

Reflections on Quantification in the Study of Conversation

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Several years ago, a graduate student with a quantitative background who was taking my course on "Conversational Structures" raised the question, "Why don't you people quantify?" This student seemed clearly to find the materials and the course's stance toward them engaging, and did not mean the question so much as a challenge as a recommendation. Perhaps it had been prompted by the so-called informal quantification represented in the common use in some conversation-analytic writing of terms such as *massively*, *overwhelmingly*, *regularly*, *ordinarily*, and (as in the current sentence) *commonly*. I responded to the question at some length, reviewing a number of constraints on "serious" or "formal" quantification in the study of conversation—matters that seemed to me common knowledge (in a difference sense of *common*), although mostly disregarded in practice; in any case, nothing new.

Since that time, I have had occasion to discuss these reflections with practitioners of several disciplines in the social/human sciences, including many for whom quantification is an important tool.¹ On the

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whole, they confirm my sense that there is nothing in principle new here;² still there appear to be some nonobvious consequences for studies of discourse, conversation, and talk-in-interaction more generally. There also seems to be agreement that, as I note at the end, the *considerations that I try to articulate do not pertain to quantification* alone, but have a more general bearing on inquiry into interaction. This broader provenance is welcome, as long as it does not serve to blunt the bearing of these considerations on the prospects for quantification in the study of interaction, for it was about this that my student was asking, and about this that my reflections were formulated.

Do bear in mind, then, that what follows was meant in the first instance as cautionary remarks to students (in the broadest sense) of conversation analysis regarding quantification. It was *not* meant as an attack on quantitative studies of interaction, which may have very different analytic commitments; nothing could be less interesting than a renewal of pointless bickering about quantitative versus qualitative or *recycled arguments about principled objections to the possibility of quantitative analysis*, and these are not my objectives here.³ In fact, it should come as no surprise that in the course of explicating some concerns that constrain the prospects for quantification in studying talk-in-interaction, these reflections help to specify some of the conditions—including the study policies and analytic strategies—under which it may be warrantably undertaken, and at the end of the article some prospects of this sort are briefly explored.

But if the reflections that follow do have a more general relevance than for students of conversation analysis, then some who do quantitative work or who read it for its bearing on their substantive concerns may wish to consider how particular quantitative work—completed or contemplated—fares by reference to the issues raised in what follows and may thereby find ways of clarifying their substantive concerns by finding explicated here analytic challenges—imposed by aspects of the organization of interaction itself—that are otherwise often left inexplicit.

I

One use of quantitative analysis whose indispensability is often underscored has been to establish the “weight” or gravamen of an

observation or assertion, that is, to provide grounds that it be taken *seriously* and not be dismissed as artifactual, incidental, or epiphenomenal. The statistical term most often associated with this use in the social sciences is *significance*, and quantitative analysis is commonly invoked to underwrite the significance of empirical claims of fact and relationship.

Let me begin, then, by remembering that *one* is also a number, the single case is also a quantity, and *statistical* significance is but one form of significance. Indeed, it is significance in only the technical sense that a “finding” in a sample may be taken as indicating the likely presence of an element of order in the larger universe being studied. Where talk-in-interaction is concerned, however, other testimony to the seriousness of a claim can figure centrally. Indeed, although finding an element of orderliness that is significant in the statistical sense may give rise to claims about *relevance*—that is, relevance to the participants in some speech event, it is not the *only* way of establishing relevance, or the best—sometimes not even warranted at all.

An alternative sense and evidence of relevance that I stress here at the beginning of my reflections is that provided, for example, by the displayed orientation of a co-participant to some feature of what a speaker has done.

There is nothing new in what I am saying here. The best evidence that some practice of talk-in-interaction does, or *can* do, some claimed action, for example, is that some recipient on some occasion shows himself or herself to have so understood it, most commonly by so treating it in the ensuing moments of the interaction, and most commonly of all, next. Even if no quantitative evidence can be mustered for a linkage between that practice of talking and that resultant “effect,” the treatment of the linkage as relevant—by the *parties* on *that* occasion, on which it *was* manifested—remains.

This is a benefit and constraint of working with naturally occurring events, rather than experimental or otherwise elicited data. The natural organization of the domain of events being studied will have had some practice understood by co-participants as having some import or sense or consequence, and ensuing actions built on that understanding will have constituted a next bit of interactional reality on the unfolding cusp of interactional time. And no number of *other* episodes that developed differently will undo the fact that in these cases it went the way it did, with that exhibited understanding, and with an object of study made

available to inquiry, demanding an account along those lines (Schegloff, 1968, 1987a, 1988).

This reliance on evidence internal to the single case is a familiar position for people in my disciplinary neighborhood and that of many readers of this journal. There are by now a number of studies that exploit this analytic possibility and exemplify this stance, and it ill behooves me to belabor it when I have undertaken to explore an alternative possibility in reflecting on quantification in the study of talk-in-interaction. I only want to make clear that that exploration is launched from what I take to be a safe home harbor.

But more than reaffirming a secure base is involved. For one point of departure that should be understood to inform my reflections on quantification is that, in examining large amounts of data, we are studying *multiples or aggregates of single instances*. Quantitative analysis is, in this sense, *not an alternative to single case analysis, but rather is built on its back*. We can be led seriously astray if we allow the possibility of quantitative analysis to free us from the need to demonstrate the operation of what we take to be going on in singular fragments of talk (except for those phenomena argued – no, *shown* – to be orderly *only* at an aggregate, statistical level). The questions will be: Under what conditions can such building *on single case analysis* be defensibly done? What are its dangers and how can they be avoided or addressed? If quantitative analysis *can* be defensibly done in this area, what distinctive payoffs might be expected? And is now the time for this? That is: Are quantitative studies of conversation *possible*? Are they *desirable*? What might their distinctive *payoffs* be?

II

I organize my reflections on quantification in the study of talk-in-interaction into three topics. They have to do with the simplest statistic of which I know: the proportion or fraction or percentage. It reports that something happens $n\%$ of the time, x out of every y times, and so on. I want to reflect on the denominator, on the numerator, and on the domain or the universe on which such a fraction or proportion is taken to report.

But first let me set out in the most reduced form possible the essence

of my reflections. First, quantitative analysis requires an analytically defensible notion of the denominator. I call it "environments of possible occurrence" or, more explicitly, "environments of possible *relevant* occurrence." A statistic even as simple as a proportion is meaningless without such a base, appearances to the contrary notwithstanding. Second, quantitative analysis requires an analytically defensible notion of the numerator, the set of types of occurrences whose presence should count as events and, given an adequate conception of environments of relevant possible occurrence, whose nonoccurrence should count as absences. That requires, I argue, an understanding of what sorts of occurrences or practices are alternatives to one another for the *participants*. Third, quantitative analysis requires an analytically defensible notion of the domain or universe being characterized. Here, I refer not to problems of adequate sampling, but rather a warranted conception of analytically coherent universes that it is relevant for a statistic to refer to, because they are relevant organizational domains of activity for the participants in interaction.

It is not impossible for these criteria to be satisfied when dealing with talk-in-interaction, but dealing with them does need attention and in many areas we are not yet (in my judgment) in a position to satisfy these prerequisites that allow the possibility that quantitative analysis will deliver what is wanted from it and what it promises.⁴

III

First the denominator.

Several summers ago, I attended a conference where I heard a paper in which the authors wished to assess the relative sociability of various sorts of elderly people and teenagers. The study was quasi-experimental in character and involved getting various combinations of young and elderly, with various other attributes, to interact with one another. The investigators then examined the talk to assess the degrees of sociability exhibited in it. Because the sorts of measures that were developed to assess sociability are ones about which I know something, and because they are apt exemplars of one of my concerns, I address a bit of

discussion to them. Not because they are distinctively problematic, but because they offer some characteristic problems for exploration.

The investigators focused part of their attention on differences among the participants in the amount of laughter and in the amount of what has been called, following Yngve (1970), backchannel behavior. What they compared was the performance of several categories of participants with respect to "laughter per minute" and "backchannels per minute." As I say, what I want to focus on here is the denominator, the "per minute."

It seems quite clear to me that parties to interaction do not laugh per minute. Laughter is among the most inescapably *responsive* forms of conduct in interaction. Even outside of interaction, it is treated by humans as intrinsically, indefeasibly responsive; one is always laughing "at" something, even if only a fleeting memory (a feature Goffman, 1978, underscored in naming a more inclusive class "response cries"). Laughter, then, is relevant at some points in talk-in-interaction and not at others. In the latter respect (i.e., where it is *not* relevant), it is prototypical of the sort of conduct that used to be cited for its psychiatrically diagnostic import as "affect inappropriate to the situation"; in popular culture, actors have relied on it as a way of depicting someone "maniacally off their rocker"; in politics, it can have characterological interpretations, as when a respondent interviewed by National Public Radio after a debate during the 1992 Democratic party presidential primary in Illinois remarked about candidate Bill Clinton, "Clinton struck me as kind of breezy; he smiled a lot when there was nothing to smile at" (though, to be sure, smiling and laughing are different forms of conduct).

So one can *not* choose to show sociability anywhere by laughing; *its positioning matters*. If some laugh less than others, then this is potentially an *interactionally* produced result. There may have been fewer of those sorts of talk and other action to which laughter is an appropriate response, or for which (to incorporate another of its environments of relevant possible occurrence) it is an appropriate accompaniment. If one wants to assess how much someone laughed, to compare it with other laughter by that person or by others, then a denominator will be needed that is *analytically relevant to what is to be counted* because it is *organizationally related to it in the conduct of interaction*. And minutes are not.⁵

What is? For answers to this question we need analysis, and very

likely analysis of single episodes of talk in interaction (even if in many such single episodes). We need it in order to establish from the way in which interaction is conducted, what the environments are in which laughter is possibly relevant, what sort of a tack laughter of various sorts constitutes in such environments, that is, what someone is *doing* by laughing there, what its import is and accordingly its possible consequences. It is not just *after* jokes and funny stories; it may also be in their course. But, surprisingly, it may also be after potential offenses (Jefferson, Sacks, & Schegloff, 1987) or as part of telling one's troubles (Jefferson, 1984b; but *not* as part of receiving another's troubles). And, of course, another's laughter can be a locus of relevance for one's own joining or withholding (e.g., Jefferson, 1979), each of which (in each of these environments) will have its own import.⁶ Laughter per minute, whatever impression of scientific exactitude and standardization it may suggest, ignores such environments of possible relevant occurrence and, is, I submit, a meaningless measure of conduct in ordinary interaction (i.e., one shorn of meaning).

Similarly with so-called backchannels, little productions of *uh huh*, *yeah*, and the like.⁷ These too some might choose to count per minute, perhaps as evidence of sociability. It is conventional to treat such productions as demonstrations of attention, involvement, or understanding. But, of course, *any* production of responsive talk by a co-participant would provide evidence of attention, and perhaps more detailed evidence, and so this does not help much with the occurrence of backchannels. Why does someone produce one of *these* tokens?⁸

Well, perhaps the most common environment in which they occur is that in which another participant is producing an extended turn (what we call a "multi-turn-constructural-unit turn" [Sacks, Schegloff, & Jefferson, 1974; Schegloff, 1982, 1987a], which may or may not be a "discourse unit" of canonical form like a story), and a recipient shows that he or she understands that such a unit is in progress but is not yet complete. Because such a production provides for, and indeed may prompt, a prior speaker to continue, I prefer to call such tokens not backchannels (which they may well *not* occupy), but "continuers" (Schegloff, 1982).

Whether there will be many or few such productions per minute may, accordingly, be understood to have less to do with the sociability of the participants than it has to do most proximately with the sequential structure of the turns into which the talk is organized. If there are many

such projected multi-unit turns, there will be more places at which a continuer is relevant. If the parties together organize their talk in single-unit turns (and recall that turn size is a *joint* product of speakers and hearers), there will be fewer such places. Producing a continuer after talk that is intendedly complete (after the point of a story or the thrust of an argument, for example, or a "one-liner") can do the precise *opposite* of sociability—it can show *inattention*, failure to grasp the other's point, or failure to align oneself interactionally with its thrust (Schegloff, 1991a, pp. 164–167). Once again, continuers have to be understood not relative to minutes, but relative to the environments of their relevant possible occurrence.

But where laughter, or continuers, can occur, other things can generally be relevantly done as well (e.g., with respect to continuers, assessments, see C. Goodwin, 1986), and their import may be the same as laughter, or continuers, only more so (e.g., with respect to laughter, a "topper"); or their import may be quite different (e.g., on repair where laughter might have been relevant, see Schegloff, 1987b, pp. 212–216; 1992a, pp. 1328–1329). That is, when laughter fails to occur in an environment of relevant possible occurrence, it may be because something else has been done instead that has the same import, or even in greater degree, or because a quite different tack was being taken, pointedly, by a participant. Each of these can have quite a different import for analysis. But here we again approach the problem of the numerator.

But before turning to the registering and counting of occurrences, which is what numerators ordinarily report, linger with me for a moment over the phrase "when laughter fails to occur in an environment of relevant possible occurrence." Note that an analytically grounded sense of "when laughter fails to occur" *requires* the notion of an "environment of relevant possible occurrence." For not every place that something may not be found is a place at which it is missing. Although I have not so far reported the outcome of last year's World Series, that report has not "failed to occur," it has not been "missing" or "absent," you could not cogently report its nonmention here. Anything in talk-in-interaction that is to be described nontrivially as absent, as a nonoccurrence of an analytically grounded sort, will have to be found not present at a place at which its presence was possibly relevant (see *inter alia* Sacks, 1992, Vol. 1, p. 670, Fall, 1967 on "relevance structures," and Schegloff, 1968, on "conditional relevance"). And so we will

need a specification of “environments of relevant possible occurrence” whenever this sort of basic descriptive statistic is to be formulated and deployed—both to provide a cogent denominator and to undergird the “negative space” in the numerator, that is, the nonoccurrences.

IV

Otherwise, the problem of the numerator takes several forms.

First, *what* counts as an occurrence of whatever it is we think we are counting? I referred earlier to the notion of backchannel signals, introduced some years ago by Yngve (1970) (though it had been treated before him by Fries, 1952) and picked up for further elaboration by Duncan and his colleagues (e.g., Duncan & Fiske, 1977). Duncan included in this class of utterances both tokens such as *mm hm*, *uh huh*, and *yeah*, as well as such brief (hence merely interpolated?) questions as *huh?* or *what?* Note, however, that, unlike the former, the latter do not encourage or even allow the speaker to prosecute the talk along the projected lines. They appear rather to stop it for repair. Which is precisely what *uh huh* seems to *pass the opportunity* to do. Are these then to be counted as alternative realizations of a same class of object? (See Schegloff, 1982, for fuller discussion.)

Let us say, then, that we purge the class of these items and leave it containing just bits like *uh huh*, *mm hm*, *yeah*, and appropriate head gestures (M. H. Goodwin, 1980).⁹ Actually, we may still have a problem, for Jefferson (1983 [also, this issue—Ed.], 1984a) argued that there *is* a practice by which *yeah*—in contrast with *uh huh*—marks incipient shift by its producer from “passive reciprocity” to active speakership.¹⁰ And what then of “assessment terms?” There are places in the course of an extended telling in which a speaker has built an episodic climax of a determinate character. Passing the speaker on to continue at such a juncture may involve not a colorless *uh huh* but an assessment term or expression that reflects appreciation of the produced character of the immediately preceding talk; anything less runs the risk of displaying inattention, lack of understanding, or a failure to align with the stance the teller has adopted.¹¹

So, as C. Goodwin (1986) suggested, assessments may stand as

alternatives to—or should we call them alternative realizations of?—backchannels or continuers.¹² But, of course, this is so only in certain environments; an assessment offered in an environment of talk not built for it will be understood as a very different kind of move, as itself possibly taking a different alignment toward what is being told than the teller is taking.

But there is a second form of problem with respect to the numerator. I have been proceeding as if we could presume a kind of slot and filler format,¹³ and the problem was to define what the frame was within which the slot was located, and what values the variable (the filler for the slot) could take. But participants may have ways of talking that break out of the format altogether. Let me broadly indicate what I have in mind first by an anecdote.

I am of a generation that found it difficult to address or refer to in-laws by the kinship terms for natural parents—*mom* and *dad*—but was also not yet prepared to address them by first name (and title plus last name was too formal). There was, as a resource, the set of practices that Goffman informally referred to as “no-naming” to cope with this exigency. There were, among others, various practices that managed to recast the format of the talk so that there was no “name slot” left at all. For example, I would commit myself to pick up my father-in-law at work, and, needing to know when to go by, would ask my mother-in-law not “When does X get off today?” but “And when does the business day end today?” The slot for a person reference is made to disappear by reformatting the utterance. So the problem is put in too restricted a manner if limited to “which person reference form is used”—too restricted, that is, for the set of practices of talking that participants employ as actual alternatives.

Here is a less dramatic illustration, in that the doing of person reference is retained, but not in a slot for a filler fixed within a recurrent frame. I can refer in passing to Uriel, if I am talking to Bill (Labov, i.e., my former colleague at Columbia University). To others I might refer, equally in passing, to Uriel Weinreich. In other contexts, but within the same frame, I might refer to “a major historical linguist” or “a preeminent student of Yiddish” or “a former colleague of mine at Columbia” (all of which Uriel Weinreich was), and so forth. All of these and others fit comfortably within a slot-and-filler format; it is only a question of which reference form or forms to insert into that slot within some sentence/utterance frame.

But speakers can also do differently: "Do you know who Uriel Weinreich was?" they may begin. That is, instead of choosing some person reference form to put into the "telling turn," they may construct a "presequence" to address the reference question (a sequence that may itself get expanded to get its work done), and then, in the telling sentence (if and when it arrives), a simple "he" appears (see Schegloff, 1980, for a fuller account).¹⁴

That is, the alternative realizations are not necessarily similar *sorts* of objects: Some are words, others are whole sequences, yet others are reconstructions of the forms of talk to circumlocute the problematic reference altogether. So if we count different forms of personal reference, we will need to figure out how to incorporate all the possible *forms of the occurrences*, quite apart from figuring out which nonoccurrences to count as relevant. And that means we will first have to know what jobs of talking-in-interaction participants are addressing, and what the range of practices is composed of by which these jobs get done.

At the risk of being (too) relentless, let me just mention in passing that, when we find new possibilities with respect to the numerator—additional ways speakers do some job or respond to some "move"—we may find ourselves with a recurrence of the problem of the denominator, needing to establish what are the environments of possible relevant occurrence of *that* form, and what are *its* contingencies, and so on.

For example, an alternative to a continuer in response to another's utterance is the initiation of repair, that is, an effort to deal with a problem in hearing or understanding (Schegloff, Jefferson, & Sacks, 1977)—so, perhaps, instead of *uh huh, huh?*¹⁵ But as soon as this possibility is entertained, we need to consider the whole range of constructions by which recipients of some talk can initiate repair on hearing or understanding problems that it poses (Schegloff et al., 1977, pp. 367–369)—of which *huh?* is only one, and others include category-specific question words (*Who?*) or repeats of the source of the trouble or candidate hearings/understandings offered for confirmation, as well as others. Each of these may have (partially) different environments of relevant possible occurrence and may be heard differently accordingly—for example, differentially portending disagreement.

A third sort of numerator problem can get only brief mention here but is no less consequential. Much that we study in talk-in-interaction takes the form of practices of conduct and organizations of such

practices. It is tempting to check our grasp of those practices by seeing if their designed products are in fact produced, and that is what leads to a search for "hits" or occurrences to be reported in the numerator. But it is important to remember that more than incidence and nonincidence are of interest. Indeed, relevance is at least as important as incidence in establishing an oriented-to order. Past experience has been that some of the best evidence *for* some phenomenon or practice can often be derived from negative cases, which may display an orientation by the participants to the very practices from which they depart. For example, Arthur may ask Bill a question, thereby "selecting him as next speaker." If Charles then speaks, the key observation may be not that it was he and not Bill who took the next turn, but that he begins his talk with an apology to Bill for the interruption, or that he makes *his* utterance also select Bill as next speaker,¹⁶ thereby showing his orientation to the relevance of the speaker-selection job done by the prior utterance in the very course of superceding it.

So we have here another constraint on negative cases, in addition to the earlier mentioned one concerning the establishing of an environment of relevant possible occurrence. Having established that, a nonoccurrence still needs close examination to determine whether or not, in the very nonoccurrence, evidence *for* (orientation to) a practice or an organization or a phenomenon may not be demonstrably provided. Therein lies again the need for single case analysis.

Enough for now on numerator problems — and their consequences.

V

The third sort of issue we should reflect on concerns the domain or universe from which our data are drawn, for which our claims are made, and to which they are responsible. Not all talk-in-interaction is similarly organized, and the adequacy of findings therefore derives not only from the correctness of their substantive claims but from the bounding of the domain for which they are asserted.¹⁷

It may be useful to begin by being explicit about why it makes sense to discriminate ordinary conversation from interviews, meetings from courtroom proceedings, and the like. It is not just a matter of sorting

corpora of talk by the common-sense labels that characterize the settings or contexts in which they were collected. The issue is not, or is not *merely*, a taxonomic one. These domains need to be discriminated when we believe, and *because* we believe, that interactants conduct themselves differently, are oriented to different sets of relevancies, and therefore produce and understand the conduct differently in these different domains.

For example, the domains just distinguished—ordinary conversation, interviews, meetings, and courtroom proceedings—are ones that appear to have talk organized by reference to different turn-taking organizations—to constitute in that sense what we called some years ago (Sacks et al., 1974) different “speech-exchange systems.” Indeed, some of these terms themselves surely mask internal variation; very different forms of talk, for example, get referred to vernacularly as “interviews.”

To register and appreciate the full import of a quantitative analysis, even a simple descriptive statistic, even assuming that the issues of defining environments of relevant possible occurrence and the range of alternative practices that enter into determinations of the numerator, to register the import of such a statistic we must take into account the domain for which it is proposed to hold. We are otherwise at serious risk of being confused or even misled.

Consider, for example, the review (Schourup, 1988) in the journal *Language* of Schiffrin’s book *Discourse Markers* (1987).¹⁸ The reviewer reported, as the author did, that the materials are drawn from sociolinguistic interviews, and took the troublesome potential relevance of this to be the distortions that might be introduced by observer participation. The reviewer reported, and apparently ratified, the neutralization of such potential problems discussed by the author.

But no account was taken of the sort of activity that “doing an interview” may constitute for the participants, and of the sorts of activities that the interview as a speech-exchange system and speech event lends itself to. I remember some years ago reading in an article about these materials an utterance that is reported to end with the speaker saying, “And that’s my answer to *that*.” It reminded me that “that it is an interview” may hover over the occasion and its participants as a continuing texture of relevances, showing up in this or that respect that we could recognize for its “interviewness,” once we came to understand that it had specially to do with *interviews*, but that we might otherwise take as features of *discourse in general*.

Consider, in this regard, an apparent divergence between a finding by Schiffrin about the marker *oh* and the account offered of the same marker by Heritage (1984). Heritage, working from a collection of single cases drawn from ordinary conversation (but not employing quantitative techniques on an aggregate level of analysis), displayed a range of exemplars of *oh* occurring “in response to informings that are elicited by questions” (1984, p. 307). Then he noted that this array of exemplars “shows that the production of an ‘oh’ receipt *is not necessarily associated with the degree to which the answer is expected*” [italics added], and he then explicated a whole range of sequential contexts in this regard.

This issue is of special interest to the reviewer of Schiffrin’s book, who remarked (Schourup, 1988, p. 634) with respect to it (not with respect to Heritage’s finding),

Here and elsewhere in the book descriptive statistics are used to great effect. By showing, for example, that “*oh*” *is far more likely to occur before answer acknowledgements when the answer is not selected from options precoded in the question than when the answer is selected from the coded options*, Schiffrin at once supports her general claim about “oh” and rescues the analysis from a classic objection to analyses of discourse markers: that they fail to show where such items do not occur. [italics added]

There are several observations to be registered about this excerpt as a vehicle for the discussion of scope that I am developing.

First, the reviewer took this as an especially noteworthy demonstration of the power and effect of descriptive statistics. It seems reasonable then to infer that he thought the case had been made that *oh* occurs far more after unexpected than after expected responses. Although his wording could be taken to reflect the interview character of the data base (e.g., his reference to the “options precoded in the question”), throughout the review he otherwise referred to how things work *in discourse*—not *in interviews*, but *in discourse*. Readers—of the book or of the review—may well take away a *finding* demonstrated “to great effect” quantitatively—*about discourse*. But even social science interviews turn out to vary with respect to the registering of answers to questions, namely the treatment of survey interviews by Suchman and Jordan (1990), in which one may find no *ohs* at all. An opportunity is passed up here to locate more explicitly just what sort of speech event, just what sort of talk-in-interaction, these occasions were for the

interviewer and her interlocutors—precisely by specifying what the occurrences of *oh* reveal. Here is a prime resource for exploring just that sort of variation in context—its source(s) and indicator(s)—that is often called for in research programs committed to the comparative study of language use.

Second, it is worth considering in this regard the difference between the range of questions asked in an interview and the range asked in ordinary conversation; between the interactional thrust and import of questions in the two settings; between the formats used to construct *intendedly nonbiasing questions* in interviews and questions *built to display preferences* in ordinary conversation; between the uses that various valences of answer relative to the question have in interview contexts, and the valences they have in ordinary conversation (e.g., the relative opportunities to “pursue the matter” vs. having shown a misreading of another’s stance). There are at least *prima facie* grounds for exploring whether “receipt” turns after question–answer sequences are the same sort of position in the two speech-exchange systems, in what respects they might differ in sequential organization and interactional import, and what bearing that might have on the quantitative results. Surely this is close to the central concern with the bearing of “context” on the production of talk.

Actually, and this is a third observation, such exploration would immediately encounter the need to discriminate different *types* of interview speech-exchange systems, incorporating different turn-taking systems, employed for different purposes (see, on survey interviews, Suchman & Jordan, 1990; on employment interviews, Button, 1987; on language assessment interviews, Lazaraton, 1991; on ethnographic interviews, the by-now extensive literature such as Briggs, 1986; and the references regarding news interviews that follow). In examining another type of “interview”—news interviews—at least three investigators have found yet another contrast (in addition to the one just discussed) to Schiffrin’s findings. In these interviews, the occurrence of *oh* after question–answer sequences is virtually zero, without respect to the expected or unexpected character of the answer relative to the question (see Clayman, 1988; Greatbatch, 1988; Heritage, 1985).

These authors offered an analysis of this finding by reference to what interviewers in news interviews are *doing* in that activity. It is systematically different from what questioners in ordinary conversation ordinarily are doing. So is what the interviewer in Schiffrin’s materials

is doing very likely systematically different from what questioners in ordinary conversation are doing. It seems reasonable to set the interpretation of the quantitative results into such a context, but that depends on those concerned with such materials—authors, reviewers, readers, discussants in subsequent research—situating the findings in some domain, a domain bounded and informed by our understanding of how those producing the talk-in-interaction understand the context they are in, the context that they *constitute* by employing *its practices* in the production and appreciation of their conduct (Heritage & Greatbatch, 1991; Schegloff, 1988/89, 1991b, 1992b).

Let me underscore the point that what is at issue here is not best, or even adequately, understood by the phrase familiar from social psychology, “definition of the situation.” Such a formulation is at risk of making the key concern out to be a conceptual and psychological matter. But at least for the case of “interview” and “conversation,” it is a matter *not* of vernacular conceptual definitions, but of practices that implement the talk, and—when practices such as turn-taking are at issue—*recurrent* practices that implement the talk (Heritage & Greatbatch, 1991) and can do so differentially. And so it is not just a matter of an observer knowing “the definition of the situation held by the parties” for the light in which properly to interpret some otherwise neutral object, but rather that the produced talk constituted a practice-d [sic] accomplishment of “such a setting,” of “*this* setting.” The “definition of the situation” is not separate and anterior; it inhabits the talk (see Heritage & Greatbatch, 1991; Schegloff, 1988/89, 1991b, 1992b).

VI

One upshot of these several considerations is that quantification is no substitute for analysis. We need to know what the phenomena are, how they are organized, and how they are related to each other *as a precondition* for cogently bringing methods of quantitative analysis to bear on them. I have been reflecting not so much on quantification per se as on its application to the study of talk-in-interaction at the present time. The result of the reflections is that, with some exceptions, it is premature. For many of the phenomena, practices, and organizational

domains that we study, the prospects for “maturity” (if that is what follows the premature) are remote at best, and in some cases arguably unrealizable because of the complexity intertwined with the considerations to which I have been calling attention.¹⁹ Still, the difficulties I have elucidated bear differently in different conversational environments,²⁰ and in some areas of inquiry it may be worth exploring whether or not the conditions for cogent quantification can be met. That is, whether we know enough about the domain of phenomena, and whether the features of its organization or practices are such as to allow us to try this mode of inquiry on it.

For example, I have for the last several years been engaged in a study of “other-initiated repair” in which I am working with about 1,300 instances of a type of sequence and its variants.²¹ Other-initiated repair refers to recipient’s attempts to deal with (or “repair”) (Schegloff et al., 1977, pp. 367–369) a problem encountered in hearing or understanding an utterance. Such efforts are virtually always initiated in the turn after the problematic utterance or “repairable,”²² and compose the whole of that turn. In fact, because nothing can be excluded in principle from the class “repairable” (Schegloff et al., 1977, p. 363), such repair initiation by the recipient of some talk appears to be the only type of turn in conversation with an unrestricted privilege of occurrence; it can in principle occur *after any turn at talk*. In that respect, then, its “environments of relevant possible occurrence” are well-defined. Furthermore, there appears to be a determinate set of turn formats used to initiate such repair (Schegloff et al., 1977, pp. 367–369), and those formats that are also used to accomplish other actions are specifiable, as are the other actions they are employed to enact. So what counts as an instance of other-initiated repair is relatively well-defined.²³ Unlike the earlier discussed practices of reference to persons, it appears that this domain of practices of talking-in-interaction—other-initiation of repair and its sequelae—can be “qualified” for quantitative treatment. Indeed, an early job facing a prospective quantitative analysis may well be to provide such an account of the domain to be studied as seriously addresses the considerations I have explicated and seriously grounds the defensibility of quantitative treatment by reference to the already analyzed properties of the domain.

Once a domain has been qualified for quantitative treatment,²⁴ we can proceed to the next issue, whose outcome is by no means clear. That issue is whether there are any *distinctive payoffs* of quantitative

analysis, for the fact that we *can* do quantitative analysis (if it should turn out to be a fact) does not entail that we *should* do it. Here again, the issue needs to be addressed by reference to the particular domain being studied. In the case of the project on other-initiated repair sequences, the answers to these questions are as yet unclear. Two examples of possibly interesting directions of work may be mentioned *but have not yet been worked through*. Both involve "comparison," but of quite different sorts, and potentially different warrantability.

One of the graduate students working with me on this project asked one day whether any of the other-initiated repairs in the corpus that took the form *huh?* occurred in a telephone conversation. She had been working on a coordinate project on other-initiated repair in Swedish conversation and had found no Swedish equivalent of *huh?* Because the materials she had by that time collected were from telephone conversations (with co-present interactions yet to be brought into the corpus), she thought that might be an account. A quick scan of the American materials showed that "few" of the *huhs* had occurred in telephone conversations.

Several "methodological" questions immediately come to mind, and of a quantitative sort; most pressingly whether the *huhs* are *disproportionately* few, that is, whether they are less prevalent than other turn formats for other-initiation of repair, or are fewer than the contribution of telephone conversations to the corpus as a whole would warrant. This is not the place to pursue such an inquiry in detail, but should it turn out that the scarcity of *huh* on the telephone is robust and not an artifact of the proportion of the corpus composed of talk on the telephone, research questions worth pursuing could clearly come into view. For example, there is already substantial evidence that initiating repair involves a selection procedure for choosing from among repair-initiation formats. What evidence might there be that this "selection" can incorporate an orientation to such contextual features of the interaction as whether it was on the telephone?²⁵ Or is the observation about the aggregate distribution not the product of an orientation to the telephone context directly, but an indirect product of other features, such as body behavioral components of *huh*-production whose efficacy is subverted on the telephone, though not distinctively there?

These particular considerations are entirely hypothetical, but they suggest a central potential consequence of a quantitatively grounded observation, a consequence just like the appropriate consequence of

observations grounded in ethnographic fieldwork or intuitive insight, namely a return to the data—to singular episodes of interaction—to track in the details of single occurrences the phenomenon or practice (if such it turns out to be) that had come to light through other modalities of inquiry, and to ground the analytic claims in those details.

This potential payoff on a quantitatively grounded observation may suggest the other I wish just to mention—formally similar but substantively potentially quite different. That is, the hypothetical line I have just sketched involves *comparison*, in this case of different conversational “contexts,” in one sense of that term. Other comparative questions clearly might exploit quantitative analysis along similar lines. Some of these are arguably grounded in the textures of relevance *to which parties to talk-in-interaction are oriented*; others are not. For example, the comparative treatment of parties to Swedish–Swedish and American–American conversation with respect to other-initiated repair is not obviously grounded in the orientations of the participants. Warranting such a course of inquiry is itself a methodological challenge, one to which I cannot address myself here.²⁶

Recall again that the considerations of the preceding paragraphs have been hypothetical and prospective in character. We have as yet little compelling evidence that there are distinctive payoffs to be derived from quantitative analysis that would counterbalance the considerable analytic pitfalls that lurk about. There is no clear answer to this question yet in the research project from which I have been drawing.

It *may* turn out that there is nothing both distinctive and defensible to be gained from quantitative studies of talk-in-interaction. Should that happen, we *could* face a curious irony (though one that has been argued on principle in the past). In contrast to much of the subject matter of the social sciences—which has been taken to be fundamentally *indeterminate* at the level of individual occurrences and orderly only at the aggregate statistical level—conduct in talk-in-interaction *could* then appear to be demonstrably orderly *at the level of the singular occurrence only* and, in effect, *not* orderly in any distinctive, relevant, or precisely determinable way in the aggregate. As of now, I do not believe this conclusion is warranted; the proper grounding and payoffs of quantification have not yet been thoroughly explored. In this regard, it bears underscoring that the concerns to which I have drawn attention are based not (only) in *some principled methodology*, but in the very *organization of talk-in-interaction* (as we currently understand it).

Accordingly, these concerns are matters for continuing reconsideration in light of changes in that understanding.

VII

I return here at the end to the query that prompted these reflections in the first instance: Why not introduce quantification into studies of talk-in-interaction? I have been treating this question as not rhetorical, but *seriously methodological*, that is, as a *matter of method* for the domain being studied. The issues I have brought to attention I mean to be treated as *concerns to be addressed* by any research undertaking in this area, not as *dicta* to which to submit. In brief, try to make analysis—*any* analysis, quantitative or qualitative—responsive to these issues *somehow*, or show them not to apply. In any given inquiry, it is appropriate to consider, in light of the materials being examined and the phenomena, practices, and so forth, being elucidated, what bearing, if any, these concerns have—how any one of them is neutralized or does not apply in the first instance, or how it fails to bear on the conclusions being drawn about just that study's preoccupations. Taking these concerns seriously and explicitly may itself jog the inquiry into *sensitivity to aspects of its materials or phenomena that may not otherwise have been addressed*, or addressed so readily.

I hope that it is clear that, at the very least, a great deal of research and clear thinking stands between the informal “quantification” embodied in usages such as *massively*, *ordinarily*, or *occasionally*²⁷ and the appropriation of more formal quantitative techniques. They are *not* simply weaker and stronger versions of the same undertaking; they represent different *sorts* of accounts.

Formal quantitative analysis is the outcome of a set of procedures focused on “precise” numerical characterization as (at some level) a desideratum in its own right: Definitions, criteria of class membership, and the like are required on its behalf even where they had not (or would not necessarily have) *otherwise emerged from the process of analysis*; mathematically grounded counting procedures are invoked; and so on. Informal quantification is the product of a quite different—but nonetheless methodological—orientation to empirical materials. Terminol-

ogy such as *occasionally* or *massively* reports an *experience* or *grasp* of frequency, not a count; an account of an investigator's sense of frequency over the range of a research experience, not in a specifically bounded body of data; a characterization of distribution fully though tacitly informed by the analytic import of what is being characterized.²⁸ It can yield not only the observation that there are only occasional exceptions to an overwhelmingly observed practice, but imbue that observation with a sense as to whether such exceptions warrant a full respecification of the analysis.²⁹ At the other end of the distributional spectrum, with all the difficulties in giving a formally quantitative account of practices of referring to persons in conversation discussed earlier, something substantive, important, and correct is conveyed in reporting that: "Reference forms are combinable, and *on some occasions* are used in combination. *But massively in conversation, references in reference occasions are accomplished by the use of a single reference form*" (Sacks & Schegloff, 1979, p. 17) [italics added]. A prospective move to more formal quantification does not involve merely replacing these informal terms with numbers.

We may need to defer full dress quantitative analysis in many areas until its appropriate constraints can be met. However, it may still be useful, even now, to learn from the more informal terms, which until now have been used, what they can convey about the characteristic shape of things that investigators have observed in their materials—at least occasionally, perhaps even frequently.

NOTES

- 1 The pedagogical context of my courses at the University of California, Los Angeles, aside, previous versions of these reflections were presented to quantitatively oriented sociolinguists at the 17th NWAVE (New Ways of Analyzing Variation) conference in Montreal, Quebec in October, 1988, and at the first sessions of the Language and Social Interaction group of the International Communication Association in Miami, Florida, in May, 1992. My thanks to David Sankoff and Robert Hopper, respectively, for these opportunities. I am indebted as well to Anne Lazaraton, William Labov, and Donald Treiman for informative reactions to these earlier versions; to the respondents to the ICA presentation, Kathy Kellermann, Janet Beavin Bavelas, Robert Hopper, and Sally Jackson; and especially to my colleagues Steve Clayman, John Heritage, and Peter Kollock for helpful discussion and suggestions while I was preparing the present version. I wish to acknowledge as well the support of the National Science Foundation through Grant No. BNS-8720388.

- 2 On the other hand, Dubois and Sankoff (1992, p. 2) found that some of these reflections "cast doubt on the applicability of [Labov's, 1969] principle of accountability to the analysis of discourse," and they suggested a "methodology [that] constitutes a solution to the problem of Schegloff's denominator without abandoning the principle of accountability" (Dubois & Sankoff, 1992, p. 11).
- 3 I do not address myself here (except briefly at the end) to the anticipated rewards of quantitative analysis. That is for another occasion, one in which *substantive payoffs can be presented, rather than promissory notes*. Here, then, I am concerned only with methodological constraints that need to be addressed if quantitative analysis is to be defensibly done, whatever the reason(s) for doing it.
- 4 Some who favor quantitative analysis have occasionally seemed discomfited by some of these considerations, finding them tendentiously antiquantitative whatever the protestations to the contrary. But quantitative analysis, and especially its statistical branch, is often looked to as a control on the sloppiness and imprecision to which nonquantitative reasoning is thought vulnerable. Precisely because this intendedly prophylactic use can induce a false sense of security, we should be wary of the missteps to which quantitative analysis is itself liable, for surely no one wishes merely to replace one set of subversions of quality work with another. Much of the relevant vigilance is supplied by workers in the quantitative tradition themselves. Perhaps something of value can be contributed by those whose work experience is somewhat different, who can address the bearing of features of the natural domain on analysis directed at it.
- 5 One referee of this article asked whether the claim is "that *in principle* interactants do not laugh (or produce other conversational behavior) on a per minute basis" and whether it is "not possible, then, that, for some phenomena, interactants orient to their production on a durational or a per-unit of time basis?" The argument in the text is empirical and analytic and is certainly *not* "in principle." So also would need to be the claim that some interactants produce elements of their conduct on a per-unit of time basis, and no apt candidates for such organization in naturally occurring interaction readily present themselves. The empirical/analytic point in the text should also serve to constrain the ways in which laughter (or other overt conduct) is construed as indicative of other, unobservable states of the parties.
- 6 Once again, although smiling is different from laughing, an account of a smile may provide a useful exemplar. *New Yorker* magazine staff writer Weschler (1990) recounted the following anecdote that occurred while he was interviewing a Uruguayan general about the use of torture in that country. The general

revealed that his men had a habit of questioning suspects "energetically."
"Energetically?" Weschler asked.

"[The dictator] was silent for a moment, his smile steady," Weschler wrote. "For him, this was clearly a game of cat and mouse. His smile horrified me, but presently I realized I'd begun smiling back (it seemed clear the interview had reached a crisis: either I was going to smile back, showing that I was the sort of man who understood these things or the interview was going to be abruptly over). So I smiled, and now I was doubly horrified that I was smiling. I'm sure he realized this, because he now smiled all the more, precisely at the way he'd gotten me to smile and how obviously horrified I was to be doing so" (p. 6).

This account was taken from Sipchen (1989). See also Teitelbaum (1990).

The point, again, is that when some phenomenon or practice is orderly on a case-by-case basis (and not only at the statistical aggregate level), what is relevant to its occurrence or nonoccurrence in particular environments will be relevant to its understanding in the aggregate, if there is to be one. Indeed, *both position and composition* are ordinarily constitutive of the sense and import of an element of conduct that embodies some phenomenon or practice.

- 7 On the issue of exactly what should be counted a backchannel and what not, see Schegloff (1982); but this concerns the problem of the numerator, about which more in a moment.
- 8 In the context of a discussion of quantification, I will err on the side of caution and remark that this use of "token" in the text has nothing to do with the type/token distinction often drawn in methodological contexts.
- 9 With this last phrase, I touch merely in passing on an expansion of several orders of magnitude in the issue under examination. There is the systematic possibility that for any form of conduct that can be enacted in, or contexted by, talk (and in/by so far unmentioned aspects of talk such as prosody, sustained or inflected pitch, pace or volume level, vocal quality such as creaky voice, etc.), there are realizations in other modalities, such as spatial positioning, posture, gesture, facial expression, gaze direction, and so on, and short- or long-term changes or shifts in these. These domains – by no means exhaustively mentioned here – enter recurrently into the considerations taken up in the text, but with no further explicit mention.
- 10 For specifying discussions of other forms also commonly assimilated without differentiation into the category of backchannels – forms such as *oh, oh, really?* and pro-repeats (such as *He/she/they did?*) – see Heritage (1984) and Jefferson (1981).
- 11 Not all uses of *uh huh, mm hm*, and the like do the work of continuers; this is simply one sort of generic use they have. Some usages of these tokens are responsive to the use in a speaker's turn of particular practices, the understanding of which is an issue over and above the general understanding of the turn in which they occur. For example, in referring to persons, speakers regularly employ (if possible) reference forms that allow their recipient to

recognize whom among those they know is being referred to (Sacks & Schegloff, 1979). It is not uncommon for such references to persons to be registered by a recipient with an acknowledgment token. For example,

- Bee: hh This feller I have- (iv-) "felluh"; this ma:n. (0.2)
 t! 'hhh He ha::(s) - uff-eh- who- who I have fer
 Linguistics {is real}ly too much, 'hh {h =
 Ava: Mm hm? Mm hm,

The second of Ava's "mm hm"s serves as a continuer, providing for Bee's telling the story that she has prefaced. The first "mm hm," it may be noted, is produced just at the completion of Bee's (somewhat troubled) person reference. A number of the examples in Yngve's article introducing the term *backchannel* are of this sort (1970, p. 574). The occurrence of backchannels may thus also be contingent on the occurrence of certain forms of person reference—and whatever other practices of talk make such interpolations possibly relevant next.

- 12 And, Goodwin proposed, assessments may be accorded a distinctive sequential deployment.
- 13 For those unfamiliar with the term(s), "slot and filler" refer to a mode of analysis that takes some form of talk (sentence, clause, utterance) as given—as a frame, so to speak, but for one element—the slot (e.g., word, intonation or stress pattern, etc.) whose alternative forms or realizations (the fillers) are examined to see how they would affect the overall import of the frame sentence/clause/utterance. Of course, neither the frame nor the fillers need be linguistic; for example, the frame could be a postural configuration and the variable a gesture.
- 14 A case in point (taken from Sacks & Schegloff, 1979, p. 19):

- Arlene: Ya still in the real estate business, Lawrence?
 Lawrence: Wah e' uh no my dear heart, uh y'know Max Rickler h?
 (0.5)
 Lawrence: hhh uh with whom I've been associated since I've been out here in Brentwood,
 Arlene: {Yeah
 Lawrence: has had a series of um bad experiences. . . .

- 15 Here we see the consequences of decisions about what constitute instances of some form of conduct, that is, the numerator problem. Yngve and Duncan and associates (see Duncan & Fiske, 1977; Yngve, 1970) treated such "brief questions" and *uh huh* equally as instances of backchannels rather than as alternatives to one another. See Schegloff (1982) for further discussion, where it is argued that one generic job that continuers such as *uh huh* do is to *pass* just such opportunities to initiate repair as *huh*—among other forms—exploits.

- 16 On the latter possibility, see Sacks (1992, Vol. 1, p. 669, Fall, 1967, Lect. 6).
- 17 Note again that I am not referring here to problems of sampling, of how to be confident that the materials we examine are representative of some universe, of which parameters of the materials should be matched with parameters of that universe, and indeed what parameters of samples or universes or populations are relevant at all. That is but one of a number of other issues that cannot be addressed in the present context.
- 18 In what follows, I recurrently refer to "the reviewer" and "the author" in the hope of minimizing the reading of this discussion as *ad hominem* or *ad feminam*. As in the earlier treatment of "per minute" as a denominator, I mean to address specifiable actual practices of quantitative analysis and not general issues, and have therefore focused on work that presents characteristic, and even recognizedly exemplary, instances of a practice. One other caveat: In some contexts, references to "discourse" or "discourse markers" by either the author or the reviewer may be read as referring to an analytic level of organization (i.e., *discourse* markers as compared to *syntactic* markers) rather than the extensional scope of the claim. My discussion means to address instances in which the latter reading is the most likely interpretation.
- 19 There are other challenging problems to be addressed on the way to quantitative analysis that I have not been able to take up here. For example, there is the problem of *sampling*, which I mentioned earlier only to set aside. Or there is the problem of "units," units of analysis, units of relevant context for the units of analysis, and the like. To extend an example offered earlier in the text, if C is talking and B "interrupts," to understand correctly C's withdrawal from the overlap we may need to know that in the preceding turn, A had addressed B and selected B as next speaker, and that C had intervened (Sacks, 1992, Vol. 1, pp. 323-324, Spring, 1966). To study the unit "interruption" or "overlap," then, we may need to examine instances in the context of the sequences in which they occur.
- 20 By this I mean not social or situational "contexts," but technically specified niches in the several modalities of organizations and practices in talk-in-interaction—sequential, interactional, and so on.
- 21 "Other-initiated Repair Sequences in Talk-in-Interaction," supported by the National Science Foundation under Grant No. BNS-8720388.
- 22 See Schegloff (1992a, pp. 1320-1326) for the "exceptions."
- 23 As are the alternatives to other-initiated repair (see Jefferson, 1987; Schegloff, 1992a, pp. 1326-1334).
- 24 This clause is meant analytically, not temporally. The concerns discussed here have a continuing relevance to analysis and are not simply "threshold" questions, to be set aside once met. So, the issue concerning environments of relevant possible occurrence, or the numerator issue of what the alternatives are

to other-initiations-of-repair, receive continuing attention during the course of analysis, and findings arise during the course of analysis that bear on these issues and enrich our understanding of the phenomena—and not only in quantitatively expressed ways. Thus, other-initiated repair seems to occur with special frequency and with special relevance in environments of potential disagreement or misalignment, and this partially specifies the environments of possible relevant occurrence over and above the previously formulated “after any turn.” As well, in addition to continuers as systematic alternatives to other-initiations of repair (alternatives that “pass” the opportunity to initiate repair), we can explore explicit disagreements, misalignments, rejections, and so on (what in the conversation-analytic literature are termed “dispreferred” responses) as turn-types to which other-initiations of repair may be alternative. (Note here the treatment of alternatives as asymmetrical.) As the inquiry progresses, additional such analytic possibilities may present themselves, and attention to the sorts of concerns discussed in this article on a continuing basis throughout the work may both progressively qualify the undertaking for quantitative treatment and enhance the analysis without respect to the possibilities for quantification.

- 25 Again, we know that some aspects of talk-in-interaction are specifically sensitive to this feature of context (Schegloff, 1979, 1986), and others appear not to be.
- 26 As studies grounded in the materials of talk-in-interaction are pressed in a comparativist direction (e.g., across social or occupational contexts; see Boden & Zimmerman, 1991; Drew & Heritage, 1992), quantitative techniques of analysis may come to appear increasingly inviting, and perhaps irresistible. One need only mention dimensions such as developmental stages (whether of socialization or of relationships), language, culture, psychobiological attributes (e.g., Alzheimers, aphasia, etc.) group affiliation and “style,” and so forth, to recognize matters that already present themselves as attractive topics at the boundaries between conversation analysis and other disciplines. But this list masks important variations that bear on the analytic viability of comparative work, for example, in the degree to which they are, or can be, oriented to relevancies for the participants, and/or ones oriented to comparatively. Another occasion will be needed to take up some of the knotty challenges presented by comparative analysis per se, against whose backdrop the use of quantitative analysis may require reexamination. (I am indebted to John Heritage for prompting discussion of these matters.)
- 27 Actually, these three “points” virtually exhaust the reach of “informal quantification.”
- 28 *What I have in mind is the counterpart in professional inquiry to observations developed by Sacks years ago about members’ commonsense measurement methods. Sacks (1988/89, pp. 54–60) called attention to the different ways in which “precise” and “approximate” measurement systems operate. Invited to*

meet someone at 2:30, I am not late if I arrive at 2:35; invited to meet them at 2:33, I *am* late at 2:35. For the specification of temporality, precise and approximate time references are generically different, they are different *orders* of orientation to time. Sacks developed similar accounts of other such measurement systems, such as the characterization of how fast one is driving or how severe an impact was in an automobile accident to have produced the resulting observed wreckage. As Sacks remarked with respect to a variety of commonsense forms of precise and approximate characterization (1988/89, p. 60):

this possible "vagueness" . . . is not a defective kind of vagueness but is the way to show that you measured the thing in an appropriate way to measure [such things].

And

Instead [of offering a "precise" characterization], what one does is offer the product of an educated analysis—that can then be seen to have been specifically done as an educated analysis, and thereby be seen to have been done by someone who knows how to look—if it's told to someone who knows how to hear.

- 29 As was the case in Schegloff (1968), where a single exception to a practice otherwise formulated adequately for the other 499 "cases" in the corpus engendered a reanalysis that yielded the 499 cases and the 1 that differed as alternative specifications of a more generally formulated organization.

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