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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 indigent 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. 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 pre-suppositions 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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