses which are at variance with actual experience; and (8) Billig attributes to me a claim of 'ideological neutrality' which I nowhere make, as I am not engaged in ideological analysis; Professor Billig is therefore contesting a view which he has attributed to me, rather than one I have articulated, while claiming the opposite. And this is just the Abstract.

The lines by Wallace Stevens referred to in Point 4 above are these:

> Description is revelation. It is not
> The thing described, nor false facsimile.
>
> It is an artificial thing that exists,
> In its own seeming, plainly visible,
>
> Yet not too closely the double of our lives,
> Intenser than any actual life could be,

2. Just as his Abstract errs in characterizing my article as 'a defense of CA' and as an 'attack on critical discourse analysis', so it errs in its introductory section in referring to my 'dismissal of critical discourse analysis'. On the contrary, I went out of my way in the later parts of the article to argue against the view that CA and CDA are incompatible, and tried to suggest one form their co-existence might take. Readers might then be on the alert in assessing Billig's arguments because if, as he proposes to do, 'Schegloff's own texts can be treated as data', his own examination of the data is on occasion rather loose and cavalier. He repeatedly characterizes my article and its arguments in ways unwarranted by its actual texts. This concern merits a bit of more principled elaboration, presented in the next section.
3. See, for example, the chapter on 'Righteous Slaughter' in Jack Katz' *The Seductions of Crime* (1988: 12–51), written from quite a different analytic point of view but documenting in its own way the point I am making.
4. Billig writes, 'Why would CA assume that in the "ordinary world" rape, bullying, racist abuse and so on are not mundane occurrences? Where did this assumption come from?' Where indeed?!? It is surely not CA's assumption. Where is the 'text [which] can be treated as data' for this claim of Billig's Rhetoric of Inquiry? Here again Billig takes CA to task for a position which he has assigned it, but which a moment's thought would reveal is inconsistent with its basic commitments and ways of working. One almost starts to wonder whether 'Rhetoric of Inquiry' is not being used here as a rhetorical device to mask what is in fact simply an ideological polemic responding to what has been perceived (even if incorrectly) as an 'attack'. Not only is there no text to warrant this charge; the juxtaposition of sexual harrassment with sequential analysis as an absurdity stands in stark contrast to an episode detailed in my first published article in a regular professional journal and volunteered by a sociology graduate student at Columbia after I first spoke to the students about my work in the mid-1960s. Here is the paragraph in question (Schegloff, 1968: 1078–9):

> Finally, consider as evidence of the binding character of the distribution rule the following personal anecdote recounted by a student. At one time, she began receiving obscene phone calls. She noted that the caller breathed heavily. She, therefore, began

> the practice of picking up the receiver without speaking. If she heard the heavy breathing, she would hang up. The point she wanted to make in relating this anecdote was that she encountered considerable irritation from her friends when it turned out that it was they calling and she had not made a first utterance upon picking up the receiver. She took this to be additional evidence for the correctness of the rule 'the answerer speaks first'. However, she has supplied an even more pointed demonstration than she intended. It is notable that she could avoid hearing the obscenities by avoiding making a first utterance; however obscene her caller might be, he would not talk until she had said 'hello', thereby obeying the requirements of the distribution rule.

From this young woman's point of view, what I was talking about was quite directly germane to her concerns.

5. That text is (Sacks et al., 1974: 712):

> Since relative distribution of turns is the cumulative outcome, at any current point in a conversation, of the turn-by-turn determinations of turn-order, the biases operative in turn-order determination (one of which was noted in §4.5 earlier [the bias for selecting prior speaker as next – EAS]) may result in skewings intrinsic to the turn-taking system, in the overall distribution of turns to any point.

6. I might add, since Billig makes a number of points about the matter, that one finds here one basis for discriminating conversation from other talk-in-interaction, especially in contexts in which specialized practices and rules constrain how the talk is to be organized, often backed by threat of legal penalty. This is not an arbitrary or conventional sociological distinction. It is mandated by examining the materials of such interactions and trying to get at the real life exigencies and constraints by reference to which participants shape their conduct. Where there are institutionally specific rules overlaid onto – and reshaping – the practices ordinarily shaping the distribution of participation, these surely must be incorporated into the analysis, and such analyses are then systematically different from those in which there are no such overlaid constraints. I address the issue of what constitutes 'ordinary conversation' as compared to other forms of talk-in-interaction in Schegloff (1999). I address the issue of what constitutes 'ordinary conversation' as compared to other forms of talk-in-interaction in Schegloff (1999), and have therefore not addressed it more fully here.
7. Billig cites Burke (1993) to the effect that 'the very idea of "conversation" developed in early modernity in Europe and was bound up with assumptions of equal rights of talk within the specific, semi-private contexts of "conversation" '. The general point he wishes to make is that conversation-analysts presuppose certain forms of social organization. (I suppose that it reflects Billig's own national/cultural preoccupations that the example he offers is that 'the sort of domestic conversations studied by conversation analysts . . . take place in private living spaces, not overheard by domestic servants'). I suppose this is true. However, a careful reading of Burke's book will reveal, I believe, that what he means by 'conversation', what he studied and is writing about, is quite a different thing than what conversation analysts mean by it. And another 'however': Billig may not be aware that conversation analytic work has been done, and is being done, across a considerable range of societies, cultures, languages, situations, etc. We *do* try to give ourselves opportunities to be made aware of the bearing of such contextual variations – whether sociological, historical, anthropological, linguistic, etc. I tried to suggest the relevance of doing so in the epigram of my first published article

(Schegloff, 1968), which featured an instance of the sequence I was trying to describe reported in the Bible, exchanged between the Lord and Abraham.

8. But not everything Billig has to say about it is equally serious. For example, he writes: 'The speakers, conventionally studied by CA, do not talk of "adjacency pairs", "preference structures", "recipient designs" [sic], "self-repairs", etc. These are categories which the analyst imposes'. True, they do not talk *of* adjacency pairs, they (mostly) talk *in* them; they do not talk *of* preference structures, they construct their talk by reference *to* them with characteristic turn and sequence shapes as the observable outcome; they do not talk *of* recipient design, they *exhibit* it in their selection of words, reference forms, topics, etc.; they do not talk *of* self-repairs, they *implement* them in arresting the trajectory of their talk to introduce some operation on it. CA's insistence on 'relevance to the parties' does not need to be met by showing the parties talking about the thing whose relevance is in question, but by showing that the parties are oriented to it in doing whatever they do. How such orientation can be detected and described is a matter of ongoing discovery in the analysis of the materials of interaction; some readers may wish to look at my paper on 'Confirming Allusions' (Schegloff, 1996b) to see the range of ways parties show themselves to be oriented to the use of certain forms of repeat in certain sequential contexts to indicate that one is confirming another's understanding of an allusion and that it had in fact been previously conveyed inexplicitly, none of which involve saying 'Oh, you're confirming your allusion'. The argument that some practice or unit of organization is not 'indigenous', is not oriented to by the parties, because they do not use those words in interaction, is, in my judgement, an undeserving and unworthy argument to be raised in this context.
9. This is not to deny a possibly robust intuition here that one of the issues informing this exchange is responsibility for what has happened to the car and what is to happen to it in the future. Marsha may be understood to have first embodied an orientation to this with the prosody on confirmation that the foul deed was done 'right out in front of *my* house'. While in the first instance underscoring the nerviness of the perpetrators and the sense of violation by the victims, this also registers whose turf and whose 'watch' – and therefore whose 'business' and responsibility – is at issue. And Tony's return to the matter of the car's retrieval may well be understood to show his attention to the mingling of her responsibility and his interests. But such themes need to be *shown* and not simply put forward, and be pursued by reference to discrete features of the talk and other conduct; they ought not be imported wholesale, unaccountably and undifferentiatedly, in identifying the participants. Furthermore, although these orientations may well inform what the interactional business is about (if that can be shown), they contribute little (as far as I can see) to how it is implemented; they do not mobilize *practices* of interaction. For the purposes of the article I was writing, this would have made them, even if established, of lesser interest.

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Conversation Analysis and the claims of naivety

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Space inevitably restricts my reply to Emanuel Schegloff. I will try to concentrate on some of the major issues that divide us, for I think that his characterization of these differences contains omissions and misunderstandings. I cannot deal with all the issues raised by Schegloff, but I will attempt to clarify what is, and is not, the basis of my position.

First, a couple of preliminary remarks can be made. Schegloff objects to my describing his original article (Schegloff, 1997) as an 'attack' on Critical Discourse Analysis (CDA). I gladly withdraw the word 'attack'. Deborah Tannen (1998) suggests that academics too readily use the military metaphors of 'attack' and 'defence' to the detriment of reasonable debate. I would not wish, therefore, to 'defend' my use of 'attack'. In the same spirit, I hope to show that my article was not intended as an 'attack' on Conversation Analysis (CA) as such – although I can understand how it might be interpreted as one. Another preliminary point can be made. Schegloff objects that, although I claim to analyse his article, I readily cite the works of other conversation analysts, for whom he can bear no responsibility. He is correct. I did not (and still do not) wish to personalize the issue by concentrating only on his work. When Schegloff (1997) outlined the strengths of CA, he was not, of course, just referring to his own work: he was referring to a whole corpus of inquiry. However, I should have made clearer that I was using his article to illustrate wider trends in CA, and to have stressed that his own work may not represent the clearest examples of some of these trends. If there is ambiguity, I apologize. Inevitably some ambiguity will continue, for I still do not wish to deal only with Schegloff's own contributions to CA, however eminent and distinguished they are.

Schegloff begins his reply by commenting on the sort of critiques which he categorizes under the heading 'The trouble with CA is . . .' A number of ideologically driven critiques will, no doubt, have suggested that the details of CA should be replaced by wider, structural analyses. That is no part of my argument. In fact, I have criticized cultural studies for ignoring the detailed study of language prac-

tice (Billig, 1998). My piece had a specific rhetorical context and purpose. It was a response to Schegloff's (1997) characterization of CA, which he used to make rhetorically weighted contrasts with CDA. These contrasts prompted my response, as I am sure they prompted Wetherell (1998). After all, neither of us has ever previously felt impelled to write a 'trouble with CA' piece – and both of us, after our own fashions, have used CA in our own work.

My critique was directed against Schegloff's (1997) claim that CA is based on a naive epistemology and methodology. He suggests that CA approaches social reality directly, examining it in the participants' own terms. He contrasts this with CDA, which, he claims, imposes its own categories on participants. Schegloff suggests that CDA, because it is driven by prior theorizing, can only find out what it already knew. Schegloff repeats this last claim in the final paragraph of his reply to me. I want to emphasize that my arguments were directed against these claims, not to disputing the value of fine-grain analysis, which CA practices. Regarding the claim that CA studies participants in their own terms, I wanted to suggest that matters are not (and cannot be) that simple. CA constructs and uses analytic terms that are not the participants' own. The use of such terms and the meanings they convey are by no means straightforward. I suggested that their usage contradicts the claims to epistemological naivety.

I feel that Schegloff tends to disconnect my critique of CA's terminology from the specific argument about epistemology. He suggests my descriptions of CA have a polemical import. According to him, I convey an impression of 'impropriety and misleadingness', draping a 'vague illegality' over CA's terminology. He exemplifies this by citing my discussion of his use of the term 'vernacular'. My point was not that there is something misleading or illegal in the use of 'vernacular'. I was suggesting that CA, in its own concepts, moves beyond the participants' own terms. I cited Schegloff criticizing analyses which themselves are stuck in the 'vernacular'. Let me stress: I find nothing illegal or inappropriate in using technical terms which are not part of the ordinary vernacular; nor for that matter do I find anything wrong in attempting to stick to the vernacular in analysing discourse. My point is that the naive epistemology has problems accounting for this move from the vernacular (or from the participants' own terms) to the specialized language of CA. If this central direction of my argument is ignored, then it is understandable how one might be left with the impression that the criticism is diffuse, or to use Schegloff's phrase, that there is a 'blizzard of innuendo'.

I wanted to suggest that in practice CA is not so methodologically or epistemologically naive as Schegloff suggests. Analysts bring presuppositions to the analysis. My point is not that they should seek to eliminate all presuppositions. I think the epistemological and methodological naivety that Schegloff recommends is neither desirable nor in an absolute sense realisable. For this reason I raised the issue of how speakers might be named in CA transcripts. My point was to show that there can be no 'neutral' naming practice. Each practice embodies assumptions about the social world. Schegloff points out that identifying speakers in

terms of institutional affiliations may have some advantages, but it runs the risk of casting a 'category shadow' over the analysis. Similarly, I would suggest that informal categories and alphabetic codes, too, cast 'shadows'. There is no point of sociological neutrality.

In his reply, Schegloff points out the difficulty of identifying speakers on the basis of the talk's content: that would be to presuppose the analysis, before the analysis has been conducted. Indeed, that was, in part, intended to be my point. But not all prior judgements can be avoided. Judgements about the type of talk being studied (i.e. institutional, doctor/patient, domestic, etc.) – and, thus, about the suitability of naming practice – will be made, at least provisionally, prior to the details of the analysis. In short, the analyst, in order to conduct the analysis, must bring presuppositions about the nature of the interaction.

My point was that the terms used in CA – its foundational rhetoric – carry theoretical baggage. The terms, including those used to identify the speakers and also to categorize them as 'participants' or 'co-participants', reflect analysts' understanding about the nature of the interaction being studied. Analysts shift their foundational rhetoric (and practices of naming) depending on whether institutional or domestic talk is being analysed. An implicit sociological understanding, thus, is the precondition for the analysis. I criticize the notion that informal conversation, as compared with institutional talk, can itself be treated as a sociological point-zero, as some conversation analysts, who consider conversation as a 'bedrock', tend to imply. Again, I should emphasize: there is nothing illegal or improper in using presuppositions as such. In my view, presuppositions are necessary for analysis.

It is in this context that I introduced the hypothetical example of the rape. The point was not that violent episodes cannot be studied in terms of the details of spoken interaction. As Schegloff states, recounting his experience of studying an episode of wife-battering, the detailed examination of interaction can show how violence was embedded in more 'ordinary talk'. I would not wish to dispute that at all. My point was more specific. It was that some of the theoretical terminology regularly used in CA for analysing non-institutional interaction would be inappropriate for the case of rape. This would imply that such terms (including those which I called the 'participatory rhetoric') make assumptions about the nature of social interaction: if they did not, they would not be inappropriate in the case of distressing situations.

I suggested that the form of CA, which Watson (1997) calls 'traditional' CA, is particularly unsuited for a critical analysis of situations such as rape, racial abuse, etc. It would also be unsuited without modification for investigating gender imbalances. This form of analysis, again to quote adherents of CA, involves a particular pattern of 'attending and disattending', which specifically 'disattends' to content. Schegloff in his reply correctly points out that not all CA disattends to content. In my view, a serious study of gender and child supervision would not be based on a single example (such as the extract presented by Schegloff, 1997), but would demand a corpus of materials, to be studied in terms of content. Such

investigations provide a bridge between CA and CDA. When material is collected on the basis of content – such as gathering a corpus of material on wife-beating, rape or child supervision – then, as Schegloff noted with respect to practices of naming, the analyst must bring in presuppositions about the nature of the phenomenon before the analysis is conducted in detail.

If there are different forms of CA, with different patterns of 'attending and disattending', then one must ask how analysts are to choose between them. In passing, I should mention that the very notion of 'attending/disattending' itself reflects the claims for epistemological naivety. It suggests that the analyst merely attends to social reality which is given in a non-problematic way: it ignores the extent to which the analysis depends on the construction of rhetorical practices. The choice is not merely between patterns of attending/disattending, but also between rhetorical practices, including naming practices and the participatory rhetoric. It is not sufficient to claim that the data 'mandates' certain practices and not others. The analyst brings tasks to the data. CDA aims to make explicit such tasks, in order to enable a theoretically based choice between available rhetorics and attending/disattendings (for instance, there would be theoretical grounds for avoiding the participatory rhetoric when analysing certain topics).

In my view, such considerations vitiate the distinction between CA and CDA that Schegloff makes. It is not the case that CA looks at social reality directly, while CDA is condemned only to find what the analyst expects to find. CA has various patterns of attending and disattending, as well as different rhetorical practices. However, a commitment to epistemological naivety downplays such issues. Similarly, epistemological naivety, especially when accompanied by the conviction that CA operates in the participants' own terms, might discourage analysts from reflexively examining their own theoretical terms and the choices that are made in the conduct of analysis. It might be thought that the technical terms and the foundational rhetoric merely describe what actually exists. However, as Schegloff in his criticism of my rhetoric argues, ' "mere description" is not viable in ordinary discourse'. Schegloff suggests that because there is an 'indefinitely expandable set of noticings', no one particular noticing can be warranted by its mere 'correctness'. The same conditions are present in the technical discourse of social analysis. Any piece of social interaction might be the object of indefinitely expandable academic 'noticings'. One particular set of attendings and disattendings cannot be warranted merely on the grounds that it is correct. In this sense, 'mere description' is not viable: it too carries theoretical baggage and assumptions about the nature of social reality.

Although Schegloff warns against ungrounded analyses, his contrasts between CA and CDA are themselves ungrounded in relation to CDA. He offers no analyses of CDA to support his contrasts, nor to warrant his descriptions of CDA's practices (which, of course, are not 'mere descriptions'). The criterion which he uses to distinguish between CDA and CA – the possession of theoretical presuppositions, which dominate detrimentally the analysis – is more appropriate for

distinguishing good from bad analyses, whether they be conducted under the label of CA or CDA. After all, conversation analysts might apply particular patterns of attending and disattending to certain data sets, with the result that they only notice analytically what they are expecting to notice: what they disattend to will remain ignored. There is a case for saying that the traditional form of CA was looking for the basis of social ordering in codes of speech which are shared by participants. If this pattern of attending/disattending is applied to episodes in which power is violently exercised, it might be possible to find such shared codes. If that is all the analyst attends to, then the inequalities of power will be disattended to.

I would prefer the distinction between CA and CDA to be drawn differently, although a firm distinction would be misleading because CDA, like CA, encourages the close examination of spoken interaction; indeed, CDA often uses the methods and findings of CA. However, there are differences between CDA and 'traditional' CA. The specific tasks of CDA are frequently part of a wider analysis of social inequality. Moreover, CDA wishes to theorize the presuppositions that must be brought to the micro-analysis of interaction. CDA does not claim epistemological naivety in the fulfilment of its methodological tasks, but explicitly wishes to incorporate insights from social theory and other social sciences, including macro social science, into the analysis of particulars. This can be clearly seen, for example, in Wetherell's (1998) response to Schegloff. CA, by contrast, using variants of epistemological and methodological naivety as guiding principles, often excludes these wider inquiries, ignoring the insights that can be gained from other disciplines (its exclusionary character can be seen in the limited range of references that typically are cited in CA empirical studies). In consequence, I would turn Schegloff's final conclusion around. Those who think they have little to learn from studying the broad pattern and history of social relations, should choose the path of traditional CA. Those who feel that they do not know these things and who wish to understand how the wider pattern might be reproduced in particular moments of social interaction, should turn to CDA.

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Naivete vs sophistication or discipline vs self-indulgence: A rejoinder to Billig

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Let me begin with the agreements and alignments.

- First, I appreciate and accept the gestures of rapprochement in Professor Billig's opening paragraphs.
- Second, there is indeed problematic work in all camps.
- Third, there is not enough space to take up all the disagreements and problems of understanding remaining between us – and each successive installment of this exchange is allowed less space by the editor. If Billig feels squeezed, imagine how I feel!

Next, a few denials and reassertions, briefly stated.

- First, I did not '. . . suggest that Critical Discourse Analysis (CDA), because it is driven by prior theorizing, can only find out what it already knew' (or, later, that '. . . CDA is condemned only to find what the analyst expects to find'). What I suspected (in the final paragraph of my Reply) was that the particular varieties of theorizing that appear to underlie Billig's discussion and are implicated in much CDA suggest that its authors 'know basically how things work'. Surely this is quite different from finding out only what one already knew. The latter is mere ritual and pretense of learning; the former allows for genuine learning *within the parameters of the already known, but taking those parameters* – the categories of race, class and gender; the bearing of hierarchy, power, oppression, macro-social structure, etc., generally those categories and themes lending themselves to political discourse – *as given and as inescapably relevant*.
- Second, I continue to disagree with the assertion that 'some of the theoretical terminology regularly used in [Conversation Analysis] CA for analysing non-institutional interaction would be inappropriate for the case of rape'. It seems to me that Billig has simply reasserted this and has given no warrant for doing so in the face of the arguments to the contrary in my previous Reply.
- Third, Billig asserts with apparent confidence, 'The analyst brings tasks to the data. CDA aims to make explicit such tasks . . .'. I readily yield to Billig if he means here to be describing the workstyle of CDA. However, if he means to be characterizing CA, then I wonder what basis he has for this claim (other than to-him-transparent-and-inescapable presupposition). A great deal of the most important work in CA has had its onset in what conversation analysts call 'unmotivated observation'. I haven't the space to describe it here; discussions can be found in variety of CA writing by a variety of authors. It is the critical

site of the origin of much of what is genuinely new in CA work at any given time. Anyone who has participated in CA 'data sessions' or so-called 'play groups' – analytic jam sessions, if you like – which have 'taken off', will recognize what I am talking about, and will know the reality of such unmotivated observations and how they can set off a line of inquiry which has no precedent in the experience or past work of the participants (Schegloff, 1996, was the product of just such an unmotivated observation). This is so and real, the orthodoxies about the inevitability of task- or presupposition-driven inquiry to the contrary notwithstanding.[1] A key component in the training and progressive competence of new CA workers is the developing capacity to make unmotivated observations, and to articulate them – even in the absence of any compelling upshot at that moment.

- Fourth: Billig wishes to make the point that the '. . . naive epistemology' (I shall return to that phrase) has problems accounting for [the] move from the vernacular (or from the participants' own terms) to the specialized language of CA'. And I wish to contest that point. Although I may be missing what Billig has in mind by 'problems', I don't see what they are if the 'specialized' – or 'technical' – terms do indeed capture for purposes of analysis the displayed orientations of the parties to the interaction.

  For example, reverting to the data previously examined, Marsha and Tony may not use the term 'assessment', or say that there is a 'preference' for 'second assessments' to 'agree' or 'align' with 'prior assessments', or that one 'canonical' way of 'implementing' such agreement/alignment is to employ in 'next turn' an assessment term of the same 'valence' but 'upgraded' (Pomerantz, 1984; Schegloff, 1996: 168–74). But, I submit, we can see their orientation to just such practices of talk-in-interaction displayed in the segment analyzed in Schegloff, 1997: 174–80. What then is the problem? Billig asserts that there is one, but does not characterize it or defend its problematicity.

Finally, a bit more extended discussion of the thematic core of Billig's Rejoinder to my Reply to his Response to my article.

Billig characterizes CA's position, or at least my version of it, as one of 'methodological naivete'. Rhetorically speaking (to adopt Billig's preferred mode of analysis), this could be understood as, 'Something I [Billig] know to be the case, you [Schegloff] deny; you're naive'. Rhetorically this works by signing up the reader on the side of sophistication, on the author's side.

How it works aside, what is the warrant for Billig's position? Apparently it is this. Given the point which I made (but was surely not the first to make), that there is an indefinitely large number of things about which observations can be made, and an indefinitely large set of observations to be made about them, some principle of selectivity must necessarily be involved – hence the presuppositions with which Billig is comfortable, and the attributed rejection of which he characterizes as my naivete'.[2] From my point of view, however, what I practice is not

naivete but discipline; and what Billig is arguing for is not unavoidable perspectivalism, but analytical self-indulgence.

I have elsewhere argued this position more expansively than I can here (Schegloff, 1988, and in the 1997 article around which the present discussion revolves). In brief, Billig's solution is no solution; it is a reconciling of oneself to a problem claimed to be incapable of solution. By contrast, the position I take up *does* propose a solution, with associated consequences for the practice of analysis – along the following lines.

Given analysts' capacities to offer virtually limitless observations (were it only the case that analysts were so fertile and creative!), what should constrain them? What, if anything, can appropriately differentiate observations to be taken seriously from others? One solution, to which I suppose Billig would subscribe, is that 'communities of relevance' are, and set, the standard. The communities of relevance in question are academic, disciplinary, political, aesthetic, etc., communities, whose members share an orientation to inquiry about the world or action in it, an orientation which imparts relevance to certain lines of inquiry, with associated observations, rhetorics, etc. Then, indeed, given the capacity of the social and cultural world to engender new communities of relevance, and, in the contemporary world, endless ones that can become the rage of the day, there is no solution to the problem. In the end, we each retreat to our community of relevance, within which we fight about more refined differences in point of view.

The alternative is to adopt as the 'community of relevance' the one *composed of the parties to the interaction being examined*. It is what *they* demonstrably orient to as relevant (as best we can establish it, to be sure) that sorts out which of the 'indefinitely many' observables has standing as evidence in the conduct of the inquiry. This is the 'discipline' to which I referred earlier; its absence is the self-indulgence to which I referred. Nor is the discipline which I invoke self-indulgent; there is no assurance that what our community of inquiry (the CA community, for example) has been interested in will readily be resonated to in the data which we examine. And past experience has shown that we count as our greatest advances inquiries in which things not previously suspected to occur or exist can be cogently and convincingly brought to serious notice.

Rather than a cascading set of communities of relevance from which an investigator can choose the most inviting on whatever grounds invitingness is based, there is a single – albeit shifting – community of relevance, which challenges the inquirer to show that the observation being registered and the analytic line being taken is resonant with the orientations of the people who matter the most – the ones who engaged in that conduct, and on whose understanding of its relevances the actual ensuing trajectory of the interaction was built. I opt for the second; Billig apparently opts for the first.

So there is a choice between alternative pairs of contrasting terms to characterize the difference between Billig's stance and mine: Billig's sophistication vs my naivete on the one hand; Billig's self-indulgence vs my disciplined inquiry on the other hand. Reconciling oneself to an analytic world in which the choice of 'rel-

evance' or 'meaning' is inescapably arbitrary, before which we might as well pick criteria by reference to grounds other than epistemic ones; vs a world whose interactional events were infused at their moment of constitution with relevance and import by the parties who co-constructed them – a point of reference indigenous to those events, by reference to which we can assess our investigatory efforts – an Occam's razor with which to cut through the quandaries of indefinite perspectivalism. If such a leverage is available to us – perhaps distinctively for talk in interaction, and surely for conversation, with its built-in mechanism for each party's display of their understanding of what has just been going on – then it is self-indulgent not to accept the disciplining of analysis which it makes possible.

But here I have returned to the Conclusion section of 'Whose Text? Whose Context?'. May I therefore invite readers of these exchanges to return to the article which prompted them and weigh its arguments against these discussions, and these discussions against its arguments. In particular, may I call attention to the paragraph preceding the Conclusion section, where I wrote,

> I understand that critical discourse analysts have a different project, and are addressed to different issues, and not to the local co-construction of interaction. If, however, they mean the issues of power, domination, and the like to connect up with discursive material, it should be a serious rendering of that material. And for conversation, and talk-in-interaction more generally, that means that it should at least be compatible with what was demonstrably relevant for the parties . . . Otherwise the critical analysis will not 'bind' to the data, and risks ending up merely ideological. (1997: 183)

Whatever my own commitments are, I was not arguing in 'Whose Text? Whose Context?' that critical discourse analysts should give it up in favor of conversation analysis. Asked to assess the competing claims of political and formal lines of analysis in dealing with interactional discourse, I urged that CDA avail itself of the resources of CA taken in its most serious form. Billig (and Wetherell, 1998, before him) have professed an interest in doing so – indeed a past practice of having done so. I have tried to make clear what I think is seriously involved, with the implication that there may be more – and other – for CDA to do than has been done. This is not urging that one enterprise replace the other, but that one ground itself in the results of the other – with certain anticipatable results, and subject to determinate constraints. As I concluded there (p. 184), '. . . serious critical discourse analysis presupposes serious formal analysis, and is addressed to its product. Whether politics and aesthetics are compatible turns, in this view, on whether this arrangement can be made to work by those whose central impulse is critical'. We shall have to wait and see.

Finally: The danger in exchanges like this is that the contributors and readers get drawn further and further into secondary discussions about the work, and further and further away from *doing* the work – whatever the work they choose to do is. Indeed, the ultimate danger is that this *becomes* the work they choose to do. I am reluctant to contribute to moving this discussion and its audience down that

path. I have had my say. Professor Billig has had his chance to reply to it. With ensuing rounds. Enough.

Readers need to decide what they find most cogent and compelling to do, and then go do it, or prepare themselves further for doing it, if that is the life stage they are at. That means:

(1) Reading carefully, closely, seriously, open-mindedly; reading to find what the writer may be telling you that you did *not* know before, that you had not thought about *that way* before, that you had not entertained before – rather than to find which thing you already know this is a version of, so that you can align with it or choose the critique to aim at it.
(2) For whatever naturally occurring setting in the world turns out to be engaging, observing it carefully, closely, seriously, open-mindedly; observing – over and over again – to find what the natural world may be 'telling you' that you did *not* know before, that you had not thought about *that way* before, that you had not entertained before – rather than to find which thing you already know this is a version of, so that you can align with it or choose the critique to aim at it.

Whatever it is, *do* it – or try – before *talking* about doing it. And bear in mind that addressing issues of moral and political moment does not entail relaxing the imperatives of rigorous analysis, but intensifying them – the more so, the more you believe is at stake.

NOTES

1. Like the one expressed by Billig when he writes, 'Judgements about the type of talk being studied (i.e., institutional, doctor/patient, domestic, etc.) – and, thus, about the suitability of naming practice – will be made, at least provisionally, prior to the details of the analysis. In short, the analyst, in order to conduct the analysis, must bring presuppositions about the nature of the interaction'. This is just the sort of stance which good CA ordinarily must resist. Ryave's (1978) discussion of storytelling did not foreground that the parties to the conversation were adult retarded men, precisely to allow appreciation that it was the organization of storytelling that was at issue, not the identity of the participants (cf. his note 2), a point which I also tried to underscore with respect to 'studying schizophrenic thought and language' (Schegloff, 1991: 66–7). And it was central to both the work of Clayman and Whalen (1988/89) and to mine (1988/89, 1992) on the engagement between then Vice-President George Bush and news anchor Dan Rather to take the premise that this was a 'broadcast news interview' as a contingent possibility, needing to be realized in the talk moment-by-moment; without that orientation, had we assumed it was the news interview it was proclaimed to be, we would not have been in a position to see it slip into the virtually conversational confrontation it was characterized as in the news reports the next day.
2. While on this point, let me just say that I disagree with Billig's extension of a point I made into a domain for which it does not hold. In writing that '"mere description" is not viable in ordinary discourse', I meant precisely to imply a contrast with disciplined empirical inquiry, in which it *is* viable, or can be. It is, to be sure, true that the 'indefinitely expandable set of noticings' is a generic characteristic of the world

addressed by disciplined naturalistic inquiry, but its import is not the same there as it is in ordinary discourse. In ordinary discourse, 'correctness' by itself is not adequate grounds for noticing. In research inquiry, it *is*. That is what makes such inquiry a distinctive domain of activity. 'Mere description' is exactly what basic inquiry aims for – to the recurrent chagrin of those who insist on its practical relevance or payoff.

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