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Schegloff's Texts' as `Billig's Data': A Critical Reply

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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 with-
out 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 ana-
lytic skills of deploying these analytic tools on actual materials and less with bela-
boring 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-