

## **Category and Rule in Conversation Analysis**

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### **1. Introduction**

The development of a new analytical model (not merely a new technique or topic) is a noteworthy event in the social sciences. The field of conversation analysis (CA) has brought forth, in still rudimentary form, a new analytical model. The various analytical concepts of CA have been well described (e.g., Heritage 1984; Levinson 1983). Recently there have been some attempts to clarify and develop these concepts (Bilmes 1985, 1986, 1988; Coulter 1983; Schegloff n.d.). This paper is an attempt to broaden and extend this reflective trend by placing CA within the context of social scientific theory.

The methodological foundations of CA are the study of recordings of natural conversation and an attention to how members of society achieve the ordinary doings and appearances of everyday life. In the conversation analysts' attention to the ordinary, in their emphasis on the interactional rather than the purely linguistic aspects of conversation, and in various other aspects of their approach, some of which will be described later, they were inspired by the work of Harold Garfinkel and Erving Goffman. Although neither Garfinkel nor Goffman did detailed analysis of recorded, naturally occurring interaction, that too had its precedents (e.g., Pittenger, Hockett, and Danehy 1960). What Harvey Sacks, Emanuel Schegloff, and Gail Jefferson added was not only their general interest in conversation but a demonstration of the pervasiveness of structure and, most significantly, an original understanding of structure itself. It is this last matter that is the focus of this paper.

## 2. Structure and Function

Two of the major model types--what I will call approaches-- used in social science generally, and more specifically for the analysis of linguistic phenomena, are the structural and the functional. While CA's innovations are primarily in its treatment of structure, it has significant functionalist elements. Since CA's functionalist aspects will be referred to later in this discussion, a very brief characterization of the functional approach may be in order.

The functional approach involves an analysis of society in terms of problems and coping mechanisms. Traditional anthropological functionalism typically started from some native institution or cultural practice, assumed that it was a solution to some problem, and then sought to discover that problem or "functional need" (e.g., Radcliffe-Brown's [1940] analysis of mother-in-law avoidance and joking relationships or Malinowski's [1948] of magic). The inverse procedure begins with a problem, asking whether and how society copes with it. All societies, for example, must make some provision for the maintenance of cohesion and solidarity. And particular societies, because of their specific physical and social environments or because of the specific ways that they are organized, have their own unique problems. There is perhaps something arbitrary about looking at social organization entirely in terms of problems and coping mechanisms, but it is a fruitful analytic strategy.

My main concern in this paper will be with structural models. Structural analysis explains the nature of entities, their meaning, their orderliness, their properties within a system. Entities are defined by their patterning in relation to one another. The definition of these entities, in turn, leads us to a new understanding of the ways in which they are patterned. The place of a particular sound (phone) in a particular language depends on its distribution relative to other phones. The meaning of a word and even its identification as a part of speech (noun, verb, etc.) depend on its place in the sentence and in the lexicon. The meaning of a three-second silence depends on its place in the conversation. The meaning of an event in a myth depends on what other events it contrasts with. Structural models are characterized by reflexivity. The

elements in a structure are mutually defining. If element *x* is defined by its relationship to elements *y* and *z*, it is equally true that *y* is defined by its relationship to *x* and *z*, *z* by its relationship to *x* and *y*. Each element is defined by elements that it (in part) defines. For present purposes, we can characterize structural analysis as the study of the relationship between pattern and meaning. Thus, the study of the structure of molecules would not constitute structural analysis in the sense in which I use the term here.

Another crucial aspect of most structural analyses is that they deal with ideal objects.<sup>1</sup> The structural approach does not necessarily attempt to predict what will be done. Rather, it says something about the status of what has been or may be done by comparing it with an ideal. For example, it characterizes an utterance as grammatical or ungrammatical.

A full structural analysis consists of at least the determination of native categories and a set of statements about the organization of those categories. The categories are native in the sense that they are derived from distinctions that natives actually make. There is no requirement that members be explicitly aware that they use such categories or make such distinctions.<sup>2</sup> Pre-

<sup>1</sup>I mean to contrast *ideal* with *ill-formed* and *imperfect*, as well as with *actual*.

<sup>2</sup>This is emic analysis. It seems necessary to use emic categories if one is to describe the organization of behavior within a cultural group. The analyst who chooses to use etic categories (created without reference to the culture under study) can still get valid results if (1) the categories are based on universal distinctions or at least distinctions that happen to be made in the culture under study, or (2) the analyst is attempting to arrive at statements not about the organization of native behavior but rather about possible outcomes. One can, for example, talk about the ecological consequences of destroying a certain type of plant by systematic burning of underbrush, even if the natives do not separately categorize that plant, even if they are not aware of its existence. One might go even further and connect these consequences to, say, native patterns

Chomskyan structural linguistics followed this "item and arrangement" formula fairly straightforwardly. Chomsky (1965) found that the structure of sentences could not be adequately described in this way. Instead he used a set of "phrase structure rules" to produce what he came to call *deep structure*. The more semantically equivocal surface structure was then generated by transformations on the deep structure.

Levi-Straussian "structuralist" analysis also posits a kind of deep structure (Levi-Strauss 1963). Levi-Strauss used two vivid metaphors to describe and justify his interest in an underlying structure. The diverse patterns of the kaleidoscope are created by a few bits of colored glass and a set of mirrors. To describe the kaleidoscope, we must focus not on the patterns that it can produce but on the mirrors and bits of glass. Again, the pieces produced by a jigsaw may reveal no comprehensible uniformities of shape. For understanding, we must look to the structure of the cams that control the jigsaw. But Levi-Strauss' actual analyses are quite limited. Using largely *ad hoc* procedures, he is able to find underlying structures in various cultural phenomena, such as myths. The structures that he discovers are invariably binary oppositions, sometimes mediated by a third term. But, because he has no formal theory of the transformations involved, he cannot work in the opposite direction, from underlying structure to surface phenomenon. He cannot use the binary oppositions to create "proper" myths. Instead, he must attempt to validate his analysis by finding the same underlying structure in other myths or other cultural forms. Once again, though, he must use *ad hoc* procedures, and the demonstration is far from rigorous.<sup>3</sup>

At any rate, we have an initial distinction between structural analyses which are performed directly on the phenomena themselves as against those

of migration.

<sup>3</sup>It should also be noted that Levi-Strauss is concerned almost exclusively with categories. His analytic system is sorely lacking in resources for dealing with sequential arrangements (Bilmes 1982).

which rely on notions of deep structure and transformations. There is a second, more troublesome distinction between those structural analyses that are based on rules and those that are not. I say "troublesome" because it is sometimes the case that two analyses, one relying on explicit rule statements, the other not, are isomorphic, so that one is essentially the equivalent of the other. An old style structural syntactic analysis, for example, might consist of descriptions of patterns, but, insofar as the patterns were not merely statistical, rules were at least implicit in the descriptions. Chomsky, in his phrase structure grammar, restated these descriptions as rewrite rules. On the other hand, it is difficult to imagine any set of rule statements that might substitute, without gross sacrifices in economy, for the network of connections in a stratificational grammar (Lamb 1966). For a relatively well defined phenomenon like language, it may be that rule and network descriptions are both plausible. For conversation, which is much more loosely constrained, I suspect that only a rule-based description will serve.

We have got to expect, though, that the model for conversation analysis will be fundamentally different in nature from not only Lamb's but Chomsky's model as well. Both Chomsky and Lamb conceive of a grammar as a way of translating between meaning and sound. That is, the job of grammar is to take something which exists in one form and to process it into another form. There is no parallel to this in CA. Conversations are emergent. A specific conversation is a new thing that did not exist in any form prior to its being produced as a social performance. This difference alone is enough to guarantee that the grammatical systems espoused by Chomsky and Lamb cannot serve as models for the analysis of conversation.<sup>4</sup>

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<sup>4</sup>This is not necessarily to say that Chomsky's and Lamb's grammatical systems translate between meaning and sound, only that they are thought by Chomsky and Lamb to do so. This aspect of the Chomsky-Lamb view of language rests on the questionable notion that there is some independent realm of pure, nonlinguistic meaning upon which grammar operates (see Harmon 1968).

### 3. Categories

A crucial part of linguistic structural analysis is the determination of analytical categories. These categories, for example, phonemes, are developed from data on the specific language under study and are based on distinctions that matter to the native. (I refer here to traditional structural phonology. It is dangerous at best to attempt to characterize the status of the concept of phoneme in generative phonology.) This is “emic” analysis. CA has not done much to develop an emic system of analysis (particularly of speech acts) appropriate to its needs, and this is perhaps one of its major weaknesses. It has, however, come up with some important innovations in its approach to categorization which, in certain respects, take it beyond emic analysis.

Let us first consider the way that CA handles categories. Some of the ideas presented by conversation analysts are simply imaginative extensions of traditional structural analysis. So, for example, Sacks (originally in his lectures and then in Sacks and Schegloff 1979) makes a distinction between type I and type II references. A type I reference (e.g., a personal name) tells the hearer that he<sup>5</sup> is to try to identify the person referred to. A type II reference (e.g., “someone”) tells the hearer that he is not to try to identify the person referred to. Sacks makes the case that this is an emic distinction (although he does not use the term “emic”), that is, a distinction that natives use to organize their behavior. (But see the critique of Sacks’ argument in Bilmes, 1988, especially footnote 12.)

Another of Sacks’ discussions is closely related to linguistic pragmatic analyses of discourse cohesion. Sacks (1972) points out that when we hear “The baby cried. The mommy picked it up,” we assume that the mommy is the mother of the baby. This phenomenon is very similar to anaphora. Indeed, with

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<sup>5</sup>I am following here the convention, suggested by Dorothy Smith (1978), that the form of the unmarked pronoun be determined by the gender of the author.

their treatments of anaphora and deixis in general, linguists have taken a step beyond traditional *emic* analysis into what we may call *extended emic analysis*. That is, they have broadened their analyses to include considerations of context. The meaning of, say, a pronoun is derived in part from the immediate context of use rather than from general relations in the language. Sociolinguists have added a concern for social relations (Hymes 1974), a factor which is handled in CA through the notion of recipient design.<sup>6</sup>

Linguistic pragmatics extends the reach of linguistic theory by including context.<sup>7</sup> But, for CA, this is still not enough. Beyond context, there is interaction. The position of the conversation analysts is explained in several of

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<sup>6</sup>Hymes (1962) offers the following formulation of the relation between a linguistic form and its context: "The use of a linguistic form identifies a range of meanings. A context can support a range of meanings. When a form is used in a context, it eliminates the meanings possible to that context other than those that form can signal: the context eliminates from consideration the meanings possible to the form other than those that context can support" (19). This statement brings us to, but not across, the borders of the ethnomethodological and conversation analytic positions on meaning. The CA treatment is described in this paper. For a description of the ethnomethodological view, see Wieder (1970). The distinction between a CA and an ethnomethodological perspective may be confusing to those who are used to regarding CA as a kind of ethnomethodology, and there may in fact be strong similarities between the Sacks-Schegloff treatment of meaning and that of Garfinkel, but there are, at the least, clear differences of emphasis.

<sup>7</sup>I am using "context" in a somewhat restricted sense. For present purposes, "context" includes relevant matters of fact, knowledge, belief, and so forth which are in existence at the moment when the utterance in question is produced. It does not include what occurs subsequent to the utterance. As far as sequential considerations are concerned, the context of an utterance consists of *previous* utterances in the sequence, but not subsequent ones. This use of the term "context" diverges from the usage of most conversation analysts but is convenient for the purpose at hand.

their critiques of the field of linguistic pragmatics most directly relevant to CA--speech act theory (Levinson 1983, Schegloff 1978, Streeck 1980, Turner 1970). I offer here my own brief critique, one which is closely fitted to the subject at hand.

Speech act theorists have focussed on the question of how speech acts are constituted and recognized. What, for example, constitutes a certain utterance as a promise? The assumption is that, if we know the relevant context, we will be able to achieve an unproblematic recognition of the utterance as a speech act. To put it another way, there is an assumption that an utterance *is*, once and for all, a particular speech act from the moment of its production. In Searle's (1969) analysis, speech acts are based largely on intention. Whether, for example, an utterance is a promise depends crucially on what the speaker intends. The act is moored to the psychological states of the speaker. This gives the speech act a reality that, at least in certain ways, transcends the local occasion and contingencies of interaction. Even if others heard the utterance as a promise, and if the speaker himself acknowledges that it was a promise, still it was not a promise unless the speaker had the appropriate intent when he produced the utterance. Instead of being a consensual or negotiated phenomenon, Searle invests the speech act with a kind of objectivity.

This is, of course, the way we see it in everyday interaction--we cannot negotiate about what something *is* if we take the position that the thing's very nature is defined in negotiation--but as a sociological stance it is, I think, unsatisfactory (Bilmes 1986). One problem is that the analyst will not--indeed, cannot--have the necessary information to achieve a certain recognition of the character of the utterance as a speech act. How is one to know, for example, when you say "I'll be there tomorrow" whether you intend to put yourself under an obligation to be there? Moreover, simply because the speaker has no pre-existing intention to obligate himself does not mean that he must of necessity intend not to obligate himself. He may not have considered the matter. Indeed, for analytical purposes, it is best to think of intention not as something preceding



or occurring simultaneously with the utterance, thus making it a speech act, but as something that is added on to it by participants as necessary in the course of interaction. If the question of whether the utterance was a promise becomes salient for the participants, then the question of the speaker's intentions may also be raised.

An utterance is not a speech act upon its production but only as a result of the constituting work of persons in interaction. What kind of speech act the utterance will turn out to be (and, in the participants' minds, to have been all along) is contingent on subsequent interaction. Searle supposes that the "conditions," such as appropriate intent on the part of the speaker, provide the grounds for the speech act, which is what it is only because the conditions are in effect. But it is the attempt to interpret an utterance as a particular speech act that makes the conditions possibly relevant, and it may be only on the occurrence of such an attempt that the speaker considers whether he intends to obligate himself, whether he believes that his addressee would prefer his doing the predicated action to his not doing it, and so forth.

The analyst cannot classify the utterance as a certain type of speech act, and cannot know what it entails, simply by inspecting it in its context. We might go so far as to say that at least some of Searle's conditions do not provide a pre-existing context and grounding for speech acts, but rather the reverse is true. It is the identification of the act, or the attempt to identify it, that invokes and entails the felicity conditions.<sup>8</sup> I may refute your interpretation of my speech act by denying that some felicity condition is in effect. Participants do recognitions of speech acts, but these recognitions are defeasible. Analysts'

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<sup>8</sup>That the speech act may be, in a sense, precedent to its "conditions" is suggested (inadvertently, I suppose) by Searle himself. "In the performance of any illocutionary act, the speaker implies that the preparatory conditions of the act are satisfied. Thus, for example...when I make a promise, I imply that the thing promised is in the hearer's interest" (65). That is, an utterance may be recognized as a promise, and as a result of that recognition (rather than as a condition for such recognition), it may be deduced that the speaker believes that what he is promising is desired by the addressee.

recognitions are similarly defeasible in principle, unless they wait to see what participant recognitions have come to. Fortunately, social scientists are not required, in their professional capacity, to negotiate speech acts, only to observe participants' negotiations.<sup>9</sup>

CA has recognized an interactive and negotiated aspect to categorization, and further recognized that the social use of any category renders it subject to such negotiation. Thus, the categories used by interactants are not merely emic and contextual, they are interactionally defined. An example will make this clearer. In the following fragment, from Schegloff (1978), A is a radio talk show host and B is a caller discussing his history teacher's views:

1. B: He says, governments, an' you know he keeps- he feels about governments, they sh- the thing that they sh'd do is what's right or wrong
2. A: For whom
3. B: Well he says- //he
4. A: By what standard
5. B: That's what- that's exactly what I mean.

B takes "For whom" to be a question and begins to answer it. A interrupts with another utterance in question format and, in #5, B achieves a rehearing of #2 and #4 as not questions after all. In the emic and the extended emic system, i.e., in terms of the language and of the existing context, "For whom" is clearly a question, but, as a result of the interactive work on this occasion, it turns out not to have been a question this time.<sup>10</sup> So, we can see that even when what is

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<sup>9</sup>As this discussion suggests, I find a role for a variant of speech act theory within CA (see footnote 10)

<sup>10</sup>As an example of how speech act theory can inform rather than taint a CA analysis, consider how it can be applied to Schegloff's transcript. One of the felicity conditions for a question is that the speaker wants the information

apparently a question actually goes through as a question, this is an occasioned event--it could have been otherwise.<sup>11</sup>

In connection with CA's treatment of categorization, we should note, first of all, the use of the notion of "appreciation" or "hearing." Any response to an utterance provides evidence of how the utterance was interpreted by the recipient. When B, in the above exchange, begins to offer in #3 what is apparently an answer to #2, A realizes that B has taken #2 to be a question. Thus, as a conversationalist, you are able to monitor not only what the other says but what the other understands you to have said. I can think of no notion analogous to "hearing" in structural linguistics. Nevertheless, the CA model is deeply structural in the following sense: the items (in this case speech acts, turns, utterances, pauses, etc.) in the "system" are not to be identified (by the analyst) in isolation, according to some set of objective criteria. Rather, they are mutually defining; their identity is to be found in their relationships.<sup>12</sup> The

requested. When B starts to provide that information, A interrupts, in effect denying that the felicity condition is fulfilled, i.e., he demonstrates that he does not want the information that B thought he was requesting. B, seeing that #2 was not after all a question, seeks and finds another interpretation. The felicity condition has been used here as a device to provide for the defeasibility of an offered interpretation.

<sup>11</sup>See also Goodwin (1981:18-19) on the problems attendant on trying to make fixed categorical classifications of conversational items at the moment of their production. He points out, for example, that what might seem to be a gap following a turn may be revealed at some later point to have been a within-turn pause.

<sup>12</sup>Coulter (1983) rightly argues that adjacency pairs, such as question-answer, are logical, *a priori* structures, discovered empirically but not dependent on statistical confirmation. "You cannot specify the concept of a 'question' independently of any reference to the concept of an 'answer'" (365). The "first pair-part...is given its meaning only by reference to its appropriate

distinctive feature of the CA treatment of categories is that the items are not defined solely by their relations in an abstract emic system, nor even by their relation to an already existing context, but in the local occasions of interaction in which the items are employed. CA thus extends its analysis beyond emic and beyond "meaning by rule" (Wieder 1970).

With this change in our understanding, we must once again reconsider our term for the locus of social action, the person as a functioning part of a social group. Parsons used the term "actor," by which he referred to a socialized, motivated member of a normatively organized social group. Anthropologists use the term "native," ethnomethodologists "member," emphasizing the cultural aspects of the person, his competence in the ways of his group. For ethnomethodologists in particular, the member is, above all, a master of natural language. The previous discussion suggests yet another term for the person as a locus of social action--"participant." This term emphasizes the situated aspects of social action. It recognizes that a construction of what is going on and what response may be appropriate, or at least construable, is something that must, to a certain extent, be worked out among those persons, in that place, on that occasion.

This is not to say that CA has somehow transcended reliance on emic categories. The CA literature is full of unanalyzed recognitions of questions, invitations, agreements, and so forth. As I have already suggested, it might be argued that one of the major weaknesses of CA is its *ad hoc*, commonsense categorization of speech acts. CA needs to rethink speech acts in accordance with its own objectives. No doubt a set of categories based on native distinctions is needed, but this does not necessarily have to be the somewhat opaquely organized set of named categories by which members identify their speech acts to one another. But whatever system of categorization is used, we must remain aware that the analysis of any particular utterance remains

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second pair-part" (365). There is an interesting convergence here of Wittgensteinian logical grammar, of which Coulter is a practitioner, and Saussurean structuralism.

contingent on the participants' negotiations. We may say that a certain utterance is possibly a question, i.e., it could be treated that way, but the participants have the final authority regarding how it is to be understood and may find a way of treating it that never occurred to the analyst.

#### 4. Rules for Ideal Structures

In discussing rules, I will once again compare CA to linguistic models. To begin with, CA does not, as does transformational grammar, rely on a concept of deep structure or on transformational rules. There is, however, an even more basic difference, one that differentiates CA from traditional structural models as well as from transformational models. The linguist's concern is with grammaticality. If you follow the rules, you get a well-formed sentence, and insofar as you can recognize well-formed sentences, that recognition constitutes a test of a proposed system of rules. The rules of grammar are rules in Winch's sense of the term: "The test of whether a man's actions are the application of a rule is not whether he can *formulate* it but whether it makes sense to distinguish between a right and a wrong way of doing things in connection with what he does" (1958:58). Grammatical rules relate to this criterion in a straightforward way. If you follow the rules, you have done the thing in the right way; otherwise, you have not. Although conversation may also be said to meet Winch's criterion as a rule-based activity, it does so in a more oblique way, as we shall see.

A first difference between the way rules are handled in linguistics and CA follows from the difference in the way categories are handled. In linguistics, the rule *y follows x* is more determinative than it is in CA. This is because the categorization of linguistic tokens is (at least as far as the analyst is concerned) not negotiable. In any specific instance, the analyst knows whether or not the rule applies because he knows whether this thing is an *x*. He also knows whether the rule has been obeyed because he can tell whether that is a *y*. In CA, whether that thing is an *x* and that a *y* is for the participants to decide. The conversation analyst does not (in theory at least) have the same authority as the

linguist in deciding either whether the rule applies or whether it has been followed.

Rules in CA also operate differently than they do in linguistics. It may be worthwhile to put this matter in a broader context. Chomsky's attack on traditional structuralism and particularly on American descriptivism was epistemological as well as technical. He effectively destroyed the descriptivists' positivist-behaviorist pretensions with one simple observation. He noted that a very great deal of what natives actually say is not usable as data in constructing a grammar. We do not, for example, want a grammar that generates half-sentences, even though natives are frequently heard to produce half-sentences. This means that the linguist is not simply taking a corpus of recorded data and building a theory that accounts for its structure. Rather, he accounts only for grammatical utterances, grammaticality being determined by native judgments. There is more consistency of patterning in ideal sentences than in actual utterances. (Goodwin [1981: Chapter 2] and Schegloff [1979] point out, though, that there is a great deal more patterning in actual, "ungrammatical" utterances than linguists have recognized.)

A structural study of myth, on the other hand, may be more strictly tied to a corpus of empirical data. Levi-Strauss' theory of myth presumably applies to all myths, i.e., there is no such thing as an incorrect, "ungrammatical" myth. The reason for this is that a myth is already a kind of ideal object; it is a cultural product, refined and perfected over time. Any mistakes, any structural errors in the original version will have long since been ironed out. (The same argument applies to fairy tales--see Hammel 1972, Bilmes 1982, on *The Three Bears*.) Of course, it may be argued that Levi-Strauss is not in fact dealing with a corpus of observations in that he does not deal with recordings of actual myth-tellings. Any specific telling might have imperfections. According to this argument, Levi-Strauss is proceeding pretty much as the linguists do, selecting or synthesizing ideal examples from a compromised body of data. Whatever the merits of this argument, I think it is clear enough that it is possible in principle to do a structural analysis on an actual corpus of data, for example, a book of fairy tales,

if that book consists of already-perfected stories, just as it is possible (again, in principle) to do a syntactic analysis using as a corpus a book consisting of grammatically perfect sentences.

A modern, linguistically-inspired view of culture handles native behavior in general in very much the same way that linguists handle native utterances. "The model of an ethnographic statement is not: 'if a person is confronted with stimulus X, he will do Y,' but: 'if a person is in situation X, performance Y will be judged appropriate by native actors'" (Frake 1969a:124. See also Goodenough 1957). Appropriateness is, in Frake's approach to cultural analysis, what grammaticality is in Chomsky's approach to linguistic analysis. "The test of descriptive adequacy must always refer to informants' interpretations of events, not simply to the occurrence of events" (Frake 1969b:471). The central criterion in both cultural and grammatical analysis is native judgment of correctness.<sup>13</sup>

The linguist's preoccupation with language as an ideal object is, I think, defensible. Speakers are clearly trying to produce some sort of approximation to this ideal, so, to understand one aspect of what speakers are doing, it is necessary to achieve an understanding of the ideal that they are approximating. In certain respects, conversational analysts have done something similar. Sacks, Schegloff, and Jefferson (1974) propose a set of turn-taking rules, rules such as *current speaker may select next speaker, obliging the party selected to speak next* and *speaker change occurs at transition relevance places* (these are very rough and abbreviated paraphrases). These rules constitute an ideal that actual conversations only approximate. Yet CA has taken a different course

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<sup>13</sup>In anthropological sociolinguistics, too, there has been a strong tendency to use *appropriateness* the way linguists use *grammaticality*, and to focus attention on the cultural knowledge and rules underlying appropriate behavior. However, it is perilous to generalize about a field as diverse and broadly defined as sociolinguistics. It might, for example, be noted that Gumperz and Hymes' well-known edited volume, *Directions in Sociolinguistics* (1972), includes a paper by Sacks and one by Schegloff. And CA has certainly influenced recent work in mainstream sociolinguistics, an influence that is very apparent in Gumperz' *Discourse Strategies* (1982).

than linguistics; it has not devoted itself to the elucidation of ideal conversations.

There are reasons, aside from the empirical interests of its practitioners, why CA could not follow the linguistic example and concern itself only with models of ideal conversations. One reason is that natives do not seem to have as definite and articulated and complex a sense of what a well-formed conversation is as they do of what a well-formed sentence is. They are likely to have very imprecise notions of what is proper and improper in conversation, and these notions are derived from a number of disparate sources. Natives tend to have a grasp of what linguists mean by grammaticality and are able to judge whether a sentence is grammatical aside from whether they like or dislike the sentence on other grounds. (Of course, formal schooling may distort our native perceptions of grammar, tainting them with extraneous notions of "correctness.") It seems doubtful, on the other hand, that we could get natives to make consistent judgments of conversations on the basis of a single standard of well-formedness unless we specified in advance what well-formedness consisted of. But that, of course, would defeat the whole purpose of soliciting native judgments.

Another reason for diverging from the linguistic approach is that an ideal model of conversation will be rather rudimentary and unspecific, covering only a few basic parameters. In terms of intricacy and elaboration, one can hardly compare the system rules proposed by Sacks *et al.* with a grammar of a language. The interest of a CA model of conversational behavior lies not in the model itself but in the way it is used in interaction, and this is what CA focusses on.<sup>14</sup> When we look at what interactants do, we see that not only does their

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<sup>14</sup>I think it is at least arguable that conversation analysts have not been primarily interested in the way that the turn-taking model is implemented, in that they have not devoted a great deal of attention to how transition relevance places are identified. Although there is now an extensive literature on this subject (Beattie 1983), most of it has been produced outside of CA. Conversation analysts have been concerned, rather, with the way that participants orient themselves to the rules, invoke them, hold each other



behavior roughly approximate the model but also that they are visibly working toward that end. Instances when the model is violated come to constitute the strongest evidence for its existence. So, we find that when an overlap occurs, interactants take steps to minimize it. Grammarians could use the same strategy, attending to how people correct their own and others' grammar. That the linguist does not employ such a strategy is perhaps indicative of the fact that he takes for granted and finds no need to prove (1) that languages have grammars, and (2) that natives have a definite knowledge of the grammar of their language and can make clear and consistent decisions about what is and is not grammatical. Therefore, the linguist does not have to look to natural speech to discover what is grammatical; he can simply invent constructions and solicit native judgments of grammaticality. (There are, as well, some severe difficulties with an empirical strategy in linguistics. For example, the great majority of grammatical errors go uncorrected. Also, it is not always clear, without presuming a previous knowledge of the grammar that one is studying, what constitutes a correction and whether a possible correction applies to the grammar or the content of the reformulated utterance.)

## 5. Rules in CA

To start a conversation is to invoke the constitutive turn-taking and sequencing rules. To end it is to suspend them. The problems of invoking, maintaining, and suspending conversational organization are structural-functional in nature.<sup>15</sup> They involve "mechanical" operations on the current

accountable to them, understand what is occurring by reference to them, and so forth. The native recognitions (e.g., of speech acts, of transition relevance points) that are necessary in order for the rules to operate have not been the main concern of conversation analysts, except as those recognitions are displayed in interaction.

<sup>15</sup>"Structural-functional" has been used as a descriptor for certain schools of

status of a socially constituted, rule-based system. So, for example, the basic rules governing turn-taking in conversation are: "(1) at least, and no more than, one party speaks at a time in a single conversation; and (2) speaker change recurs" (Schegloff and Sacks 1974:236).<sup>16</sup> When two conversationalists find themselves speaking simultaneously, they generally act to minimize the overlap.

The case of closings is more revealing. (My discussion is based on Schegloff and Sacks 1974. Although my observations on silence go beyond theirs, I believe that I am merely expanding on their line of thought.) We do not ordinarily say of a lone person that he is silent. Silence is a nonoccurrence of sound, and nonoccurrences are not ordinarily notable. It is only when speech is relevant that the absence of speech is notable. Therefore, "doing silence," silence as a social act, observable and mentionable silence is at least generally an interactional phenomenon. A second point: If A and B are conversing, and A is speaking, then B's not-speaking is not registered as B's silence. Generally

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anthropology and sociology. I am not making any such reference. I am merely pointing out that to study conversation in terms of the organizational problems it presents is to take a functionalist approach, but one which, at least for conversation analysts, involves structural determinations of meaning.

<sup>16</sup>That these are rules and not merely observations about what normally happens in conversation is evident from the Schegloff and Sacks' (1974) discussion: "Conversationalists construct conversations in their course, and in doing so they are oriented to achieving the co-occurrence of the features cited above, and employ the turn-taking machinery to do so....If the features are normative, i.e., are oriented to by conversationalists, then the machinery for achieving their co-occurrence should include procedures for dealing with violations, and indeed should locate failure to achieve the features, singly and jointly, as 'violations', as in need of repair" (236). *One speaker and one only* is not merely a "feature" of conversation, although that is what Schegloff and Sacks call it, but, as their discussion shows, it is a rule and failure to achieve it is a violation, albeit a common one.

speaking, one can "do silence" only when one's interlocutors are not talking. Social silence, then, is quite different from objective silence and is located with reference to the turn-taking rules. Schegloff and Sacks make the further point that silences may be attributable. That is, although no one is speaking, the silence may be noted as B's silence. This happens as a result of the operation of adjacency pair organization. So, if A asks B a question, after which there is silence, the silence is B's. But, in the absence of special provisions, any extended silence between turns, whether attributable or not, is notable and untoward, since the turn-taking rules call for at least one speaker.

The problem of closing, then, is "how to organize the simultaneous arrival of the co-conversationalists at a point where one speaker's completion will not occasion another speaker's talk, and that will not be heard as some speaker's silence" (Schegloff and Sacks 1974:237). In other words, how to suspend the relevance of the turn-taking rules. If one were to attempt to close by simply falling silent, this "would be interpretable as an 'event-in-the-conversation', rather than as outside, or marking, its boundaries, and would be analyzed for actions being accomplished in the conversation, for example, anger, brusqueness, pique, etc." (238). The problem, to phrase it yet another way, is: having constituted conversation on this occasion, how do interactants go about "deconstituting" it? This is perhaps a trickier matter than terminating a game of chess, because it must be done "from the inside," using the very conventions which are to be suspended.

It is not necessary to describe here Schegloff and Sacks' analysis of how conversationalists cope with this problem. The functionalist aspect of their approach to rules is well illustrated by the description of the problem itself.<sup>17</sup> This description also illustrates the structural aspect of their approach. Their structural approach to rules is of special interest. From the structural, meaning-constructional point of view, the analyst is less concerned with whether the rules

<sup>17</sup>Levinson (1983) mentions the following as one of "two basic methods" of CA: we must ask "(i) what problems does this organization [of conversation] solve, and (ii) what problems does this organization raise" (319).

are obeyed, or even approximated, than with whether they are "in effect," whether the participants are oriented to the rules. This concern is seen clearly in the treatment of adjacency pairs. "A basic rule of adjacency pair operation is: given the recognizable production of a first pair part, on its first possible completion its speaker should stop and a next speaker should start and produce a second pair part from the pair type the first is recognizably a member of" (Schegloff and Sacks 1974:239). An example of an adjacency pair rule is: *answer questions*. If you do not answer, you are expected to offer instead an account of why you are not answering. A successful account, in a sense, lifts the relevance of the rule. If you offer neither an answer nor an account, you have violated the rule. To understand the status of adjacency pairs in CA, we may begin from Searle's (1969) distinction between constitutive and regulative rules. Constitutive rules, such as the rules of chess or grammar, create a new form of activity; regulative rules regulate an already existing activity.

As soon as we begin to look closely at this distinction between rule types, it becomes muddy. First of all, it is clear that constitutive rules are sometimes used for regulation, as when we hold someone to the rules of a game. Furthermore, whether we can properly call a rule constitutive or regulative depends not only on the nature of the rule but on what we choose to call the activity that we are engaged in. A rule against using rough language may be wholly regulative in relation to "conversation," but it is constitutive in relation to "polite conversation."

Levinson (1983) observes that "some of the rules formulated in CA, e.g., the turn-taking rules..., are as much **regulative** as **constitutive**" (367). Adjacency pair organization seems to be constitutive of conversation in that, if we completely ignore it, we can hardly be said to be holding a conversation. It is also regulative--that is, it has normative force--in that, if, for example, our question goes unanswered, we can hold the addressee to account: "Hey, I asked you a question." Rather than calling adjacency pair rules constitutive or regulative, we might do better to speak of the constitutive and regulative aspects of adjacency pair rules.<sup>18</sup> We will need a further distinction, however, to get at

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the aspect of adjacency pair organization that is most salient for CA.

The distinction we will need is between what I will call "strongly constitutive" and "weakly constitutive" aspects of rules. (I will refer, for convenience, to strongly and weakly constitutive rules, but it should be remembered that whether a rule is strongly or weakly constitutive depends on the attitude of the analyst as well as on the nature of the rule.) Grammatical rules, as treated by linguists, are strongly constitutive. If you follow the rules, you are doing the activity, speaking the language; if you do not follow the rules, you are simply not doing the activity. By "simply," I mean to suggest that, from this perspective, it is only by obeying the rules that one manages to perform an activity. To fail to perform according to the rules is to fall off the edge, to be incompetent, to behave but not to act. To follow the rules is to do something meaningful; to not follow the rules is to do something meaningless, or at best to do something to which the rules are irrelevant. If you follow the rules of chess, then you are playing chess. If not, all that can be said (in terms of the rules) is that you are not playing chess.

Whereas strongly constitutive rules may be said to set the parameters of meaning, weakly constitutive rules slice up a meaningful domain. Giddens (1979) mentions "don't take the goods of another" as "the sort of prescription which is usually offered as an instance of a regulative rule" (66). He then points out that this rule "enters into the constitution of ideas of 'honesty', 'propriety', etc." (66-67). This is true, but it overlooks a crucial consideration, namely, that violation of the rule is also constitutive. If you follow the rule, you get one kind of meaning; if you violate it, you get, by reason of the violation, another kind. All regulative rules are weakly constitutive, since it is always possible to interpret the breaking of such a rule as signifying something other than incompetence. (It is not as clear whether all weakly constitutive rules are necessarily regulative as well.) The rule against stealing is regulative, but it is also constitutive of doing

<sup>18</sup>On this point, Giddens' position is similar to mine: "All social rules have both constitutive and regulative (sanctioning) aspects to them" (1979:66).

crime. To be a criminal, you must break the law. The notion of weak constitution is related to Watzlawick, Beavin, and Jackson's (1967:49) observation that we cannot *not* communicate. In a world that is preconstituted as meaningful, whatever one does or refrains from doing is presumed to have meaning. To break the rule is no less meaningful than to obey it.<sup>19</sup>

In some conversational exchanges, an adjacency pair rule may be violated. We could simply discard such exchanges as "ungrammatical," not "real" conversation, and therefore not proper data. To do this would be to lose the fact that the exchange not only makes sense but makes the kind of sense that it does precisely because the rule has been violated. The rule is not (necessarily) something to be obeyed, but something to be aware of, something that the participants (to use Goffman's [1972:185] phrase) "are alive to." In linguistics, the rules *govern* language as an ideal object. To the degree that an utterance does not follow the rules, it is not language. The adjacency pair rules of CA do not govern; rather, they are objects of orientation.<sup>20</sup>

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The earliest published statement, and perhaps still the most elegant, of the

<sup>19</sup>Although speakers sometimes break grammatical rules for effect, the linguist would face special problems in dealing with weakly constitutive aspects of rules (if he chose to deal with them at all). This is because the rules of grammar are so complex that one can hear violations as mistakes. In contrast, violations of conversational rules are generally heard as *deliberate*, unless the would-be violator appears to be out of play.

<sup>20</sup>Levinson (1983) uses an argument similar to mine to distinguish CA rules from linguistic rules: "The CA rules describe unmarked expectations rather than the set of possible well-formed sequences or conversations; in this way such rules are much more like Grice's maxims [of conversational implicature (Grice 1975)] than like linguistic rules. Consider, for example, the rule that given a first part of an adjacency pair, a second part should follow; as the notion of conditional relevance...makes clear, failure to provide a second is itself a communicational resource that can be used to contribute effectively to conversation" (367).

CA treatment of rules in their weakly constitutive aspect is Schegloff's ([1968] 1972) discussion of conditional relevance. Schegloff points out that, of the numerous things which are not present at a particular moment in a conversation, only some may be said to be relevantly absent. "By conditional relevance of one item on another we mean: given the first, the second is expectable; upon its occurrence it can be seen to be a second item to the first; upon its nonoccurrence it can be seen to be officially absent" (364). When, for example, one asks a question or issues a summons and no answer is forthcoming, an answer is relevantly and noticeably absent. This circumstance may, among other possibilities, provide an occasion for the repetition of the summons or question. Conditional relevance applies backwards in a sequence as well as forwards. That is, not only does a first call for a second, but if someone unexpectedly produces a recognizable second, we may suppose that he "heard" a first. If someone says "Yes?", and I am the only other person in the room, I may reply "I didn't call you." Furthermore, as Schegloff and Sacks (1974) note, the production of a proper second to a first shows that one was both attending to and understanding the other's speech.

Schegloff goes on to point out that we cannot "naively choose" not to produce a second upon the occurrence of a first. "The culture provides that a variety of 'strong inferences' can be drawn" from the absence of a second. Perhaps the other has not heard our first or is mechanically prevented from producing a second (e.g., he has a bone stuck in his throat). If we can eliminate or ignore these possibilities, then inferences of motivation and intention are in order. The legitimacy of some particular proposed inference may be denied, but this is merely a claim that one has made the *wrong* inference, that some other inference is in order. This is what Schegloff means in saying that we cannot "naively choose" not to produce a second. Since such inferences are made in an ordered manner, not at random, and are therefore somewhat predictable, we can accomplish such actions as insulting, sulking, displaying reluctance, and so forth by withholding seconds.

Consider the following exchange:

A: What time is it.

B: [no response]

A [sarcastically]: Thanks a lot.

This exchange is perfectly comprehensible, and our analysis must deal with this fact. Let us say, then, that the rule defines a normal form.<sup>21</sup> (This is not a statistical notion. It is quite incidental to the analysis that normal forms tend to be most common.) The rule is a kind of signpost, a feature in the semiotic landscape. Knowledge of the normal form helps A assign a meaning to B's behavior, e.g., he was snubbing me, he has something to hide, etc. Adjacency pair organization is first of all an aid to interpretation, and only secondarily a guide to behavior. I will answer or not answer your question according to how I want you to interpret my behavior. On the other hand, adjacency pair rules are constraining in that, although we cannot make the other produce a particular class of second to our first pair part, by producing a first we can make whatever he does interpretable by reference to what we have done. That is, he must produce a second of a sort.

It would certainly be an overstatement to claim that conversation analysts were the first to notice the weakly constitutive aspects of rules. To take just one example, sociolinguists, despite the emphasis they often put on appropriate, rule-governed speech, could hardly help noticing that breaking the rules for appropriate speech is frequently a meaningful thing to do and not necessarily a disorderly, random, or inexplicable occurrence. Sometimes in this literature, rule-violation is seen as an ordered phenomenon, but without specific recognition of its weakly constitutive aspects. It may, for example, be seen as a characteristic of a certain group of persons (Kernan 1974) or as a strategy by which individuals demonstrate charisma and potency (Salmond 1974). In both

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<sup>21</sup>Geoghegan's (1973) "coding rules" are similar to adjacency pair rules in that they define what I am calling normal forms. In Geoghegan study, the normal forms are items of reference.



these cases, rule-breaking is seen in its indexical (in the Piercean sense) relation to persons rather than as a discursive device, available to all members in their efforts to communicate. A second way of treating rule-violation within sociolinguistics has been to posit the existence of rules for breaking rules, although these metarules are not always specified. This has the effect of making rule-violation a more complex form of rule-governed activity. There may well be times when one can be seen to be following some metarule in not answering a question, but to see all failures to answer in this way is to lose track of the fact that not answering is usually meaningful precisely *because* it is a violation. But even when sociolinguists have perceived that a violation may be meaningful in itself, they have not (to my knowledge) developed the kind of principled and orderly appreciation of this fact that we find in CA. They have not developed it as an integral and central part of their understanding of human communication.

Returning to CA's treatment of rules, does conversation, from the CA perspective, fit Winch's notion of action that is the application of a rule? There are right and wrong ways to perform conversational actions, and so, according to Winch's criterion, the conversationalist's actions constitute the application of rules. However, adjacency pair rules do not define right and wrong in any simple and straightforward way. We cannot say that if you answer a question you have acted competently or rightly and if you don't you have acted incompetently or wrongly. To not answer is a correct way to do a snub.

There appear to be four major ways of treating rules in the social sciences. Whereas linguists look at rules as constitutive in the Searlean sense, the dominant, "normative paradigm" (Wilson 1970) in sociology has been primarily concerned with rules in their regulative aspect. Some major features of the normative perspective: The rules of society (norms) are internalized by society's members in the course of socialization. Actors generally obey the rules both because of internal and external compulsion, the latter taking the form of social sanctions. The constraints posed by the rules account, at least in large part, for the ordered character of social life. As Wilson (1970) points out, it is presupposed in the normative paradigm that (1) social actors agree on the

rules, (2) actors can uniformly recognize situations in which specific rules apply, and (3) actors can uniformly recognize whether particular behaviors are or are not the actions specified by particular rules.

The ethnomethodological position, associated with Harold Garfinkel and his students and foreshadowed by Ralph Turner's well-known article on role-taking (1962), rejects or ignores most of the elements of the normative paradigm. For the ethnomethodologist, rules are constitutive, but in a very different way than they are for the linguist. The ethnomethodologist does not speak of the member as following (or not following) rules. Rather, members cite or allude to norms and other rules to identify or display the character of their behavior as social action and the character of the situation as a setting for social action. A related approach takes rules to be items in a discourse of explanation and advocacy (Bilmes 1976).

Finally, there is the CA view of rules as weakly constitutive. CA is sometimes considered a branch of ethnomethodology, and in certain respects it is, but in its treatment of rules CA diverges sharply from Garfinkel's ethnomethodology. One aspect of CA's treatment of rules combines elements of the grammatical and the traditional normative approaches. The rules, to a degree at least, constitute the activity, and yet the activity presents certain systemic requirements that allow for a functional explanation of the rules. In their other, weakly constitutive aspect, rules are not viewed primarily as things that participants cite or allude to or that produce order by constraining behavior or that produce meaningful activity by defining it, but rather as mechanisms of meaning-in-progress. They are not signs, yet they are vehicles of meaning. They do not necessarily constrain action, yet they must be taken into account.<sup>22</sup>

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<sup>22</sup>Some researchers have seriously misunderstood the CA treatment of rules; they see it as invoking a normative or even statistical model. Thus, Goldberg (1983) writes that "the sequencing rules are only normative ones--reflecting the statistical probabilities that act *b* will follow act *a*....[In the case of a violation of the sequencing rules] all that can appropriately be claimed is that, according to

## 6. Conclusion

Michael Toolan (1986), in his review of *Pragmatics* (Levinson:1983), quotes Levinson as follows:

As many instances as possible of some particular phenomena are examined across texts, not primarily to illuminate "what is really going on" in some interaction (a goal judged impossible), such illuminations evading some participants as well as analysts on many occasions, but rather to discover the systemic properties of the sequential organization of talk, and the ways in which utterances are designed to manage such sequences (287).

Toolan makes some interesting comments on this passage. "At base," he writes, "this is a proposal for behaviourist distributionalism of a quite Bloomfieldian kind" (150). The CA program is strongly empirical (and, in this regard, has succeeded where the linguistic descriptivists failed), but it is not behaviorist in the usual social scientific sense of the term. There is no underlying formulation in terms of stimulus-response and no notions of conditioning. CA's generalizations have to do with social conventions rather

the model, the chances of its occurrence are extremely low and not predicted" (32). The fallacy here is quite apparent and requires no comment. She goes on to allow that "adjacency pair models do, to an extent, describe violations of the standard, speech act derived sequencing rules. For example, insertion sequences are regularized violations of the question-answer sequences" (32). Insertion sequences are not, I think, viewed by conversation analysts as violations at all, nor do participants view them as such. Her misunderstanding of the nature of the CA model leads her to a final and perhaps oddest-of-all claim that CA "glosses over the emergent, interactive accomplishment of discourse" (32).

than with the nature of organisms as behavior generators. As for distributionalism, there are, as we have seen, some very basic differences between the approach of the structural linguist and that of CA. "Bloomfieldian distributionalism" can hardly serve as an accurate description of an analytical technique that handles both the identification of items and the rules of their arrangement in a very unBloomfieldian manner.

Toolan continues: "Like any distributionalism that denies the influence of intuition or meaning, it simply uses intuitionist judgements covertly. What is going on in an interaction cannot be 'illuminated', but systematic organization can. In practice tentative identifications of bits of conversational systematicity are made using distributionalist criteria to support a prior underlying assumption as to 'what is going on'" (150). Toolan's understanding of what is meant by "what is going on" is clarified by his comment that "the broad picture as to the topics of talk, speakers' probable goals, as to what is relevant and intended--much of this is tacitly assumed and underpins the accounts and systematicities that c.a. posits" (150).

It seems to me that Toolan is only partly correct in his understanding of what Levinson is referring to with the phrase "what is really going on." The participants' goals and intentions are, ideally, neither an object of analysis nor a theoretical resource for CA, and, as far as I can tell, CA has no systematic need to covertly depart from this ideal. If a conversation analyst, in some particular study, happens to deviate from the ideal, this is his error (or perhaps his deliberate decision); it does not, at any rate, indicate some essential flaw in the model. Where Toolan seems to go wrong is in the conflation of meaning and intention. The meaning of an utterance, as against what the speaker "means by" the utterance, is a matter of convention, context, and negotiation. The conversation analyst, it is true, typically relies on his unanalyzed native competence to see the possible meanings of utterances. He does not do the work of the grammarian, the semanticist, or the speech act theorist. If this is what Toolan means when he says that the conversation analyst "uses intuitionist judgements covertly," then he is right. But CA does not "deny the

influence of...meaning"; it simply declines to speculate about intention and motivation. Toolan has not discovered a fundamental methodological inconsistency at the core of CA.

Levinson (1983) writes that "it is perhaps no accident that the analyses produced by CA so far have a striking (if superficial) resemblance to the structuralist theories of linguistics that predominated before the 1960s. Both kinds of approach are concerned with corpora of recorded materials; both have as a central methodological tool the use of a 'slot and filler' heuristic--i.e., the investigation of how sequential (or *syntagmatic*) considerations restrict the class of items that may expectably follow" (367). We have seen in the preceding pages how each of these similarities is indeed superficial. Although, like traditional structural linguistics, CA uses basically an item and arrangement approach, CA has a distinctive way of categorizing items, a distinctive understanding of the rules of arrangement, and a distinctive notion of how rules and categories interact. Associated with CA's different analytical methods is a concept of its analytical object as an empirical, interactional process rather than an ideal, contextless structure. CA is empirical in a way that linguistic structuralism never was or could have been. In these very fundamental ways, CA is something new in structural analysis.

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