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Conversation Analysis:
A Resource for Reconceptualizing
SLA Studies

2.0. INTRODUCTION

In the previous chapter, I reviewed some of the important hypotheses that constitute SLA studies. In this chapter, having first given a brief historical sketch and definition of CA, I examine some of the implicit assumptions that inform current SLA research from a CA perspective. I, therefore, rebut some criticisms that have been made of CA and then proceed to problematize how mainstream SLA studies have been constructed to date.. This critical examination of basic tenets in SLA provides the theoretical underpinnings for the kind of conversation-analytic respecification of SLA studies that is worked out in later chapters of this book.

2.1. TOWARD A DEFINITION OF CA

The term *conversation analysis* has been used to describe work that is informed by a broad range of disciplinary perspectives, including pragmatics, speech act theory, interactional sociolinguistics, ethnomethodology, the ethnography of communication, variation analysis, communication theory, and social psychology (Schiffrin, 1991). For my purposes, however, I restrict the use of this term to describe only the kind of work that has been carried out within an ethnomethodological tradition. In this, I follow the practice of Stubbs (1983), who noted that CA is almost always used as a synonym for an ethnomethodological orientation to what I more generally call the *analysis of conversational data* (ACD). Thus, according to this distinction, ACD subsumes CA and, indeed, all the other disciplinary perspectives previously mentioned.

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What, then, is CA? Historically, CA began life in the late 1960s and early 1970s as a subdiscipline of sociology. Like SLA, which is beginning to claim autonomy from applied linguistics, there are signs that CA is seceding from sociology and is also establishing itself as a separate discipline in its own right (Schegloff, 1987, 1991a, 1992a). Initially, CA researchers focused on describing the organizational structure of mundane, ordinary conversation, which may be defined as the kind of casual, social talk that routinely occurs between friends and acquaintances, either face-to-face or on the telephone. More specifically, researchers described this organizational structure in terms of sequences, turn-taking and repair practices (Goodwin, 1981; Jefferson, 1974, 1978, 1987; Sacks, Schegloff, & Jefferson, 1974; Schegloff, 1968, 1990, 1992b, in press; Schegloff, Jefferson, & Sacks, 1977; Schegloff & Sacks, 1973). Other representative work also carried out in the area of ordinary conversation includes studies of the sequential organization of various speech acts (Davidson, 1984; Drew, 1984; Pomerantz, 1975, 1978a, 1978b, 1984a, 1984b, Psathas, 1986; Schegloff, 1972), the construction of syntax-for-conversation (Goodwin, 1979; Lerner, 1991; Schegloff, 1979, 1996), reference (Sacks & Schegloff, 1979), and the structure of joke and story telling (Goodwin, 1984; Sacks, 1974; Stubbs, 1983).

These lines of research continue to be major foci of conversation-analytic work today. However, as Drew and Heritage (1992) noted, since the late 1970s there has been increasing interest in analyzing the structure of talk used to construct institutional contexts, including (among others) news, medical, courtroom and classroom contexts. Although the term *conversation analysis* continues to be used as the name of the field, the domains of CA now include both ordinary conversation and institutional talk (for useful collections of papers on institutional talk in L1 contexts, see Atkinson & Heritage 1984a; Boden & Zimmerman, 1991; Button, 1991; Button & Lee, 1987; Drew & Heritage, 1992a. For parallel examples of CA work on institutional talk that uses L2 data, see Firth, 1995, 1996; Firth & Wagner, 1997; Gaskill, 1980; Lerner, 1995; Liddicoat, 1997; Markee, 1994, 1995; Marriot, 1995; Schwartz, 1980; Wagner, 1996). For this reason, the more encompassing term *talk-in-interaction* (Schegloff, 1987) is widely used to refer to the full range of speech exchange systems just identified, which therefore all fall within the analytical purview of CA.

I now situate CA explicitly within the intellectual tradition of ethnomethodology, as it is the ethnomethodological foundations of CA that set it apart from other ACDs (Button, 1991; Heritage, 1987; Taylor & Cameron, 1987). According to Roger and Bull (1988):

Conversation Analysis

Numa Markee

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The term "ethnomethodology" was coined by Garfinkel (1974). In combining the words "ethno" and "methodology," Garfinkel was influenced by the use of such terms as "ethnobotany" and "ethnomedicine" to refer to folk systems of botanical and medical analysis. What is proposed is that any competent member of society (including the professional social scientist) is equipped with a methodology for analysing social phenomena; the term "ethnomethodology" thus refers to the study of ways in which everyday common-sense activities are analysed by participants, and of the ways in which these analyses are incorporated into courses of action. The most prominent development within ethnomethodology is undoubtedly that which has become known as conversation analysis, which examines the procedures used in the production of ordinary conversation. The influence of conversation analysis is being increasingly felt in disciplines outside sociology, notably psychology, linguistics[1] and anthropology. (p. 3)

More specifically, ethnomethodology is the product of a marriage between two seemingly incompatible intellectual perspectives, the *hermeneutic-dialectic* and the *logico-analytic* (Heritage, 1987; Mehan, 1978; Mehan & Wood, 1975). From the former, it borrowed its theoretical interest in folk ways of making sense of the world; from the latter, it took its empirically based methodology. As Mehan (1978) commented:

one of Garfinkel's (1967) seminal contributions was to translate the idealistic and subjectivistic notions associated with the phenomenological branch of the hermeneutic-dialectic tradition into the realm of the social by exhorting researchers to find in the interaction between people, not in their subjective states, the processes that assemble the concerted activities of everyday life. (p. 60)

Based on these characterizations, I define CA as a form of ACD that accounts for the sequential structure of talk-in-interaction in terms of interlocutors' real-time orientations to the preferential practices that underlie, for participants and consequently also for analysts, the conversational behaviors of turn-taking and repair in different speech exchange systems.

2.2. CA: EPISTEMOLOGICAL AND METHODOLOGICAL CONSIDERATIONS

The "rules of evidence" used by conversation analysts are not as well understood as those used by experimental researchers or, indeed, by ethnographers. Therefore, I briefly review what "counts" as evidence in CA and what kinds of claims are made by conversation analysts (see also Jacobs 1986, 1987).

2.2.1. Making Arguments in CA

The methodology of CA is qualitative and thus subject to the usual evaluation criteria for such research. Beyond this, however, CA attempts to explicate in emic terms the conversational practices that speakers orient to (i.e., the rules of talk they deploy for each other and, by extension, for analysts) by "unpacking" the structure of either single cases or collections of talk-in-interaction. Such cases provide the primary evidence for the asserted existence of particular conversational mechanisms identified by analysts. In short, a case is only convincing to the extent that it is directly motivated by the conversational data presented for analysis. As Benson and Hughes (1991) stated:

> the point of working with "actual occurrences," single instances, single events, is to see them as the products of "machinery" that constituted members' cultural competence enabling them to do what they do, produce the activities and scenes of everyday life ... the explication, say, of some segment of talk in terms of the "mechanism" by which that talk was produced there and then, is an explication of some part of culture. (p. 130)

2.2.2. The Role of Ethnographic Information in CA

CA is epistemologically quite close to ethnography, as both these approaches focus on the particular rather than the general and also seek to develop a participant's rather than a researcher's perspective on whatever phenomenon is being studied. Developing a participant's perspective involves developing a rich description of context. However, conversation analysts and ethnographers do not necessarily understand context in the same way (indeed, this is one of the most contentious issues in CA today. For an overview of the arguments, see Duranti & Goodwin 1992; Hopper,1990/1991).

For ethnographers, understanding members' practices involves developing a "thick description" of their local knowledge (Geertz, 1983). Developing such a description entails developing a detailed profile of members' cultures and biographies through a variety of data collection techniques. Typical data include video and audio tapes of behavior, transcripts, interviews, and retrospective talking-aloud protocols. These various kinds of data are then often triangulated (i.e., cross-checked against each other) in an effort to document the multiple perspectives of different participants (e.g., students, teachers and researchers) on a given event.

Some conversation analysts (see Bilmes, 1992, 1993; Cicourel, 1992; Mehan, 1993; Moerman, 1988) incorporate ethnographic information into their analyses (though not all do so to the same extent; see Wilson, 1991), claiming that such information is necessary for a complete understanding of talk-in-interaction. In contrast, researchers who work within the "purist" tradition of CA (e.g., Schegloff, 1987, 1990, 1991a; 1992a) make no appeal to ethnographic accounts of members' cultures or biographies to make an argument unless there is internal evidence in the conversational data to provide a warrant for the introduction of such data.[2]

For example, in Excerpt 2.1,[3] five observable facts in the talk of L9, L10 and L11[4] combine to warrant an analysis that appeals to the specific biographical details that L10 and L11 are Chinese speakers, whereas L9 comes from a different language background. First, L10 translates the word *coral* into Chinese (see lines 407 and 410). Second, L11 orients to this translation (see line 409). Third, L9 does not understand what the Chinese word means (see line 412). Fourth, as Chinese speakers, L10 and L11 can (and actually do) translate *sanku* back into English (see 413–414). Finally, L9 indicates that she understands what this Chinese word means by reference to its English equivalent (see line 415).

Excerpt 2.1

```
407  L10:  *  oh I see (+) I see the chinese is uh (+) sanku
408            (++)
409  L11:  *  unh?
410  L10:  *  sanku
411            (+)
412  L9:   *  what
413  L10:  *  c//orals//
414  L11:  *  //corals//
415  L9:   *  corals oh okay
416  L10:     yeah
               (NM: Class 1, Group 3)
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Absent such a warrant, analysts are said to be as well placed to analyze observed talk-as-behavior as the individual(s) who first produced it because they are using the same evidence that the participants were displaying to each other as they co-constructed the conversation in the first place. From a purist perspective on CA, then, context means the immediate sequential environment of a turn. It is this local environment (sometimes referred to by discourse analysts as the *co-text* of talk) that provides participants with a metric with which to judge the appropriateness of the talk that is produced in next turn. In this sense, therefore, conversation is highly context-dependent. At the same time, conversation may without contradiction also be said to be context-free (Sacks et al. 1974), in that socioeconomic status, gender, biographies or other such ethnographic data are not used a priori to explain how members organize and make sense of the talk that they construct for each other.

For these reasons, the production of additional texts by the original participants to explain or comment on what they "really" meant in the primary text is avoided, because such texts can only serve to confuse the analysis. This is because self-report data do not explicate the original behaviors so much as reconstruct and re-interpret them (see also Lantolf, 1994a, who comes to a similar conclusion), and these reconstructions are not necessarily more accurate or insightful than the original interpretations of the observed behavior.

2.2.3. Four Defining Characteristics of CA

Four important implications follow from this discussion. First, CA is profoundly agnostic about the value of explanations that are derived from etic theories of social action because these explanations are not grounded in members' constructions of their own naturally occurring behaviors. Second, conversation analysts do not, therefore, develop arguments about the structure of conversation on the basis of quantitative analyses of frequency data. This is because such analyses cannot reveal anything about how participants orient to the underlying preferential structure of conversation.[5] Instead, conversation analysts seek to demonstrate that conversation could not be conversation if such universal interactional resources for making meaning as turn-taking or repair did not exist.

Third, in order to demonstrate the existence of such resources, conversation analysts use prototypical examples which give discursive form to the phenomenon being analyzed. However, such examples are not by themselves sufficient to make a convincing argument. Analysts

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must be able to corroborate their claims by pointing to a convergence of different types of textual evidence or by showing that a single structure identified by the analyst plays a role in different types of cases. Note that the use of convergent evidence is a particularly important resource for countering the charge that an analysis is merely an artifact of the examples collected and chosen for presentation to readers. So indeed is the use of related data. For example, it can be shown that reading a turn as an invitation is cotextually warranted by an invitation-relevant presequence that enquires into the potential availability of the invitee and by a following acceptance or rejection sequence which brings the business to a close. Finally, analyses must be subject to critical falsification. That is, analysts must demonstrate that potential counterexamples and different accounts for the same data set have been anticipated and that other researchers can replicate findings with different transcripts.

2.2.4. Summary

CA is radically different from other forms of ACD that are relatively more familiar to SLA specialists. Most importantly, it avoids developing its arguments on the basis of any a priori theory, be this nomothetic (see Long, 1980; Long & Sato, 1983; Pica, 1983a, 1983b; Pica & Doughty, 1985; Pica, Young, & Doughty, 1987 for work of this kind), formal (Chomsky, 1965, 1975, 1980, 1986; Gregg, 1993, 1996; White; 1989) or constructivist (see Coughlan, 1995; Hall, 1993, 1995a; 1995b, 1997; Lantolf, 1994b; Lantolf & Appel, 1994; Ohta, 1995; van Lier, 1988, 1996, all of whom work within the Vygotskyan paradigm of sociocultural theory).

Although CA is most different from the dominant nomothetic tradition of SLA research, it nonetheless also differs from more mainstream qualitative approaches to SLA. In particular, CA tends to avoid appealing to ethnographic data (as used by Douglas & Selinker, 1994; Hawkins, 1985; van Lier, 1988, 1996), thereby formulating the notion of context much more strictly than is commonly the case in SLA work (see, e.g., the work of Selinker & Douglas, 1985, 1989 in this area). Clearly, therefore, CA has the potential to provide a far-reaching epistemological critique of mainstream SLA studies, whether in the experimental or ethnographic tradition. Before I develop such a critique, however, let me first deal with some objections to the use of CA as a methodological resource for SLA studies.

2.3. SOME POTENTIAL OBJECTIONS TO THE USE OF CA AS A METHODOLOGICAL TOOL FOR SLA STUDIES

There are three principal objections to using CA as a methodological tool for understanding SLA processes. The first two have been most forcefully articulated in the rebuttals of Firth and Wagner (1997) by Kasper (1997), Long (1997)[6] and Gass (1998), whereas the third has been advanced by Crookes (1990). The first objection is that CA is a behavioral discipline while SLA studies is a cognitive discipline. More specifically, CA is suspicious of individual cognitive constructs (e.g., knowledge, understanding, learning, etc.). In contrast, SLA theory seeks to describe and explain the cognitive processes that underlie language learning. It is therefore not clear what CA has to offer SLA studies, because the two disciplines have such seemingly incompatible outlooks on the nature of the phenomenon that is to be explained. The second, clearly related, objection is that CA is designed to account for language use, not its acquisition. As such, any insights that CA might provide into the structure of conversation is peripheral to the central concerns of SLA studies. The third objection is that the turn is not a suitable unit of analysis for SLA studies.

2.3.1. Are CA and SLA Studies Incompatible?

Although it is perfectly true that CA is fundamentally a behaviorally oriented discipline that focuses on language use, I wish to argue that this does not therefore automatically disqualify CA as a methodological tool for studying the kinds of learning processes that are central to SLA studies. Furthermore, I do not accept that the turn is not a viable unit of analysis for researchers interested in SLA. Let us first examine Gass' (1998), Kasper's (1997), and Long's (1997) objection that CA and SLA studies have mutually incompatible outlooks on how the phenomenon of language acquisition might be analyzed.

In my view, what separates SLA researchers from conversation analysts is not so much whether language is best described in terms of cognition or behavior as whether cognition is understood exclusively as an individual or as both an individual and a socially distributed phenomenon that is observable in members' conversational behaviors. Early CA work on the sequential organization of talk, turn-taking, and repair (see Goodwin, 1981; Jefferson, 1974, 1978; Sacks et al. 1974; Schegloff, 1968; Schegloff et al. 1977; Schegloff & Sacks, 1973) specifies the rule-governed nature of members' observed, real-time conversational practices that constitute their interactional compe-

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tence. This interactional competence interacts with members' individual and collective ability to analyze and deploy syntax-for-conversation in order to achieve socially relevant, locally occasioned acts of communication (Goodwin, 1979; Lerner, 1991; Sacks et al. 1974; Schegloff, 1979, 1996).

Although the precise nature of members' grammatical knowledge is not specified by these authors, it is clear that conversationalists constantly monitor and analyze the grammatical phrase structure of interlocutors' unfolding turns in order to take their own turns and to interject repair initiations at appropriate moments in the talk (Sacks et al. 1974). As Gass' (1997) model of the phases of language learning (discussed in chapter 1) shows, conversational repair is viewed by SLA researchers as the sociopsychological engine that enables learners to get comprehended input. This understanding of the nature of repair in SLA is broadly consistent with more recent work by Schegloff (1991a) in CA, who argued that repair can be analyzed as an example of socially distributed cognition. By extension, the same claim can be made for sequencing and turn-taking.

This development potentially clears the way for a convergence of perspectives between SLA researchers and conversation analysts on the utility of CA as a methodological resource for SLA studies. However, this convergence has radical implications for mainstream SLA. More specifically, it seems clear that the idea that cognition is not solely an individual but also a socially distributed phenomenon that is observable in members' conversational behaviors must oblige social interactionist researchers in SLA to reconsider the idea that cognition is exclusively instantiated in the minds of individuals. Of course, this position has already been championed by Vygotskyan researchers, who argue that learning first occurs interpsychologically as a result of interaction between mentor and novice, only later becoming appropriated intrapsychologically by the novice (Aljaafreh & Lantolf, 1994). I develop this idea further in CA terms in chapters 7 and 8.

2.3.2. Is CA Unsuitable as a Resource for SLA Studies?

This section examines the second objection raised by Gass (1998), Kasper (1997) and Long (1997), namely, that CA focuses on language use, not acquisition, thus making it of marginal use to SLA researchers. This objection is clearly prompted by these writers' fear that if Firth and Wagner's argument for broadening the present scope of SLA studies were to be widely adopted, it would in effect no longer be SLA studies but a new field called second language studies, which would no longer necessarily be committed to addressing the traditional acquisitional issues of SLA studies as its primary intellectual goal.

I am sympathetic to these concerns. Thus, as an SLA specialist, I accept that issues of language use are subsidiary to questions of language acquisition in SLA studies. However, as a conversation analyst, I also agree with Firth and Wagner (1997, 1998) that the boundaries between language acquisition and use are in fact quite indistinct — a point that should be quite familiar to SLA colleagues because it lies at the heart of sociolinguistic critiques (Halliday, 1973; Hymes, 1972) of generative notions of competence and performance (Chomsky, 1965). Furthermore, as argued in the previous section, when researchers investigate the structure of conversational practices such as sequencing, turn-taking, and repair, they are in fact also investigating processes of socially distributed cognition; these processes surely lie at the heart of sociolinguistically influenced approaches to SLA studies. Consequently, I believe that a strong case can be made that SLA studies would be greatly enriched by incorporating into its methodological arsenal conversation analyses of the sequential and other resources that speakers use to modify each others' talk and thereby to comprehend and learn new language.[7] Finally, it is worth pointing out that this type of research would of course play directly into the research program outlined by Long (1985a) on the role played by comprehensible input in SLA.

2.3.3. Is the Turn a Suitable Unit of Analysis for SLA Studies?

Finally, let us examine Crookes' (1990) objection that the turn is not a suitable unit of analysis for SLA studies. According to Crookes, SLA researchers have used at least five different kinds of units to describe the structure of L2 discourse, sometimes singly, sometimes in combination: T-units, c-units, turns, tone units, and utterances. Crookes argued that, of these five categories, the utterance is the most suitable analytic category for use with L2 discourse data. The reason that he cited in support of this position is that, if they are to be useful, analytic categories must have a high degree of instrumental validity, that is that researchers' analytical categories must reflect the psychological processes that underlie individual language learners' speech production. He concluded:

> On these grounds, the turn may be eliminated from consider-
> ation. Since its boundaries are determined by the processes of
> speaker interaction, it does not reflect the psychological
> processes of an individual's speech production alone, but is
> additionally influenced by the many social variables which

determine the flow of multi-party discourse. (It also becomes meaningless when monologue is considered). (pp. 191–192)

There are three theoretical and empirical problems with this objection. First, Crookes' own preference for the category of utterance reflects a speaker's, not a hearer's, perspective on who controls the production of talk-in-interaction. However, as Sacks et al. (1974) and many other conversation analysts have amply demonstrated, this position is empirically not sustainable. Second, by adopting — as I believe is necessary — a hearer's perspective on talk-in-interaction, speech production cannot be understood solely from an individual, cognitive perspective, even if the object of study is how individuals learn new language. This is because talk-in-interaction is fundamentally collaborative in nature (Sacks et al. 1974). Consequently, in order to be logically consistent, researchers must view the conversational resources that individuals potentially draw on to learn new language as collaboratively achieved micro-moments of cognition. Just as communicative competence is said to subsume linguistic competence (Hymes, 1972), so these collaboratively achieved micromoments of cognition are best understood as socially distributed phenomena that subsume at least some individual cognitive processes in SLA.

Finally, Crookes' assertion that a turn-taking account of speech production is vitiated by the many social variables that determine the flow of multiparty discourse is also suspect. Invoking the principle of "ethnomethodological indifference" (Garfinkel & Sacks, 1970), conversation analysts maintain that the putative effects of social variables on the structure of talk are not a matter of a priori theorizing. Rather, they are an empirical matter, whose relevance to speakers has to be located in speakers' own conversational practices (Schegloff, 1972, 1987, 1991b, 1992a). In practice, empirical research has shown that speakers seem to orient to a turn-taking machinery that is remarkably unaffected by external social variables (see Sacks et al. 1974).

For these reasons, I maintain that the turn is a particularly valuable analytical category for L2 ACDs. There can surely be no better yardstick for determining the validity of turns as a unit of analysis than demonstrating that the existence of such units can be empirically located in the participants' own conversational behaviors.

2.4. RESPECIFYING SLA STUDIES

Having dealt with these objections to the use of CA as a methodological tool for SLA studies, let me now offer an ethnomethodologically

motivated critique of mainstream SLA. In common with many other disciplines in the social sciences, mainstream SLA relies on the idea that if an explanation of how the world functions is to be scientifically adequate, it must be based on ways of knowing that are compatible with a rationalist approach to constructing scientific knowledge (see, e.g., Gregg, Long, Jordan, & Beretta, 1997). Furthermore, if (as often happens) a folk explanation diverges from a scientific explanation, then the former is to be discounted as irrational and, therefore, as scientifically inadequate. As Heritage (1987) remarked:

> a radical gulf is thus created between rational actions with their self-subsistent reasons and non-rational actions in which the actors' reasoning is discounted in favour of causal normative explanations of conduct ... [The effect of this epistemology is] to marginalize the knowledgeability of social actors to a remarkable degree and to treat the actors, in Garfinkel's memorable phrase, as 'judgemental dopes' (Garfinkel, 1984, p. 68) whose understanding and reasoning in concrete situations of action are irrelevant to an analytical approach to social action. (p. 229)

The rationalist position on the scant value of members' knowledge about the world derives from Talcott Parsons, whose theories of social action dominated postwar sociology for 20 years. In contrast, ethnomethodologists such as Garfinkel argue that rationalist explanations of the world are not in any sense more insightful or indeed useful than those of social actors.[8] As Heritage explained:

> Garfinkel proposed that, if mundane social actions were premised on the characteristic features of scientific rationality, the result would not be successful activity but, rather, inactivity, disorganization and anomie (Garfinkel, 1952, 1984, pp. 270–271). A scientifically adequate orientation to the events of the social world is thus far from being an ideal strategy for dealing with the flow of ordinary events. Its imposition as a standard with which to evaluate actors' judgements is therefore wholly unwarranted and, Garfinkel insisted, it is both unnecessary and inhibiting in analysing the properties of practical action (Garfinkel, 1984, pp. 280–281). Moreover, if ideal conceptions of rational action are dropped from the picture, the way is open to begin investigations based on the properties of the actors' actual knowledge in the making of reasonable choices among courses of action, i.e.

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Of course, the implications of such criticisms are potentially open to serious misinterpretation. Let me therefore immediately nip in the bud any suggestion that ethnomethodologists subscribe to an "anything goes" approach to doing science, in which potentially any and all explanations are equally valid.[9] As already noted, CA is empirically based, has clearly defined methodological procedures for developing participant-relevant analyses of talk-in-interaction, and is concerned with the possibility of replication. Furthermore, the rules for developing such analyses are just as, if not more, rigorous than those followed by experimental researchers.

For example, it is common practice in experimental research to treat the behaviors of so-called "outliers" as atypical of the sample. Consequently, these outliers are often discarded from the final analysis. In CA, however, all participants' behaviors are viewed as making sense to the individuals concerned, and thus must be accounted for in the analysis. So, for example, in his analysis of sequencing in conversational openings, Schegloff (1968) initially developed an analysis that accounted for 499 out of 500 cases in his database. However, Schegloff did not ignore the solitary apparent exception to his analysis. Instead, he went on to reanalyze the entire corpus to yield the 499 cases plus the apparent exception as alternative specifications of the phenomenon under study at a more general level of organization.

Having established that ethnomethodology represents a serious, though undoubtedly heretical, approach to doing science, it is clear that the adoption of CA as a methodological resource for SLA studies necessarily entails a fundamental respecification of the SLA research enterprise.[10] This respecification has at least three dimensions: developing an emic alternative to rationalist science, developing a critical attitude toward quantified data, and using highly detailed transcripts of talk-in-interaction as primary data.

2.4.1. Developing an Emic Alternative to Rationalist Science

Most SLA writers have until recently treated the rationalist norms of the dominant nomothetic paradigm as intellectually unproblematic. One of the most important of these norms is that an experimental, quantitatively oriented methodology holds the principal key to scientific progress. However, this is not necessarily true. For

example, Foster's (1998) work on conversational modifications (which, incidentally, is not informed by ethnomethodological ideas) presents two alternative pictures of the same phenomenon. These two pictures are hard to reconcile. On the one hand, Foster's study shows that, in aggregate, certain task types seemed to prompt learners to modify their speech more frequently than other types did. However, when the speech of individual learners was examined, it became clear that there was tremendous individual variation in the number of repairs that were initiated by learners during different tasks. A small minority of learners did the lion's share of initiations, whereas the majority initiated few or no repairs at all. Furthermore, the organizational factor that seemed to explain whether learners engaged in negotiation work at all seemed to be whether learners were working in pairs rather than in small groups, not the characteristics of the tasks that they had to solve.

These results demonstrate that an experimental, quantitatively oriented methodology inevitably loses important details of individual behavior. For some researchers, of course, this is an acceptable price to pay to get generalizable results that contribute to the theoretical evolution of SLA studies as a scientific discipline. However, Foster's paper raises the possibility that the results of mainstream SLA research on the psycholinguistic properties of different task types or the acquisitional function of conversational repairs could merely be artifacts of an experimental, quantitative methodology. In order to address this crucial issue, SLA theory construction needs more input from emically focused research on the contextual and interactive dimensions of talk-in-interaction (Firth & Wagner, 1997). This input would allow an assessment of whether the results of quantitative and qualitative SLA research ultimately converge or diverge.

2.4.2. Developing a Critical Attitude Toward Quantified Data

The second point is closely related to the first. Even though quantification has provided many important insights into SLA processes, there is an urgent need for SLA researchers to develop a more critical attitude toward the use of certain types of quantified data than is currently the case. For example, as as I noted previously (Markee, 1994), an experimental approach to SLA studies fundamentally depends on clear definitions of terms. Furthermore, any analytical categories that are based on these definitions must not overlap. After all, from an experimental perspective, the whole point of positing such categories is to investigate whether the distributions of these

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categories are statistically significant (thus allowing researchers to generalize beyond the sample studied) and whether these distributions are consistent with researchers' theoretically motivated predictions.

However, the functional subcategories of repair that are commonly used in mainstream SLA research are notorious for their ambiguity and lack of discreteness. Consider, for example, the categories of comprehension checks and clarification requests proposed by Long and Porter (1985) and Porter (1986). Porter (1986) gives the following examples of these categories in Excerpts 2.2 (see line 2) and 2.3 (see line 1).

Excerpt 2.2
Comprehension check

```
1 L:      To sin- uh ... to sink
2 N:   *  Do you know what that is?
3 L:      To go uh-
5 N:      To go under ...
          (p. 207)
```

Excerpt 2.3
Definition request

```
1 L:   *  ... what is the meaning of research?
2 N:      Um, study?  You study a problem and find an answer.
          (p. 207)
```

From a participant's intersubjective perspective, there is no evidence that members orient to these categories as distinct constructs at all. The participants in both these excerpts orient to a need to resolve lexical trouble that occurs in their conversation. Although in functional terms the initiations of repair work may look different, in sequential terms, the participants end up doing definition work that is spread over a number of turns (see, in particular, Excerpt 2.2).[11] Arguably, therefore, the general category of "definition talk," whose defining organization is ultimately sequential rather than functional, seems better motivated by the data than the two more specific speech acts proposed by Porter. This conclusion poses severe methodological problems for SLA researchers working within an experimental framework because repair subcategories must not overlap if subsequent statistical manipulations of the data are to have any validity or reliability.

These kinds of considerations also underlie Aston's (1986) critique of what he called the "more the merrier approach" to analyzing repair. Aston also pointed out that it is difficult to

differentiate empirically between subcategories of repair. In addition, he noted that decontextualized experimental research on conversational modifications implies that students should be taught to negotiate meaning at any and every opportunity. Citing Garfinkel's (1967) breaching experiments, Aston remarked that this kind of behavior would quickly be perceived as irritating.[12] This notion is empirically supported by the talk in Excerpt 2.4 although in this instance, it is a learner, L11, who becomes annoyed with the teacher's repeated repair initiations. More specifically, L11's asterisked turn at line 541 illustrates the social consequences of the teacher's excessive repair initiations at lines 524 and 538.

Excerpt 2.4

```
520 L11:    ok (+) excuse me (+) uh: what what does it mean hab- (+) habi-
521         (+)
522 T:      habitats
523 L11:    habitats
524 T:    * yeah (+) you had that word as well (+) what do you think it means
525         (++)
526 L10:    <hhh>//hh//
527 T:              //you// all spoke about habitats didn't it
528 L10:    uh:m
529 T:      the //m//ost important (1) habitat
530 L10:         //I//
531         (++)
532 L10:    I think (+) the habitats is the:[əm] (+) e//nvironment uh// and the
533 L9:                                               //environment//
534 L10:    environment and uh (1) uhm
535         (++)
536 L9:     is it is //it the: nearest environment//
537 L10:            //for for (+) for the fish// you (mea be:) (hh)
538 T:    * <h> yeah what would be another word for a habitat then (+) it's like
539         (1)
540 T:      //it's hli-//
541 L11:  * //I ha//ve no idea ((in an exasperated tone))
            (NM: Class 1, Group 3)
```

Notwithstanding these kinds of criticisms of experimental research, Long (1997) continued to articulate the received position in SLA studies when he criticized the lack of quantification in most sociolinguistically oriented naturalistic ACDs. More specifically, it seems that Long believes that the inclusion of descriptive statistics would automatically improve the quality of qualitative research. But, as I have shown, this position ignores the methodological weaknesses of quantification. Furthermore, insisting on the virtues of quantified

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data in this way is rather like saying that American football would be a much better game if only it were played with a round ball, as in soccer, rather than with the oval-shaped ball that is used in American football. Finally, this criticism misses the point that naturalistic researchers may avoid using even the simplest statistics (e.g., percentage scores) because to use them would be inconsistent with developing an emic analytical perspective on the phenomenon being studied.[13]

Speaking to this issue in the context of analyzing mundane conversation, Schegloff (1993) pointed out that developing an index of sociability that is based on quantifying the number of laughter tokens that subjects produce per minute completely ignores the issue of when it is sequentially appropriate for conversationalists to laugh. Laughter at inappropriate times or, conversely, the lack of laughter at appropriate moments in the talk, are accountable events that may hold serious consequences for participants. For example, in its most extreme form, inappropriate laughter may be interpreted as a manifestation of emotional instability, even mental illness. Less drastically, laughing at the wrong moment may be judged as rude and have adverse effects on a member's social relationships with his or her peers. Quantitative techniques are thus ill-suited to capturing these subtleties and, when applied to conversational data, tend to distort their communicative import.

In the context of SLA studies, similar criticism may be leveled against Foster and Skehan (1996), who defined fluency in terms of continued performance and repair avoidance. On the basis of these definitions, they develop a statistically based index of fluency which reflects an interaction between a particular task type and the amount of planning time that is available to students to prepare for the task (see Table 1.1 in chapter 1). However, the conceptualizations of fluency and planning used by Foster and Skehan are theoretically problematic on three counts.

First, the notion of continued performance as an indicator of fluency completely ignores the fact that there are times when it is imperative for speakers to stop talking. Conversationalists who insisted on continuing a prior conversation about the weather during the middle of a funeral service would not be judged fluent but rather insensitive. Indeed, if they persisted in this behavior, they would likely be asked to leave. Second, Foster and Skehan's definition of fluency wrongly assumes that normal discourse is free of trouble when, in point of fact, all interaction — including repaired talk itself — is not only potentially repairable but is actually repaired quite regularly in order to transact the business at hand both successfully and fluently (Schegloff et al. 1977). Thus, repaired talk, and the associated pauses and silences

that are often a signal that a repair is in progess, is not by itself indicative of a lack of fluency.

Finally, although a distinction can be made between spontaneous and prepared speech, particularly when these notions are tied to technical specifications of different speech exchange systems, the idea that conversation is ever unplanned is also problematic. All talk is designed to achieve a particular goal at a particular moment in a particular conversation. Furthermore, it is designed for a particular recipient. Thus, the notion of "recipient design" (Sacks et al. 1974) is inextricably bound up with the idea of moment-by-moment planning.

2.4.3. Using Highly Detailed Transcripts of Talk-in-Interaction as Primary Data

Since CA rarely quantifies members' conversational practices, except in the most general, participant-relevant terms, it is clear that suitably transcribed audio or video recordings of talk become the primary data for analysis and discussion. As Heritage (1988), noted, four basic assumptions govern CA work and are therefore reflected in how talk-in-interaction is transcribed:

- Conversation has structure.
- Conversation is its own autonomous context; that is, the meaning of a particular utterance is shaped by what immediately precedes it and also by what immediately follows it.
- There is no a priori justification for believing that any detail of conversation, however minute, is disorderly, accidental or irrelevant.
- The study of conversation requires naturally occurring data.[14]

I already discussed the first two points in earlier parts of this chapter. I concentrate now on fleshing out the implications for SLA studies of the last two elements of this position. For reasons of organizational convenience, I begin with Heritage's fourth point, noting that a preference for naturally occurring data requires researchers to be extremely sensitive to the social context of data collection. I then address the implications of Heritage's third point, that is, if no detail of conversation is disorderly, accidental, or irrelevant, then clearly, extremely fine-grained transcriptions will be required to capture the complexity of talk-in-interaction. This principle also implies that the use of sampling procedures should be avoided because such techniques are likely to exclude vital details from the analysis. Based on this discussion, I finally sketch out what a CA-inspired methodology for SLA studies might look like.

Data collected in laboratory settings[15] inevitably reflect a member orientation to a speech exchange system that is demonstrably different from that of ordinary conversation (Schegloff, 1993). Thus, there can be no expectation that results obtained in laboratory settings will necessarily generalize to other settings. To make statements about how mundane conversation or ordinary classroom talk is organized, it is best to gather such data directly from these settings (see also Foster, 1998, who makes the same general point).

Speaking to this issue, Firth and Wagner (1997) argued that SLA data gathered under laboratory conditions are tainted because researchers attribute stereotypical roles such as NS and NNS to their subjects. These psycholinguistically defined roles do not take into account other sociolinguistically defined roles (father, friend, wife, etc.) that might be more relevant to participants in ordinary conversation.[16] The attribution of such psycholinguistically defined roles to speakers is said to entail a number of other theoretical problems, which led Firth and Wagner to suggest that the scope of SLA studies should be broadened to include the study of naturally occurring lingua franca talk, that is, interactions between L2 speakers who are communicating in a shared L2 for instrumental reasons, such as conducting business.[17]

As I noted previously, Gass (1998), Kasper (1997), and Long (1997) all objected, perhaps justifiably in some ways, that broadening the scope of SLA studies to this extent would effectively remove the A from SLA studies. However, Liddicoat (1997), who is in substantial agreement with Firth and Wagner's position, made a subtler point in suggesting that the reason why NSs are massively constituted as interviewers and NNSs as interviewees in so many transcripts of NS–NNS talk is due to the fact that the overwhelming majority of SLA conversational data — be they naturalistic or gathered in laboratory settings — instantiate various kinds of institutional talk, not ordinary conversation. Liddicoat therefore claims that many broadly accepted findings in SLA studies, such as the reported dominance of NSs in NS–NNS conversation and NSs' preference for other-initiated repairs of NNSs' talk, are nothing more than the products of the specialized speech exchange systems to which members are orienting at the time they are being recorded. For this reason, Liddicoat joins Firth and Wagner in calling for naturalistic, ordinary conversational data.

I accept that it would be desirable for SLA researchers to have access to such data in order to study naturalistic, non instructed SLA processes, but I believe that a broader lesson should be drawn than either Firth and Wagner or Liddicoat envision. It is necessary to understand how speech exchange systems differ from each other. For

this reason, a model of interactional competence needs to be developed that distinguishes between the conversational practices to which members orient in different speech exchange systems. This is the subject of Part II of this book. For now, however, I address the more immediate implications of Heritage's third point.

As even a cursory inspection of the excerpts displayed in this and the preceding chapter demonstrates, CA transcripts (see Excerpts 2.1–2.3) are more detailed than those normally found in the SLA literature (see Excerpts 1.1–1.4). The wide spread adoption of such fine-grained transcriptions would enable SLA researchers interested in understanding the effects of conversational repairs on language learning to investigate whether the moment-by-moment sequential organization of such talk has any direct and observable acquisitional consequences. In other words, fine-grained transcripts may potentially allow SLA researchers to show empirically that learning occurs or does not occur as a direct result of learners first getting comprehended input and later producing comprehended output (Markee, 1994). As I noted in this article, SLA researchers have shown little interest to date in pursuing such a line of enquiry despite the fact that this line of research would be consistent with the research program on the function of comprehensible input as a resource for SLA proposed by Long (1985a) and Pica (1987).

Finally, I develop the idea that the use of sampling procedures should be avoided because such techniques are likely to exclude vital details from the analysis. For example, if lines 93–136 of the data set reproduced in Excerpt 2.5 appeared in a 5-minute sample of talk taken from a 50-minute lesson, it might be concluded, on the basis of the co-constructed talk at lines 135–136, that L15 had understood the meaning of the phrase "we cannot get by Auschwitz."[18]

Excerpt 2.5

```
093 L15:    excuse me ((L7's name)) do you understand what's this (+) we cannot get
094         by auschwit[ʃ] ((reading)) I don't understand what we can't get by (1.5)
095 L7:     oh <hh> (+) uh we-
096 L15:    we can't get by I'm not su- I don't understand what is the meaning
097 L7:     we have every reason to be afraid of ((unintelligible)) ((L7 is reading; her
098         turn trails off into an unintelligible mutter))
099 L15:    we cannot get by (+) what's the meaning ((L15's turn overlaps the end of
100         L7's turn))
101 L7:     (+) what is auschwitz (+) it is a::
102 L15:    I think it's a place because::
103 L7:     ((unintelligible)) a ((unintelligible)) right,
104 L15:    yeah I guess that (+) I already understand that
105 L7:     //concentra//tion camp,
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106	L15:	//((unintelligible))//
107		(+)
108	L7:	is that the one
109	L15:	yeah it cannot get by what is this
110	L7:	we cannot get by ((reading)) <h> I think (+) you cannot s:kip like you
111		cannot (+) you have to go through it, (+) you cannot get by (+) like (+)
112		you have to go through it, say (+) we cannot get by <hh> 'cause
113		(1)
114	L15:	yes I understand
115		//you (+) why he would <hh> because uh (++) the reason why//
116	L7:	//have every reason to be afraid of ((unintelligible))//
117	L15:	he doesn't want t- a united germany is //be//cause that
118	L7:	//oh//
119	L7:	because (+) you know the concentration camp, (+) then hitler he he tried
120		to kill (+) in nazi germany,
121	L15:	yeah?
122	L7:	ok (+) and the jewish (+) //right,//
123	L15:	//yeah?//
124	L7:	ok (+) so (+) say (+) he: he said we cannot get by that we- we never will
125		forget about this (+) //to the end//
126	L15:	//he says it's// (+) history:
127	L7:	uh huh see, (+) //says this is really terrible//
128	L15:	//says it's-it's like-// he doesn't wa:nt to: (+)
129		//a u//nited germany
130	L7:	//I mean//
131	L15:	* like=
132	L7:	* =I guess (+) he says (+) this is terrible
133	L15:	* oh=
134	L7:	* =a lot (+) they killed a lot of people right,
135	L15:	* uh=
136	L7:	* =we cannot get (+) by (+) auschwitz <hh> this means that <hh> he=
137	L15:	=we can't forget we can't forget

(NM: Class 2, Phase 2, Group 2).

However, as shown in the complete collection of Auschwitz-related talk reproduced in appendix C, L15 participates in a total of eight lengthy episodes of such talk. Thus, the exchange shown in Excerpt 2.5, which occurs during the sixth episode shown in appendix C, represents a tiny proportion of the total work that L15 actually does during a 50-minute lesson to understand the meaning of the phrase "we cannot get by Auschwitz." As I argue in chapter 8, there is good reason to believe that L15 never understood what this phrase meant in the context of a debate on German reunification despite all the work that she did to understand this phrase. Thus, a reliance on the kinds of sampling procedures that are so often used in SLA work on L2 interactions would have led to important errors of interpretation because such an analysis would have been based on an incomplete picture of participants' behaviors.

2.5.   CONCLUSION

I do not propose that CA holds the key to formulating yet another theory of SLA. I do claim that CA can help refine insights into how the structure of conversation can be used by learners as a means of getting comprehended input and producing comprehended output. CA is therefore in a position to contribute to research on two of the modules in Gass' (1997) model of SLA (see Fig. 1.1 in chapter 1).

More specifically, CA-oriented research in SLA would link up rather directly with the work of Hatch (1978) and later social interactionists such as Gass and Varonis (1985a, 1985b, 1989, 1994), Long (1980, 1981, 1983b, 1983c, 1989), Long and Porter (1985), Pica and Doughty (1985), Pica et al. (1986), Pica, Holliday, Lewis, and Morgenthaler (1989), and Plough and Gass (1993). Given Hatch's own caution in specifying the role of conversational modifications in SLA, it is perhaps ironic that the utility of conversationally modified input as an important resource for the acquisition of L2 syntax by adults rapidly became a theoretical given in the SLA literature. The research that was done to test this hypothesis was mostly experimental and unfortunately largely circumvented any significant body of prior qualitative work on how learners use conversational modifications and whether such modifications can be shown to result in learning a second language. CA-oriented SLA work could fill this gap. However, in filling this gap, this kind of research would now take on an epistemologically unusual or, as some will undoubtedly argue, unwarranted, hypothesis-confirming character rather than fulfilling the hypothesis-generating role traditionally assigned to qualitative research by experimentalists.

My own position on these issues is as follows. First, I am not particularly concerned with committing epistemological heresy if this yields interesting insights into the role of conversation as a resource for SLA. In this regard, I strongly believe that there is still much to be learned from further qualitative work in this area. Some 20 years after Hatch initially formulated the discourse hypothesis, detailed analyses of how SLA processes are instantiated in the moment-by-moment talk-in-interaction of adult L2 learners are still exceedingly rare.

Second, although many of the results of experimental researchers on the function of conversationally modified input as a resource for the acquisition of syntax by adult L2 learners are likely to be quite robust, I nonetheless prefer to use a CA methodology in order to return to some of the ideas that Hatch sketched out in 1978. In particular, I would like to further explore how adults learn to deploy new vocabulary (and indeed syntax) by doing conversation.

In part, this interest is prompted by methodological concerns. It is undoubtedly far easier to demonstrate how participants achieve the acquisition (or, indeed, the non acquisition) of L2 vocabulary in real time than it is to show how they construct new syntax. However, it would be a mistake to maintain that CA has nothing to say about how learners pragmatically construct syntax from talk-in-interaction. As I show in chapters 7 and 8, even when learners focus on vocabulary, they inevitably have to pay attention to, and also deploy, a broad range of semantic and syntactic resources in order to understand and learn the meaning of unknown words or phrases.

What, then, would a CA-oriented methodology for a social interactionist approach to SLA studies look like? I propose that such a methodology should be:

- based on empirically motivated, emic accounts of members' interactional competence in different speech exchange systems;
- based on collections of relevant data that are excerpts of complete transcriptions of communicative events;
- capable of exploiting the analytical potential of fine-grained transcripts;
- capable of identifying both successful and unsuccessful learning behaviors, at least in the short term;
- capable of showing how meaning is constructed as a socially distributed phenomenon, thereby critiquing and recasting cognitive notions of comprehension and learning.

Chapter 3 provides a practical review of how CA researchers set about doing conversation analyses of talk-in-interaction. The remainder of the book demonstrates how such an ethnomethodologically respecified SLA methodology works and the kinds of insights it can provide into the processes of L2 learning. More specifically, Part II lays out the theoretical ground by sketching out what CA has to say about the construct of interactional competence (bullet 1). Part III analyzes two collections of definition talk. The first analysis instantiates a case of successful learning behavior, whereas the other, which uses identical methodological procedures that are applied to similar, but more complex data, illustrates a case of unsuccessful learning behavior (bullets 2–4). This analysis therefore not only problematizes mainstream SLA's traditional understanding of comprehension and learning as exclusively cognitive constructs (bullet 5) but also suggests how such notions might be reanalyzed more fruitfully in socially situated, behavioral terms.

NOTES

1. CA is also the intellectual heir of the work of the structuralist linguist Zelig Harris (1951), who pioneered research into the discourse level of language.

2. Ethnographic evidence may certainly draw a conversation analyst's attention to the existence of an interesting phenomenon. For example, Hawkins (1985) established through retrospective talking-aloud protocols that, although NNSs' answers to NSs' questions might be conversationally appropriate, this did not mean that the NNSs had necessarily understood what the NSs had said to them. In Markee (1994), I drew on Hawkins' insight to establish that one participant had not understood talk addressed to her at particular moments in the interaction. However, I did so by using only the real-time conversational evidence that speakers displayed to each other (and, therefore, to the analyst) as they constructed their conversation.

3. See appendix A for the conventions used to transcribe this and other CA excerpts in this book. Excerpt 2.1 is the fifth excerpt in a collection of eight excerpts that are thematically related to L10's attempts to understand and learn the word *coral*. This collection is reproduced in full in appendix B and constitutes the complete database for the analysis of successful learning discussed in chapter 7.

4. In all transcripts made from my own recordings, I use abbreviations such as L9 (Learner 9) or T (Teacher) as a means of keeping the participants' identity confidential. Note that this convention is a matter of convenience and in no way implies that other social roles that participants might construct in and through their talk are of no consequence to the analyst (for discussion of these issues, see Firth & Wagner, 1997).

5. This does not mean that CA researchers never report regularities in behavior (see Heritage & Greatbatch, 1986; Jefferson, 1988) nor that they always eschew simple statistical data such as percentages (see Heritage & Roth, 1995; Markee, 1995). Furthermore, as Schegloff (1993) was at pains to point out, neither does it mean that CA researchers should not follow up their initial studies with experimental research (see Wilson & Zimmerman, 1986, for an example of such a follow-up study). But experimental research should only be carried out once a thorough qualitative understanding of the issues has been achieved.

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6. See also Long (1983a) for an earlier critique of Mehan's (1978) work on constitutive ethnography.

7. Indeed, Susan Gass (personal communication, November 30, 1998) seems to accept this argument, noting that "my reading of K[asper] and L[ong] (and my own 1998 rebuttal [of Firth & Wagner]) is not so much a rejection of CA, but a rebuttal to F&W's proclamation about what SLA should be."

8. Thus, as Hatch (1978) pointed out, it might be worth while to take seriously adult learners' insistence that vocabulary (rather than grammatical) learning is a primary goal of SLA.

9. See also the exchange between Firth and Wagner (1997) and Long (1997) on the issue of CA's relationship to rationalist approaches to doing science.

10. In developing this critique of mainstream SLA studies, I am not advocating that SLA researchers should stop doing experimental research. This would be akin to throwing the proverbial baby out with the bath water. Experimental researchers have made many crucial contributions to SLA studies and will doubtless continue to do so. The issue here is that, contrary to Gregg et al. (1997), there is more than one way of doing good science.

11. It is interesting to note that experimental research is in one sense uninterested in the acquisitional *consequences* of repairs. More specifically, no evidence was presented by Porter (1986) that the learners in Excerpts 2.2 and 2.3 actually understood, much less learned, what the words "sink" and "research" mean.

12. During these experiments, Garfinkel instructed his students to repair all possible ambiguities that occurred in conversation. These experiments had such disastrous social consequences for the experi-menters that they had to be discontinued.

13. Long also claimed that quantified data are more useful than qualitatively described data in advancing the theoretical agenda of SLA studies. I believe the issue is not whether one type of research is more useful than another, so much as what each approach can contribute to the SLA debate. Some questions are better addressed via experimental studies, whereas others are more suited to a qualitative treatment.

14. See also Hopper (1988), Hopper, Koch, and Mandelbaum (1988), Jefferson and Schenkein (1978), Levinson (1983, pp. 318–325), McLaughlin (1988), and Zimmerman (1987) for other comprehensive accounts of CA. For a summary of the transcription conventions widely used by conversation analysts, see Atkinson and Heritage (1984b).

15. Of course, the distinction between a laboratory and a naturalistic setting is not at all clear-cut because the observer's paradox may be a factor in the way in which participants behave in so-called naturalistic settings. Apart from this issue, however, if data collection involves the use of subjects who do not know each other, occurs in an unfamiliar location, and is designed to test performance on completely unfamiliar tasks, then the research location will likely be constructed by participants as a laboratory situation (Liddicoat, 1997). Conversationally, this phenomenon will manifest itself as some variety of talk that is not ordinary conversation, which is clearly problematic if one is trying to study ordinary conversation. Conversely, if data collection involves the use of participants who already know each other, occurs in a familiar place, and entails members engaging in familiar tasks, then the research location will likely be constructed naturalistically by participants; that is, they would be expected to construct their relationships through the practices of ordinary conversation.

However, even if an attempt is made to distinguish between naturalistic and laboratory settings in this way, there are still plenty of data collection situations that involve a certain amount of ambiguity. For example, participants may know each other and may perform tasks with which they are already familiar, but be recorded in an unfamiliar setting (e.g., a video studio), which may have an adverse impact on the naturalness of the ensuing talk. Thus, the notions of naturalistic and laboratory settings probably represent the ends of a naturalness continuum, which merge into each other in infinite shades of grey.

16. See, Goodwin (1979), who shows how one conversationalist's interactive construction of a sentence simultaneously constructs two of his interlocutors as friends and the remaining participant as his wife.

17. In this context, see also Gass' (1998) rebuttal of these criticisms.

18. This excerpt was recorded in 1990. The learners are discussing the then current issue of whether East and West Germany should reunite,

using information from an article by the left-wing West German writer Günter Grass. In this article, Grass argued that Germany's Nazi past, symbolized by the concentration camp Auschwitz, precluded reunification.

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