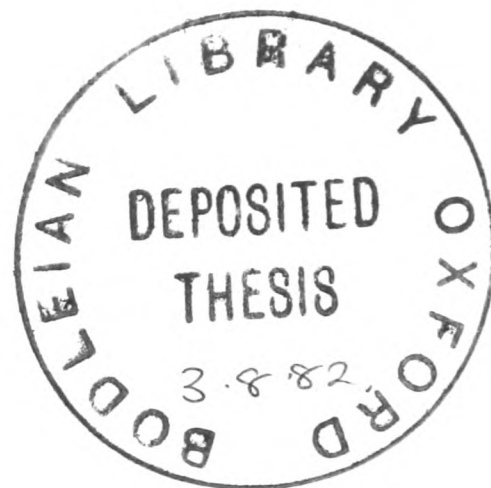


Discontinuity in Conversational Speech:
an investigation of some theoretical
problems and their analysis.



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Trinity College
Thesis submitted for the
degree of D.Phil. in
Michaelmas term, 1981.

ABSTRACT

The occurrence of discontinuity in conversational speech raises a variety of theoretical problems for the study of verbal communication. According to the dominant explanatory models of language, discontinuities — in the form of pauses, self-corrections, repeats, false starts, and the like — are errors of language performance. Consequently, when, in conversational speech, discontinuities do occur, it is natural that questions should arise regarding (a) why they occur and (b) given that they do occur, how speakers and hearers are able to deal with them. It is argued that no coherent answers may be offered to these questions under the prevalent conceptualization of discontinuity.

As a feature of verbal performance, discontinuity has been studied primarily by researchers from the related disciplines of psychology, sociology, ethnomethodology, and discourse analysis. Although the explanatory approaches differ greatly, there is an underlying presuppositional unity to them. Whether discontinuity is studied as an indication of the speaker's cognitive processing, as a feature of social differentiation, or as an essential aspect of the speaker and hearer's management of conversational interaction, it is always assumed that discontinuities occur as the result of speaker error. This assumption, in turn, is shown to be derived from an unrecognised written-language (or 'scriptist') bias in the study of verbal communication. This conceptual bias, it is argued, is a shared weakness in the otherwise differing theoretical approaches to the explanation of discontinuity in conversational speech.

The theoretical discussion of the thesis is supplemented by an appendix containing a selection of transcribed examples of discontinuities compiled with the aid of a computer concordance program. These examples are used as comparative evidence throughout the central chapters of the thesis.

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ABSTRACT

This thesis investigates the theoretical problems arising from the study of a set of ordinary features of conversational speech. Of primary interest are those 'performance errors' (cf. Chomsky, 1965, p.3-4) which are held to disrupt the continuity of speech output: e.g. hesitations, false starts, self-corrections, repeats, repair, etc. Although these features do not appear to share a common characteristic, they may be grouped together on the basis of their mutual opposition to the notion of speech fluency or continuity. For this reason, they are referred to here by the general name 'discontinuity'.

The study of discontinuities is considerably influenced by the fact that they do not occur in written language. They are uniquely a phenomenon of speech. This raises an interesting set of problems concerning the conceptualization and explanation of discontinuities. The analysis and resolution of these problems presents the central task of the thesis. It is argued that discontinuities are conceived as 'errors' of speech performance primarily because of their absence from written language. There is a written language (or 'scriptist') bias prevalent in the study of speech. This leads to treating as 'errors' any features of spoken language which would contravene the grammatical conventions of writing. Among such features are those

grouped under the heading of 'discontinuity'.

The fact that discontinuities occur so frequently in conversational speech raises some interesting problems for assumptions regarding the link between grammar and communication. Because discontinuities are a regular feature of conversational utterances, these utterances do not often meet the standard (scriptist) conventions of grammaticality. Nevertheless, communication never appears to be jeopardized by the occurrence of discontinuities. Consequently, the notion of successful communication as dependent upon the speaker's instantiation of and the hearer's recognition of utterances which are tokens of the grammatical sentences of the language must come into question. For what speakers commonly utter and hearers commonly understand are manifestly not grammatical sentences.

If it is accepted that discontinuities are errors of speech performance — contravening grammatical conventions and thereby jeopardizing communicational success — two related questions compete for attention. First, why do discontinuities occur? That is, why is it that speakers risk communicational breakdown so frequently by committing the error of discontinuing in mid-utterance? The second question concerns the effect of discontinuity on communication. Given that discontinuities do occur in conversational speech, how do speakers and hearers attempt to remedy the potential consequences for communicational success? This question has generally been divided into two sub-questions. (1) How do speakers repair the potential damage to speech which discontinuities are assumed to cause? (2) How are hearers able to make sense of utterances in which discontinuities occur? In both cases, the answers provided are framed in terms of discourse rules supposedly used by speakers and hearers to minimize discontinuity's damage to the formal

sequential structure of speech utterances.

The main body of this thesis consists of an analysis of some of the theories which have been proposed as answers to these questions regarding the causes and consequences of discontinuity. Because modern linguistics has taken the linguist's task to be the study of linguistic competence rather than performance (or langue rather than parole), these proposals have tended to come from researchers working in disciplines other than linguistics proper. The linguist takes discontinuities to be errors of performance and, for that reason, irrelevant to questions concerning the competence of the ideal speaker-hearer. The result has been to push the study of discontinuity into the investigational domains of psychology (both cognitive and social psychology), sociology, ethnomethodology, and discourse analysis. It is no surprise, then, that the resultant 'theories' of discontinuity often reflect the presuppositions and explanatory goals of their disciplinary backgrounds. However, it is argued that the explanations of the causes and consequences of discontinuity, in every case, are determined by the underlying influence of a 'scriptist' conception of discontinuity-as-error. Consequently, the way is blocked to a treatment of individual discontinuities as bona fide features of conversational interaction.

Psychological investigations have focussed on the question of the causes of discontinuity. The occurrence of (both silent and filled) pauses in speech has been the primary concern. Psychological studies of pausing have been motivated, in general, by the desire to use pauses as external, observable evidence of internal, unobservable mental processes. One group of these studies has taken pausing to be an external sign of speaker anxiety. The second, larger group of studies focusses on hesitation pauses as evidence of the cognitive activity

involved in speech encoding. By observing the distributional patterns of hesitation pauses, it is hoped that investigators will obtain a 'window on the mind' of the speaker. The origin of this explanation lies in Lounsbury's synthesis of behaviourism, distributional linguistics, and information theory. In his 1954 paper, Lounsbury argues that speaker hesitation is caused by latency in the stimulus/response processes underlying speech encoding. The idea that speech encoding is the cause of hesitation has remained popular in psycholinguistics during the 25 years since Lounsbury's study. Curiously, however, Lounsbury's argument supporting the link between encoding and hesitation has been completely abandoned and no new support has been offered. Instead, investigators working within a post-behaviourist, Chomskian psycholinguistic model have simply taken the 'window on the mind' hypothesis as an unquestioned assumption. Recent work by Good and Butterworth has challenged this assumption on the ground that at least some occurrences of pauses can be shown to be intentional features of a speaker's verbal performance. This observation means that pauses cannot be taken as reliable evidence for speech encoding theories since there is no incontrovertible way of telling whether a particular pause is the result of latency in speech encoding or whether it is an intentional, functional part of the speech act. The latter possibility is most damaging for the concept of discontinuity as an 'error' of speech performance. This in turn brings into question why there is a need for 'special' psychological explanations of the causes of discontinuities.

Sociological investigations into discontinuity are of two types. The first is exemplified in the work of Bernstein on the social codes of language use. Bernstein argues for the existence of two social codes of speech: a restricted code and an elaborated code. The former

code is primarily said to be used by members of the working class while the elaborated code is used only by the middle classes. Because speakers of an elaborated code pause more in speaking, they are said to be able to make a wider choice from the structural and lexical possibilities afforded by the language. Consequently, they give the impression of being more articulate and more intelligent, thereby accruing the advantage of educational prestige.

Bernstein's theory is based on psychological arguments linking pausing and speech encoding. From this explanation of the causes of pausing, he draws conclusions regarding the social consequences of pausing. He does not, however, offer a coherent explanation of why the latter should follow from the former.

The second group of sociological studies of discontinuity focusses on the speaker's management of discontinuities, no matter why these latter are held to occur. These are the studies of the ethnomethodological school of conversational analysis. The ethnomethodologists argue that repair, a notion related to that of error-correction, is regulated by an independent organizational mechanism operating within particular conversational environments. This mechanism is said to incorporate a built-in system of preferences according to which self-correction is preferred to other-correction. The function of repair is to accommodate and minimize the disruptive effect on communication of discontinuity and other speech errors.

While the notion of repair concerns the speaker's efforts to fix ('repair') the potential damage to communication caused by discontinuities, the parallel notion of editing rules concerns the hearer's role in dealing with the consequences of discontinuous utterances. Like the more familiar notion in discourse analysis of ellipsis rules, editing

rules are supposed to explain how the hearer derives grammatical competence sentences from the typical discontinuous utterances which he regularly encounters in conversation. Chapter Five consists of an original detailed analysis of the notion of editing. A variety of possible formats for editing rules are formulated and compared. With the use of the Lund Corpus of English Conversation, each of the editing rules is examined for its effectiveness in editing the wide variety of discontinuous utterances which are spontaneously produced in English conversation.

It is concluded that no consistent set of formal editing rules — i.e. rules which are not merely ad hoc, situation-specific, and subjective — could be formulated to enable different hearers to derive the same sentences from the typical discontinuous utterances produced in spontaneous conversation. To complement this argument, evidence is produced to show (a) that hearers appear to have no trouble interpreting a range of actual 'ungrammatical' utterances and (b) that discontinuities can serve a variety of interactional functions. These important functions would be ignored by an editing procedure which deleted discontinuities from the hearer's perception and/or interpretation of an utterance. On these grounds it is claimed that the notion of editing rules — like that of repair rules — is of little use in explaining how hearers are able to understand spoken discourse. Rather, rules of editing and repair are only of use for a theory of discourse which takes communication to be based on the speaker's intended production of and the hearer's interpretation of competence sentences. Such a theory is the result of a scriptist conception of speech.

The theoretical discussion carried on in the main body of the thesis is supplemented by an appendix containing a corpus of passages selected

from the Lund Corpus of English Conversation. This compilation of discontinuous utterances was carried out by means of a computer concordance program specially written for this purpose. The aim of the appendix is to provide comparative empirical evidence for the theoretical arguments in the text. These arguments, of necessity, are focussed on the examples used in the works being discussed. The appendix corpus provides a broader, more varied empirical background for these discussions.

Were discontinuity not considered to be an error of verbal performance, there would be no reason to ask why it occurs and how speakers and hearers are able to deal with its consequences. Such questions are not asked of those features of speech which escape characterization as 'errors': e.g. the use of prepositions, verbs, morphemes, syntactic constructions, etc. It is only because discontinuities would be considered ungrammatical in written language that they are termed 'errors' in spoken language. Such a scriptist perspective leads the linguist (or other investigator of speech communication) to ignore or misunderstand those features of speech which — for one reason or another — do not properly occur in written language. Consequently, the investigation of the forms and functions of spoken language remains stalled at an embryonic stage. It is urged that a consideration of the wide range of possible interactional functions of discontinuities should be the focus of future research.

Discontinuity in Conversation Speech:
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Preface

When thinking about what a language is, we are influenced by what we think a language is used for. Somewhat paradoxically, the converse is also true. Our concept of what languages are for is at least partially determined by our concept of what languages are. This situation applies both to theoretical and common-sense reflection on the nature and function of languages. Common-sense tells us, for instance, that one important function of a language is to facilitate conversational interaction. We use a shared language in order to communicate with each other. Consequently, what we think communication consists in will influence how we conceive of the sort of thing a language is. For the theorist, assumptions about the nature of languages will have an inevitable effect on hypothetical explanations of how linguistic communication — be it spoken or written — is possible.

The task undertaken in this thesis is the investigation of how certain theoretical and common-sense assumptions about the nature of languages have influenced the conceptualization of and explanation of certain formal properties of conversational speech. The formal properties concerned are here grouped under the ^{heading} of 'discontinuity'. How discontinuity has been conceptualized is the subject of both the opening and closing chapters of the thesis. The main body of the text examines the hypotheses that have been offered to explain why discontinuities occur and, given that they do occur, how conversationalists are able to deal with them. Underlying both the conceptualisation and explanation of discontinuity is the recurrent assumption that discontinuities are errors of language use. This assumption, in turn, is shown to be based upon certain fundamental assumptions about what a language is.

The discussion of the theoretical problems raised by the study of discontinuity is supplemented by a large selection of empirical examples

presented in the appendix. The appendix has been compiled with the aid of a computer concordance program specially written for this purpose. The program is applied to three files from the Lund Corpus of English Conversation, gathered as a part of the Quirk Survey of English Usage. The result is an output of 242 examples to be used as informal comparative evidence for the theoretical discussions. It would not have been possible to integrate the appendix examples more thoroughly with the theoretical discussion in the chapters because of certain criterial problems about the identification of types of discontinuity. These problems are discussed at various points in the main body of the text. The purpose of the appendix, then, is to provide a broader empirical background to the central theoretical aims of the thesis.

As ever, my greatest intellectual debt is to my research supervisor, Professor R. Harris. The topic of this thesis was his idea, and it was his inspiration that brought it to fruition. I am also grateful to Professor J. Fought whose observations regarding certain key points of the argument have been most helpful. My research was partially supported by a U.K. Social Science Research Council studentship and by a series of generous grants from Trinity College, Oxford. I thank them both.

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There are two systems of footnotes employed in the text of this thesis. Numerical footnotes refer to references listed at the end of each chapter. Alphabetic footnotes refer to notes listed in Section II of the Appendix. The appendix notes are designed to link passages of the computer-compiled corpus with particular points of the theoretical discussion in the chapters.

CHAPTER ONE

Introduction: Conceptualization, Definition, Identification

This thesis attempts to analyse and interrelate a number of theoretical problems which arise in the investigation of one widespread feature of conversational speech. This feature will be referred to as 'discontinuity'.

The originality claimed for the thesis resides in its recognition and identification of the basis on which discontinuity is accepted as being a feature of conversational speech, even though discontinuity has many different manifestations and these have been studied separately by different academic disciplines.

The value claimed for the thesis lies in the contribution which this analysis of discontinuity makes towards understanding how - as both lay language-users and analysts - we conceptualize the kind of activity which speaking is.

* * * * *

1. Discontinuity: a preliminary account

The notion of discontinuity, as understood for purposes of the present study, is perhaps best introduced by reference to the 'performance errors' which are said to characterize ordinary conversational speech, such as hesitations, false starts, repetitions, error-corrections, filled pauses, etc.¹ Of primary interest here will be the ways in which discontinuity is studied from the differing theoretical perspectives of linguistics and other language-related behavioural disciplines.

Three characteristics of discontinuity in speech become immediately apparent to the inquirer.

(i) The first of these might well be considered as an essential defining characteristic of the notion of discontinuity: discontinuity does not ^{normally} occur in written language. For example, it is not to be found in any of the standard registers of written English: journalism, fictional narrative, travel brochures, personal correspondence, thesis writing, press statements, etc., etc. Discontinuity, unlike sequential arrangement, syntactic marking, etc., is normally 'filtered out' of written language even when the latter is intended to be an account of what a speaker has said (such as in court transcripts). In this respect discontinuity is like many of the features of spoken language, including intonational structure, rhythmic grouping, and articulation rate. And yet all of these, along with discontinuity, are commonly occurring features of the speech acts we perform in communicational interaction.

The following is a transcription of part of a conversation between two middle-aged married couples. The subject is pigs. The transcript, edited by Crystal and Davy (1975), makes use of a simple system for

the notation of tone unit structure (explained on page 17 of Crystal and Davy, 1975). The discontinuities are not edited out.

1. - |they ÀRE re'volting| |ÀREN'T they| al|though
2. - a ↑friend of ÒURS| who . w was |so .
3. - ↑passionately ↑FÒND of PÍGS| that - |he
4. - 'came . |he 'came from NÒRFOLK| you |SÉE|
5. - and he |came to the er ↑MÍDLANDS| - er to
6. - |TÉACH| and he I |think he was ↑very SÀD|
7. - for the . the |lost 'fields of ↑NÒRFOLK or
8. - SÓMETHING be|cause when ↑ever we were ÓUT|
9. - he would |stop the CÁR| if |ever he 'saw .
10. - 'or SMÉLT| |sign of a PÍG| .²

There is nothing which strikes the listener as out of the ordinary about the speech of which this extract is a transcription. A recording of the actual conversation from which this passage was transcribed may be found on the cassette which accompanies the Crystal and Davy text, extract 4. It does not give the impression of being anything but normal, spontaneous, conversational speech. (Indeed, the speakers were, according to the editors, unaware of being recorded.³) And yet, the first reaction the reader of this transcript has is that it is not normal written language. Even as written dialogue, it strikes a reader as out of the ordinary. This is primarily because of the transcriptionists' efforts to 'translate' into written form many of the features of ordinary speech which are not part of the written language code. A similarly confusing transcript would be produced if one tried to mark down all the kinesic aspects of a conversation. But this would primarily involve adding features to the written text, while not really transgressing the

conventions of written language. In the text above, because of the inclusion of discontinuities and other 'performance errors', the result is a passage which does not conform to the grammatical conventions of writing. This point will prove significant throughout this thesis.

For introductory purposes, it may be best to identify some of the standardly mentioned discontinuities in the above passage. Line numbers have been added to the Crystal and Davy version in order to facilitate this task. We may start by pointing out the two 'filled pauses' on line (5): er . The second of these filled pauses is preceded by a silent (or 'unfilled') pause. Because this silent pause is marked by one hyphen it is said to have a duration of one full conversational beat.⁴ The full stops after who and so in line (2) indicate silent pauses shorter than one full beat. Such filled and silent pauses are often referred to simply as hesitation. There is an instance of what is often called a 'false start' on line (6): and he I think he was This is called a 'false start' because one interpretation of such phenomena holds that the speaker starts to say one thing and then, deciding against a continuation of what he has begun, he discontinues and starts a new sentence. In other words, the example from line (6) would be interpreted as if the speaker discontinued his sentence after and he and then began a new one with I think he was. Some researchers will also call the sequence who . w was in line (2) a false start. In this interpretation, the speaker is taken as beginning to say was, discontinuing, and then beginning again. Others might call this an instance of stuttering and still others a case of self-correction or repetition. The lack of a unifying terminology is itself indicative of problems in the analysis and interpretation of discontinuity features.

An instance of what most researchers would call a simple repeat

occurs in line (7): for the . the. A longer repeat is found running from line (3) to line (4): that - he came . he came. There is no case of a straightforward self-correction in this passage, but if, in line (4) the speaker had said he came from Norfolk . no Suffolk that would have qualified as a self-correction. Finally, it should be noted that, if one takes although at the end of line (1) as the beginning of a sentence or major clause, that sentence or clause is never completed. It is, in a sense, abandoned on line (3) after so passionately fond of pigs that - . Some researchers would say that what follows the quoted sequence is a 'repair' on that sequence. In this case, since it is the speaker who performs the repair, it would be called a 'self-repair'.⁵ It is significant that the decision, if made, to take the clause begun by although (line 1) and ending at pigs that (line 3) as abandoned is not based on grammatical criteria, for what follows could, strictly speaking, be a grammatically adequate completion of the clause begun by although. That is, although a friend of ours who was so fond of pigs that he came from Norfolk is a perfectly adequate subordinate clause, if perhaps a bit of a slur on Norfolk. Furthermore, although one might claim that the sentence or clause begun by although (line 1) is abandoned by line (3), it is clear that the information presented in that clause is not lost. If it had been lost there would be no pronominal reference for the he in lines (3), (4), (5), etc. Nor would it be clear that it was because the friend was fond of pigs that he stopped wherever he saw one. Indeed, for the logic of the passage to be complete the 'abandoned' clause must be taken as a premise. The fact that, as far as the communicational success of the passage is concerned, no obstacle has been raised by the speaker's 'hemming - and - having' stands in contrast with the term 'performance

error' used to designate individual features of that performance. A major question is thus raised: how can a speaker's performance be said to fail through discontinuities, and yet remain communicationally successful? While it is true that the speaker fails to produce a sequence of sounds which may then be transcribed into a grammatical sequence of written language (i.e. in terms of sentences), the relevance of this point to the notion of the success or failure of speech performance is to be questioned.

(ii) A second important characteristic of discontinuity is that we do not often talk about it. That is, although we - i.e. as ordinary language-users rather than trained linguists - often talk about the meaning of what someone said, or the style in which it was said, or the grammar of its construction, and so have developed a terminology to be used in these meta-linguistic 'language-games',⁵ we rarely talk about the continuity or discontinuity of an utterance. (The exception here is when reference to a speaker's stuttering or laboriously drawn out articulation is discussed. When discontinuity approaches or attains the pathological, it becomes a topic of common interest.) Consequently, we have not developed an organized terminology for talking about discontinuity. This situation has important implications.

In the standard domains of orthodox linguistics - phonology, syntax, semantics, etc. - the theories developed by linguists must stand up to the theories represented in the everyday language-games referring to the phenomena in these domains. As a result, semantic theory, for example, has been profoundly influenced by the fact that

ordinary language-users already have certain fundamental notions about how words have meaning and about the relations between the meanings of words. These notions are represented in the language-games used for talking about meanings. "Here, this is what 'hardwood' means" someone will say, holding up a piece of hardwood. Another will say "'Anxious' means 'worried'" in order to explain the meaning of the word 'anxious'. The influence of such ordinary, everyday metalinguistics is inescapable. The semantic theory which is not able to present itself as being in agreement with such language-games, or is unable to give good reason why it is in disagreement, has little chance of being accepted even within the academic community. For this community itself is made up of ordinary language-users and, so, ordinary metalinguists.

Our theories of language, like our theories, of social behaviour in general, 'grow out' of what we already know about language and social behaviour, and this knowledge is itself reflected in the language-games we use for talking about language and behaviour. A semantic theory which was not in agreement with our language-games about meaning would not ordinarily be thought of by scholars whose 'intuitive' knowledge of meaning is already framed by those language-games. Our formal theories of language are built upon our common-sense ideas about language and how it works, and those ideas are themselves represented in the ways we talk about language.

For example, there could be no linguistic theory as we know it which did not presuppose something very much like Bloomfield's 'fundamental assumption of linguistics'.⁷

In certain communities (speech-communities) some speech-utterances are alike as to form and meaning.

On this assumption is based the notion of a type-token relation holding

between the language-system and speech. Without it, the linguist's chosen subject - the language-system said to underlie verbal behaviour⁸ - would be conceptually incoherent, not to say, irrelevant. Now, Bloomfield did not formulate this 'fundamental assumption' simply for the purpose of inventing a discipline. Rather, the assumption is grounded in the way everyday language-users will ordinarily talk about each other's verbal behaviour. Chris says X. Gill asks Susanne "What did Chris say?" Susanne replies "Oh, he said X". Then Gill, "Well, we'd better do it then." In such a situation what Susanne replies is taken by Gill to be the same thing as what Chris said and to have the same meaning. The fact that, due to the different shape of her vocal tract, Susanne's utterance was not an identical copy of Chris' utterance is ignored by Gill. Instead, the linguist would say, she takes Susanne's and Chris' utterances to be tokens of the same type of speech unit and, so, to mean the same. Gill reacts to the meaning of Susanne's utterance as if its meaning were identical with that of Chris'.

This example is intended to be taken as typical of ordinary conversational interaction. It is on the basis of such typical interactions that the theoretician conceptualizes an underlying structure to human verbal behaviour, in this case, a type-token relation linking specific utterances to sentences-of-the-language. In the context of such ordinary interactions Bloomfield's fundamental assumption seems a natural conclusion. Without such a context, such an assumption could be accused of prejudging all the important questions of linguistic inquiry. In light of the way we talk about other people's talk, Bloomfield could not have been expected to formulate any other fundamental assumption.

Since the topic of discontinuity does not figure in our metalinguistic

language-games in English, there is a greater freedom allowed to the investigator who wants to construct a theoretical account of discontinuity in speech. It is important to note that this is a result of our cultural conditioning as English speakers. A linguist from a different culture might not be so free. For instance, since in Tzeltal there appears to be a greater facility for talking about discontinuity, the Tzeltal linguist could be expected to experience contextual constraint in his theoretical conceptualization of discontinuity phenomena in speech. In his 1974 article 'Speaking of speaking: Tenejapa Tzeltal Metalinguistics', B. Stross gives evidence that the ordinary Tzeltal speaker has the ability to discuss discontinuity with the use of a variety of descriptive terms. This gives the Tzeltal speaker a greater ordinary technical capacity for incorporating the discussion of discontinuity into his metalinguistic language-games than is afforded to the speaker of English. Stross lists at least 8 expressions which the Tzeltal may use in talk about discontinuity.

- ϕ' akbil k'op: speech that has been continued;
conversation continued after a pause.
- $\check{c}'$ abal k'op: silence or near silence (during
conversation or a visit); pause in a conversation.
- keh $\check{c}$ em k'op: speech cut off (in midstream) by speaker.
- k'ohkol k'op: speech cut off in midstream, mid-sentence,
or mid-word by the speaker.
- t'isebil k'op: speech interrupted by someone else
speaking.
- tu $\check{c}'$ bil k'op: speech interrupted by someone else
speaking.

- t'ih[✓]sem k'op: words that won't come out: words that
are blocked by some problem internal to the speaker.
- [✓]sohol k'op: incompetent or non-fluent speech: not being
able to say what one wants to say.⁹

What this list shows is that for some people, the Tzeltal, the notion of discontinuity is a perfectly ordinary one. Indeed there are many different types of discontinuity for the Tzeltal speaker to speak about. It is probable, then, that his conceptualization of language - or, perhaps more appropriately, verbal behaviour - is thus fundamentally different than that of the English speaker for whom the topic of discontinuity is not a feature of his ordinary metalinguistics. It is unfortunate that, as yet, we have no more technical accounts by a native Tzeltal linguist of what a language is, how it is learned, and what the role of discontinuity is in communication. If we did have a Tzeltal equivalent of Bloomfield's Language or of Aspects of the Theory of Syntax, we might - among other things - be able to observe the effect on theory of different cultural notions of discontinuity and 'performance errors'.

Since, in English, we do not ordinarily speak of discontinuity, the investigator into this topic is relatively unconstrained. This means that it is easy for the orthodox linguist, or psychologist, or sociologist to appropriate the notion of discontinuity for his own purposes. Thus we find very different discontinuity theories arising out of what are considered to be more central concerns in psychology, sociology, and linguistics 'proper'. Sociologists have explained discontinuity in such a way that their account confirms what they see as more central sociological theories. Psychologists have used

discontinuity as evidence for theoretical accounts of psychological processes. In turn, linguists have 'explained' discontinuity in a way which suits theories considered to be of greater import to linguistics 'proper'. The theories which have thus been developed do not agree with one another. Often the central concepts of one theory appear totally incommensurate with those of another. The common-sense anchor, which an ordinary metalinguistics of discontinuity might have provided, is absent. The only link with common-sense techniques which all the theories can trace is the practice, discussed above, of excluding all discontinuity from writing. Discontinuity, in so far as it is a general feature of language, belongs only to the spoken medium. Every theory of discontinuity, regardless of its disciplinary background and assumptions, must take account of this. Thus, although the concept of discontinuity does not have a firm foundation — such as have the concepts of meaning, grammar, style, etc. — in the practice of metalinguistic language-games, nevertheless its foundation is that of a practical verbal technique: that of transposing spoken language onto the page. This common practice will be seen to underlie both the concept of discontinuity — in each of the theories discussed — as well as each of the theoretical explanations of discontinuity.

There is little inter-disciplinary debate on the topic of discontinuity which might otherwise have served to bring the conflicting theories into contact. In this case, a situation in which conflicting accounts are given of what is supposedly one and the same phenomenon is allowed to stand. One of the aims of this thesis is to interrelate these conflicting theories. Since common-sense, in the form of ordinary meta-linguistic language-games, does not do the work of providing a common ground linking these disparate theories, it is up to the

investigator with an inter-disciplinary approach to construct such a common ground by means of comparative analysis and juxtaposition.

(iii) A third important characteristic of discontinuity is that much of it does not appear to exist, at least not in the way that other features of language and language-use — ~~speech sounds~~, intonation, syntactic marking — are said to exist. A common focus of discontinuity studies has been the silent pause. In the most general sense, a silent pause can be said to occur when the speaker is not speaking. So a pause has no linguistic substance. The pause is merely silence. However, all silence does not constitute pausing. In the empty quad where no speakers stand, there are no pauses. An absence of speech does not fulfill the criteria we employ in calling something (or nothing) a pause. The notion of the pause is intrinsically related to its opposite: fluent or continuous speech. When there is no question of fluent speech, e.g. the empty quad or a couple fast asleep in their bed, then there is no question of a pause in speech. It appears then that the pause, like the related notion of discontinuity, must be defined in relation to its opposite: fluent or continuous speech. Each is what the other is not.

Because the pause is the silence between units of speech (but not all such silences), it is not observable in the same way as are other units of speech. While (e.g.) a particular vowel sound is observable and even measurable along a variety of dimensions — length, fundamental frequency, pattern of formants, loudness, timbre, ... — the pause only has length. What happens between the beginning and the end of a pause is,

compared to vowel sounds, very simply described: nothing. This makes the pause unique among the significant parts of the utterance. It also makes it generously amenable to theoretical explanation. Since there is nothing to observe in the pause, apart from its beginning and its end, there is virtually nothing, no data which can serve to falsify any theory of the pause. This is another reason why the pause has proven to be fertile ground from which a variety of mutually contradictory theories have been drawn. Having a theory of the pause is like having a theory of what is in a locked room into which neither we nor anyone else who proposes to test our theory may enter. There is little ground for resolving arguments between conflicting theories, and so the arguments tend not to occur. This situation becomes even more complicated with pausing because the theories of pausing are not, in general, formed in order to develop a better understanding of pausing itself but rather to use the pause as evidence for other, more 'general', theories of psychology, sociology, and linguistics. Thus if a theory of the pause can be produced which serves the purpose of the more general concerns of the investigator qua psychologist, sociologist, or linguist, then the desire to question that theory or to evaluate it against other theories of the pause is reduced.

The study of discontinuity, then, is an exemplary study. In studying discontinuity the researcher is faced with many of the obstacles which hinder any study of the fleeting phenomenon that is the act of utterance: (a) the 'scriptist' bias created by the comparison of spoken language to written language, (b) the influence on the theoretical conceptualization

of speech that is enforced by the everyday ways we have of talking about speech in our ordinary metalinguistic language-games, and (c) the difficulty facing any attempt to characterize speech precisely by means of physical measurement, observation or criterial identification. In short, the investigator of discontinuity is all the more conscious of the fact that speech, studied as a medium of communication, is neither objective nor directly observable, but is the conceptual product of the way we talk about it, think about it, and use it in intentional contexts. Consequently, it becomes all the more apparent that it is a difficult task to obtain a balanced view of the phenomenon of discontinuity, even to the point of knowing if it is a real phenomenon or an illusion created by any of a multitude of possible perspectives.

The aim of the present chapter is to introduce the notion of discontinuity. This will work towards a definition of what counts as discontinuity as well as what does not. A consideration will be made of a variety of previous attempts made to define and clarify concepts related to that of discontinuity.

* * *

2. The conceptual field

In view of the previous discussion concerning the methodological obstacles facing the study of discontinuity, it will be understood that the concept of discontinuity is a disparate one. Indeed the term 'discontinuity' is common neither in everyday nor in theoretical usage. 'Discontinuity' is here meant to serve as a cover-term for a variety of notions discussed by psychologists, sociologists, anthropologists, and linguists alike, specifically those notions which may all be set in opposition to the notion of continuity. Therefore, in order that we

may obtain a clearer understanding of the meaning of both 'continuity' and 'discontinuity', it will be necessary to carry out a short survey of a variety of related notions.

(i) It is perhaps best first to reiterate that discontinuity, as here conceived, falls squarely within the domain of the now famous Chomskian notion of the 'performance error'. The latter notion is succinctly outlined in the following two quotations from Aspects of the Theory of Syntax.

Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogenous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, errors (random or characteristic) in applying his knowledge of the language in actual performance.¹⁰

We thus make a fundamental distinction between competence (the speaker-hearer's knowledge of his language) and performance (the actual use of language in concrete situations). Only under the idealization set forth in the preceding paragraph is performance a direct reflection of competence. In actual fact, it obviously could not reflect competence. A record of natural speech will show numerous false starts, deviations from rules, changes of plan in mid-course, and so on. The problem for the linguist, as well as for the child learning the language, is to determine from the data of performance the underlying system of rules that has been mastered by the speaker-hearer and that he puts to use in actual performance.¹¹

A few preliminary remarks may be made concerning the above paragraphs. We note, first of all, the implication that the "numerous false starts, deviations from rules, changes of plan in mid-course, and so on" which are characteristic of natural speech are, in Chomsky's terms, errors in the application of the speaker's linguistic knowledge in performance. There is nothing very controversial in drawing such an implication. It is repeated time and again in the standard texts of orthodox linguistics.¹²

What it means, however, is that a great deal of ordinary conversational speech is rife with such mistakes, i.e. that much of the time, when we speak, we do so incorrectly. Furthermore, according to the above passages, conversational speech does not directly reflect linguistic competence. In other words, conversationalists may be conceived as making errors in applying their knowledge of language in performance, but performance itself cannot reflect competence. This is different from saying that, when the speaker does not make errors, then his speech is a reflection of his linguistic knowledge. Instead, Chomsky appears to be saying that although parts of conversational speech bear a direct relation to competence, in that they consist of errors in the application of competence rules, still the 'correct' parts — since they are not a direct reflection of competence — should not be considered as simply the correct application of competence rules. As Chomsky has said elsewhere there is more to performance than the mere production of sentences generated by competence rules.¹³

What is important in the above discussion is the notion that we constantly make errors in speaking. For something to be an error it must be incorrect by comparison with something else: in this case, by comparison with the sentences generated by competence grammars. Although it is perhaps not too difficult to conceive of the act of speaking as consisting of the speaker's (not always successful) attempt to produce grammatical sentences such as those generated by his linguistic competence, this notion of the relation between competence and performance raises some perplexing questions when one tries to form an idea of how hearers come about to understand the error-riddled utterances of speech when, like the speakers, they are supposedly only armed with a linguistic knowledge of grammatical, competence-sentences. How can the hearer 'filter out' the errors and discontinuities of ordinary speech in order

that it may be interpreted according to the semantic, phonological, and grammatical rules which make up his internalized grammar? Some possible answers to this and related questions will be considered in the chapter here entitled "Utterances, Sentences, and Editing".

A further question concerns the task of the child learning his language. Chomsky ^{argues} / that the speech the child hears is full of performance errors, such as those grouped under our term 'discontinuity, to be evidence for his claim that (at least certain important parts of) linguistic competence is innate. Otherwise, so the argument goes, how could the child ever derive the correct grammar of his language from the incorrect data presented to him as inductive premisses? But the child, like adult speakers, also makes so-called performance errors and appears equally capable of understanding utterances in which such errors abound. Where, then, does the child's knowledge of how to discontinue, to restart appropriately and to interpret discontinuous utterances come from? This knowledge cannot be part of his/her innate linguistic competence. Is it then a product of experience? But if what we might call this 'performance knowledge' can be something acquired from experiential conditioning, why could this not also be the case for 'competence knowledge'?

Chomsky's claim that what he calls 'performance errors' arise from the interference of competing features of the communicative act does not meet with much opposition in the writings of others interested in discontinuity. Indeed, it provides a theoretical justification for turning to sources other than linguistics in order to find the explanatory key or keys to discontinuity. Chomsky, as a chief spokesman for linguistic theory, essentially abdicates any authority with regard to the study of discontinuities in speech. Since they do not stem from the linguistic

competence of the ideal speaker-listener, and since linguistic theory is "concerned primarily with the ideal speaker-listener ... who is unaffected by such grammatically irrelevant conditions as ... errors in applying his (linguistic) knowledge",¹⁴ discontinuities are therefore of no concern to the (Chomskian) linguist. A vacant dwelling does not stay empty for long. Soon the neighbors or neighboring disciplines move in and begin to set up house. In this case the neighboring, language-related disciplines are psychology, sociology, and discourse analysis, and they have each their own accounts of what the competing influence is which causes speakers to discontinue in speech. And each account starts from the premise that, since discontinuities do not reflect Chomskian competence, they must reflect something else.

A final but important comment must be made concerning Chomsky's separation of the ideal and the real. What linguistics is supposed to focus on is the knowledge of the ideal speaker-hearer. The real features of speech as uttered by real speakers are merely incidental. Discontinuity, seen as a performance error, is just such a feature of the real. It is discarded from the linguist's concerns in the same way that the inevitable bumps and imperfections on a real rubber ball are ignored in the physicist's theory of the sphere. In large part, it is discarded for the stated purpose of explanatory generality. If physicists spent their time explaining why individual rubber balls roll each in a peculiar way, this argument holds, physics would not be where it is today. Similarly, the Chomskian argues, if the linguist were to concern himself with the details of individual speech acts, performed in unique situations by ordinary, fallible, and personally motivated people, linguistic theory would also not be 'where it is today'. The fact that it is just such details which the ordinary speaker-hearer must constantly concern himself with is,

for the good of the science, ignored. It is the neighboring disciplines who are left to explore and explain these non-linguistic details of the reality of conversational speech. Their concerns turn toward the explanation of particular, real features of speech acts. The dilemma which they face, and which the linguist avoids, is how to provide a generalized explanation which, by its inherent generality, does not betray the one-off individuality of particular speech acts.

(ii) Although it is natural, in the current academic context, to give Chomsky pride of place in discussions of linguistic topics, many other scholars — linguists, sociologists, anthropologists, and psychologists — have written provocatively on subjects concerned with discontinuity. The anthropologist Floyd Lounsbury, one of the initiators of an approach to the study of discontinuity, has remarked that one form of discontinuity, viz. hesitation, has been neglected by linguists because of the linguist's technique of collecting data.

Hesitation pauses have figured very little in linguistic analysis. Probably one reason for this is the way in which the informant technique has been made use of in the past. Whether the informant be an American Indian speaking a strange language to an inquiring linguist, or whether he be a linguist speaking his own language to himself, the time-consuming task of committing the observations to paper has necessitated a great many repetitions of stretches of speech a sentence or less in length. The repetitions demanded of the informant amount to rehearsal and result in his memorization of the phrase or sentence, and thus the hesitation pauses are weeded out.¹⁵

It is not surprising that the repetition of an utterance will eventually result in a continuous, i.e. pause-less, production of that utterance. Because linguistic data collection is often based on the informant's repeated production of utterance tokens — especially at the time of Lounsbury's writing: 1954 — Lounsbury argues that the data collected

by the linguist will be (unnaturally) fluent. Since the orthodox linguist conceives of his object of study as the type-sentence underlying the variety of token-utterances produced by his informant, those features of the token-utterances which (a) seem to be variable in their location and dimension and (b) drop out of the tokens altogether as the repetitions accumulate, are readily interpretable as mere performance factors. They are not seen as part of 'the language' — as the underlying type-sentences are — but as variable details of token speech acts. In the linguist's effort to make general statements about the informant's verbal behaviour, those features such as discontinuity which appear to be creatures of specific moments — situationally, or individually, or performatively determined — are set aside. Pauses, like many other features of the total speech situation, are considered to be part of the context rather than bona fide units of linguistic behaviour.

Lounsbury's theory will be discussed more extensively in Chapter Two. For the moment, it is sufficient to note the background hypothesis underlying the above quote. Lounsbury pictures the speaker as mentally planning his utterance word-by-word as he is producing it. So, when the speaker has finished saying "I went to ...", he then decides what the next word will be, say, "the". Then, he decides what will follow "the", say, "farm"; and the process continues. He argues furthermore that depending on the preceding sequence, it will be easier or harder for the speaker to produce the next word. This will depend on the probability of that word occurring in that particular sequential context. (Lounsbury only considers the linguistic context of a word and is not concerned with the probability of words in situational or intentional contexts. Linguistic probability is thus assumed to be independent of probability within a particular communication situation.) If the word that the speaker

chooses is relatively improbable in the given verbal context, then the processing of that choice will require more time; so the speaker will hesitate. It is on this basis that Lounsbury explains the occurrence of hesitation pauses. Consequently, it is not surprising, from Lounsbury's perspective, that hesitation pauses should be 'weeded out' of utterances which the informant is asked to repeat over and again. As the repetitions progress the sequential probability linking the words becomes higher and higher. Eventually there is no need for hesitancy.

Thus, for Lounsbury, and for many researchers who are inspired by his work, the hesitation pause is an indication of how the speaker is mentally processing the speech production procedure. The study of pause behaviour holds out the prospect to psycholinguistic researchers of providing them with a 'window on the mind'.

Empirical evidence indicates that the temporal dimensions of speech reveal the higher processes of mental activity.¹⁶

However, not all those who have begun their work on hesitation under the inspiration of Lounsbury have retained his behaviouristic, information-theoretical explanation of the speech production process. But, as Chapters Two and Three will show, they have all retained the hesitation pause as an indication of mental activity.

(iii) Next to Chomsky's notion of the pause as a 'performance error' and Lounsbury's depiction of hesitation as resulting from the choice procedure in speech production, we may place the view of certain discourse analysts of the school of sociology known as ethnomethodology.

Although the ethnomethodologists are concerned with a variety of types of discontinuity, their work which is most relevant to our purposes concerns what, in discourse analysis, is called 'repair'. The concept

of repair is related to that of correction in speech. Unlike the majority of psycholinguists who, like Lounsbury, are interested in why discontinuities occur, the ethnomethodologists in their repair studies focus on how speakers deal with discontinuities, given their occurrence. The notion of repair refers to the ways in which speakers are able to restructure their on-going speech in order to accommodate and minimize the disruptive effect of discontinuities. In this respect, repair is said to be an essential tool in the negotiated management of two- or multi-party conversational interaction. Repair is thought to have its own 'orderliness', or syntax, based on built-in preferences. The most important of these is the preference for self-correction over other-correction of an error. A major paper on repair details the source and consequences of this preference.

In the recent history of linguists ... the phenomena of correction — or, more generally, repair — have been largely ignored, in spite of their massive occurrence in the overwhelmingly most common use of language-conversation.

(...)

... the organization of repair is the self-righting mechanism for the organization of language use in social interaction. If language is composed of systems of rules which are integrated, then it will have sources of trouble related to their modes of integration (at the least). And if it has intrinsic sources of trouble, then it will have a mechanism for dealing with them intrinsically. An adequate theory of the organization of natural language will need to depict how a natural language handles its intrinsic troubles. Such a theory will, then, need an account of the organization of repair.¹⁷

(iv) A separate group of sociologists who have interested themselves in discontinuity is that working in the tradition established by Basil Bernstein. Bernstein's sociological perspective bears no resemblance at all to the perspective taken by ethnomethodologists. Instead, his concern is with hesitation discontinuities as a distinguishing feature

in the speech of the middle class. Essentially, the middle class are said to pause more often than the working class; and, thus provided with more time to verbalize their thoughts, they produce more elaborately structured utterances, utilising a wider choice of vocabulary.

The hesitation behaviour of the working class would seem to rule out the (structural) possibilities available to the middle class.¹⁸

Bernstein applies this argument to a discussion of social bias in the educational context and comes to the conclusion that, because of their conditioned habits of avoiding discontinuity, working class children speak in a style (or 'speech code') which connotes a low standard of intelligence. In this case then, discontinuity is taken by some (middle class teachers) to be a symbol of the intellectual ability of the speaker.

(v) Another approach to discontinuity is that promoted by Bernd Voss. In contrast to the ethnomethodologists who focus on how speakers deal with the unavoidable occurrence of discontinuity in their speech, Voss represents those researchers who focus on how hearers are able to decode utterances, in spite of intrusive discontinuities. Voss adopts a popular view in the psychology of speech perception to the effect that the hearer 'idealizes' the incoming acoustic signal — i.e. the result of a speaker's utterance — so that he may interpret that signal as an instance of a sentence of his language. As a consequence of these idealizations, 'performance errors', such as discontinuity, are 'edited' out of the utterance in order that the hearer may derive from that utterance a 'target sentence'. It is the latter which is said to allow the hearer to interpret what the speaker meant to say. The implication of such a position is that hearers of e.g. English are able to tell which parts

of an utterance constitute performance errors and which do not. In this way they can tell how to edit an utterance to the appropriate idealized competence sentence and so understand the speaker's meaning.

Voss produces evidence to the effect that second-language learners of English

will find it difficult to know whether a given stretch of acoustic information is part of the speaker's performance that can be disregarded or whether it needs to be accounted for.¹⁹

In other words, the non-native learner of English will have difficulty recognizing which parts of a speaker's utterance constitute a performance error and, on those grounds, should be 'weeded out' in the idealization process. Thus, discontinuity, in this view, serves as a signal to the hearer about how to idealize a speaker's utterance into an appropriate, interpretable competence sentence. Non-native learners will be unclear on this function and so will mis-perceive many spontaneous (i.e. with discontinuity) English utterances. The importance of hesitation phenomena will stand in inverse proportion to the state of the subject's command of the language.

(vi) Finally, mention should be made of the investigations carried out by the anthropologist C. Goodwin. In his paper "The Interactive Construction of a Sentence in Natural Conversation", Goodwin considers how, in a chosen passage of conversational speech, discontinuities contribute to the contextualization of the speaker's utterance. Goodwin's aim is to show how the construction of a speaker's turn cannot be analysed independently of a consideration of the totality of the speech situation. Specifically, Goodwin focusses on how utterance construction — including discontinuity — is related to speaker and non-speaker gaze

orientation. Discontinuity can serve as a mechanism by which the speaker can control, or attempt to control, the direction of the gaze of his co-interactants.

A speaker can request a gaze of a recipient by providing a phrasal break, such as a restart or a pause, in his utterance. After such a phrasal break nongazing recipients regularly bring their gaze to the speaker.²⁰

It is evident from the selection of passages quoted above that there is, to say the least, a varied interest in discontinuity phenomena in speech. In general, interest in discontinuity is focussed (a) on why it occurs, (b) on how speakers orient to it and employ it for communicational ends, and (c) on how hearers process discontinuous utterances. These major areas of interest will provide the subjects for the central chapters of this thesis. A final chapter will be concerned to point out the underlying unity to the different theoretical perspectives analysed.

3. Defining discontinuity

As a first step towards defining discontinuity, it should be pointed out that 'discontinuity' is not a technical term in any of the approaches which this thesis is to consider. It is precisely for this reason that the term has been chosen. Discontinuity is not to be equated with hesitation, with repair, with pausing, with corrections, with editing, with performance errors, with phrasal breaks, or with any other of the concepts which are the focus of the following chapters. These concepts, or the phenomena to which they refer, sometimes do not even overlap in their domains, and they certainly are not all versions of 'the same thing'.

In this case, there is no one feature or set of features, shared by all the concepts, to which one may refer in a definition of discontinuity. Indeed, some of the concepts — for instance, 'repair' and 'performance error' — depend so much on the very differing theoretical contexts from which they spring that, on comparison, they appear to be related only as opposites.

Given such a situation, it may appear risky, if not foolhardy, to attempt to group the studies of these concepts all within the domain of the study of discontinuity. It would therefore seem perfectly reasonable that the approaches to the study of these concepts should not manifest any similarities or come to any mutually acceptable conclusions. Such a state of affairs might appear to vitiate the logic behind this project as a whole.

However, although there is not an internal characteristic which the concepts referred to above all share, for purposes of definition, they may be grouped together on the ground that they all presuppose an opposing concept to their own, that of continuity. There is a common assumption behind the formation of all the concepts to be grouped here under 'discontinuity' that speech, in some sense, should be continuous. In any case, if it is continuous there is nothing special to talk about. But if it is not, then the concepts of repair, performance error, hesitation, correction, editing, etc., come into play. All these concepts refer to certain phenomena in the flow of speech which keep speech from being continuous.

In this case, then, the first point of grouping all of these concepts together is that each assumes that there can be at least two kinds of speech: continuous and (what is here being called) discontinuous speech. Not all, but most of the researchers to be discussed here have assumed

that 'proper' speaking involves speaking 'with continuity'. When a person's speech does not show continuity, it is thus thought that something unplanned, or unintended, or, simply, in error is happening or has happened in the speech production (and/or interaction) process. The implication is not necessarily that this is a bad thing, but that something has affected, even interrupted, the regular continuous outflow of speech.

Before pursuing this line of discussion we should first reflect that speech does not necessarily have to be conceived in this way. That is, there is no a priori reason why speech should be divided into two types: continuous and discontinuous. It is not difficult to imagine a tribal speech community where either (a) the description of the outflow of speech was divided along conceptual lines which did not coincide with the continuity/discontinuity distinction, or (b) types of outflow of speech were not conceptually distinguished at all. Probably, a prerequisite for the existence of the latter type of community is that it should be illiterate. The notions of continuity and discontinuity seem to arise when the temporal and sequential patterning of speech is compared with the spatial and sequential patterning of writing.

In any case, it seems certain that there is no need for — and this is not to say no use for — the distinction between continuous and discontinuous speech. Such a distinction does however occur in many of the scholarly works on the topic of language. What then is the basis for this distinction? It does not appear to spring directly from our ordinary metalinguistic language-games in English. Of course we can speak about other speakers' pauses, stuttering, hesitations, hemming-and-hawing, 'um'-ing and 'er'-ing; but we do not ordinarily group all of these notions together and contrast them with fluent, continuous speech (except in

certain special contexts, for instance, when, from a rhetorical perspective, we are describing the delivery of an actor on stage or a politician or other public speaker at the podium). There would appear to be, in this sense, no conceptual arrangement to our terms for describing the flow of speech and no firmly based language-game from which the technical distinction between continuity and discontinuity could be said to spring. Instead, the conceptual roots of this distinction would appear to lie in the assumptions which support the theoretical apparatus of modern language-study.

To summarize, the notion of continuity is standardly taken as a neutral base against which a variety of features of speech, specifically those of a temporal nature, may be set. It is on this basis that the central notion of discontinuity is here defined. Each of the researchers to be discussed take disruptions in continuity as their field of interest. The term 'discontinuity', then is not to be defined by reference to any specific feature of speech, or specific set of features. Although at times the researchers discussed will make such a definition, this will not apply for discontinuity in general. Indeed, the notion of continuity will vary, chapter to chapter, in the same way as that of discontinuity. But in each case the former will be taken to be the neutral basis of speech production, against which various types of disruption may be distinguished and studied. A study of the notion of discontinuity, then, is equally a study of the notion of continuity.

4. Further precision: beyond the borders of discontinuity

Although the definition of discontinuity given here simply refers to the opposition to the notion of continuity, there are phenomena which could possibly be included in this definition and yet which, for the

purpose of topical coherence, will be excluded from this discussion.

(i) Although it could certainly be considered among disruptions of the continuity of speech, we will not include, in the extension of the term 'discontinuity' those disruptions which result from 'outside interference', e.g. when a speaker's utterance is disrupted because another speaker interrupts the first in mid-course. Similarly, disruptions which arise because some other external factor, e.g. the shriek of a factory whistle or the fainting of the speaker, will not be considered.

Such phenomena are not excluded because they are felt to be without interest. On the contrary, their interest is such that a proper consideration of them would require not only a lengthy addition to this thesis, but also a considerable broadening of its scope. For instance, in order to compose an adequate treatment of the phenomena of speaker interruptions, we would have to consider at length the general aspects of a theory of conversational interaction so that, at the very least, a definition of the concept of interruption might be formed. Would we, for example, want to call the frequent 'back channel' replies, uttered throughout a speaker's talk by his hearers, by the label 'interruption'? If not, why not? In order to provide an adequate answer to such questions — so that a study of interruptions might begin — a broad theoretical discussion would have to be undertaken concerning what counts as having and as losing 'the floor' and what counts as a back-channel reply. Such discussions would lead this thesis deeply into the study of the interaction between conversationalists, while the main concern here is with the behaviour (and with the consequences of the behaviour) of one person — the speaker — in the context of conversational interaction.

(ii) Under one conception of the notion of continuity, the researcher certainly might include within 'discontinuity' the effect produced when a speaker changes topic in mid-utterance or mid-turn (e.g. "What time is it? Don't do that John!"). Topic-continuity or topic-discontinuity will not, however, be included in this discussion. The definition of 'topic' alone raises theoretical problems which would require a separate thesis to deal with.

(iii) Related to the problem just presented is the question of the continuity or discontinuity between e.g. a speaker's question and the next speaker's reply. When should a conversation be said to lose its coherence? Discussion of this problem is to be excluded on the ground that it is essentially one of topic-continuity.

(iv) An interesting topic, related to that of discontinuity, concerns what the ethnomethodologists call the 'turn-transition (relevance) space'. This notion concerns how an utterance signals that the turn it occupies is possibly complete. If, for illustrative purposes, we imagine a turn to be like a move in a game and an utterance as the combination of the actions which constitute a player's 'making a move', we may take the turn-transition relevance space to be those moments in the player's actions, or following his actions, during which the opposing player may begin his move. Should the latter begin his move before such a turn-transition space he might well be taken as interrupting the first player's move. Should he not take his move for an abnormally long period after such a turn-transition space, he may be taken as not having understood that it is 'his move'. The same applies to turn-taking in conversational interaction, argue the ethnomethodologists. If a conversationalist begins

speaking before the current speaker has reached a turn-transition space, he may be seen as interrupting. Should he not take his turn when it has been signalled that he may do so, this delay may also be taken as significant.

The transition space, roughly, is the environment of a turn's possible completion, at which possible transition to a next speaker becomes relevant. Although the transition space may begin a bit before the possible completion point, and last a bit into the beginning of a next turn, for our purposes here it may be thought of as the 'beat' that potentially follows the possible completion point of a turn.

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It is claimed, in addition, that utterances forecast when such a turn-transition space may be reached. They do so by means of their semantic, syntactic, and intonational structure. It is said to be because of this forecasting that 'next-speakers' are able to time the beginnings of their turn to coincide precisely with the end of the first speaker's turn.

All turn transfer is coordinated around transition-relevance places, which are themselves determined by possible completion points for instances of the unit-types.²²

There are various unit-types with which a speaker may set out to construct a turn. Unit-types for English include sentential, clausal, phrasal, and lexical constructions. Instances of the unit-types so usable allow a projection of the unit-type under way, and what, roughly, it will take for an instance of that unit-type to be completed. (Footnote: It is empirically evident from sequential materials, that projectability is the case; that is, we find sequentially appropriate starts by next speakers after turns composed of single-word, single-phrase, or single-clause constructions with no gap — that is, with no waiting for possible sentence completion.)²³

The notion of turn-transition relevance spaces can be taken as having a bearing on the study of discontinuity. A discontinuity which occurs at such a space is fundamentally a different sort of interactional phenomenon than one which occurs before such a space has occurred in the

speaker's utterance. And yet, purely from the point of view of the analyst's observation, they may appear identical. But from the hearer's point of view a transition-space discontinuity is seen as an opportunity to take a turn at talk, while a pre-transition-space discontinuity does not offer such an opportunity. What this points to is the importance of adopting a participant's perspective in the identification and classification of discontinuities. Otherwise, the analyst may make the mistake of grouping together two apparently similar phenomena, which, from the point of view of the participants to the interaction in which the discontinuities were observed, are nevertheless fundamentally distinct in their interactional import.

Unfortunately, to date the ethnomethodologist's notion of turn-transition relevance spaces has been only sketchily drawn. It is not yet clear what criteria will be or could be adopted to determine when they occur. In this case, they will not provide a central topic for this thesis, but will be referred to only during our discussion of the ethnomethodologist's more substantive work on repair.

(v) This thesis also will not go into the question of whether patterns and/or causes of discontinuity are language-neutral. Nor will it consider in any depth whether the strategies employed by speakers and hearers, when faced with discontinuities, are regulated by language-neutral principles of language use. Once again, this is not to deny the obvious interest of such a study. The assumption that at least some features of discontinuity are language-neutral is a pervasive one.

A generative grammar is part of a theory which accounts for X's performance as a speaker of English. But other factors intervene ...

One sort may be ascribed to human nature in general.

A speaker hiccoughs in the middle of an utterance. He might be speaking English or it might be French; the hiccough has got nothing to do with either. The same may hold in cases of hesitation. To establish this we would have to show that the incidence of pauses, 'er's', and so on could be predicted from general notions such as constituency or phrase boundaries.²⁴

This is a position which appears to be held by many of the psycholinguists to be discussed here. It may be linked to the more fundamental assumption that discontinuity is essentially an extra-linguistic phenomenon. That is, its source does not lie in particular languages, in linguistic competence, or in the essence of language but in the extra-linguistic: e.g. in discourse rules, in language processing operations, or in interactional pressures. While this more general assumption will be discussed, it will not be possible to deal directly with the question of the language-neutrality or culture-neutrality²⁵ of discontinuity. To do so would amount to writing a separate thesis involving a great deal of cross-cultural comparison and data from many languages.

The preceding paragraphs concern those phenomena which, for one reason or another, we have decided to exclude for our study of discontinuity in conversational speech. Other researchers have also made similar exclusions of phenomena which might otherwise have been deemed in opposition to continuity. Breath pauses and so-called grammatical pauses are frequently so excluded from the category of discontinuity.

(vi) Speakers are obliged to pause occasionally to draw air into the lungs. Because this is a biological necessity, many researchers do not consider breathing to be a disruption in the continuity of speech. Whether or not a pause during which the speaker inhales is deemed a

discontinuity will often depend on whether the breath is taken at a grammatical juncture. Should a breath be taken, say, at a sentence or major clause boundary, it is often considered that, since pauses — with or without breathing — tend to characterize such junctures, then the breath taken there cannot be considered a discontinuity. However, if, on the other hand, the breath is not taken at a location which the researcher wants to call a grammatical juncture, then the pause is generally attributed not to breathing but to hesitation. Breathing is pictured as occupying gaps whose occurrence is determined not by the necessity of breathing but by grammatical, hesitational, or other exigencies.

It is evident that the more important distinction here is between grammatical and non-grammatical pauses. The distinction between breath and non-breath pauses cuts across that between the grammatical and the non-grammatical. Indeed the whole question of whether a pause is taken to count as a discontinuity is inextricably linked to the notion of grammatical versus non-grammatical junctures. Furthermore, it goes without saying that the question of what is and what is not a grammatical juncture is not easily answered. Still, it has been a very important question for the study of discontinuity.

Some researchers, like Cooper and Paccia-Cooper, have defined the notion of grammatical juncture according to criteria derived from transformational grammar. Cooper and Paccia-Cooper propose a graded metric of grammatical boundary strengths based on the branching depth of particular constituents in a tree-structure analysis of syntax.

According to this metric, termed Branching Depth, a boundary is strong to the extent that a large number of nodes intervene between the most inclusive nodes bordering the boundary and the lowest flanking nodes in the tree.²⁶

Armed with such a method of grading grammatical boundaries, the authors perform experiments to see if branching depth coincides with pause length.

At the opposite extreme from Cooper and Paccia-Cooper are researchers like Goldman-Eisler who simply divides junctures into two types: the grammatical and the non-grammatical. Her aim with such a distinction is to separate hesitation pauses from grammatical pauses, which Cooper and Paccia-Cooper's ultimate aim is to see if studies of the length of pauses can be used as evidence for claims relating to the 'psychological reality' of competing grammatical models. Goldman-Eisler's concern to identify hesitation pauses only requires a binary distinction between grammatical and non-grammatical junctures so that pauses not at grammatical junctures may be identified as hesitation pauses. Since Cooper and Paccia-Cooper aim to use the data of pause length for the purpose of making delicate distinctions between grammatical models, a graded metric — based on the measurement as relative branch depth — is more suited to their tasks.

In contrast to the branch depth metric of Cooper and Paccia-Cooper, Goldman-Eisler designates all and only the following gaps in the flow of speech as grammatical junctures.

- (1) "Natural" punctuation points, e.g. the end of a sentence.
- (2) Immediately preceding a conjunction whether
 - (i) co-ordinating, e.g. and, but, neither, therefore, or.
 - (ii) subordinating, e.g. if, when, while, as, because.
- (3) Before relative and interrogative pronouns, e.g. who, which, what, why, whose.
- (4) When a question is indirect or implied, e.g. "I don't know whether I will".
- (5) Before all adverbial clauses of time (when), manner (how) and place (where).
- (6) When complete parenthetical references are made, e.g. "You can tell that the words — this is the phonetician speaking — the words are not sincere".

Those gaps which occurred but were not covered by the rules given above we shall call non-grammatical.²⁷

It is indeed difficult to say which of the two systems, Goldman-Eisler's or Cooper and Paccia-Cooper's, begs more questions or raises more theoretical or applicational dilemmas. And these are not the only two such systems proposed in the literature. It appears that almost every researcher who wants to distinguish between (1) pauses that occur for grammatical reasons and (2) pauses that occur for non-grammatical reasons will come up with a different system on which the distinction is based.

It should be apparent that even to enter into this debate would stretch the topical dimensions of this thesis beyond the limits of practicality. Furthermore, much of the discussion would both be distant from our primary concerns as well as, in the end, irrelevant to our conclusions. Consequently, just as our own definition of 'discontinuity' refers to those phenomena which, by one researcher or another, are taken to break the continuity of one speaker's speech, we will adopt whatever distinctions between grammatical and non-grammatical discontinuity are made in the various theories under discussion. That is, when we discuss e.g. Maclay and Osgood's account of hesitation pauses, we will adopt, for the duration of that discussion, those authors' own distinction between grammatical and non-grammatical pauses.

5. Identifying discontinuities

Finally, it should be emphasized that the main concern of this thesis is with the theoretical implications of how discontinuities are interpreted and conceptualized, not with the practical problems of identifying and counting discontinuities in a corpus of data. However, this is not to say that no such problems exist, nor that they do not impinge upon certain

aspects of the theoretical attention paid to discontinuity. For the final section of this introduction, we will attempt to provide a necessarily sketchy account of these problems.

In his paper "Some acoustic and grammatical features of spontaneous speech", James Martin discusses the hearer's perception of certain discontinuities (silent pauses, filled pauses, repeats, and false starts). His work is based on the assumption that, in decoding the incoming speech signal, the hearer edits or 'filters out' pauses and other discontinuities because they give false information about the speaker's message.²⁸ They give false information in the sense that they do not correspond to the form of the speaker's intended message and, if interpreted as so corresponding, the unedited message would be a distortion of that intended message.

To test this hypothesis, Martin developed a series of experiments in which hearers were asked to reproduce a speaker's utterance. Sometimes the subjects were explicitly told also to reproduce the pauses in the utterance. At other times, no mention was made of the pauses. Nonetheless, in all cases what Martin calls a 'hesitation shift' was observed. That is, in their reproductions of the original utterances, the hearers tended to reproduce less of those pauses which were within (major) constituent boundaries and produce more, even adding some pauses, at constituent boundaries. The 'shift' observed, then, is from (so-called) hesitation pauses at constituent-medial positions to juncture or grammatical pauses at constituent boundaries. The more the subjects were obliged to pay attention to the content of the utterances, the greater, in general, was the hesitation shift. The ordinary listening situation, i.e. when hearers concentrated on content rather than on the precise location of pauses, seemed to produce a relative uncertainty about the location of the pauses.

Summarizing these experiments, each required the subjects to attend simultaneously to pauses and messages in speech. Since speaking bias, memory and failure of instructions seemed to be largely ruled out, the hesitation shift suggested inattention to pauses during the linguistic organization of the input. Obviously subjects can judge pauses in a speech signal on the one hand, or remember its message on the other. The question perhaps is the way in which these two different mental operations are incompatible. It could be concluded that the subjects were alternating, however unevenly, between words and pauses as different segments of the signal. We preferred, however, the conclusion that the subjects were alternately attending to information at different levels, to information in the acoustic signal sometimes, but, mostly, to information at some higher level.²⁹

It would not do to rely too heavily on these experiments since their only indication of how the hearers perceived the incoming speech lies in how the hearers reproduced that speech. There was no control possible on the variables which might come between perception and subsequent reproduction. However, there is at least a fairly strong indication that what might acoustically be defined as a discontinuity is not necessarily identical to what might be auditorily or communicationally so defined. That is, there is evidence that no one-to-one correspondence exists between 'real' discontinuities and what might count as a discontinuity in communication.

As this is a familiar dilemma in the interface between acoustic phonetics and linguistics, it is interesting to compare this evidence with the 'emic/etic' distinction as used in orthodox phonology. In the same way that the acoustic definition of discontinuity appears not to correspond to a definition of discontinuity based on communicational significance, the phonologist has found great difficulty in defining phonemes in purely acoustic terms. It appears that the variation can be too great between what the phonologist would claim to be two instances of the same sound. In other words, just as the subjects in Martin's experiments appear to alter the location of discontinuities, ordinary

hearers are said to disregard differences between acoustic signals in treating those signals as tokens of the same phoneme-type. There is no definable correspondence between what language-users appear to take as communicationally identical sounds and what sound spectrograms tell us about the acoustic nature of sounds. It is on this ground that the difference between the phonemic and the phonetic is based. Consequently, the phonologist studies the phonemic differences in a language, as evidenced in the communicational behaviour of the speakers of that language, without relying on an acoustic basis for identifying those differences. Is such a solution possible for discontinuities?

The methodological import of this question should not be ignored. The issue at hand is not simply rephrasable as the following "Does the ordinary hearer in everyday speech situations perceive the same distinctions on the acoustic signal produced by speech as we, scientific observers, do?". Although this question is, in itself, of no small importance to the study of verbal behaviour, there is more to it than that. For the issue lies on the border between theory and method.

The language-theorist has to do more than simply give conceptual definitions of the terms of his theory. If he wants his theory to be of any practical use, he has to give 'instructions' as to how his theory is to be applied in the analysis of what is observable in language-behaviour. That is, in addition to defining the term X as "a kind of $\emptyset$ ", the theorist must give some indication to those analysts who are to employ his terminology as to how to recognize if and when an X occurs. The notion of the phonemic would not have gained the familiar use which it has if it had simply been defined (e.g.) as the minimal unit of language structure. For the notion of the phoneme to be a practical methodological tool, a procedure must be given as to how to identify phonemes. To the (greater

or lesser) satisfaction of linguists, the practice of contrastive phonemic analysis fulfills this requirement. It is possible for the phonologist to take the notion of the phoneme and apply it, by means of contrastive analysis, to observable verbal behaviour and come up with some sort of (more or less) acceptable analysis of that behaviour in terms of its minimal phonemic units. Theory is translated into practice.

This is not, of course, a specific point relating only to linguistics or to the sciences of behaviour. It applies, in general, to the explanation of all terms. That is, giving someone a definition of a word is not enough if that definition does not enable its receiver to then use the relevant word in the same ordinary way in which those who give the definition use it. Although a definition cannot possibly detail all the possible applications of the word, it must do enough to point its new user in the direction of appropriate applications. It must guide him to some clear uses on which he may make creative analogic decisions regarding new uses.

So, the problem facing the definition of discontinuity is as follows. Given that discontinuity is defined as the opposite of (whatever is taken to be) continuity, how is the discontinuity researcher to extend the use of the term when faced with evidence such as that presented by Martin? Is he to say that any acoustically perceptible disruption in the continuity of the speech flow is, by definition, a discontinuity? Or should he say that only what hearers recognize as disruptions of continuity should be so defined, even if these perceived discontinuities do not correspond to any such acoustically perceptible disruptions? The definition of discontinuity which we have given, and which is modelled on the practice of the researchers we are studying, does not guide us in this decision.

The phonologist's distinction between the phonemic and the phonetic relies essentially on the language-user's ability to distinguish between

the meanings of the expressions of his language.³⁰ That is in order to find out the phonemic contrasts in a particular language the linguist must find out from the users of that language which phonetic (i.e. acoustic or articulatory) differences in the speech of that language can serve to make two expressions different in meaning. It may be irrelevant to the phonemics of the language if e.g. a particular medial vowel is pronounced a variety of ways as long as no two of those varieties, substituted in the same contexts, could lead to a language-user saying that the two expressions do not mean the same. For instance, the sound /æ/ may be pronounced differently by different speakers and in different sequential as well as situational contexts. (Consider the difference between the pronunciations of /æ/ in "Is 'that contraption 'what you call a /hæt/" and in "Yes, 'it was my mother's /hæt/".) However, this does not mean that two different phonemes are involved, since in spite of the different sounds produced in the context /h – t/ the sequence /hæt/ retains the same meaning (according to the orthodox notion of meaning) regardless of which sound is substituted.

In other words, while phonetic differences may be identified on an acoustic or articulatory basis, such differences do not count as phonemic unless they may be seen to contrast from the perspective of meaning-differentiation. Phonetic differences which do not serve to differentiate non-synonymous expressions do not have phonemic status. They are not held to be linguistically significant.

We might imagine applying the etic/emic distinction in a similar way in order to differentiate between different types of discontinuities. Some researchers have, on a fairly arbitrary acoustic basis chosen 0.25 seconds as the limit which distinguishes between the pause and the non-pause silent interval. This was the length chosen by Goldman-Eisler

in many of her studies of pausing in English speech. The professed purpose in drawing such a boundary is so that the interval produced by a stop consonant can be excluded by definition from the class of pauses. Following Goldman-Eisler, researchers have since adopted 0.25 seconds as the appropriate boundary. Of course, Goldman-Eisler did not pick such a figure randomly.

Altogether we may consider three types of gaps at which articulatory movement comes to a halt. There is (1) the discontinuity of phonation which occurs in articulatory shifts, e.g. when two plosives or stops follow each other (e.g. top part, tat tat). In such a case the breath stress is completely stopped and then released anew. (2) There is the discontinuity of phonation which is unrelated to any requirements of the articulatory processes themselves and is due to hesitation, and (3) there is the gap in speech when the phase of expiration comes to an end and a new supply of air is inhaled.

The first phonetic stoppage is part of articulation itself and the gaps in phonation are determined by the need to adjust the position of articulation. To be quite certain that no such gaps are included in our record, breaks in phonation of less than 0.25 seconds were not considered as discontinuities. This might mean the loss of some data, but it ensures that clear separation of hesitation pauses from phonetic stoppages.³¹

What is implied by the criterion of 0.25 seconds is that there are no silences due to the speaker's actual hesitation which are shorter than 0.25 seconds and that any silence longer than 0.25 seconds is either due to breathing requirements or to the speaker's actual hesitation. The word "actual" is twice used here to point out that it is some 'psychologically real' state of hesitation in the speaker's mind which is at issue and not simply whether the speaker is perceived by hearers as hesitating. Goldman-Eisler makes no explicit claims about when a speaker will be perceived as hesitating.

Let us imagine that Goldman-Eisler had not adopted this criterion for identifying hesitation pauses but had instead chosen some sort of 'emic'

criterion: for instance, by asking informants to identify all cases of speaker hesitation. Such a move would be along the same lines as the method used by the phonologist to identify phonemic contrasts, except that the criterion of meaning-differentiation, used by the phonologist, is here replaced by a criterion of perceived hesitation. On the assumption that what the phonologist/linguist is interested in is how the sounds used by speakers are bearers of meanings, the choice of a hesitation criterion — given that researchers like Goldman-Eisler take pauses as directly related to hesitation — seems an appropriate one. Thus, we may imagine the researcher working under this 'emic' paradigm of discontinuity studies to apply the term "discontinuity" only to those stretches of speech (occupied or not by actual silence) which informants have identified as periods of speaker hesitation. What would be the outcome of such an 'emic turn' in discontinuity studies?

Let us suppose, for the sake of argument, that our emic researcher was able to obtain fairly consistent agreement between informants as to when speakers were hesitating. (For if such agreement were not obtainable the case for an emic approach would collapse in the same way as orthodox phonology would collapse if people could never agree whether two expressions meant the same³²). What would this agreement show?

It would not show that it was possible to identify, by analysis of their speech, when (and certainly not why) speakers were hesitating. For there is no guarantee that what informants will identify as a hesitation pause is in fact due to 'psychologically real' hesitation. (Nor can we, given Martin's evidence, even say that an 'acoustically real' discontinuity has been identified.) In order to be sure that what the informants identified as a hesitation pause was indeed that, the researcher would have to have some sort of privileged access to the

speaker's mind (as well as some method by which, when he got a good look at his mind, he could tell what hesitation was). But, as it is, he can have no access to the 'real facts' about speaker hesitation. Consequently, he cannot know if his informants are or are not mistaken in their identification of speaker hesitation.

All the researcher would have would be information on when certain groups of informants were willing to use the expression 'hesitation' in talking about the speech of a variety of speakers. This could not be taken as a guarantee of the communicational effect of certain aspects of the speech flow. That is, we could not infer from our informants' reactions that they were all reporting on the same communicational effect — what we may call an effect of speaker hesitation — produced by the speaker's speech. Nor could we know exactly what in the speech signal was leading to our informants' reactions.

In the end, all the emic researcher would have is evidence on a particular language-game involving the use of the term 'hesitation' and related terms. He would not know what, in the hearer's perception of speech, was the cause of the pattern of use reflected in the language-game. Nor would he know what in the speaker's output — let alone in the mental state accompanying that output — was the source of his informants' consistent use of the term 'hesitation'.

In other words, what the emic researcher is left with after his experiments is not evidence on the link between aspects of speech flow and perceived hesitation — which is, after all, what he was after. He would not, in the end, have arrived at a definition of the hesitation pause based on the hearer's perceptual identification of it. Instead, he would only have (what to him would be useless) evidence on patterns of usage, or language-games, with the term 'hesitation'.

On the other hand, a researcher might well decide to pursue an extreme form of the etic approach. That is, he could study the distribution of discontinuities without any reference to the hearer's perception of them or to their purported mental causes. In this case a new problem would arise: what counts as a discontinuity? Neither the full-blown emic criteria discussed above nor Goldman-Eisler's criterion of 0.25 seconds could be used to resolve this question. But, on what grounds could any other criterion be chosen? The limit of 0.25 seconds could not be used for the pause, except by arbitrary fiat, since it is based on a question-begging distinction between the hesitation pause and 'phonetic-stoppage'. No minimal length limit could be fixed, other than purely arbitrarily. The same would apply to a maximum length limit. Nor would there be any basis on which to group the silent and filled pauses as instances of the same type of phenomenon. Indeed, there would be no ground for grouping together various sounds made by the speaker all as instances of 'the filled pause'. And certainly no reason would be apparent to group false starts, corrections, repairs, repeats, etc. as varieties of one and the same phenomenon: viz. discontinuity. From this 'pure' etic point of view every moment of verbal behaviour is distinct from every other.

The general point here being made is that a method of identification which takes only acoustic measurement as its criterion — rather than any of a number of possible emic criteria — would have no grounds for grouping under the same heading all the speech phenomena which might be included under discontinuity. Without an emic criterion, an etic method is left only (a) with the arbitrary, and so undefendable, choice of a criterion or (b) with chaos. Yet if the theoretical concept of discontinuity is to be applied to the practical study of speech, there has to be some non-arbitrary way of grouping the phenomena observed along lines of identity

and non-identity. We have to be able to say that those two things are the same and that those other two are not. But without a justifiable criterial perspective we cannot even divide up the observed (in this case, human speech) into identifiable 'things' or units, let alone decide whether two such things are the same or different.

* * *

Given the problems facing any attempt to identify discontinuity, it is not surprising that many of the researchers who will be discussed have attempted to side-step these problems. They have done so either by not giving any guidelines how their theoretical categories are to be translated into methods of identification or by adopting a pre-established framework for identifying discontinuities in speech. In the latter case, an appeal to a comparison between written and spoken language has generally been made. In every case, these methodological decisions (or non-decisions) have had repercussions for the interpretation and conceptualization of discontinuity.

NOTES

Chapter One

1. Chomsky, Aspects of the Theory of Syntax, 1965, p.3.
2. Crystal and Davy, Advanced Conversational English, 1975, p.42.
3. Crystal and Davy, Advanced Conversational English, 1975, p.13.
4. cf. Crystal, Prosodic Systems and Intonation in English, 1969, pp.171-2.
5. see Chapter.4.
6. cf. Wittgenstein's use of the term 'language-game' in his Philosophical Investigations:: for example, paragraph 7.
7. Bloomfield, Language, 1934, p.144.
8. e.g. Chomsky, Aspects of the Theory of Syntax, 1965, p.4.
9. Stross, "Speaking of speaking: Tenejapa Tzeltal metalinguistics", in Bauman, R., and Sherzer, J. (eds.) Explorations in the Ethnography of Speaking, 1974.
10. Chomsky, Aspects of the Theory of Syntax, 1965, p.3.
11. Chomsky, Aspects of the Theory of Syntax, 1965, p.4.
12. cf. for instance, Hockett, A Course in Modern Linguistics, 1958, pp.143-4, and Goffman, Forms of Talk, 1981, p.22.
13. Chomsky, Reflections on Language, 1975, p.56ff.
14. Chomsky, Aspects of the Theory of Syntax, 1965, p.3.
15. Lounsbury, "Transitional probability, linguistic structure, and systems of habit-family hierarchies", in Osgood, C., and Sebeok, T. (eds.) Psycholinguistics: A Survey of Theory and Research Problems, 1954, p.99.
16. O'Connell and Kowal, "Prospectus for a science of pausoloty", in Dechert, H., and Raupach, M. (eds.) Temporal Variables in Speech, 1980, p.8.
17. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.381.
18. Bernstein, "Linguistic codes, hesitation phenomena and intelligence", J. Gould (ed.), Penguin Survey of the Social Sciences, 1962, p.43.
19. Voss, "Hesitation phenomena as sources of perceptual errors for non-native speakers", Language and Speech, 22, 1979.

20. Goodwin, "The interactive construction of a sentence in natural conversation", in Everyday Language, Psathas, G. (ed.), 1979, p.106.
21. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.366, fn.2.
22. Sacks, Schegloff and Jefferson, "A simplest systematics for the organization of turn taking in conversation", in Schenkein, J. (ed.) Studies in the Organization of Conversational Interaction, 1978, p.15.
23. Sacks, Schegloff and Jefferson, "A simplest systematics for the organization of turn taking in conversation", in Schenkein, J. (ed.) Studies in the Organization of Conversational Interaction, 1978, p.1.
24. Matthews, Generative Grammar and Linguistic Competence, 1979, p.33.
25. cf. on the question of cultural differences between the use of discontinuities, Hymes, Foundations in Sociolinguistics, 1977, pp.60-61.
26. Cooper and Paccia-Cooper, Syntax and Speech, 1980, p.171.
27. Goldman-Eisler, Psycholinguistics, 1968, p.13.
28. Martin, "Some acoustic and grammatical features of spontaneous speech", in Horton, D., and Jenkins, J. (eds.) The Perception of Language, 1971, p.48.
29. Martin, "Some acoustic and grammatical features of spontaneous speech in Horton, D., and Jenkins, J. (eds.) The Perception of Language, 1971, p.48.
30. In fact, there have been non-meaning-based distinctions between the phonemic and the phonetic: cf. B. Bloch, "A set of postulates for phonemic analysis", Language, 24, 1948. Most of contemporary phonology is, however, meaning-based. Furthermore, there are existent arguments to the effect that even the (so-called) non-meaning-based analyses do, in fact, make covert appeals to meaning: cf. Harris, Synonymy and Linguistic Analysis, 1973.
31. Goldman-Eisler, Psycholinguistics, 1968, p.12.
32. cf. Harris, Synonymy and Linguistic Analysis, 1973, Chapter 2.

CHAPTER TWO

Psychological Investigations of Pauses

More than any other discipline, psychology has interested itself in discontinuity as an important feature of verbal behaviour. This interest has tended to focus on pausing in speech. Since the early 1950s, close to a hundred published reports on pauses have been produced by psychologists. Many of these researchers, realizing that their output on pause behaviour comes close to qualifying their study for the status of a separate academic discipline, now call themselves 'pausologists' and their field 'pausology'.¹ Others, intent that their investigations remain an integral part of psychology with psychological aims, limits, and methods, have refused this label. It is possible to see the dividing line in this debate as running parallel to a division between the implicit research goals of the investigators. Some — those who are not so willing to accept the label 'pausology' — want the study of pauses to serve as a means by which to attain results in the more traditional concerns of psychology. Others — many of whom accept the new label and the implied status of a new discipline — argue that pauses should be studied in their own right, and not merely as fodder for other theoretical concerns.

The effect of this division can be seen across the development of psychological investigations of pausing. This is not to say that the debate has been explicit since the beginning of these investigations. It has not been. But, it is nevertheless reflected in the nature of

those investigations, in what is counted as evidence, and in what is taken as given. Only very recently has this division come out into the open.

This division cuts across the three main areas of the psychological investigation of pauses. Some investigators have studied pauses from a socio-psychological view, taking the pause to be one of the many tools with which speakers can manage verbal intercourse. Others have taken pausing to reflect the emotional state of the speaker. By far the largest group of investigators have worked under the assumption that the patterns of silence and verbalization in a speaker's utterance reflect his cognitive processing of the utterance. That is, they take pauses to provide a window on the mind.

These three areas will be treated separately in this chapter. No claim is made that the whole of the field has been covered. In the interdisciplinary approach adopted in this thesis, this would not be feasible. However, there is an effort to include those studies which are treated in other, later studies as having been particularly significant. If some explanations of pause behaviour are ignored here, they have also been ignored in the rest of the literature itself. It is not altogether easy to divide the work in this field into the three areas mentioned. Some papers are of import to all three areas. This means that a certain amount of cross-referencing is necessary and that the arguments presented in some papers end up being outlined before their time is due. This is especially the case with the important Maclay and Osgood paper. For this reason, the discussion of their work has been placed first, to serve, by example, as an introduction to the field as a whole.

1. Maclay and Osgood

One of the four or five most influential studies of pauses is presented in an article by Maclay and Osgood (1959): 'Hesitation phenomena in spontaneous English speech'. Many of the more recent studies have taken this article as their starting point, sometimes devoting an entire study to a single claim or hypothesis presented by Maclay and Osgood.

Their paper begins with a discussion of previous work on the topic. They criticise the short shrift which pauses have received in linguistic studies. Linguists tend to take pauses as insignificant events. If any attention is paid to them, only juncture or 'grammatical' pauses are considered. Little or no mention is ever made of what the psychologist calls the 'hesitation pause'. However, Maclay and Osgood agree with Bloomfield that 'non-linguistic factors' are what cause hesitation. Thus they tend to turn away from a structural linguistic approach to pause analysis. Instead, as many others do, they focus their search on psychological (or socio-psychological) determinants of hesitation pauses.

They discuss the work of Mahl, Lounsbury, and Goldman-Eisler, neither disagreeing with any of their findings nor shedding any new light on their work. Instead, the work of these three is presented as prefatory ground-clearing on which to base their own more detailed study of the distribution of various hesitation types. Because of their undeniable influence on subsequent work, as well as their non-partisan status regarding the various causal theories of pausing (a status not enjoyed by Mahl, Lounsbury, or Goldman-Eisler), an examination of Maclay and Osgood's paper on hesitation phenomena will best serve as an introduction to the present chapter.

They reduce Mahl's categorization of eight disturbance types to four.

1. repeats: all repetitions of any length which are judged to be semantically non-significant.

e.g. I I saw a very big boy

and not

I saw a very very big boy^A

: The repetition in the second of these examples would be judged semantically significant. Presumably, this would not be the case in the first example, but the authors do not make clear whether intonation is taken into account by their judges (the authors themselves?). If a strong accent and pitch change were added to I when repeated it could serve to signify that it was I who saw the very big boy and not someone else. Maclay and Osgood do not clarify how the judges are supposed to interpret the term 'semantically significant' in such and similar cases.

2. false starts (FS): "All incomplete or self-interrupted utterances"²

e.g. (a) I saw a very ... have you ever eaten rock candy?

(b) I saw a very big ... a very small boy.

They distinguish between retraced false starts (e.g.

(b)) and non-retraced false starts (e.g. (a)).

However, they give no clue for deciding whether (c) is to be considered.

(c) I saw a very small ... I saw a very big boy.

Is (c) to be categorized as a non-retraced FS followed by a hesitation-free utterance? If so, something about (c) would be missed. Or is it to be labelled a variety of retraced FS? In this case, should there not be a distinction drawn between retracing to the beginning

of a sentence and retracing towards, but not all the way to, the beginning. Only in the latter case is the post-disruption utterance syntactically linked with the utterance before the disruption. Their definition of retracing as when "the speaker back(s) up in an attempt to correct one of the words he had already used" is no help here.^B

3. filled pause (FP):^C "All occurrences of the English hesitation devices [ɛ, æ, r, ʔ, m]".⁴ Even though this might at first appear to be a perfectly straightforward method of identifying filled pauses — that is, by enumerating the members of the class — it does suggest an important, but implicit, reliance on context. Not every instance of the listed sounds would one want to call a filled pause. In the phrase /ʔ c æ t/ neither the /ʔ/ nor the /æ/ would appear to be functioning as a FP. But one has to rely on context in order to arrive at this conclusion. For instance, if the preceding context had been We bought ..., few would want to label the subsequent /ʔ/ a filled pause. But, if the context had been We bought a large ..., the /ʔ/ following large would more readily be labelled a filled pause. Still, here again contextual consideration could be expanded. It is not difficult to imagine a pattern of pitch and stress combinations which would lead many judges to take We bought a large ... /ʔ/ cat as an instance of retracing so that, in a manner of speaking, the final 'target sentence' would be We bought a cat. Furthermore, neither is it difficult to imagine examples of this type of ambiguity for Maclay and Osgood's other filled pause devices. Whether a particular

sound is to be taken as a filled pause is not a question which may be decided by reference to acoustic or auditory criteria alone. Context — formal, semantic, interactional, ... — must be taken into account. In this sense, whether a sound is to be taken as a filled pause (or, for that matter, a repeat or a false start) is also not something that a hearer can decide at the moment of hearing the sound. This decision requires waiting until further sounds of the utterance have been produced in order that the appropriate relation to context may be determined. (Presumably, this is precisely what transcriptionists usually, but not always, do).^D

4. Unfilled pause (UP): Maclay and Osgood rely on a subjective determination of unfilled (or silent) pauses. Whenever it was judged that there was "an abnormal hesitation in speech that could not be referred to the three previous categories", this was labelled an unfilled pause. This category includes what the authors call the "non-phonemic lengthening of phonemes."^{5, E}

The major part of the Maclay and Osgood paper comprises a distributional analysis of their four categories of speech disturbances. Their data base consists of a 50,000 word sample excerpted from the proceedings of a conference on psycholinguistics held at the University of Illinois in 1955. Any utterance under 80 words was omitted from the corpus. This left 163 utterances, of which the mean length was 309 words. (Presumably, by 'utterance', the authors mean one speaker's turn at talk, irrespective of sentence boundaries.) It could, of course, be objected that such an exclusion considerably de-naturalizes the data, with the result being that the end product consists entirely of monologues. Consequently, the

influence that dialogic structure might have on hesitation-type distribution would be missed. In fact, this pragmatic manoeuvre reflects one of the assumptions underlying most of the psycholinguistic investigations of pause phenomena. That is, hesitation is assumed to be psychologically determined. Hesitation phenomena are thought to provide the psychologist with 'a window on the mind'. This assumption, and the experimental studies that have been generated by it, will be considered in detail later in this chapter.

Maclay and Osgood make no mention of the format of the discussions included in their sample. It is not said whether a chairman was regulating turns at talk, whether hands had to be raised in order to secure the floor, whether more than one person was talking at any one time, whether some speakers were entitled to a privileged place on the podium while other speakers were dispersed about the hall.

The distributional location of each of the four types of speech disturbance was determined as follows:

- 1 - Repeats were classified according to the type of word repeated.
- 2 - False starts were classified according to the type of word immediately preceding the disruption, or by the word in which the disruption occurred if it occurred internally.
- 3 - Filled pauses and unfilled pauses were analysed according to both the type of word they preceded as well as the type of word they followed.

The type of word was determined by the Fries⁶ classification of English word types.

A variety of objections may be raised against this classificatory system. Only two will be mentioned here. Why are repeats not classified according to the word type which they precede?

Oh, look The the distributor cap has a crack in it.

In this example, it might very well be more revealing to note that the repeat occurs before distributor cap rather than after look. Such an analysis would be more relevant to a speech encoding theory which assumed that hesitations were linked to lexical selection. The same question is also applicable to the locational classification of false starts.

The results of their distributional analysis are as follows:^F

1. Repeats typically (2/3 of examples) involves function words (Fries' categories A-J) (with the exception of subject personal pronouns) and only the function word itself was usually (71%) repeated.
2. False starts typically (2/3 of examples) followed lexical words (Fries categories 1-4).
3. Retraced corrections of lexical items were found usually to include an antecedent function word, but retraced corrections of function words usually involved correction of the function word only with no other retracing.
4. Lounsbury's hypothesis #1 (cf page 7) of this chapter) was confirmed that both UP and FP occurred more often before a lexical word than before a function word.
5. 50% of pauses (filled and unfilled) occurred within phrasal boundaries.
6. FP and UP were found to be neither in free variation nor in complementary distribution. But, of the two, FP is the more likely to occur at phrase boundary before a function word,

and UP is more likely than FP to occur within a phrase before a lexical word.

7. For most speakers, there were more occurrences of FP and UP than repeats and false starts.

Maclay and Osgood conclude from these results that repeats, occurring in the same locations as UP and FP, have the same function as these latter: "providing time for selection among diverse lexical alternatives". They take the results as suggesting that the encoding units of speech are phrase-like: function word(s) plus lexical word. The fact that repeats and false starts both tend to recycle to phrase boundary is taken as evidence of such phrasal units.

However, they also argue that the skewed distribution of FP and UP (see result #6) suggests that there are two layers of encoding: phrasal and lexical. They picture the speaker as first being faced with the choice of a phrase type. Here both grammatical and semantic decisions would be required. If he is unsure at this stage, the tendency is to delay with a FP. Secondly, he should choose a lexical item. Uncertainty here may arise as late as after the utterance of the function word(s) beginning the phrase, since selection of the phrasal type would include function words but not lexical words. If the speaker hesitates before lexical selection, this is typically done with an UP.

Although Maclay and Osgood take pauses of all types to be psychologically determined, they argue that the choice between at least one pair of pauses — the FP and UP — may be interactionally determined.

Let us assume that the speaker is motivated to keep control of the conversational 'ball' until he has achieved some sense of completion. He has learned that unfilled intervals of sufficient length are the points at which he has usually lost this control — someone else has leapt

into his gap. Therefore, if he pauses long enough to receive the cue of his own silence, he will produce some kind of signal (/m/, /ər/, or perhaps a repetition of the immediately preceding unit) which says, in effect, 'I'm still in control - don't interrupt me'.⁷

Such a hypothesis would account for the relatively larger number of FP than UP occurring at phrasal boundaries. This hypothesis has stimulated a reasonable amount of controversy. Related to this hypothesis is a further claim: the more prolonged the unfilled pause, the greater the tendency for a FP or repetition. These hypotheses and the resultant controversy will be the subject of the next section.

2. Filled pauses and floor-holding

In a series of papers, published between 1969 and 1974, Mark Cook and Mansur Lalljee have criticized certain points of Maclay and Osgood's paper. Cook and Lalljee's criticisms, primarily concerning the distribution and function of filled pauses, are based on experiments carried out at the Oxford psychology laboratory and elsewhere.

In the first of these reports, Lalljee & Cook (1969),⁸ the authors set about to test the claim that filled pauses are used by speakers in order to avoid being interrupted. Their experiment is designed to see if speakers who want to continue speaking use a high frequency of filled pauses in the face of possible interruption. A colleague of the authors conversed with each of the subjects in one group: the 'high-pressure group'. He was told to interrupt each subject three times in the first two minutes of conversation and thereafter to start speaking as soon as the subject stopped or paused. With a second, 'low-pressure', group he was told not to interrupt at all. The subjects in the low-pressure group were told that they were to converse with another student subject on a topic over which they were in disagreement. The high-pressure group was

told the same thing with the additional information that "the other subject is a very fluent speaker, so you might have difficulty getting a word in. Nevertheless, we would like you to do your best to get a fair share of the conversation...".⁹ If Maclay and Osgood's hypothesis regarding the floor-holding function of filled pauses were true, Cook and Lalljee supposed that the subjects in the high-pressure group — faced with a co-conversationalist who interrupted often and was said to be 'a very fluent speaker' — would use a large amount of filled pauses. The low-pressure group would use relatively fewer.

These predictions were not confirmed. The filled pause rate was found to be much the same for both groups. Lalljee & Cook attribute the conflicting results found by Maclay and Osgood to be due to these latters' use of utterances of a minimum 80 words in length: that is, monologues. In dialogue, Lalljee & Cook conclude, FP do not seem to have a floor-holding function.

In 1974, a published paper by Lalljee & Cook returns to Maclay & Osgood's filled pause hypothesis. After summarizing their previous tests of the hypothesis, the authors describe a new experiment designed once again to put the FP floor-holding hypothesis to the test. They speculate that, if the hypothesis were true, then "if the subject had entire control of the situation by being allowed to turn off the recording device when he wanted, filled pauses would decrease".¹⁰ Furthermore, the subject was to be alone, uncontested in his floor-holding by a co-conversationalist. Instead, the subjects were asked to reply to a set of printed questions, alone in a room, and able to turn on or off the tape recorder at will. However, although they were able to turn off the one recorder which they could see, another microphone would be on continuously, picking up whatever FP they produced while the visible recorder was off.

The results produced by the subjects in this group were compared with two control groups. In one of these the subjects were asked the same questions as in the first group but were faced by an ever-present interviewer and a continuously recording microphone. In the last group, the subjects conversed in an 'unstructured manner', with an experimenter, on the topics used for the other two groups. Presumably, if those subjects who were speaking alone and who were in control of the recording device tended to use filled pauses significantly less than the other two groups, Maclay and Osgood's floor-holding hypothesis would be confirmed.

"If FP are related to interruptions, one would expect them to be higher during the conversation than during the interview. In the alone (group), FP are expected to be the lowest".¹¹

No significant differences were detected for filled pause frequency between the three groups. The authors conclude that Maclay and Osgood's hypothesis is incorrect, and that explanations of filled pause rate should be sought elsewhere.

In an article published in 1977 in the British Journal of Social and Clinical Psychology, Geoffrey Beattie takes up the topic of Maclay & Osgood's hypothesis regarding FP floor-holding. He dismisses the conclusions of Lalljee & Cook (1969) on the grounds that their experiment was based on the questionable assumption that subjects would desire to avoid interruption. That is, as Beattie rightly points out, the fact that subjects are told "we would like you to do your best to get a fair share of the conversation"¹² does not entail that the subjects will genuinely follow those instructions. Being instructed to desire or intend something does not mean that one will so desire or so intend.

Furthermore, as Beattie points out, Lalljee and Cook neglect Maclay and Osgood's other floor-holding device: repetition. It may be that speakers in their experiments are attempting to hold the floor, but are using repetition, rather than FP, to do so. Such behaviour would be confirmatory of the floor-holding hypothesis. Similarly, in Lalljee and Cook (1974) there is no consideration of repetition.

In order to avoid the artificiality of Lalljee and Cook's experimental contexts, and the drawbacks that such contexts entail, Beattie decided to base his analysis on naturally occurring interactions. This included filmed supervisions between a graduate student (as supervisor) and an undergraduate, as well as the filming of participants in a seminar.

Interruptions were found significantly more probable during an unfilled pause than during speech itself (47 v.25). Seventy-one percent of filled pauses and repeats were preceded by an unfilled pause, and the probability of interruption was determined to be considerably lower for filled pauses as compared to unfilled pauses. Indeed only once did an interruption occur any time within 600 msec after a speaker uttered a filled pause. Based on these results, Beattie concludes that Maclay & Osgood's hypothesis concerning the floor-holding function of filled pauses is confirmed.

There is a disturbing similarity between all the discussions of filled pauses and floor-holding. It is never made entirely clear where the filled pauses tested occur. Maclay and Osgood found the FP occurred 54% before lexical words and 46% before function words. Furthermore, FP are found to occur more frequently at phrase boundaries, rather than within phrases. They therefore speculate that FP and repeats "occur

just before points of highest uncertainty, points where choices are most difficult and complicated."¹³ In the Lalljee & Cook papers as well as in Beattie (1977), no mention is made of where the filled pauses analysed occur.

However, there is reason to believe that their location is highly relevant to determining, at any one time, whether the FP has a floor-holding function. To consider extremes, a FP at an obvious sentence boundary — say at the end of the question Are you doing anything tonight? — could have a much greater effect regarding floor-holding, than would a FP occurring within an utterance, or even more so within a phrase. For instance, in the following utterance Have you seen the ... uh new Metro?, there is no requirement for the FP to take a floor-holding role, since the syntactic incompleteness of the utterance after the is already sufficient to signal that the speaker probably intends to go on. But in the first example, both syntax, and presumably intonation, are complete, so some such signal as a FP is needed to delay the hearer coming in with his answer.^G

Such an argument leads to the conclusion that the filled pause does not have a universal floor-holding function, since it occurs (according to Maclay and Osgood) primarily within syntactic constructs where no such function is required. However, it leaves open the possibility that, at certain contextually defined positions, it could very well have a floor-holding function. The Lalljee-Cook papers, because they do not discriminate between different placements of FP, do not — whatever the other merits of their experiments — impinge upon such a possibility. The FP floor-holding hypothesis must be contextualized before its validity can be checked.

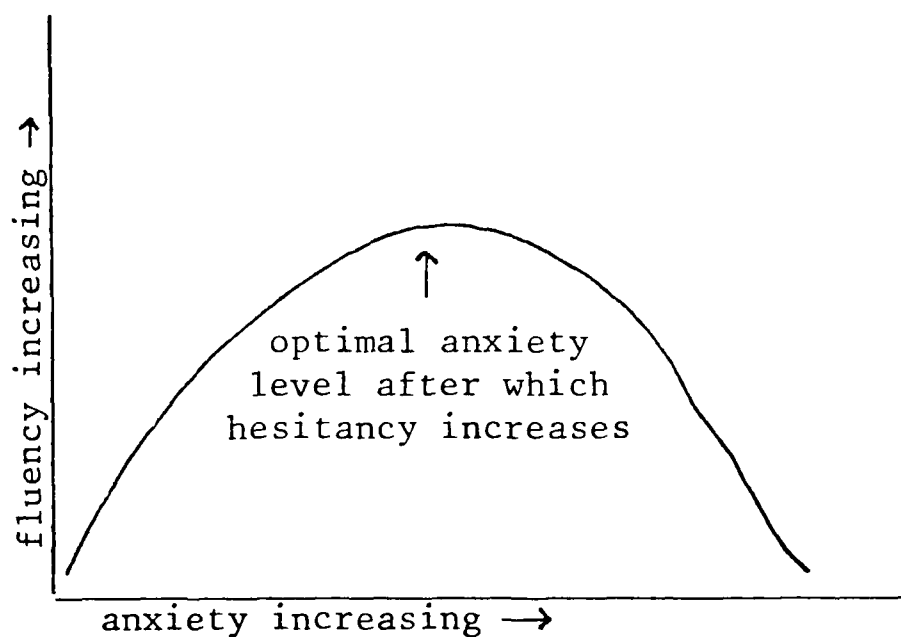
3. Anxiety and hesitation

There have been a fair number of studies attempting to relate pausing to levels of anxiety in the speaker. George Mahl, the originator of these studies, began by speculating that since anxiety had otherwise been shown to lead to disruption in complex behaviour, it might also be one of the causes of hesitancy in verbal behaviour.

The investigations stimulated by this hypothesis have produced ambiguous and even conflicting evidence. Most of these studies have differentiated between predispositional anxiety – i.e. an individual's 'natural' disposition to anxiety – and situationally-induced anxiety. There has been some evidence to show that the more predispositionally anxious a subject is the less he is likely to produce filled pauses.¹⁴ The same has been found, albeit in inconclusive experiments, for the relation between predispositional anxiety and silent pausing. For situationally-induced anxiety, results¹⁵ have tended to show that, while filled pauses do not appear to increase with anxiety levels, silent pauses do. This, regarding predispositional anxiety, Mahl's hypothesis relating hesitation and anxiety would appear to be disconfirmed (indeed the results would appear to show the converse of this hypothesis to be true). For situational anxiety, only the results for silent pauses are positive.

In a survey of work on this topic¹⁶ has attempted to provide an explanation which does not in fact conflict with Mahl's original hypothesis. He argues that, to a certain extent, anxiety will result in cortical arousal. Consequently, verbal production will increase and, concurrently, a resultant drop in the level of pausing will be observed. But this only applies up to a certain threshold level of anxiety. Beyond this level increases in anxiety will have a disruptive effect.

That is, once an individual's anxiety passes a certain level it no longer has the beneficial effects regarding verbal output. Instead it begins to hinder the individual's verbal ability, thereby causing hesitancy. Most of the results obtained would conform to this explanation. The only exception would be that situationally-induced anxiety does not, or has not been shown to, lead to an increase in filled pauses. With the exception ignored, Murray's explanation suggests a plot of the relations between anxiety and fluency that would look like the following.



Besides the confusing results of the test experiment, the major problem with Mahl's hypothesis and the attempts to verify it is that anxiety is not independently measurable. It might be thought that independent observers could judge anxiety level: but there is no guarantee that they will not base their judgements, at least in part, on pause level, thus producing a circular method of identification. Mahl has used measurements of palm sweat as an indication of anxiety level, but no proof has been given that anxiety is objectively correlated with palm sweat (nor is it easy to see how such proof could be given). Siegman and Pope¹⁷ used self-assessments by the subjects themselves in order

to estimate anxiety level. Such a procedure is questionable both because of the tendency of subjects to help achieve what they perceive to be the experimenter's goal and because of the impossibility of telling if each subject is grading his privately felt anxiety in the same way as every other. Finally, a synthesis, such as that presented by Murray is questionable on the grounds that it must assume that each of the various experimental ways of judging anxiety level — by observers, subject reports, sweat readings — refers to the same private experience: viz. anxiety. To take a result linking sweat readings with hesitancy as confirming an earlier result linking self-reported anxiety with hesitancy is to beg more questions than a scientific argument should allow.

4. A window on the mind

By far the largest proportion of published studies of speech pauses is based on a hypothesis which I shall call 'the window-on-the-mind hypothesis' (or simply 'the window hypothesis'). In short, this hypothesis takes the time a speaker spends pausing in speech as somehow indicative of his cognitive activity while speaking. A common justification of the link between pausing and mentation would rely on the assumption that a speaker needs to pause in order to plan what he is to say next. Hence the investigation of the location and duration of pauses should, according to the window hypothesis, provide the psycholinguist with evidence as to the location and duration of the cognitive planning of speech in relation to its production. A model of speech encoding which predicted a distribution of cognitive activity would then receive some confirmation if that prediction were to accord with the observed distribution of pauses in speech production. For such investigators, the cliché of the 'pregnant pause' takes on a more literal significance.

a. Lounsbury

The pioneer of this approach to the study of pauses, and the originator of the window-on-the-mind hypothesis, is the anthropologist Floyd Lounsbury. His article, 'Transitional probability, linguistic structure, and the systems of habit-family hierarchies', published in 1954, derives the window hypothesis from a *mélange* of three fields of study: information theory, behaviourist psychology, and distributional linguistics. This one article is the source of over 25 years of speculation based on the evidence provided by pauses for a model of speech encoding. In order to evaluate the numerous studies based on the window hypothesis, it is necessary first to consider Lounsbury's theoretical reasons for proposing it.

Lounsbury begins with the observation that a corpus of speech — like any non-random sequence of events — can be described not only in terms of its distributional structure, in the manner of structural linguistics, but also in terms of its statistical structure: i.e. its system of transitional-probability relationships.

Transitional structural analysis assumes units of a given order (phonemes, morphemes, words) and seeks to ascertain their transitional probabilities: 'Given an occurrence of the unit x, what is the probability that y will be the next unit to follow?' (...) Or stated in information-theory terms: 'In the case of the occurrence of a sequence xy, what is the "amount of information" in the occurrence of y, given the previous occurrence of x?'.¹⁸

It should be noted straightaway that transition-probability analysis, so framed, is not independent of linguistic analysis. Rather, the former relies on the latter to identify the units of analysis. In order for transition-probability analysis to be applicable to the study of verbal behaviour, that behaviour has to be pictured as a sequence of units. Thus, given the conclusions of linguistic analysis, the transition-probability specialist can view verbal behaviour as a sequence of units

such as words, ^{speech sounds} / clauses, topics, etc. But he must rely on linguistics to provide him with the prior analysis of verbal behaviour in terms of such units. Were the linguist to say, for example, that it is not possible to divide the flow of verbal behaviour into discrete units of the same type ordered in a linear progression through time, the transition-probability analyst could not begin his study. If the linguist (albeit a radically unorthodox linguist) argued that every bit of verbal behaviour is unique — because, say, of changing situations, purposes, participants, levels of experience, etc — the transition-probability analyst would not have his required assumption of units of a given order sequentially arranged. The key question in the study of transition probabilities is: given x, what is the probability of y following? But this question cannot be answered unless a prior definition of x and y are given. For x and y have to be recurrently identified in order that the analyst may say that so many times y did follow x and so many times it did not. Without the linguist's criteria, the analyst could not tell whether any bit of verbal behaviour was or was not an instance of y. Does a change in tone of voice prohibit two items of behaviour to be taken as instances of the same unit? What about a difference in the contextual use of those items? Without the prior analysis of verbal behaviour into the linguistic and the non-linguistic and into sequences of linguistic units, transition-probability analysis would not be applicable to the study of language.

At the same time, transitional-probability analysis is concerned with an aspect of language which is purposely ignored by orthodox linguistics. As Lounsbury remarks, linguistic analysis does not take into account the frequency of occurrence of a given item. It is concerned only with distributional possibilities. If the linguist bases his analysis

on a particular corpus of attested utterances, he does not concern himself with the number of times phoneme x appeared after phoneme y, but merely with whether such a sequence did occur at all. All that is needed to confirm the possibility of a particular distribution is that it occurs (at least) once. (Lounsbury was writing during the heyday of American structuralism, and his remarks regarding the nature of linguistic analysis reflect this situation. Nonetheless, his claims regarding the neglect by linguistics of frequency of occurrence could just as well be applied to 'post-structuralist' linguistics.)

In the procedure of contemporary structural linguistics analysis, frequency of occurrence (of a given unit in a given context, or of a given contrast) is not a relevant criterion. Only the possibility of occurrence — as represented by some one instance or by many instances of it — is relevant.¹⁹

Thus psycholinguists were able to justify the creation of a separate disciplinary perspective on language behaviour. Not only were their theoretical concerns and methodological background different from those of the linguist, but psycholinguists were able to apply them to a domain of empirical facts about language heretofore ignored by the linguist. Furthermore, the psycholinguist would focus his analysis not on an abstract system of distributional possibilities — i.e. the language or 'langue' — but on the cognitive processes which bring about the sequential production of speech (parole). In this way, by distinguishing itself from (but not contradicting) linguistics and by identifying its own empirical domain, the emerging discipline of psycholinguistics was able to develop a unique study of language behaviour without thereby entering into a potentially damaging academic conflict with the sitting tenant of language — study, linguistics. An analysis of a corpus of speech could be made, revealing not what distributional combinations were possible, but

what the probability of such combinations were, given the occurrence of one of the related units.

The statistical analysis of transitional-probabilities could be applied to any sequence of non-random events. In order that its application to speech should be relevant to the behaviourist psychologist's concerns, Lounsbury proposes to merge statistical analysis with a stimulus/response model of speech production. To further this explanatory synthesis, the notion of convergent and divergent habit-family hierarchies is introduced. A convergent habit-family hierarchy consists of a set of different stimuli which give rise to a common response. Conversely, a divergent habit-family hierarchy is defined as the case of one stimulus giving rise to a variety of differing responses. Variations in habit-family strength, from extreme convergent to extreme divergent, are said to correlate with the probability of a particular response, given a particular stimulus. In turn, these notions are applied to an explanation of the sequential 'generation' of speech.

Information theory measurements deal with the probability of occurrence of one event among the class of possible events of the same order. If we conceive of an antecedent message event (of any order or size of unit) as constituting or indexing a stimulus situation and the subsequent message event (of the same order or size of unit) as constituting a response, then the transitional probability measurements of information theory can be viewed as reflections of the systems of encoding or decoding habit strengths.²⁰

In other words, the generation of sequences of speech units (e.g. words) is taken as a unit by unit chaining of stimuli and responses. If the speaker has uttered ABCD, then, for instance, the last unit D, may be taken as a stimulus for the continuation of the sequence. With such a stimulus a variety of following responses are possible (any letter of the alphabet), but due to strength of habit (or association between two such units), it is highly probable that E will be the response. E will

then serve as a stimulus for the further continuation of the series. Thus the fact that, given D as a stimulus, E almost invariably occurs as a response can be taken as a reflection of the strong associative link (or habit-family strength) between D, as a stimulus, and E as its response. In this way, the psycholinguist can interpret his statistical analysis of transitional probabilities as providing insight into the encoding of stimulus/response chains in speech. The more naturally (due to habit-family strength) a given encoding unit arises as a response to another, the higher the probability should be of their sequential occurrence in speech. By combining information theory and behaviourist psychology, the psycholinguist — according to Lounsbury — is able to construct a model of the encoding process on the basis of statistical frequencies in speech.

In order to establish the relevance of such information theoretical perspectives on speech encoding to the study of pauses, only one additional principle is required: "habit strength is inversely correlated with the latency between stimulus and response".²¹ That is, the stronger the associative link (habit-family strength) between x (as stimulus) and y (as response), the less pause time will be needed for the speaker to produce y in response to x, e.g. in the utterance 'u v w x ...'. Furthermore, given the relation between habit-family strength and probability, if statistical analysis has shown that, given x, y is almost certain to follow, then it is just as certain that no pause should occur between x and y. It is more probable that a pause should occur if x were to be followed by anything but y.

Based on the behaviourist principles of stimulus and response, interpreted according to the theory of information, Lounsbury predicts that the greater the transitional uncertainty between two items in

sequence (i.e. the less the transitional probability), the greater the latency or pause duration should be in the production of the sequence.

The argument may be schematized as follows:

premiss A: high probability reflects strong habit-family association

premiss B: strong habit-family leads to production without pause

observation: x followed by y is highly probable

hypothesis: there should be no pause between x and y. (xy should be produced fluently).

Lounsbury does not present any experimental evidence confirming this prediction, although he remarks that the almost invariant absence of pause at intra-syllabic or intra-morphemic position does lend evidential support. Instead, he derives five hypotheses from this window-on-the-mind argument and invites future investigators to test them with experimental methods. (The term 'window-on-the-mind hypothesis' refers to no one of the following but to the more general hypothesis underlying them all: viz. that temporal of speech production reflect temporal features of cognitive activity underlying speech.)

Hypothesis 1: Hesitation pauses correspond to the points of highest statistical uncertainty in the sequencing of units of any given order.

Hypothesis 2: Hesitation pauses and points of high statistical uncertainty correspond to the beginning of units of encoding. If it should be shown that the stretch of speech from one hesitation pause to the next is a convergent one, i.e., one characterized by decreasing statistical uncertainty (increasing transitional probabilities), then we would have strong support for claiming this as a unit of encoding.

Hypothesis 3: Hesitation pauses and points of high statistical uncertainty frequently do not fall at the points where immediate-constituent analysis would establish boundaries between higher-order linguistic units or where syntactic junctures or 'facultative pauses' would occur.

Hypothesis 4: The units given by immediate-constituent analysis, and especially those bounded by facultative pause points, do correspond to units of decoding, however.

Hypothesis 5: Units of encoding for easy oft-repeated combinations approach coincidence with those of decoding.²²

Although each of these hypotheses has been tested experimentally since their publication, it is the first two hypotheses which have created the most controversy. V. Valian, in her critique of Lounsbury's paper, summarizes the picture of speech production presented by these hypotheses:

Transitional probabilities from one word to the next become successively higher until the end of a semantic encoding unit is reached. At that point the transitional probability of the next word drops. The speaker pauses.

b. Goldman-Eisler

Although not the originator of the window-on-the-mind hypothesis, Dr. Frieda Goldman-Eisler is, for most investigators, the psychologist with whom that hypothesis is identified. For over thirty years she has studied temporal variables in speech. The majority of her publications have been devoted to the study of cognitive processes seen through the window of pausal phenomena in speech.

In the early stages of her work in London, Goldman-Eisler studied

the determinants of articulation rate (AR).²⁴ She came to the conclusion that fluctuation in AR is primarily due to the number and duration of hesitation pauses, rather than to breath pauses or fluctuations in the articulation of words.

... the rate of speech turned out to be a function of the time taken up by hesitation pauses.²⁵

In a later experiment,²⁶ she again focussed on AR. Here she found that when subjects were asked to repeat utterances a number of times their AR rose accordingly. This was interpreted as evidence that the more practised an utterance is, and hence the greater its transitional habit strengths, the less time is required for the verbal planning of the utterance.

AR thus becomes an efficient and unequivocal indicator of habit strength only.²⁷

Furthermore, in view of the earlier conclusion that AR is a function of hesitation, Lounsbury's hypothesis correlating habit strength (or transitional probability) and speech latency seemed to be confirmed. That is, since (a) AR is a function of hesitation and (b) AR rises with practice (i.e. increased habit strength), then the correlation of hesitation with habit strength would appear to be a natural conclusion.

One might regard pausing as an attribute of spontaneity in the creation of new verbal constructions and structures, i.e. of verbal planning. Continuous and rapid vocalization on the other hand would be the result of practice and thus occur in the use of well-learned word sequences. In this respect speech would be no different from any other activity which is subject to learning. This assumption was amply confirmed by measurements of speech produced in conditions of great spontaneity as compared with well-learned speech.²⁸

In other words, she takes it that because subjects pause less when repeating an utterance for the n^{th} time than they did when first uttering it, therefore pausing is positively correlated to habit strength.

There are many objections to be made to such an argument. For example, one could object to the correlation of repetitions made one after another, during the short period of laboratory experiment, with the utterance of a given sequence of words (in ordinary speech) which one has perhaps uttered, or heard, three weeks or three years before. It would be unjustified to equate the habit strengths developed through the continuous repetition of sequences present in short-term memory with a superficially similar repetition of a sequence which was last uttered or heard years before and thus only remains, at all, in long-term memory.

The result of these experiments is that Goldman-Eisler is able to derive support for the window hypothesis, in so far as hesitation pauses are taken as a reflection of transition probabilities. In other words, Lounsbury's argument — not based on experimental evidence but on theoretical discussion — and Goldman-Eisler's AR experiments are able to be taken as support for the window hypothesis, i.e. for the claim that pausing reflects cognitive activity. This support is only for a particular aspect of cognitive activity: the determination of a response, based on a given stimulus. Pausing, they argue, correlates with the habit strength of such Markovian processes.

In a set of experiments in the later 1950's, Goldman-Eisler purports to test Lounsbury's original window hypothesis more directly. These experiments are placed within a larger set designed to test the application of the window hypothesis not only to the relation between pausing and lexical decisions but also to the relation between pausing and other levels of the cognitive planning of verbal activity.

She claims that there are three kinds of decision made by the speaker in order to produce and to continue to produce speech.

- 1 - He must decide what he wants to say, i.e. make a content decision.

- 2 - He must choose a syntactic structure, within which to place his utterance.
- 3 - He must choose the lexical items which will express the semantic content within the chosen syntactic framework.

She hypothesizes that each of the three kinds of decision might result in pausing, on the grounds that verbal planning — of all three kinds — and its vocal execution must alternate.

Central and peripheral linguistic processes (i.e. verbal planning and its vocal execution: TJT) would be successive links in the verbal sequence where this requires central processing.²⁹

It should be remarked that this is a much more general version of the window hypothesis than that originally proposed by Lounsbury and argued for in Goldman-Eisler's articulation rate studies. Here pausing is not being proposed as the result of latency in response, due to low transition probabilities between two events in sequence. Instead, pausing is claimed to be the exterior signal of a variety of interior processes. Indeed, as she later argues, pausing is a necessary requirement for cognitive activity. In the development of her work on pauses Goldman-Eisler tests the application of the window hypothesis to each of the three purported levels of verbal processing: semantic, syntactic, and lexical.

The first experiments attempt to test the window hypothesis on lexical selection, i.e. to test Lounsbury's claims directly. In order to do this it was necessary to distinguish pauses due to lexical selection from those produced as a result of semantic or syntactic planning. For this reason, the test considered only those pauses uttered within sentences on the grounds that

Once a speaker has uttered the first word of a sentence, he is on his course; he has decided not only what to say but has created for himself at least the broad syntactical framework. The pauses, except those occurring at clause junctions can now be stipulated to be lexical ones.³⁰

In order to test Lounsbury's claims regarding the relation between transitional probabilities and hesitation, conceived of as a model of lexical selection, a method for determining transitional probabilities was required. For this Goldman-Eisler chose a revised version of Shannon's 'guessing game'. In this procedure subjects were required to guess which word was to follow a given sequence of words. Twelve sentences, chosen from a debate, a recorded interview, and a dictated letter were used. Each subject was provided with the preceding context of the text. He was then asked to guess the first word of the test sentence, and then, one by one, the succeeding words of the sentence. At each stage of the experiment, the context from which he was guessing was augmented by one more word: the one he had just finished guessing, successfully or not. The transition probability of a given word was arrived at by calculating the ratio of the frequency of correct guesses to the total number of guesses made, (by all subjects). By this method, Goldman-Eisler hoped to by-pass the obvious difficulty of estimating the true transition probability of a given sequence, conceived of as a statistical fact about the language at large.

Once the transition probability of a given word had been estimated, it was compared with the original recorded utterance to see if a hesitation pause had preceded. Presumably, words of a high transition probability — that is, those words which were more easily guessed by subjects, given their preceding context, — would not be preceded by pauses. Those which were harder to guess and hence of a low transition probability should have been preceded by hesitation pauses.

The results obtained were mixed. 28 of the 34 words preceded by pauses had low transition probabilities. However, of the 75 words of low transition probabilities, only 29 were preceded by pauses. In general, that is, words of low transition probability were not preceded by pauses.³¹

In a second experiment³², the format was altered by asking some of the subjects to guess words not 'from left to right' (i.e. given the preceding context) but 'from right to left', that is based only on knowledge of the subsequent context of the relevant word. In this way the transition probability analysis could be more detailed, yielding information on words which were hard to guess left-to-right, but easy right-to-left or difficult both ways, or easy left-to-right but hard right-to-left, and so on.

Here the results were more like what was expected. It was indeed found that many of the words which were hard to guess left to right, were not so if the guesser was provided with the subsequent context. Describing these results at a later date, Goldman-Eisler concludes that "the occurrence of some words is constrained by previous context and of others by subsequent context".³³ Furthermore, of the 23 words which proved difficult to guess from both directions — i.e. words of low forward and reverse transition probability — 17 were preceded by pauses. Thus when reverse transition probability was taken into account the results from the previous experiment were overturned. In the first experiment only 39% of low probability words were preceded by pauses, whereas in the second experiment 73% of low probability words were uttered following a hesitation pause. Thus, by altering the method of estimating transition probabilities, Goldman-Eisler was at least able to bring her results more in line with Lounsbury's predictions. She concludes that a pause is necessary before words of low transition probability (i.e. high information content).

The resulting hypothesis, that the function of pauses is to increase information, was confirmed (to start with) at the statistical level.³⁴

But the question must remain: do the transition probabilities,

so measured, bear any relation to Lounsbury's use of that notion in his hypotheses?

Lounsbury's hypothesis is based on a supposed relation between latency and response uncertainty, with the already uttered speech taken as stimulus. In Goldman-Eisler's method of estimating response uncertainty, or transition probability, she was only able to achieve acceptable results by taking as stimulus both (a) already uttered speech and (b) speech uttered after the relevant response. However, Lounsbury would take (b) as a response in its own right to (a) plus the relevant word (call this 'w'). It would appear illegitimate to take (b) both as part of the stimulus for w, and as its response. How can one and the same event be both stimulus and response to another event?

If Goldman-Eisler's claim is that pauses occur before words that are unpredictable within their total sequential context, this claim needs a model of speech production which does not picture speech as encoded from left to right, i.e. as a Markov process. That is, it requires a model which encodes hierarchically.

In fact, the result of Goldman-Eisler's methods amounts to an abandonment of the sequential word-by-word model of the generation of speech according to stimulus and response. She does not mention the dropping of Lounsbury's model, in spite of the fact that these experiments were supposed to test the predictions of that model. Instead, she places her results within a different theoretical context, viz. that provided by Hughling Jackson's notions of (internal vs. external) speech. According to her interpretation of Jackson's theory, verbalization occurs "in duplicate, each operation being completed subjectively before it is carried out objectively".³⁵ In other words, the speaker mentally plans what he is going to say — or at least certain parts of it — and then

executes that plan. Lexical selection, in such a model, would have to be carried out at a very late stage, indeed during the silences interrupting the execution of the mental plan. In this case, a speaker would be conceived of as vocalizing his plan up to a point where he has to choose a particular lexical item. If that choice is from a wide range of alternatives — the range of possibilities being determined by what has already been vocalized and what is planned to be vocalized following the lexical item — then he pauses. If however, the combination of previous vocalization and plan are able to provide him with a selection fairly automatically, he does not pause. In this sense, Lounsbury's left-to-right encoding model, its S-R paradigm, and its particular notion of transition probability in the speech chain have been abandoned in order to develop a new model which can account for the experimental results. Goldman-Eisler does not say so, but this radical departure from Lounsbury is necessitated because the results of her experiments disconfirm his prediction, at least the one on which those experiments were based.

The assumption of a mere linkage of associated elements in sequence was rejected as incompatible with the facts obtained. It was proposed that the production of external speech should be viewed as the result of at least two simultaneous processes, one concerned with planning the content and grammatical structure and the other with word choice to fit this structure.³⁶

Having determined the relation between pausing and lexical selection, Goldman-Eisler focusses her investigations on selection of content and syntactic structure. To determine the relation between pausing and content selection, she and her co-writers designed a set of experiments which contrast speaker's pausing behaviour in relation to cognitive tasks of varying difficulty. Subjects were presented with cartoons from The New Yorker magazine. They were asked to "describe the context of the story as depicted in the pictures" and then to "conclude by formulating

the general point, meaning, or moral of the story in as concise a form as you can."³⁷ It was assumed that description of concrete events would be of a "distinctly different level of verbal behaviour" from interpreting their meaning. Hesitation totals in each of the activities were determined by dividing the sum of time spent pausing by the sum of time spent speaking. Speakers were found to pause a greater percentage of the time in interpretation than in description. Thus, the more difficult cognitive task — presumably the selection of what to say — was found to lead more hesitation.

Reflecting on the implications of these experiments, Goldman-Eisler comes to the conclusion that one can determine, by a simple subtraction, the amount of time spent pausing due to content selection. An individual's rate of pausing on descriptions or in ordinary conversation is held to be fairly constant, due primarily to questions of lexical selection. Thus if you took the time they spent pausing during the more semantically complex task of interpretation, and subtracted from it the characteristic time spent pausing for description, you would be left with the specific amount of time spent pausing by the speaker in order to perform the semantic 'extras' required by interpretation. Thus, she comes up with the following equation:

$$P \text{ Interpretations} = P \text{ Descriptions} + P \text{ Cognitive Process}^{38}$$

and

Taking an individual's inclination towards or away from external activity or his disposition for peripheral versus central processes as a baseline, it seems that the additional time of pausing determines the intellectual quality of the verbal statement.³⁹

Taking the relation between semantic processes and pausing to be determinable in this way assumes, at the very least, that no syntactic

pause time is involved in the equation. That is to say, one cannot expect to determine semantic pause time as the difference between ordinary pause time and difficult cognitive task pause time, unless pause time due to syntactic processes is held constant (or indeed if syntactic processes do not affect pausing at all). In a further experimental investigation, Goldman-Eisler comes to the latter conclusion.

A set of experiments were designed to answer the question whether processing the syntax of an utterance is a cognitive operation requiring time. The proportion of subordinate clauses to the total number of clauses in an utterance (a proportion termed 'the Subordination Index', or S.I.) was taken as an adequate measure of syntactic complexity with which to test the hypothesis. Pause time was not found to vary in relation to the S.I., or, if it did, this was found to be attributable to a concomitant variation in semantic complexity. With the latter held constant, syntactic difficulty, as measured by the S.I., seemed unrelated to time spent pausing.*

* This conclusion appears to conflict with that reached by Cooper and Paccia-Cooper in their book Syntax and Speech.⁴⁰ They argue that silent pausing is a reliable indicator of syntactic boundaries in sentence structure.

... empirical work has confirmed that temporal phenomena in speech may be syntactically determined. In the case of pausing, it is important to distinguish between pauses that are typically due to word-finding difficulties and those that are syntactically determined. The former type of pause occurs most frequently just prior to major content words (for example, Maclay and Osgood, 1959), whereas the latter occur most frequently at major syntactic boundaries (for example, Boomer and Laver, 1968).⁴¹

The proponent of Goldman-Eisler's position might well reply to their argument that Cooper and Paccia-Cooper were confusing grammatical juncture pauses with hesitation pauses. Only the latter are claimed to stem from cognitive activity. But Cooper and Paccia-Cooper's data reveal pauses at many within-sentence locations which Goldman-Eisler does not classify as grammatical junctures. Thus, according to Goldman-Eisler, pauses at such locations would have to be due to hesitation. This would then count as counter-evidence to her position on the relation between syntax and hesitation.

Thus, having postulated three levels of verbal encoding processes — semantic, lexical, and syntactic — only the first two were found to correlate with pause behaviour. Consequently, given the assumption that cognitive activity is not possible without concurrent hesitation in speech, the syntactic processes were determined not to require cognitive activity. In order to explain this apparent anomaly, Goldman-Eisler invokes a distinction between skill versus will, as proposed by Karl Pribram.⁴² A wilful cognitive action involves intention and voluntarily guided behaviour; whereas skill is a matter of following pre-ordained rules. While semantic and lexical choices would be concerned with making voluntary decisions about what one wants to say, syntactic choices are merely the result of following language-given rules, thus features of skill. Only voluntary, wilful behaviour is assumed, in this case, to require cognitive activity and hence speech latency. Referring to Pribram's experiment on this distinction, Goldman-Eisler writes:

It seems that if hesitation is an aspect of volitional action or planned activity then this body of evidence is also in harmony with the distinction which has emerged from our experiments between semantic and lexical operations on the one hand and syntactic ones on the other, when pausing time in spontaneous speech production was the criterion of planning. On this criterion, syntactic operations are to be classed as proficient behaviour, a distinction well in keeping with the guided conformity to rules characteristic of skill as against the initiative of planning and uncertainty of action. Thus pauses of hesitation indicate where will or skill operate in the production of speech.⁴³

c. Boomer

Among the adherents to the window hypothesis, the strongest criticism of the work of Goldman-Eisler has been voiced by D.S. Boomer. In a review article on her book Psycholinguistics, Boomer presents damaging criticism of Goldman-Eisler's experiments, methodology, and conclusions.

His most powerful objection concerns her choice of data for the

lexical selection experiments. Only twelve sentences were studied, none of them from what one could call spontaneous speech. What Boomer objects to most of all, however, is that only "grammatically correct, well-constructed sentences 'logically consistent with the context of the whole utterance'" were included in this twelve. He cites her original comments on this selection:

Even with highly educated speakers spontaneous speech is such that well constructed, grammatically correct sentences spoken without repetition or midway changing of grammatical construction, etc. are few and far between. While living speech is largely of this kind, for the purpose of the experiment it was necessary to select the rarer but more felicitous speech products ...⁴⁴

Boomer then points out that, nevertheless, Goldman-Eisler refers in the later book to this specially selected sample as representing "a fairly wide range of spontaneous language".⁴⁵

Boomer argues that the exclusion of 'grammatically incorrect' sentences tends to bias her experiments towards the hypothesis that only semantic and lexical choices lead to latency in speech.

Having arbitrarily excluded all utterances which involve 'midway changing of a grammatical construction' she was left with a small, highly selected set of utterances in which grammar and syntax were not problematic for the speaker.⁴⁶

In this case, he suggests, it would not be possible to find evidence supporting the hypothesis that syntax involves a time-consuming cognitive process.

In his review of work on pauses in speech, S.R. Rochester presents the reason (based on personal communication with Goldman-Eisler) behind this seemingly arbitrary exclusion of non-grammatical constructions. Given the nature of the guessing-game technique of estimating transitional probabilities, it would not have been possible to include grammatically incorrect sentences in her sample. That is, if the subjects had to guess

'non-grammatical' sequences of words such as

I saw her at the the well not at the cinema no in it.

transition probabilities would have dropped to near zero. It would appear that the nature of the guessing technique leads subjects to expect logical and well-constructed sequences, such as are found in written prose. The appropriate conclusion to draw from this is not that one must therefore exclude the majority of spontaneous speech utterances from such experiments, but that the guessing-game procedure is not an adequate way of estimating transition probabilities in speech.

Boomer also criticises Goldman-Eisler for ignoring function words when identifying the word following a hesitation pause. He argues that if one is ignoring function words then the hypothesis that hesitation pauses are usually followed by words of low predictability is a foregone conclusion, given that content words form an immensely larger class than do function words. If, in fact, function words had been included, then one would have to have taken the third of hesitation pauses which preceded a function word as preceding a word of relatively high transition probability.

Let us restate the hypothesis as it was actually tested:
the word following a pause, or the next word, or the next
word after that will be a content word and thus relatively
unpredictable. Such a hypothesis is difficult to disconfirm.⁴⁷

In an earlier paper Boomer argues for the window hypothesis but presents evidence for taking encoding units to be larger than the word. Instead, he claims that the phonemic clause - an early version of the tone unit, as defined by Trager & Smith⁴⁸ - is the primary unit of encoding. This is supposed to explain, assuming that one adopts the window hypothesis, why in his data pauses tend to occur at or near the beginning of phonemic clauses.

If the encoding units are single words then hesitations should occur more frequently before those words which involve a difficult decision, i.e., a choice among many alternatives. If the encoding unit is a sequence of several words then the hesitation should predominate at the beginnings of such sequences, rather than occurring randomly wherever a difficult word choice occurs.⁴⁹

Boomer's evidence for the latter hypothesis is derived from the following data. From an examination of 1,593 phonemic clauses from the spontaneous speech of 16 male American speakers, he discovered that, of the 1127 hesitations (filled and unfilled pauses), 54% occurred before the first or second words of these clauses. In fact, 41% of the total number of pauses occurred between the first and second words. This would not have been the case if a word transitional model was underlying speech encoding. Indeed, Boomer argues, if information quantity were the determining factor of hesitation then most hesitation would occur near the end of phonemic clause, that is, just before the nuclear stress. He cites Berry (1953)⁵⁰ as having shown that nuclear stress is more likely to occur on a word if it is a less frequent word.

Thus the high-information lexical words tend to occur toward the end of phonemic clauses, and this is where a word-unit model should predict the most hesitations to occur.⁵¹

This is to say, the nuclear stress is said to occur on a particular word because that word is of very low frequency, and hence of low transitional probability. Therefore, pauses should often occur before words with nuclear stress. Since words with nuclear stress are usually near the end of a phonemic clause, Boomer's finding that pauses tend to occur towards the beginning of such clauses is taken to be evidence against the hypothesis that low transition probability leads to hesitation.

A curious feature of Boomer's data, and one which has led to much discussion, is the greater frequency of pauses after the first word of

the clause. If one were to take the phonemic clause as the unit of speech encoding, the greatest frequency of pausing might be expected before the clause begins, rather than just after its beginning. Boomer admits that this finding may be the result of his distinction between juncture pause — at the boundaries of phonemic clauses — and hesitation pause. Juncture pauses are said to be linguistically determined features and not to reflect the hesitation of cognitive planning. In order not to confuse the two, Boomer did not count a silent pause before the first word of a clause as a hesitation, except if it was preceded by a filled pause. All other silent pauses at clause initial position were deemed to be juncture pauses and so were not included in the distributional findings on the location of hesitations. Following Barik (1968)⁵² many researchers have argued that Boomer's findings are distorted by this practice. Barik suggests that juncture pauses of duration longer than 700 msec should be counted as a juncture pause of 500 msec followed by a hesitation pause of 200 msec or more. If this procedure is followed, the surprising skewing of pause distribution towards post-first-word position disappears. Instead, 52% of the pauses would then be identified at clause initial position. This adjustment only re-enforces Boomer's claim that phonemic clauses, not words, are the primary speech encoding units. It does however call in question his hypothesis that hesitations occur after the syntactic decisions regarding a clause are made but before the accomplishment of lexical decisions. This claim has received further criticism in the work of P.R. Hawkins (1971).⁵³ Hawkins finds, in a similar analysis to that of Boomer, that the first word of a phonemic clause is likely to be a conjunction. In that case, he argues, the syntax of the clause has not yet begun because the speaker has not committed himself to the type of syntactic construct which the clause

will be. Instead the syntactic construction should not be considered to have begun until a word has been uttered which restricts further choices later in the clause. In this case, if a pause occurs after a clause — initial conjunction, it cannot be assumed that the syntactic decisions regarding the clause have already been made. Boomer does not present any evidence concerning the syntactic relations between the first word of such clauses and the remainder of the clause. He argues that the fact that pauses tend to occur after the first word of a clause supports an argument made by Chomsky against Skinner. Chomsky is said to take the position that lexical selection follows grammatical selection, while Skinner holds the opposite view. However, in view of the criticism of his analysis, Boomer's findings cannot be seen as providing reliable evidence for that argument.

Nevertheless, in spite of criticism, Boomer appears to have won the day in the debate over the size of encoding units. As Barik, Hawkins, and others point out, the criticism of his placement of the highest frequency of pauses after the first word of a clause does not damage his claim that speech encoding units are of the dimension of clauses rather than of words. In fact they lend support to that argument, since a re-interpretation of Boomer's data in the light of these criticisms would result in an even greater preponderance for pauses to occur at the beginning of phonemic clauses rather than equally spaced before all lexical items, as word-transition model would predict. For this reason, subsequent literature on the window hypothesis has taken Boomer's case for a clausal encoding unit as proven.

Once one accepts Boomer's argument, and therefore rejects Lounsbury's transitional probability model as an explanation for pausal phenomena, some of the justification for holding the window hypothesis disappears.

Lounsbury's synthesis of information theory, behavioural psychology, and distributional linguistics provided a reason how and why pauses should correlate with mental activity. A pause was seen as occurring at a specific location, viz. between words a and b, because the associations between a and b were weak. Transitional probability, habit strength of S and R pairs, and pausing were seen as intricately related.

But Boomer's model rules out the relevance of transitional probabilities and implicitly rejects the behaviourist paradigm of stimulus and response. In this case, one wants to ask, why should a pause be an indication of cognitive activity? Lounsbury provided a detailed argument linking cognitive activity to pausing. Boomer rejects the details of that argument but nevertheless accepts its conclusion: the window hypothesis. Indeed Boomer even rejects Goldman-Eisler's 1958 papers which were designed as tests of the validity of that hypothesis. On what grounds, it may be asked, does he argue that the window hypothesis should not be thrown out along with its theoretical and experimental support?

In fact he offers none. He merely takes the window hypothesis as true without saying why.

The data to be presented concern the location of ... hesitation in extended utterances, but the basic theoretical issue involved is the nature of the grammatical encoding process in speech. The data and the theoretical issue are inferentially related. The linking hypothesis is that hesitations in spontaneous speech occur at points where decisions and choices are being made. On this basis, the patterning of hesitations should provide clues as to the size and nature of the encoding units which are operative.⁵⁴

Boomer's argument is based on a double hypothesis. The first is what he calls the linking hypothesis and I have been calling the window hypothesis. The second says that, given the first hypothesis, we expect to find that clauses not words are the basic encoding units of speech. In fact, if

one does grant him the first hypothesis, then his data would appear to produce at least some evidence for the second. But his data provides no evidence for accepting the first (window) hypothesis, and nowhere does he produce an argument for it. And yet without a justification for accepting the first hypothesis, the evidential support for the second loses its relevance. For instance, Lounsbury's derivation of the window hypothesis provides a reason why a pause caused by hesitation should occur exactly where it does: i.e. between the stimulus word and its latent response. But while rejecting the stimulus/response paradigm, Boomer offers no alternative theory why pauses should be expected to occur at any determinate linear point relative to the planning delay. Why should they not occur, say, immediately after planning delay rather than immediately before. Indeed, it also seems in principle quite possible that a speaker might pause at one spot because of a difficulty he anticipates in the next sentence but one. Then, when the source of the delay was finally reached it might be spoken over quite fluently, owing to the time spent planning for it at a much earlier moment. In short, it is difficult to see — without the benefit of Lounsbury's behaviourist account — how we can assume that there is any constant causal connexion between the point of occurrence of a pause and the relevant hitch in mental planning.

Consider the analogy of driving a car. If the driver finds himself in difficulty about his route planning, he may draw up by the side of the road to consult a map. But he may, in theory, do that at any point on his journey. There is no guarantee that he will do it at some constant distance from the point where he makes his decision about which road he should take (or should have taken). It is even quite possible that the driver will pull over to the side of the road, say, two hundred yards past a turning in order to check his directions to see if he ought

to have turned left at the last turning. Once Lounsbury's behaviourist derivation of a link between pausing and planning is abandoned, there is no reason why not to picture that link in the way that the link between driving and map-reading is pictured here. However, this possibility does not appear to occur to Boomer. He continues to accept the principles of Lounsbury's window hypothesis while rejecting the latter's reasons for proposing it. By rejecting the behaviourist arguments for the window hypothesis, and the theoretical background to these arguments, Boomer — like other post-Chomskian researchers — is left with no support for the foundational hypothesis underlying their investigations: viz. that pauses are the 'now' periods of planning.

d. Fodor, Bever, and Garrett

In their book The Psychology of Language, Fodor, Bever, and Garrett come to no firm conclusions about the type of planning signalled by pauses. They do, however, accept the window hypothesis.

At least some of the false starts and hesitations and lapses of grammar and pronunciation ... presumably represent on-line decision making by the speaker. They may thus provide a relatively direct indication of the character of sentence planning.⁵⁵

The question to which they would like hesitation pauses to provide an answer concerns the size of planning units. The enquiry, they point out, boils down to an interest in determining how much of a planned utterance is present in the mind of the speaker at any one moment in the utterance. They assume that the amount varies, depending on how much of the utterance the speaker has already produced and what point in the syntactic structure of the sentence he has arrived at. They also ask "at what level of analysis is the preplanned material available?"⁵⁶ The question here concerns whether the planning is of a semantic, syntactic, prosodic,

lexical, etc., nature, or a combination of these, and which presumed levels of planning occur prior to which others.

In a short discussion of the literature they decide that Boomer's conclusions, as adjusted by Barik and Hawkins (although the latter two are never mentioned by name), are the most convincing. In particular, they claim the lexicalist hypotheses regarding the relations between the statistical uncertainty of word sequences and hesitation are "to put it mildly, not self-evident".⁵⁷ On the basis of Boomer's analysis they reject the lexicalist work of Lounsbury and Goldman-Eisler along with the hypothesis that words are the primary units of planning. Although the data is inconclusive, they say, regarding the length and character of planning units, nonetheless one may safely reject, given Boomer's evidence, the hypothesis that speakers pause to plan the next lexical item.

Although they reject the idea that speaker uncertainty mirrors transitional probabilities between lexical items, they provide no alternative argument for supposing that hesitation is the result of cognitive activity of any sort. As in much of the post-behaviourist work, this equation is merely assumed as given. Indeed, a circularity is evident in their reasoning on the window hypothesis and its use in the study of planning strategies. Planning, they seem to say, must be the source of speaker hesitancy because, from what we already know about planning, we can conclude that it should cause delay in speech execution. Furthermore, much of what we know about planning is derived from the study of speaker hesitancy as a mirror of planning activity.

e. Clark and Clark

In their book Psychology and Language, Clark and Clark develop a

more conclusive argument about planning (and the execution of plans) based on the evidence from pausing, false starts, repetition and corrections. Much of this argument relies on their assumption that speakers strive for an 'ideal delivery' in speech. This notion will be discussed in the final chapter of this thesis.

In view of their 'ideal delivery' assumption, Clark and Clark take hesitation markers (silent pauses, filled pauses, repetitions, false starts ...) to be speech errors. Speakers, they say, try to plan sentence constituents as one unit and to execute these planned constituents as fluently as possible. Hesitation markers provide evidence for both these principles.

Every deviation (from the ideal delivery) points to something that has gone wrong in planning or execution.⁵⁸

Many such hesitation errors are said to be caused by an influence which is dialectically opposed to the desire to produce an ideal delivery. That is, speakers also do not want to be interrupted. The 'ideal delivery' is opposed by 'the press of the conversation'. If the speakers took all the time necessary to produce an ideal delivery — that is, if they waited at every juncture boundary until they had finished planning their utterance until the next such boundary — they might give their interlocutors the impression that they had finished speaking and were willing to cede the floor. As a result, speakers often begin speaking, in order to hold the floor, before they have completely finished planning. This sometimes leads to a hitch in the execution process — i.e. a hesitation error — during which further planning may be accomplished in order that the execution may eventually continue.

In spite of such hitches in the execution of a constituent — brought on by the press of conversation — Clark and Clark insist that a speaker

will still try to execute a constituent as fluently as possible, even after such a hitch. Sentence constituents are not only the units of planning, the evidence from hesitation shows that they are also the units of the execution of speech plans.

But recall that speakers strive ultimately for the ideal delivery, which demands that each constituent be executed in one fluent stream. So even if they should run into difficulty planning a constituent, they should try to execute it as much as a single unit as they can.⁵⁹

This remains the case even after the execution of a particular constituent has been disrupted.

Repeats, false starts, and corrections, therefore, provide excellent evidence that speakers consider the constituent a basic unit of execution. They attempt, to the best of their ability, to execute constituents as complete wholes. When for some reason they do stop, make a false start, or correct themselves, they tend to return to the beginning of the constituent.⁶⁰

That is, they supposedly return to the beginning of the constituent in order that it may finally be executed as desired: in one fluent stream. In this way, they may satisfy both conflicting forces: ideal delivery and the press of conversation. By quickly starting a constituent, without waiting to complete its planning, they can succeed in keeping the interlocutor from interrupting. This may well lead the speaker to being forced to pause, owing to an incomplete plan. However, he can still satisfy the demand for ideal delivery by recycling to the beginning of the constituent and then uttering it fluently.

They cite Maclay and Osgood's data as evidence for this claim. For instance, the fact that they found that 77% of the time when speakers corrected a content word they also repeated the function word before it (as in A) is taken as evidence that speakers try to execute constituents in one complete, fluent unit. If this were not the case, then they presumably would have merely corrected the content word and gone on,

without any repetition (as in B).

A - those clean ... those dirty cups: 77%

B - those clean ... dirty cups: 23%

Furthermore when function words were corrected, Maclay and Osgood found that speakers did not tend to repeat the content words occurring before them (C vs. D)

C - I washed those dirty ... these dirty cups: 78%

D - I washed those dirty ... washed these dirty cups: 22%

Similar evidence supporting the constituent as the unit of execution is provided by Maclay and Osgood's data on repeats.^H

Clark and Clark proceed to the evidence, provided by hesitation, for taking the constituent as the unit of planning. They point out that Maclay and Osgood's data on filled and silent pauses might at first lead one to a negative conclusion. Maclay and Osgood found that 54% of filled pauses and 64% of silent pauses preceded content words. Clark and Clark reflect on these findings.

It is as if speakers execute the first one or two function words of the constituent — the article, preposition, or verbal auxiliaries — and then stop to plan the rest of the constituent — the adjectives and nouns. This seems to go directly against the idea that speakers plan each constituent all at once. If they had, they should have paused just before the constituent — but never halfway through, as they often did.⁶¹

Nevertheless, Clark and Clark discover a way out of this apparent conundrum. There are two levels of speech planning, they say: syntactic and semantic. In this they follow Maclay and Osgood and others. The idea is that the speaker will plan the syntactic form of his constituent before planning its precise content. (They are unclear here regarding whether semantic and lexical planning are one and the same. Whereas Goldman-Eisler would speak of the choice of a content word as a matter

of lexical planning, Clark and Clark speak of this choice as a result of semantic planning and do not mention lexical planning. This leaves them in a position of picturing the speaker as deciding on the syntactic form of what he is going to say, before he has decided on the content of his utterance.) Thus, if speakers often stop before the first content word of an utterance, this is said to be because, having already planned the syntax of the constituent, they reacted to the press of conversation and executed that part of the constituent already determined by the syntactic plan. Having retained 'the conversational ball' (as Maclay and Osgood would say), they are then free to pause and do some semantic planning in order to choose the subsequent content word. So, in fact, although mid-constituent pauses might, at first glance, appear to raise a problem for taking the constituent as a unit of planning, they do not do so. They merely serve as evidence that planning takes place on two levels, so the execution of one plan can begin before the second plan is complete. Nonetheless, both levels of planning still may be thought of as operating with whole constituents as their units. Clark and Clark point to Boomer's distributional findings - regarding the placement of pauses after the first word of a phonemic clause - as further evidence for their two-levels-of-planning hypothesis.

What is most surprising about the reasoning of Clark and Clark is that they do not appear to notice that their evidence for the two-levels-of-planning hypothesis - i.e. the distribution of filled and silent pauses - is also evidence against their claim that the constituent is the unit of speech execution. One merely needs to follow the logic of their own reasoning to come to this inevitable self-contradiction.

If (i) "speakers strive ultimately for the ideal delivery" and
 hence for "each constituent (to) be executed in one
 fluent stream"₆₂

and If (ii) silent pauses and filled pauses are "the most common disruption in the ideal delivery",⁶³

then following all the (or at least most) instances of filled and silent pauses, the speaker should retrace to the beginning of the constituent in order to execute it as a complete whole. But this was not found to be the case.

That is, Clark and Clark fix on repeats and false starts as evidence that, given a disruption in the ideal delivery, speakers will still try to execute constituents as complete wholes. But the evidence on filled and silent pauses (to which they refer for other purposes) shows that, in the most common type of disruption in the ideal delivery, speakers do not retrace in order to execute constituents as complete wholes. They continue the disrupted utterance. It would appear that Clark and Clark attempt to resolve their contradiction by taking only repeats and false starts as evidence for their claims above the execution of plans, while only counting filled and silent pauses as evidence for their planning hypothesis. If one apportions the evidence in this way, so that each hypothesis is confirmed by its own restricted set of phenomena, there is no end to the number or nature of hypotheses that might be so confirmed.

Clark and Clark summarise their findings by stating that there are three places where speakers stop to plan. At grammatical junctures they plan the syntactic frame of the subsequent utterance along with its first constituent. At internal constituent boundaries they stop to plan the next constituent, primarily its syntax (whether it will be a noun phrase, verb phrase, etc.). Finally, before the first content word, they stop to choose that lexical item.

They claim that the desire to execute an ideal delivery leads to pauses at the first two locations: grammatical junctions (major constituent

boundaries) and internal constituent boundaries. This is in order to be able to produce the subsequent constituent as one complete, fluent unit. This finding is disproven by taking into account the evidence on filled pauses. They argue that the third location of pausing, before the content word comes as a reaction to the press of conversation, resulting in a momentary sacrifice of ideal delivery standards.

If they wait too long at these points (locations one and two) they know that other people will think they have finished their contribution and will begin to take their turns. To avert this possibility, speakers must boldly start in on the first function words of the next constituent...⁶⁴

This claim is false at least as regards pausing at type 2 locations, i.e. at internal constituent boundaries. It not only neglects the effect of intonation, gesture, and eye contact as efficient regulators of turn-taking (by signalling that the speaker has not yet finished), but it ignores the fact that a pause at most constituent boundaries would in no way lead the interlocutor to think that the speaker had finished.

e.g. John ... was expected to fix ... the propeller.

Lady Adsworth's dog ... ruined ... our carpet.

At none of the constituent boundaries above would a pause lead a hearer to think that the speaker had finished his sentence. Hence, the claim that speakers often utter the first function word of a constituent and then pause — for the purpose of signalling their intention to continue — is ungrounded. Such speaker behaviour is simply not required.

f. Chafe

In three recent papers⁶⁵, Wallace Chafe argues for a very different view of speech production, based nevertheless on observations which assume the window hypothesis. Chafe, as the title of one of his papers suggests, is interested in the flow of speech as a reflection of the flow of thought.

Hesitation phenomena, within this framework, are conceived as indications of the units of thought. His version of the window hypothesis, however, is not like that put forward by most of the researchers examined in this section. Whereas they tend to take hesitation pauses as indicating (and, for some, required by) speech planning, Chafe sees hesitations as arising from two related sources. Either they mark the speaker's switch from one thought-unit (Chafe calls these 'foci of consciousness') to another, or they indicate a difficulty the speaker is experiencing in 'translating' a particular thought into words. Only the latter source is similar to those assumed by the earlier investigators from Lounsbury to the Clarks. However, those investigators might very well argue that what Chafe takes as thought-to-thought hesitations are really juncture pauses and not hesitations at all.

To illustrate his idea of the flow of thought Chafe uses a metaphor from William James' The Principles of Psychology (1890)

Let us regard memory as a vast store of information, somehow established by previous experience but also creative in itself, which is potentially ready to be activated by a process which may be called 'bringing into consciousness'. Such activation takes place as a series of brief resting places. William James suggested a similar metaphor when he wrote of the stream of consciousness: 'Like a bird's life, it seems to be made of an alternation of flights and perchings. The rhythm of language expresses this, where every thought is expressed in a sentence, and every sentence closed by a period.' (...) Numerous recent observers of spontaneous speech have noticed that it is produced in well defined spurts. In the data available to me at the moment these spurts are slightly less than 2 seconds in mean duration, and contain a mean of about 5 words. They tend to be single clauses syntactically, but under certain conditions may be more or less than a clause. They usually exhibit a 'clause-final' information contour. I hypothesize that these spurts of language are expressions of underlying perching of consciousness. (...) Hesitations are especially useful in showing us where it is easy to move on, and when it is difficult.

Chafe points out that what James called a 'perching' is what he calls a 'focus of consciousness'. Hesitations may result from the speaker's delay in switching from one focus to another, or from a difficulty in verbalizing a particular focus. Foci of consciousness are themselves grouped into larger units called 'focus clusters'. A typical verbal exponent of a focus cluster is the sentence. Hesitancy will also arise and be of longer duration between focus clusters. On the average of Chafe finds focus-to-focus hesitations lasting .84 seconds, while cluster-to-cluster hesitations last 1.18 seconds. In turn, focus clusters are grouped into paragraph-like units: 'episodes'. Depending on the number of semantic factors which change when switching from one episode to the next, hesitation time will vary.

Thus hesitation can arise as a result of the speaker having to decide what to say next. The greater the change from one content-unit to another, the greater the length of the intermittent hesitation. In contrast with previous researchers, then, Chafe sees pausal phenomena, or a set of such phenomena, as stemming not from the encoding of thoughts into speech but from the movement from one thought to another. Such pauses would thus be seen as a reflection of completely non-linguistic cognitive activity. This is the fundamental difference with earlier researchers who have always seen pauses as a reflection of the interface between thought and language.

However, Chafe sees hesitation within the verbalization of a focus as of a fundamentally different kind. These pauses reflect the speaker's difficulty in deciding not what to say, but how to say it. The former are 'highly codable', while the more difficult verbalized concepts are 'low in codability'. He refers to a study by Brown and Lenneberg⁶⁷ which linked latency in reaction time to difference in codability. There is a

similarity between this finding and Lounsbury's notion of the link between habit strength and latency. It is not therefore surprising that Chafe attributes intra-focal hesitation to lexical decision-making.

Put simply, some referents are harder to find words to communicate than others, and the degree of hesitating is correlated with the degree of difficulty.⁶⁸

The difference with Lounsbury's lexicalist hypothesis is that Lounsbury sees hesitation as arising owing to a relation between words in sequence. Chafe, on the other hand, takes a relation between words and references as the source of lexicalist hesitation.

Whereas Lounsbury's lexicalist hypotheses were grounded in theory, it is hard to see from where Chafe could draw his evidence in calling some referents more or less easily codable than others. The only evidence he provides relies on the fact that greater hesitation occurs before referents which are low in codability. But if this is now one distinguishes referents with low codability from referents with high codability, then it would simply be begging the question to go on to say that since words naming referents of low codability tend to have hesitation before them, therefore hesitation is correlated with codability. If hesitation is the criterion for distinguishing the two types, then one may not later 'discover' that hesitation correlates with that distinction.

This points to the main problem with Chafe's work: he provides no hesitation-independent criteria for identifying focal clusters, highly codable referents, episodes, etc. His analysis of thought is apparently derived from an analysis of speech production based on the window hypothesis. Therefore, the fact that these two analyses coincide does not provide very convincing evidence for taking the window hypothesis to be true; or, for that matter, for accepting either of his analyses.

g. Good and Butterworth

Chafe's most detailed account of his theory is printed in an edited collection of papers presented at an international conference on "the pausological implications of speech production" in 1978. This conference, sponsored by the University of Kassel, produced many interesting papers on the distribution, function, and causes of pauses. Surprisingly, however, there appears to have been very little new evidence either supporting or derived from the 'window-on-the-mind' hypothesis. The reason for the absence of work in this the most prolific area of research into pausal phenomena is perhaps attributable to the effective argument presented there by Good and Butterworth against the window hypothesis.

The short paper by Good and Butterworth consists of a test of a hypothesis first presented in an earlier paper by Good. In the earlier paper, "On (doing) being hesitant"⁶⁹, Good concludes a survey of pausological research with a critique of a fundamental assumption of much of this research. This critique is based on the simple, as yet unsupported, observation that people often pause on purpose. Furthermore, they appear to be aware that by behaving hesitantly they can create certain communicational effects. A speaker may purposefully 'mask' his speech with pauses, restarts, and the like in order to achieve a particular interactional goal: e.g. in order to give the impression of anxiety, or of fatigue, or of extra care with the choice of words.

Such an observation is by no means implausible, and it does raise an awkward problem for adherents to the window hypothesis. If speakers can be shown to be aware of the possibility of using hesitancy to achieve an interactional effect, then how could one tell, for any particular instance of pausing, if 'doing being hesitant' is or is not what the speaker is doing? And, if, unknown to the researcher, the speaker is

sometimes pausing so to create an effect, then how can the researcher rely on pause behaviour as an indication of cognitive activity? Sometimes it might be caused by cognitive activity, but sometimes not; and the researcher would be unable to distinguish behaviourally between the two. Consequently, the use of the pausal data as evidence for particular theories about cognitive activity (speech encoding, anxiety, the flow of thought, ...) would become, to say the least, a risky research procedure. The window hypothesis would become (a) untestable and (b) unusable.

... whilst individuals may well need to take time out to plan their utterances, they can organize their time so that the flow of talk is not necessarily affected in a particular predetermined manner.

Now if this interpretation is correct then a legitimate extension of the argument is that speakers may overtly display the time needed to formulate an utterance, even to the extent of taking more time than is really necessary. Thus producing more hesitant utterances, with the intention of indicating that a particular piece of talk is meaningful. This implies that conversationalists commonly recognize hesitancy as associated with a need to think, and that this prosodic aspect acquires a social meaning that will be attended to, and manipulated by speakers.⁷⁰

Thus a ... problem is presented for researchers into cognitive determinants of hesitancy. How are we to distinguish between the subject who is 'being thoughtful', and the one who is 'doing being thoughtful'?⁷¹

Furthermore, Good argues that the possibility of the purposeful use of hesitancy on the part of speakers presents an even more awkward problem for pause researchers. This is so because subjects are known to alter their behaviour in response to what they perceive to be the nature and the preferred outcome of the experiment. So, it is possible that the informants in experiments, e.g. Goldman-Eisler's cartoon tests, may purposefully alter their hesitation output in order to aid in the achievement of the aims of the experimenter. This effect on the hesitation of subjects in experimental situations, as Good points out,

has not been considered in the literature on pausing.

The Good and Butterworth paper is designed to test the hypothesis underlying Good's earlier critique: that is, the hypothesis that "whilst speakers may well need to hesitate more when faced with a heavy task demand, they may also increase the relative amounts of hesitation in their speech to achieve some interactional goal ..."⁷²

They report on an experiment in which subjects were asked to tell the experimenter (whom they thought was unfamiliar with the region) how to get to the subject's place of work from the subject's home (task A). Secondly, (task B), they were to repeat the directions given in A. Task C involved the subject giving directions on how to get to a location with which he was supposedly unfamiliar. Lastly, task D, the subject was to repeat the task A directions once more, but while trying "to generate the impression in the listener, that in fact you don't know this route well at all".⁷³

In the experiment the greatest amount of hesitation occurred in task D. Tasks C, A, and B, in that order, were found to involve less and less hesitancy. In other words, when the subjects were supposed to pretend to be unfamiliar with the task, they paused more. Yet, in fact, they were merely repeating instructions which they had given twice before. Thus it cannot have been increased cognitive load which was the cause of the extra hesitancy. Furthermore, no mention of hesitation or pausing had been made in the instructions given to the subjects.

Thus the original hypothesis was confirmed. In order to achieve the interactional goal of communicating unfamiliarity with an explained route, speakers increased the amount of pausing in their speech.

This study has demonstrated that an individual's hesitancy may be as much a function of some interactional goal as some cognitive processing demand. This finding renders problematic the necessary assumptions of some workers in this field that hesitancy is directly, and only, consequent upon some underlying process that they are investigating.⁷⁴

h. Conclusion

What is most convincing about Good and Butterworth's critique of the window hypothesis is its simplicity. All they had to do was show that people sometimes pause for reasons that are not linked to cognitive processing. By so doing, they reveal an unmanageable and disruptive variable influencing all the empirical work based on that hypothesis. Consequently, serious doubt is thrown upon many of the claims derived from those empirical studies. Thus, by a disarmingly simple experiment, Good and Butterworth show that what experimenters have 'discovered' about the nature, size, and order of encoding units, the various levels of such units, the stages in the flow of thought, and the planning of sentence structures — in so far as these 'discoveries' are based on experiments within the window hypothesis approach — have a very shaky empirical foundation. They have shown, in effect, that, if the pause is a window-on-the-mind, the mind on the other side is an artifact of questionable experimental assumptions.

The ease with which such a devastating critique was made reflects an interesting fact about the development of study under the window hypothesis. It may be that few of those researchers whose work is so awkwardly questioned by Good and Butterworth will realize that the one researcher whose work escapes this criticism is Floyd Lounsbury. But even if they did recognize that the position taken by the founder of their approach remained untouched, they would not be consoled. For the behaviourist, information-theoretical foundations of Lounsbury's work — in which lie the reasons for his escape from criticism — have long since been rejected by subsequent workers in the field.

In essence, Lounsbury argued that, given certain theoretical principles (derived from information theory, behaviourist psychology, and distributional

linguistics), hesitation pauses should occur at points of low transitional probability. Such an argument may be effectively criticised in any one of three ways. It may be shown that the assumed theoretical principles are false or incompatible with each other. Or it may be argued that these principles do not in fact lead to the stated prediction regarding the occurrence of pauses. Lastly, it may be experimentally shown that what is predicted does not in fact occur. (This last argument would not in fact logically prove the falsity of the original argument, but, within the context of scientific discussion, it is a commonly accepted criterion providing grounds for the rejection of an argument.)

No researcher in pausology, including Good and Butterworth, has attempted to disprove Lounsbury's claims on the basis of either of the first two critical strategies. Such an attempt does however exist outside the realm of pausology. V. Valian criticizes Lounsbury on the basis of his theoretical principles. Her argument relies on certain of the points which made up the standard Chomskian critique of behaviourism⁷⁵

Since language is recursive at the level of a word, a phrase, a sentence, etc., it is not true that, at any given level, there is a constant and limited number of alternatives.⁷⁶

Consequently, she argues, it is nonsense to speak of the transitional probabilities of a certain verbal sequence: word-word, phonemic clause — phonemic clause, etc. In fact, given the infinite creativity of language, no particular sequence can be given a transition probability higher than zero. Valian claims that this shows the absurdity of the notion of transitional probability as applied to verbal behaviour. Furthermore, she invokes the most damaging criticism a post-Chomskian can make:

Transitional probabilities, which are based on surface structure, cannot capture deep structure regularities.⁷⁷

This is a bogus argument, at least as it relates to Lounsbury. Valian seems to assume (like Goldman-Eisler) that transitional probabilities are language-given relations. Knowledge of them, were they to exist, would presumably be a feature of competence. However, transitional probabilities are based on the estimation of the actual occurrence of given sequences. Probability estimates are rooted in performance and do not (properly applied) make any claims about competence or the structure of 'the language'.

Valian may be willing to admit that the observational measurement of transitional probabilities is all very well, but that the speaker's knowledge of his language does not involve knowledge of probabilities. That is, habit-families of stimulus and response are a fiction. The speaker does not know anything about the probability of *y* occurring after *x*; his realm of language competence only concerns the possibility of *y* occurring after *x*. Thus, habit families, conceived of as stronger or weaker associative relations between two sequential items — one a stimulus and the following its response — could not feature in the speaker's knowledge of his language. Hence any presumed relation between (a) habit family hierarchies and (b) transitional probabilities could not exist since the first of these two is an invention.

However, Valian offers no reason for ruling out the possibility of speaker's having knowledge of habit-families. Linguistic competence, as she takes it, indeed could not incorporate such knowledge. But this can just as easily be seen as a fault with the notion of 'linguistic competence', so defined, and not with the notion of habit-families. Furthermore, even if one accepts her notion of linguistic competence, this does not mean that speakers could not have some other type of knowledge of verbal behaviour besides that comprising linguistic

competence. Indeed, few Chomskians would today reject such a view. They argue instead that there are separate domains of knowledge about verbal behaviour, linguistic competence being only one (albeit the most important one). This is Chomsky's own argument in Reflections on Language.⁷⁸

So, it is perfectly possible that a speaker could be aware of habit-family hierarchies in a realm of knowledge separate from strictly-defined 'linguistic competence'. Such knowledge, as standard behaviourist theory represents it, would be acquired through one's experience of verbal behaviour and of the frequencies of certain verbal sequences. Indeed, as Lounsbury himself suggests, individuals could differ in their versions of such habit-families, depending on their own past experience. Valian's mistake is to take Chomsky's argument against the notion of habit-families and statistical structure as the basis of language learning and language structure to be an argument against any relevance of those notions to verbal behaviour. No argument is provided, either by Chomsky or by Valian, why the speaker should not be aware of habit-families and transitional probabilities. Hence, there is no reason why such knowledge could not constitute the source of hesitation phenomenon in speech.

The only argument that pausologists have put forward against Lounsbury adopts the third of the possible critical strategies. That is, they have attempted to prove experimentally that Lounsbury's predictions regarding the distribution of pauses - predictions derived from his theoretical argument - do not, in fact, accurately characterize the available data.

Boomer is the main source of experimental evidence in support of this criticism. His criticism of Lounsbury's hypothesis has been almost universally accepted by subsequent researchers, in spite of its demonstrable shortcomings. In his data, over 50% of pauses occur at or near the beginning of phonemic clauses. However, a large proportion of the

pauses occur elsewhere, that is at later positions in the clauses. Two arguments against Lounsbury were derived from these findings. The first is that a regular bunching of pauses at or near clause initial position would not be predicted by a word-transition model. On the contrary, pauses should be distributed throughout the clause.

This involves a misconception of Lounsbury's argument. In fact, Lounsbury explicitly hypothesised that the distribution of hesitation pauses might reveal an encoding unit larger than the word, with pauses tending to occur near the beginning rather than the end of this unit.

If it could be shown that the stretch of speech from one hesitation pause to the next is a convergent one, i.e. one characterized by decreasing statistical uncertainty (increasing transitional probabilities), then we would have strong support for claiming this as a unit of encoding.⁷⁹

That is, a unit is envisaged where the transitional probabilities between words are increasing; hence the probability of a pause occurring would decrease. This is exactly what Boomer found.

The second criticism would be related to the first. Boomer's evidence could be taken as disconfirming Lounsbury's hypothesis that pauses occur before words of low transitional probabilities. This argument is based on the findings of Berry (1953) where it was shown that the nuclear stress of phonemic clauses usually occurs on words of the highest information content. As nuclear stress also tends to occur at the end of phonemic clauses, hesitation pauses should be found occurring with them: just before the nucleus.

Berry's findings are open to debate. But even if one were to accept them, they do not provide significant evidence against Lounsbury. Boomer found 48% of his pauses occurring after clause initial and just-past initial position. Many of these may have been before words on which

the nuclear stress of the clause falls. If so, this would be perfectly compatible with Lounsbury's thesis.

In addition, Boomer seems to ignore the fact that Lounsbury never said that pauses will occur only at points of low transitional probability. Indeed, Lounsbury specifically leaves open the relevance of other variables to the determination of pause occurrence. Lounsbury's claim is that points of low transitional probability will tend also to be points of hesitancy, not the reverse. Boomer's findings do not upset this prediction. On the contrary, if Lounsbury's hypothesis regarding larger encoding units is taken into account, Boomer's data may be seen as a positive confirmation.

Similarly, the evidence of Good and Butterworth has no bearing on Lounsbury's hypothesis. They do not purport to argue against Lounsbury on theoretical grounds nor provide evidence on the relation between points of low transitional probability of hesitancy. Indeed, their belief that pausing may be due to a variety of reasons is perfectly in keeping with Lounsbury's argument. They do not show that one of those reasons could not be transitional probability.

However, the fact is that pausologists since Lounsbury, with the exception of the early work by Goldman-Eisler, have assumed that Lounsbury's derivation of the window hypothesis from the principles of behaviourist psychology, information theory, and distributional linguistics is mistaken. Indeed they take Boomer as having uncovered this error by disconfirming Lounsbury's predictions. Nonetheless — and herein lies the reason why the critique of Good and Butterworth applies not to Lounsbury but to subsequent pausologists — they do not drop Lounsbury's main hypothesis: the window hypothesis. In fact, Boomer explicitly states that Lounsbury's hypothesis # 2 "could serve as the basic hypothesis of the present study

(i.e. Boomer's) if the words 'and points of high statistical uncertainty' were deleted."⁸⁰ That hypothesis was:

Hesitation pauses and points of high statistical uncertainty correspond to the beginning of units of encoding.⁸¹

But whereas Lounsbury justified the postulation of this hypothesis by deriving it from theoretical principles, Boomer and later pausologists simply take its truth for granted. They could not, as Lounsbury had done, ground the window hypothesis in behaviourist and information-theoretic principles, since those principles had been rejected.

As a result, the window hypothesis is left conspicuously open to doubt. Why should we believe that pausal behaviour tells us something about the mind? No real answer is forthcoming. At best a circular argument is produced: at worst, no argument is produced at all. (as in Clark and Clark and Fodor et al.) When an argument is produced, it takes something like the following form.

premiss:	If the window hypothesis is true, pauses should occur at the beginning of encoding units
premiss:	we can tell where the beginning of encoding units occur by seeing where pauses regularly occur
observation:	pauses regularly occur at x (e.g. the beginning of a phonemic clause)
conclusions:	(a) hence x marks the beginning of an encoding unit and (b), since a pause does occur there, the window hypothesis is true.

With an argument of this nature behind him, all the new researcher has to do is find some undiscovered distributional regularity about pauses and he ipso facto can thereby both discover something new about encoding units and re-confirm the fundamental truth of the window hypothesis.

Furthermore, by relying on such an argument the pausologist opens himself up to the criticism of Good and Butterworth. Any distributional regularity of pauses can be taken by this reasoning to be an indication of cognitive activity (pre-encoding units). Hence we see the ease with which, in this field, researchers are able to draw new conclusions about the nature of cognitive activity. They merely have to observe pauses in order to observe the workings of the mind. But once someone shows, as Good and Butterworth have done, that pauses can occur for reasons not related to cognitive activity, this line of investigation becomes a dead end. The window hypothesis itself might remain intact, unsupported, but it no longer has an experimental application.

By rejecting the behaviourist theoretical basis underlying the window hypothesis, recent pausologists have rendered themselves vulnerable. They are forced to adopt a hypothesis which must collapse if it does not cover all types of pausing. But an objection of this sort, as we have seen, does not have the same effect on Lounsbury's theory. Not only was that grounded in a non-circular theoretical argument, but, as a result, it was not led to the postulation of any necessary cause for hesitancy. Room is left for other causes of pauses.

Finally, although the criticism raised by Good and Butterworth does not detract from Lounsbury's formulation of the window hypothesis, it does nonetheless show that the hypothesis can never be proven experimentally. It will always remain open to the critic to object that the pauses in the evidential data may be the result of other processes.

The psychologists studied here are interested in the causal source of pausing. That is, they are in general more concerned with the nature of that source — assumed to be cognitive activity — than they are with pausing itself. It may be that it is this inward-looking bias which

leads them to ignore other possible causes for pausing. If, instead, they had studied pauses in their own right, and not simply as evidence for theories about cognitive activity, they might not have missed the possible explanation that at least some cases of pausing stem from intentional aspects of behaviour. This is the difference between the assumption under which the window hypothesis researchers are working under the conclusion which Good and Butterworth suggest. People can pause intentionally. Hesitation sometimes arises because a speaker intends it to and not because of an unintentional, mental-mechanical delay in cognitive activity.

In this case, the following question presents itself: is an intentional pause a case of hesitation? Is it correct to say that someone who interrupts his speech on purpose is hesitating, or does the latter term only apply to interruptions which are not intentionally produced by the speaker?

How and when we identify hesitation in speech will depend upon how we answer these questions. In this case it is interesting that Goldman-Eisler takes a hesitation pause to be any non-juncture pause longer than 0.25 seconds in which no breath is taken. The problem with this criterion for identifying hesitation pauses is that no guarantee can be given that silences longer than 0.25 seconds are due to hesitation unless, by circular reasoning, we make such pausing the defining criterion for hesitation. In this case, it would not even matter that some pauses longer than 0.25 seconds are produced intentionally since the criterion for hesitation would be length of pause and not the presence or absence of a presumed mental state such as intention. In other words, the researcher would simply define 'hesitation' as a technical term referring to pauses longer than 0.25 seconds, whatever their cause. Although this move might solve the problem of telling when a speaker had or had not 'hesitated'

(in the newly defined sense of that term), it would thereby render 'hesitation' pauses useless as evidence for theories of cognition. For pauses to be allowable as evidence for cognition theories, a determinate relationship has to be established between pauses (of a certain type and length) and mental activity. Good and Butterworth's evidence regarding pausing and intentionality specifically prevents the establishment of such a relationship between pausing and cognitive delay.

The most comprehensive survey (although now slightly out of date) of investigations of pausing was published in 1973 by S.R. Rochester. Towards the end of this survey, he summarizes a view which is characteristic of much of the (socio-) psychological study of pause phenomena.

What is the next step? Given that pauses are relevant to cognitive processing and to social and affective-state variables, and that pause location indicates at least lexical and structural levels of decision-making, what remains to be established?⁸²

Given so many 'givens', the establishments one may 'establish' are legion. This is shown by the variety of different models of the mind, man, and social interaction for which pausological studies have given evidence. But these 'givens', the pillars on which the models stand, do not have the strength to support such ambitious constructions. Nonetheless, there is no inherent reason why a (socio-) psychological investigation of pauses should not produce interesting results, provided that pauses are studied in their own right, and not simply as evidence to support one particular model of speech encoding.

NOTES

Chapter Two

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CHAPTER THREE

Social Class and Hesitation Phenomena

... it seems that the additional time
of pausing determines the intellectual
quality of the verbal statement.

Goldman-Eisler (1968)¹

The work of Goldman-Eisler has had a strong influence on the sociological investigations of Basil Bernstein, her colleague at the University of London. Bernstein argues that the speech habits of the working and middle classes are fundamentally different. Each class is said to use a distinct 'speech' (or 'sociolinguistic') code. The middle class use what Bernstein calls an elaborated code, while the working class use a restricted code. In a paper published in 1962 - 'Linguistic codes, hesitation phenomena, and intelligence' - Bernstein makes use of Goldman-Eisler's work on discontinuity for the purpose of exploring the source of the differences between the two speech codes. The aim of the present chapter will be to evaluate Bernstein's use of hesitation phenomena as evidence for his theory of speech codes. A comprehensive discussion of the codes and their role in Bernstein's theory of social structure is, unfortunately, beyond the scope of this thesis.

1. Speech codes and verbal planning

The notion of speech codes is meant to refer to differing abilities to make use of the given structural and lexical possibilities of a language. The restricted code speaker only makes use of a limited set of the possibilities afforded to him by his knowledge of the language. Although the elaborated code speaker is assumed to share the same knowledge of the language as the speaker of a restricted code, nonetheless the former is able to make a much broader use of that knowledge in speech situations. He can do all that the working class (restricted code) speaker can do, and much more. The limitation on the speech ability of the member of the working class is said to be a product of socialization. No innate, genetic difference is suggested. Nor, however, is the difference in code a matter of choice. It is imposed by society. In turn, the structure of society is held itself to be largely determined by the difference in codes.

(Bernstein makes no attempt to conceal the circularity underlying his explanation. If a given generation is divided into two classes, each using a different speech code, these codes will be passed on to the next generation. Then, because of the linguistic and psychological characteristics of the code, Bernstein argues, their speakers will receive unequal treatment from teachers and employers and will not have the same access to social and economic privileges. Thus, because of their acquired speech codes, passed on to them from the preceding generation, the new generation will continue the latter's class division. Thus the circularity of this process is not a logical deficiency of Bernstein's argument, as some critics have suggested, but allows Bernstein to explain how social structure may remain stable from generation to generation.)

An analogy may here be used for illustration. We might imagine that, say, restricted and elaborated code users have identical perceptual faculties. This may be taken as meaning that they have the same human abilities to distinguish and react to differences of colour. However, the restricted code user — for reasons that are not imposed on him by his humanity but rather by his role in society — is obliged to wear tinted glasses throughout life. Consequently, his ability to distinguish colours is greatly reduced. The net result is that his behaviour in response to colour distinctions is much different from the spectacle-less member of the middle class. And this is in spite of the fact that the natural perceptual abilities of each group are identical. In turn the limited performance with colour distinctions will cause the member of the working class to be handicapped in employment. If the habit of wearing tinted glasses is passed on to the children of the working class, they too will be handicapped and will thereby insure that the social structure of the previous generation is reproduced.

Similarly, Bernstein takes the differing speech performances of the working and middle classes to be caused not by differing competences but by a social 'filter' which intervenes between competence and performance thereby restricting the verbal ability of the working class.

Competence refers to man abstracted from contextual constraints. Performance refers to man in the grip of the contextual constraints that determine his speech acts. Competence refers to the Ideal; performance refers to the Fall. In this sense, Chomsky's notion of competence is Platonic. Competence has its source in the very biology of man. There is no difference between men in terms of their access to the linguistic rule system.²

However, there is a systematic difference in the speech performances. And this is due neither to chance nor to differences in the speech situation, but rather to the intervention of speech codes. Bernstein's

concern with hesitation phenomena is the result of his attempt to explain the nature of this intervention between competence and performance.

It is not necessarily Chomsky's notion of competence that Bernstein is delicately avoiding here (and indeed it would be anachronistic to claim that in the 1962 paper this was his concern). He aims to show that what he is saying is not merely that the working class and middle class speak different social dialects or different languages. Nor does he want to claim that the difference in speech codes is genetically determined. Rather he wants to show that the different speech performances of the two classes are more than habits and yet are not in fact due to differences of inherited competence. Instead they stem from a socially imposed middle ground, between competence and performance.

Before turning to the question of how speech codes intervene between competence and performance, we first need to consider what are claimed to be the observable linguistic characteristics distinctive of speech in each of the codes. The basis of these differences lies in predictability. Restricted code utterances are said to be significantly easier to predict than the utterances of an elaborated code speaker. This sequential predictability applies both to vocabulary selection and to the selection of syntactic structure. In its extreme form the speech of a restricted code tends towards ritual, i.e. when

All the words, and hence the organizing structure
irrespective of its degree of complexity, are wholly
predictable for speakers and listeners.³

In this case, little or no information is conveyed by the lexical and syntactic choices. Any information that is to be conveyed must be a product of the non-verbal features of the speech situation. Supposedly, speech acts in the restricted code tend primarily to have either expressive (as opposed to ideational or analytic) functions or function

concerned with the reinforcement of the social bond between interactants (i.e. analogous to the presumed function of ritual). This unfamiliarity with the ideational functions of speech tends, in turn, to handicap restricted code users in school and in the employment market and so to insure that continued membership of the working class.

The elaborated code user, on the other hand, makes a much more varied selection from the repertoire of syntactic and lexical possibilities. This makes his utterances much less easy to predict. In addition, low predictability results in a higher information content for any particular choice. The elaborated code user is able to vary the selection of elements and structures so that this selection will result in "a verbal arrangement which closely fits specific referents. (...) The preparation and delivery of relatively explicit meaning is the major purpose of the (elaborated) code."⁴ Whereas the restricted code user, limited in his choice of elements and structures and therefore prevented from encoding much information into his message, is forced to communicate by the implicit means of intonation, gesture, and other non-linguistic features, the elaborated code user can make explicit use of the potentials of his language to "facilitate the verbal transmission and elaboration of the individual's experience."⁵

It is important that, like Lounsbury, Bernstein accepts the linguist's principle of the linearity of the sign.⁶ That is, speaking is taken as linear output rather than as a multi-dimensional activity integrating many contemporaneous activities.⁷ If Bernstein were to take linguistic performance as such a multi-dimensional activity, then the question of the sequential predictability of utterances would take on a different appearance. Given the principle of the linearity of the sign, Bernstein can measure the predictability of a word in the context of the words

preceding it. But if the linearity principle is dropped, the whole behavioural and situational context must be used in the assessment of predictability. Bernstein (and Lounsbury) would have to frame his (their) predictability statements in terms such as the following: 'in this situation, with these conversationalists and the particular purposes they have at this moment, the probability that the speech act ϕ will be produced is ...'. It remains to be seen whether it would be methodologically possible to estimate transition-probabilities in this way. However, even if it were somehow possible, the implications of such estimates would, for Bernstein, be unappealing. For, to say that how the working class participate in communication situations is highly predictable is to say that a very large slice of their daily lives is predictable. Only by adopting the principle of the linearity of the sign is Bernstein able to confine his claims to the linear dimension of linguistic units in sequence. He thereby avoids making any statements about the determinacy of working class life.

To summarize: although users of either code are said to have an equivalent competence in their language, the restricted code speaker is somehow limited in his ability to draw upon that competence. This results in utterances of a relatively high degree of sequential predictability. The question that then arises is how this limitation on the verbal ability of the working class speaker is achieved. Bernstein replies to this question with two arguments: one sociological (based on how the child is 'socialized' by its family), the other psycholinguistic. Our concern here is with the latter.

His reply to the psycholinguistic aspect of the question — how does the competence of the working class speaker become restricted in performance? — is based on the notion of verbal planning. Between a

speaker's knowledge of his language and his production of concrete utterances in that language there is thought to intervene the mental processes by which the speaker draws upon his knowledge for the purpose of planning a subsequently produced utterance. It is in the verbal planning component that Bernstein locates the source of the observable differences between restricted and elaborated code utterances.

The (speech) codes, linguistic translations of the meanings of the social structure, are nothing more than verbal planning activities at the psychological level and only at this level can they be said to exist

8

The picture of verbal planning which Bernstein draws conforms to the information theoretical conception of planning promoted by Goldman-Eisler in the 50s and early 60s. Following Goldman-Eisler and Lounsbury, latency in speech delivery is taken as an indication of planning difficulty. Specifically, if a sequential transition is of low predictability, selection of the next item will be delayed and hesitation will result. Thus, in view of the fact that Bernstein locates planning as the source of the distinctions between codes and that these distinctions are founded on the predictability of the utterance sequences in each code, it is not surprising that he should find Goldman-Eisler's theory of hesitation phenomena of great relevance to his own theory of speech codes. Since the speech of a restricted code is, supposedly, more predictable than the speech of an elaborated code, it should also be more fluent: i.e. it should involve a lower amount of hesitation. Inversely, elaborated speech should produce more pauses because of its lower predictability. What is even more important for Bernstein's purposes is that, if the hypotheses correlating hesitation and speech codes are confirmed, he can claim, on the basis of Goldman-Eisler's theory, that the source of this correlation lies in verbal planning. That is, low sequential

probability is said to lead to latency in planning and, therefore, to hesitation in speech. By this strategy Bernstein can claim to have proved that verbal planning is the source of the difference between restricted and elaborated codes. And, by so doing, he can explain how the speech performance of the working and middle classes differ, in spite of their sharing the same degree of linguistic competence. By intervening between competence and performance, verbal planning creates the observed distinctions in speech codes.

Consequently, Bernstein would hope to find, through experimentation, that working class speakers pause less than middle class speakers. Such a finding would tend to confirm his claims (a) that the restricted code is more predictable than the elaborated code, and (b) that — because hesitation is assumed to be caused by planning — the observable performance differences between the codes stem from a more deep-rooted difference in verbal planning strategies.

Fluency and hesitation would seem to discriminate
between two kinds of speech and differentiate levels
of verbal planning.⁹

Bernstein is not clear why working class speakers should choose more predictable items and structures than middle class speakers. He suggests that restricted code speakers use less time in making their choices. If this is supposed to be taken as an account of why items of greater predictability are chosen, a new question arises at one remove: why and how is the restricted code user afforded less time to plan a sequence than is the speaker of an elaborated code? Is such mental haste thought to be due to genetic factors? Or is it the result of greater interruptive pressure put on children growing up in a working class environment? Bernstein neither answers these questions nor appears to see their importance. In the final analysis, no explanation is offered why members

of different clauses should use different mental planning strategies. In this case, it becomes even less clear what has been gained in avoiding taking taking different linguistic competence as the source of the distinctions between speech codes. Of course there are political and ethical reasons why the sociologist would not want to claim that one class is less competent than another, but is Bernstein not doing just that in his postulation of different planning strategies? What is the explanatory benefit of the hypothesized distinction between planning ability and competence? A closer look at Bernstein's empirical study will serve to illuminate this dilemma.

2. Conflicting experimental results

An experiment was devised in order to compare the hesitancy of speakers in the middle and working classes. One group of 61 boys said to be from the working class were contrasted with a group of 45 public school boys. Both groups ranged in age from fifteen to eighteen years. The groups were sub-divided according to verbal and non-verbal I.Q. profiles in order that the comparisons could be related to intelligence. Discussions on the topic of capital punishment were held in each group in the presence of a research assistant. From each discussion a sample was taken of approximately 1800 words. However, following the practice of Goldman-Eisler, only utterances of greater than forty syllables were analysed, on the grounds that hesitation patterns only emerge in longer utterances.

The overall results of the analysis show the working class subjects paused less often and spent less time pausing than did the middle class subjects. These differences remain even between sub-groupings where verbal and non-verbal intelligence are held constant.

Bernstein takes these results as confirming his hypothesis that speakers of a restricted code use more predictable items and structures

than do middle class speakers. In other words, because middle class speakers use less predictable sequences they hesitate more often and for longer in planning such sequences. At the same time, the results are assumed to confirm the hypothesis that verbal planning is the source of the characteristic difference in predictability between restricted and elaborated verbal sequences.

It is interesting that Bernstein does not consider the logical possibility (given his data) that the middle class pause more because they are less skillfull at verbal planning than working class speakers. That is, it may be that the middle class would spend more time planning a particular verbal sequence than the working class would spend planning the very same sequence of words. It is for this reason, one might argue, that the middle class are found to be more hesitant.

This suggestion is not as perverse as at first it may sound. It points to the unwarranted use of amount of hesitation both as an indication of the predictability of a sequence (that is, as a measure of the difficulty such a sequence would present to planning) and as a window on the actual process of planning that sequence. This is rather like saying because one speaker stutters more than another, his utterances must be phonologically more complex to program. Therefore, his stuttering is taken as an indication not of a phonological disability but of more ambitious phonological planning. If the predictability of Bernstein's test utterances were held constant — and so also their presumed planning difficulty — it is possible that the middle class might still hesitate more. No evidence is presented to rule out this possibility. So, it is perfectly plausible to interpret Bernstein's evidence as indicating the greater planning skill of the working class.

An objection might be raised here to the effect that this interpretation

of Bernstein's data is blocked by the fact that the differences remained between the two classes even when, for a subgroup from each class, verbal I.Q. scores were the same. There is, however, no evidence for taking verbal I.Q. scores as an indication of level of ability in verbal planning.

Alternatively, it might be objected that the notion of one social class (especially the working class!) being more skilled at verbal planning is absurd. How, it would be asked, could such a situation arise? This objection would not be so easily countered. But, in view of the fact that Bernstein leaves it unanswered why it is that the working class spend less time planning and choose more predictable items, ignoring such an objection would not be without precedent.

What Bernstein would need in order to make his case more convincing is independent evidence to the effect that the middle and working classes are equally skilled verbal planners. Then his data could be taken as evidence that, in spite of equal degrees of planning skill, the working class do not make full use of that skill. Instead, his data may be interpreted as evidence that the two classes do not have the same planning skills, and that the working class is in fact the more skilled. For that reason, they pause less.

This problem reveals the general dilemma faced by any theorist who aims to provide a psychological explanation of a systematic difference in the verbal performance of two groups while at the same time maintaining that the two groups do not differ in linguistic competence. A reason must be found why the psychological realization of their verbal abilities is identical up to a point and then different. Bernstein holds that working and middle class speakers have equivalent linguistic competence as well as equal skill in verbal planning, but nevertheless plan differently. The dilemma faced by such an account is that it will always be possible to object to the location of the divergence in ability.

Bernstein places this divergence, in a sense, 'between' planning skill and planning performance. It could just as easily be placed between linguistic competence and planning skill, i.e. by claiming that the two groups share competence but differ in planning skill. Or, the location of the divergent source could be 'pushed back' to its limit, e.g. by claiming that the two groups differ in linguistic competence itself. The dilemma is irresolvable because no behavioural grounds exist for taking one boundary location over another. Instead the choice must be made on different grounds. Bernstein's reasons for maintaining that the working and middle classes share the same competence probably stem from certain moral principles underlying his sociology. What is of interest here is that, given his adoption of hesitancy as a window on verbal planning, he is forced to draw the boundary where he does: between planning skill and planning performance. Otherwise, he would have to claim that the working classes are more skilled verbal planners than the bourgeoisie. But the question remains: how does the competence of the working class become restricted in performance?

In 1974, P.A. Jones replicated Bernstein's experiment and yet produced results which directly contradict those of Bernstein. In Jones' experiment two groups of 25 boys each were selected from schools in Newfoundland. The boys were between 10 and 12 years of age. One group was deemed to be from the working class and the other middle class. The subjects were interviewed individually and asked to say what they saw in a group of six pictures. In addition, Jones says that the two groups appeared to differ in verbal ability. That is, one group displayed the features which would qualify their speech as elaborated, while the other group spoke a characteristically restricted code.

Nevertheless, she found that the middle class speakers paused less frequently and for less time than did the working class, restricted

code speakers. In other words, although the middle class speakers showed a greater grammatical and lexical complexity in their speech, this did not appear to require verbal planning latency. These results are exactly the opposite of those produced in Bernstein's experiment.

If hesitation patterns reflect only cognitive verbal planning, present findings would seem to contradict (Bernstein's) position. Speech of the present high-verbal boys ... is both more fluent and grammatically more complex than that of the low verbal boys, and could undoubtedly be classified as elaborated in the same sense as that so classified by Bernstein (1962). Yet the high-verbal boys pause less frequently than the less effective speakers.¹⁰

What can be made of such contradictory findings? A possible source of this divergence may be a difference in experimental technique. Dittmar (1976) criticizes Bernstein's experiment for a number of reasons. One of these concerns the fact that it was felt necessary to practice the discussion with the working class boys. Two practice discussions on the topic were held in the weeks preceding the experiment. No such sessions were felt necessary for the middle class boys. Bernstein remarks that, in the experiment itself, the research worker often found it necessary to intervene in the working class sessions in order to avoid the discussion breaking down 'when voluntary contributions came to an end'.¹¹ Dittmar questions the reasons for these interventions

This can be interpreted in two ways. Either it is possible that the (working class) boys did not have enough motivation to speak on the set topic, or the reason for their lack of motivation could have been that they had already had two practice discussions. At any rate we cannot exclude the possibility that in these circumstances the boys had no inducement to display their actual linguistic capabilities in the discussion.¹²

In addition, it could be argued that a reason why Bernstein found less pausing in the speech of the working class boys was that they had practised their pronouncements on the topic of capital punishment.

Having discussed a topic on two previous occasions can easily lead to predictable and fluently performed utterances on a third occasion. If Bernstein is to accept Goldman-Eisler's arguments on the causes of verbal hesitation, he will be forced also to agree with her that practice leads to greater fluency: i.e. to less frequent and shorter pauses. Unfortunately, Jones does not provide enough detail on her experimental procedure to be able to tell if this quirk in Bernstein's experimental method is the source of their divergent results.

Bernstein, like Goldman-Eisler, excludes pauses with a duration of less than 0.25 seconds from analysis. Curiously, only silent pauses seem to have been taken into account. Yet, there is no reason, in principle, to exclude filled pauses and repetitions from possible locations of planning activity (if it is given that discontinuity of performance is required for planning: i.e. the window hypothesis). Bernstein does not give any reason for considering only silent pauses. It could be the case that his middle class subjects produced more filled pauses and repeated themselves more often than the working class subjects. As Bernstein does not offer samples of the data used for his analysis, there is no way of knowing how the inclusion of filled pauses and repeats would have effected his results. Again, it is unclear whether Jones' data differed from Bernstein's in this respect.

Perhaps the most questionable assumptions underlying Bernstein's experiments on hesitation phenomena are (a) that a silent pause necessarily implies the concurrent occurrence of verbal planning and (b) that verbal planning requires a delay in external activity (i.e. speaking) in order to occur. P.A. Jones specifically questions the second of these two. She finds that even though her elaborated code users pause less than her restricted code speakers, the former have a slower rate of articulation.

She hypothesizes that this may allow the 'extra' planning required for elaborated speech.

Bernstein's suggestion that elaborated code users engage in coding behaviour primarily during pauses, could be rivalled by a suggestion from this study, that verbal planning accompanying effective speech goes on concurrently during speaking, resulting in a slower speech rate.¹³

Although this is an untested (and untestable?) hypothesis, it does, nevertheless, present an alternative explanation for those who want to hold that elaborated speakers must do more verbal planning. However, it rules out the relevance of hesitation phenomena as a direct indication of planning activity. (It would, nevertheless, involve another version of the window hypothesis, in which articulation rate was to be taken as the observable phenomenon correlatable with planning).

3. Conclusion

It appears that there is no way that the contradictory experimental results of Jones and Bernstein may be resolved. Doubts may be raised about the validity of Bernstein's experimental method, but it is not certain that these doubts could not also be raised concerning Jones' experiments. In any case, there are more deep-rooted problems in the attempts to take the distribution of silent pauses as an indication of verbal planning processes.

For instance, Bernstein argues that the restricted code user relies more on the non-verbal components of a communication situation than does the speaker of an elaborated code. Thus, the restricted code speaker makes greater use of the 'expressive features' of speech. Why is the use or avoidance of silence not considered a feature of this type? Good and Butterworth have given experimental evidence to the effect that speakers will use silence as an intentional part of their verbal

behaviour (cf. Chapter Two). Could this not be a source of the greater frequency of pauses found by Bernstein in middle class speech (or for working class speech as found by Jones)? Bernstein would appear to presuppose that speakers do not pause (mid-sentence) on purpose: i.e. for effect. This seems odd in view of his claim that the speakers of a restricted code are more concerned with 'how things are said' than with 'what is said'. Why could the avoidance of pauses not be a feature of this concern? (Or perhaps the working class tend to fill their pauses with an 'um' or 'er' more than the middle class do, in which case their pauses would not be counted by Bernstein.) A possible hypothesis (given Bernstein's data) is that working class speakers learn to avoid silence mid-turn in speech in order to forestall interruption by other interactants. It might be the case that the working class are more given to interruptions at a pause than are middle class speakers. Thus a strategy of pause avoidance would be understandable. Similarly, it could be that middle class speakers learn to put pauses in their speech in order to give their talk an air of deep thought (just as public school children are said to have learned, in the reign of the last King, to stutter like the King in order to project class status.)

Instead, the window-on-the-mind hypothesis is adopted without question. The occurrence of silent pauses longer than 0.25 seconds is assumed to be uniquely indicative of particular processes in the mind. By studying the distribution of pauses one supposedly arrives at insights regarding mental operations. Under such an assumption, it is not surprising that Bernstein should have found differing hesitation behaviour between social classes such an important discovery for his theory of codes. It meant that he could identify psychological grounds for the observable distinctions between the two codes. It meant, in

effect, that he could refer outside of his own discipline (and the discipline of his critics) to confirmatory findings from another discipline, psychology. It should be of little wonder that Bernstein's linking of hesitancy and speech codes is the one area of his theory which has received the least attention and the least criticism, for it invoked findings from outside the realm of his critics' competency.

What is however surprising is that Bernstein, a sociologist, should adopt a completely non-sociological account of a particular feature of verbal behaviour. Furthermore, as was argued in the last chapter, this account is flawed specifically for the neglect of sociological forces: i.e. by neglecting that silence can have an interactional function. And, even if we take the window assumption as given, it is not clear on what grounds filled pauses, articulation rate repeats, and self-corrections may be excluded from the analysis. Why are they assumed to be determined by non-psychological forces, while silent pauses are determined by the psychological processes of the speaker?

It should be clear that Bernstein would have avoided the dilemmas inherent to his account of hesitation if he had pursued a more detailed sociological (or interactional) analysis of the occurrence and function of pauses in speech. He may (or may not) then have found class differences in both the distribution and function of pauses. Instead he assumes that, although the distribution may differ, the function is invariant. This is the result of importing Goldman-Eisler's theory from psychology as some sort of deus ex machina explaining the difference in the speech of the two classes. That is, his sociological argument supporting the code theory goes so far and then, at a crucial moment, shifts realms into the psychological.

Why was this shift felt to be necessary? Bernstein does not say. However, it is probable that he felt it necessary to provide an account

of how the individual's mind must mediate between the social influences on the individual and the individual's own social behaviour: that is between the social patterns that affect him and his own contribution to those patterns. For patterns of group behaviour not to be random, there must be — it is thought — some psychological reason why each individual in the group continues to produce his appropriate bit of the pattern. Bernstein did not seem to feel that purely sociological explanation was powerful enough to explain why each individual in a group behaves in ways similar to the behaviour of the other individuals in the group. Instead, one must turn to psychological theory for this explanation. Sociological accounts, Bernstein seems to have felt, must be grounded in psychology. (This argument will be familiar to linguists as that used by Chomsky against the distributionalists.) But once this shift is made, the grounds for a difference between individuals comes into the question. Why should the working class speaker plan more quickly and more predictably? How can a mental process be class-determined? Are we each members of a particular social class because of the way our minds work? By turning to the psychological in order to explain patterns of group behaviour, Bernstein is led to ignore possible sociological explanations of that behaviour.

NOTES

Chapter Three

1. Goldman-Eisler, Psycholinguistics, 1968, p.68.
2. Bernstein, "Linguistic codes, hesitation phenomena and intelligence", Language and Speech, 5, 1962.
3. Bernstein, "A sociolinguistic approach to social learning", in J. Gould (ed.), Penguin Survey of the Social Sciences, 1965 (quoted in Dittmar, Sociolinguistics, 1976, p.22).
4. Bernstein, "Linguistic codes, hesitation phenomena and intelligence", Language and Speech, 5, 1962, p.33.
5. Bernstein, "Linguistic codes, hesitation phenomena and intelligence", Language and Speech, 5, 1962, p.33.
6. cf. Saussure, Cours de linguistique générale (ed.) T. de Mauro, 1978, p.103.
7. cf. Harris, 1981, Chapter 6.
8. Bernstein, "A sociolinguistic approach to social learning", in J. Gould (ed.), Penguin Survey of the Social Sciences, 1965 (quoted in Dittmar, Sociolinguistics, 1976, p.22).
9. Bernstein, "Linguistic codes, hesitation phenomena and intelligence", Language and Speech, 5, 1962, p.42.
10. Jones, "Elaborated speech and hesitation phenomena", Language and Speech, 17, 1974, p.102.
11. Bernstein, "Linguistic codes, hesitation phenomena and intelligence", Language and Speech, 5, 1962, p.38.
12. Dittmar, Sociolinguistics, 1976, p.50.
13. Jones, "Elaborated speech and hesitation phenomena", Language and Speech, 17, 1974, p.202.

CHAPTER FOUR

Repair

This chapter is in two parts: exegesis and analysis. It begins by providing an outline of the notion of repair and of its study by the ethnomethodological school of conversational analysis. This notion appears to have undergone modification in the published literature. An attempt will be made to trace its development. The remainder of the chapter comprises a critical analysis of the 'explanation' of repair in the works of Sacks, Schegloff, Jefferson, Drew, and Wootton.

The term 'ethnomethodology' refers to a research programme adopted by a breakaway group of sociologists. In the context of this thesis it is worth pointing out that the creation of the ethnomethodological programme came about in response to criticism of orthodox sociological method, such as that practised by (among many others) Basil Bernstein. The focus of ethnomethodology is on practical reasoning, whatever the situational context in which it is displayed. One such context is held to be conversational interaction and the organization of turn-taking therein. Consequently, a school of conversational analysis has sprung up within ethnomethodology taking its inspiration from the works of E. Schegloff and the late Harvey Sacks. Ethnomethodological conversation analysis, like the parent discipline ethnomethodology, pursues the study of practical reasoning by taking conversation as a setting in which the 'formal structures of practical actions'¹ are displayed. These structures

which provide the observable orderliness of conversation, are claimed to be at the same time, (a) invariant, 'context-free' features of the production of conversational order, and (b) 'situated accomplishments' of the interactants' methods of practical reasoning. The formal structures underlying conversation are then both 'context-sensitive' and 'context-free'.²

Since conversation can accommodate a wide range of situations, since it is a vehicle for interactions in which persons in varieties of identities and varieties of groups of identities are operating, since it is sensitive to the various combinations, and since it is capable of dealing with a change of situation within a situation, there must be some formal apparatus that is itself context-free, that by virtue of the ways in which it is context-free can in local instances of its operations be sensitive to, and exhibit its sensitivity to, various of the parameters of social reality in a local context. Some aspects of the organization of conversation must be expected to have this context-free, context-sensitive status, for, of course, conversation is a vehicle for interaction between parties with any potential identities, and with any potential familiarity. It began to look to us as if the organization of turn taking for conversation might be such a thing.³

The notion of repair, as a context-free but context-sensitive organization for the management of discontinuities, takes its place within the proposed systematics of turn-taking. The best way to introduce this notion is to present it through an account of the major ethnomethodological studies of it.

1. Exegesis

The most important study of repair from the ethnomethodological perspective is that jointly written by E. Schegloff, G. Jefferson, and H. Sacks, entitled 'The preference for self-correction in the organization of repair in conversation' (hereafter 'SJS'). Published in Language in 1977, SJS is not the first article in which these authors have concerned themselves with repair. Repair is also discussed in the principal article of ethnomethodological conversational analysis: 'A simplest systematics for the organization of turn-taking in conversation',⁴ also written by Schegloff, Jefferson, and Sacks. Gail Jefferson published a series of three articles relating to repair, all derived from her Ph.D thesis: 'Sequential analysis of two types of conversational disruption'.⁵ These papers contain some of the seeds of the later SJS paper but do not present a unified theory of repair as the latter does. Nor does Jefferson formulate the notion of 'repair', but speaks instead of 'error-correction', a class of operations which SJS and later researchers argue is merely a subset of the more general notion of repair. Still, an interesting light is cast on SJS and on subsequent work on repair if they are considered as developments from one of Jefferson's early papers.

In 'Error correction as an interactional resource',⁶ Jefferson claims that in (American? Californian?) English the article the is pronounced as /ði/ (which Jefferson writes as 'thee') before words beginning with a vowel, while /ðə/ (Jefferson's 'thuh') appears before those words with an initial consonant. She finds also that the 'hesitation marker' — 'uh' — is regularly preceded by 'thee', thus giving rise to the hypothesis that 'uh' has the status of a lexeme in English. Instances of 'thee+uh' are said to be cases of a word search. The use of 'thee' (as opposed to 'thuh') is therefore claimed to show that the speaker is aware of

an upcoming difficulty (i.e. the word search) and for this reason uses the form of the article appropriate for preceding a hesitation marker like 'uh'.

Having outlined this distribution of 'thee' v. 'thuh', Jefferson turns to the explanation of exceptions to the distribution: that is, instances of 'thuh + uh' . The non-conformity of 'thuh + uh' is said to reveal the operation of a separate device organizing conversation: an error-correction format. This is outlined as Word₁ + hesitation marker + Word₂ where word₂ is a correction of the error, word₁. An instance of error-correction is said to be the following:

1. He was here lay- uh earlier, but he left

In this example, lay- , said to be a cut-off version of 'later', is the word in error, i.e. word₁. It is followed by the hesitation marker uh which is said to signal that the following word, earlier , is a correction of the cut off lay- .

In the case of 'thuh + uh' as an instance of the operation of the error-correction format, things are less straightforward. 'Thuh + uh' suggests to Jefferson that the speaker rejects at the last moment, i.e. after the articulation of the appropriate article, a projected consonant-initial noun. The difference is that 'thee + uh' is said to signal a word search, while 'thuh + uh' signals that the projected noun was chosen but then rejected just before its production. In both cases the hesitation supposedly occurs to facilitate the search for a new word, but only in the case of 'thuh + uh' is an already projected word avoided.

Consequently, 'thuh + uh' is interpreted by Jefferson as a case of error-correction in which Word₁, the error, is never produced.

2. I turned onto thuh uh left lane

Jefferson claims that in this example 'right' had been projected to

follow the compatible thuh but, as Word₁ in the error-correction format, was rejected after the production of thuh and replaced by Word₂: left.

The principal claim of SJS is that there is an independent mechanism organizing repair in conversation. A development from Jefferson's error-correction format, the repair mechanism is claimed to be the 'self-righting mechanism for the organization of language use in social interaction'⁷. It is said to operate on a case by case basis on particular sequential environments, producing the 'observable orderliness' of certain features of conversational speech.

SJS holds that repair organizes not only phenomena such as in examples 1, 2, and 3 but also in examples 4, 5, and 6. That is, the mechanism of repair is thought to govern (what in SJS is called) 'same-turn self-repair' — i.e. examples 1, 2, 3 — 'next-turn other-repair' — i.e. examples 4 and 5 —, and 'third turn self-repair' — i.e. example 6.

1. He was here lay- uh earlier but he left

2. I turned onto /thuh/ uh left lane

3. Sure enough ten minutes later the bell r- the
doorbell rang ...

4. Ken: Is Al here today?

Dan: Yeah

(2.0 seconds pause)

Roger: He is?

Dan: Well he was

5. Louise: Do you go to therapy

(1.4)

Dan: Do I?

(0.3)

Louise: Mm hm?

6. L: I read a very interesting story today

M: uhm, what's that

L: Well not today, maybe yesterday ...

(for references to all examples, see footnote 8)

Repair, then is a concept which the ethnomethodologists set within their notion of the multi-party negotiated management of conversational interaction. This stands in contrast to the psycholinguist's interest in discontinuity as an individual's psychological problem. The ethnomethodological perspective consequently requires a very different interpretation of the observable evidence of discontinuity. This interpretation will be the principal topic of the chapter.

Given that the ethnomethodologists are concerned as much with other-repair and third turn self-repair as they are with same-turn self-repair, their subject encompasses a larger domain than that explicitly chosen for this thesis. In this chapter, our primary focus will be on same-turn self-repair, in accordance with the principles set down in the first chapter regarding the concept of discontinuity. However, since an understanding of the notion of same-turn self-repair cannot be formed unless that notion is set within the more encompassing notion of repair (all varieties), a certain amount of attention will have to be paid to other-repair and subsequent-turn repairs.

In SJS, repair is pictured as consisting of three component operations, some of which are not always explicitly performed: (1) repair-initiation; (2) location of the 'repairable' (i.e. of that which is being repaired); (3) production of the candidate repair (hereafter referred to as the 'correction'). All three of these operations — initiation, location, correction — may be performed by one word, as in example 7.

7. A: and uh, he said that he was, had tried to call her
on Mothers day ...

In this example, the occurrence of had after was initiates the repair, locates was as the repairable, and corrects it. On the other hand these operations may be handled separately. In example 3, the repair is initiated by the 'cut-off' r-. The repairable (bell) is located by the repetition, post-initiation, of its preceding context (that is, by the repetition of the). The repairable is corrected by the candidate replacement, doorbell.

3. Sure enough ten minutes later the bell r- the doorbell
rang ...

In example 5, repair is initiated by 'other' (by the/a hearer of the speaker's original turn; in this case, Dan = other, Louise = self) who repeats part of Louise's question accompanied by question intonation (signalled in SJS's transcription system by a question mark⁹). This repetition of a part of Louise's turn also locates that part of her turn as the repairable. The 'correction' is provided in 3rd turn (i.e. Louise's second turn) and is, in this case, not strictly correcting her earlier statement but re-affirming it.

5. Louise: Do you go to therapy
(1.4)

Dan: Do I?
(0.3)

Louise: Mm hm?

Same-turn self-repair is therefore represented as only one variety of repair, incorporating component operations which are also present in other varieties of repair. All versions of repair are thus seen as instances of one and the same 'organizational mechanism operating in (a) particular sequential environment'¹⁰. Depending on the environment, the 'observable orderliness' of the operations will change but in each case

will remain the product of a unique repair mechanism.

Although speaker and other may initiate repair on the same 'trouble types' (i.e. on the same type of repairables) their initiations are differently positioned and employ different techniques. Whereas most self-initiated repairs progress to completion of successful repair in the same turn as initiation, most other-initiated repairs end up being successfully completed by the speaker of the 'repairable'. Thus, self-initiation and other-initiation of repair are also said to 'project different trajectories' towards repair outcome.¹¹ SJS conclude that although self-initiation and other-initiation of repair are different in many ways, they are nonetheless related to each other and dependent on each other.¹² Positions for self-initiation are said to alternate on a turn-by-turn basis with positions for other-initiation. SJS suggest that this ordering should be interpreted as 'involving a serial ordering of opportunities to repair some same potential repairable'.¹³ The serial distribution is claimed to be a product of the repair mechanism. This mechanism provides among other things for the withholding of other-initiation: that is, from the same turn as the trouble source and from the short period just past the possible completion of that turn (i.e. the 'turn-transition space',¹⁴). SJS thus take the low frequency of other-initiation of repair during or just after the trouble-source turn as evidence of a preference for the self-initiation of repair. The repair mechanism is said to relate these non-equivalent operations of repair — self-initiation and other-initiation — to each other on the basis of preference. Self-initiation is preferred to other-initiation. Accordingly, the fact that there is very little other-initiation in the same turn as the trouble source is understood as evidence that, in response to the preferential relation between self-initiation and other-initiation,

the hearer withholds initiation in order to permit the speaker to initiate repair himself.

SJS report that self-initiation is most frequently followed by self-correction and that this is the most common type of repair. Furthermore, even when other-initiation does occur it is almost invariably completed by the speaker of the trouble source, rather than by the initiator of the repair. Examples 4 and 5 illustrate this. SJS point out that, in self-repair, the activity of locating the repairable is normally accomplished in the same operation as the correction (cf. example 1 where the correction 'earlier' also locates 'lay-' as the repairable). In such cases repair-initiation may be a separate feature: e.g. a cut-off word or a 'non-lexical perturbation' (e.g. a filled pause). With other-repair it is normally initiation and location of the repairable which are accomplished by the same operation (correction being left to the speaker of the trouble-source). A common form of such compacted other-initiation and location occurs when the hearer repeats the repairable with a questioning intonation (as in example 5, allowing for the change of pronoun).

4. Ken: As Al here today?

Dan: Yeah

(2.0)

Roger: He is?

Dan: Well he was

5. Louise: Do you go to therapy

(1.4)

Dan: Do I?

(0.3)

Louise: Mm hm?

1. He was here lay- uh earlier but he left

The fact that in such instances 'other' does not normally follow up his initiation and location of the repairable by offering a candidate repair¹⁵ is taken as evidence for a preferential relationship between self-correction and other-correction, with the former preferred to the latter. Thus, the repair mechanism is pictured as providing for a preference for self-initiation and self-correction over other-initiation and other-correction. When exceptions to these preferences occur they are said to reflect their non-preferred status by being 'modulated in form': e.g. accompanied by 'uncertainty markers', 'downgraders', etc.

In a paper published in 1980 — 'The relevance of repair to syntax-for-conversation' — Schegloff continues his study of repair from that first presented in the SJS paper. His primary aims in the later paper are to show (a) that the repair mechanism affects the syntax of sentences in an orderly and describable way, (b) that the ways in which repair affects sentential syntax are partially determined by certain of the structural pressures of discourse organization, and (c) that repair is relevant to any sentence of a language. Special attention is paid to the types of syntactic changes brought about within sentences through the operation of repair.

Repair ... does not merely occur in sentences; it can change their shape and composition, and can do so within a retained identity of 'the sentence'.

Schegloff argues that the fact that repair systematically occurs within sentences, rather than over a sequence of sentences, is due to the influence of two particular sequential features of conversation. His argument proceeds in two steps. First, he draws on ethnomethodological studies of conversation in claiming that there is a preference in

conversation for every 'next' turn to be free so that its speaker may do what was sequentially implied for that turn by the previous turn. That is, for example, if speaker A asks B a question, then the turn following the completion of the question is allotted to B for the purpose of answering the question. This preference would then lead by implication to a further preference for speaker A to accomplish any necessary repair within the same turn as the repairable so that the next turn remains free for speaker B to perform any sequentially implied action. So a preference for 'next turns' to be free for sequentially implied 'nexts' (answers to questions, replies to statements, assessments of appraisals, etc.) is seen as motivating a preference for same-turn (and, because same-turn implies same-speaker) same-speaker repair.

Schegloff then points out that, as matters stand, it would still be possible for the speaker of an error to conform to the preference for same-turn repair by taking his trouble-source sentence to completion and then repairing the trouble in a subsequent sentence still within the same turn. This however is not common. Again drawing on other ethnomethodological studies of conversation, Schegloff claims that the turn-taking organization which regulates conversation provides for any turn to be possibly complete whenever a turn-constructual unit (e.g. a sentence) is possibly complete. In addition, it is held to be the case that turn transfer becomes relevant whenever a turn is possibly complete. So, if speaker A were to leave the repair of an error in his sentence until a subsequent sentence in the same turn, he might very well find that speaker B had already begun his answer at the moment the first sentence was complete (thereby conforming to the motivation instilled by the turn-taking organization to minimize gap between turns). Consequently, a second repair preference arises: viz. to repair within the same sentence as the repairable.

Most commonly the integrity of the sentence is not preserved, and repair occurs not in a sentence devoted to it, but 'intrusively' in a sentence occupied with something else (the something which can be sequentially implicative in 'next turn').¹⁷

The basis for this distribution, then, lies in the preference organizations regulating the 'discourse systems' of adjacency pairs and turn-taking.

It is interesting that Schegloff's argument relating to the motivations for the positioning of repair-initiation appears to contradict arguments he produced with Jefferson and Sacks in SJS. In this earlier paper the mechanism of repair was pictured as independent of other discourse organizations. Yet in the later paper Schegloff argues that repair positioning is (at least partly) determined by two such organizations: adjacency pairs and turn-taking. He does not speak either of the 'orderliness' of self-repair as resulting from the same organizing mechanism at that which governs other-repair, a principal theme in the earlier paper. This 'orderliness' is now given its own motivation. He does not comment on this apparent contradiction but, somewhat paradoxically, reaffirms his approval of the SJS conclusions.¹⁸

Like SJS, Schegloff distinguishes three component parts of the repair operation: initiation, location of the repairable, and production of the candidate repair (or what is here being called 'correction'). Repair-initiation in the same turn as the trouble-source is said by Schegloff to be either pre-positioned or post-positioned in relation to the repairable. An instance of post-positioning is to be found in example 3. Here the repairable is bell and the repair-initiator is the post-positioned r-/

3. Sure enough ten minutes later the bell r- the doorbell
rang ...

1. He was here lay- uh earlier but he left

8. I can have the garment left next door at the uh jeweler's
shop.

Schegloff refers to post-positioned initiators as 'backward-oriented'.

A typical exponent of such backward-oriented repair-initiation is a word which is not produced to completion: a 'cut-off'. Example 1 illustrates such a 'backward-oriented' cut-off initiator.

Forward-oriented or pre-positioned initiation is said to be carried out by a filled or silent pause, or by a sound-stretch. They are claimed to 'stand in the place of a next-due item' and in this way to initiate repair on that item. The paradigmatic example of such initiation results in the word-search, as in example 8. An important feature of the forward-oriented initiator, according to Schegloff, is that it 'can retain the projective import of the syntactic shape of the sentence-so-far' (i.e. it does not signal that some part of the sentence before the repair-initiation is going to be changed.) The post-positioned initiator on the other hand, indicates that some part of the pre-initiation sentence is going to be repaired. Consequently, what is projected as the outcome of the sentence may also be altered.

Schegloff argues that, although many instances of repair following forward-oriented initiators do in fact result in altering part of the sentence-so-far, i.e. in a backward-oriented repair, these are usually the result of a 'repair' conversion'.

What starts ... as a repair on the next item due, a search, is converted to a different type of repair, a re-organization or reconstruction of the turn-so-far to achieve a solution. What gets 'changed' is the turn-so-far; what was the trouble was the next element.¹⁹

An example of such a repair conversion is held to be example 9.

9. She hadda wait up the:re fo:r- u-she:'s been there since
eight o'clock this morning ...

Here the stretches on 'there' and 'for' supposedly initiate a repair in the form of a word search. "The object of the search — the next item due

in the turn as built to that point — is some duration.²⁰ Completing the search would presumably result in the production of something like 'a long time' or 'seven hours'. But for some reason the search is not completed (or even begun?). Instead the repair is converted to a backward orientation, in spite of the forward-oriented initiators. This is said to result in the sentence being re-started and the topic of the word search avoided by means of expressing the relevant duration in a different way. It is a simple task to find examples which appear to contradict Schegloff's theory of backward v. forward repair initiators, but, by the notion of repair conversion, Schegloff hopes to avoid the embarrassment that such examples might cause.

Finally, Schegloff argues in this paper that not only are single self-repair efforts 'orderly', but successive attempts to repair the same trouble are also. In successive attempts to repair the same repairable there is, for one, an orientation to progressivity. That is each new repair attempt is said to display that some progress has been made toward accomplishing the repair. This may be because each new attempt adds to prior attempts, or alters an element of prior attempts, or 'back up' less far than prior attempts. Examples 10 and 11 illustrate this phenomenon. Furthermore, Schegloff claims that, as in examples 12 and 13, if a new attempt at repair repeats a previous attempt, then the repeated attempt will be a final, i.e. successful, attempt.

10. We finally got a' hhh a roo:m today in- in the leh-
a lecture hall.

11. Because they hg- because they have- they asked you first.

12. I wonder what sh- how she- what she puts in it.

13. And I grab a pail and I put- hh I see- ah- put all the glass
in the pail.

The most recent work on repair from the ethnomethodological perspective has been undertaken by P. Drew and A.J. Wootton. In an unpublished paper, 'Self-repair and self-correction',²¹ they concentrate on distinguishing error-correction, one possible outcome of repair, from a variety of other interactional tasks that repair may perform. They claim that no particular formal features of repair design distinguish the various types of repair tasks but that, nonetheless, hearers can and do recognize the type of task being performed by examining the various operational components of repair and their relations.

For purposes of illustration the following letters may be taken as representing successive elements of an utterance which is interrupted by a pause and then repaired and understood as ABCDEFG.

ABCDXF...uh...CDEFG

According to Drew and Wootton's terminology, X would here be the repairable, CDEF the repair segment, E the repair, and the filled pause would be the initiator of repair.

They point out that not all instances of repair involve the correction of an already produced element of the turn. A word search, for instance, does not end up by correcting a previous element (unless converted as suggested by Schegloff). Nor, in example 14,

14. I was thinking we:- that be kinda bitchin though
to go out to go out to dinner on a bike

does the repair perform a correction of the repairable: we. We is 'displaced and rendered obsolete in favour of an alternative way of designing the turn',²² but it is not corrected. Drew and Wootton seem here to use the term 'correction' only to refer to repair when (a) the repairable is somehow shown to have been 'faulty', and (b) the repair consists of the replacement of the repairable by another term in the

repair segment (usually this replacement, the 'repair' itself, shows that, and how, the repairable had been faulty by displaying a contrast between replacement and repairable). Otherwise, repair does not involve 'correction', in their sense of the term. In example 14, we is designated as the repairable because the construction of the post-initiation turn eliminates we from the turn (as X is eliminated above in the repair to ABCDEFG). But, because it is not self-evidently 'faulty' in any way (e.g. badly articulated) nor shown to be 'faulty' by contrast with its replacement in the repair segment (it is not, in fact, 'replaced' according to Drew and Wootton because no element fills its particular syntactic role in the repair segment), it is not therefore corrected.

1. He was here lay- uh earlier but he left

In example 1, on the other hand, the repairable lay- is corrected because (a) it is self-evidently faulty since it is cut-off, (b) it is shown to have resulted from a faulty choice of words, this by means of its contrast with the repair earlier. Replacing an error of some sort with the corrected version seems to be the characteristic method of 'correction'.²³ Only in example 1 does such a replacement occur.

The recipient is said to be able to tell what sort of repair operation is being carried out — whether pure correction or something else — by inspecting the relationship between the repairable and the repair. A word may be cut-off, but this does not necessarily mean that it will be corrected or even that it is the repairable (cf. example 3 where r- is cut off but is not repaired).

3. Sure enough ten minutes later the bell r- the doorbell
rang ...

The determination of the repairable is only possible by examining the repair itself. The repair is what exhibits both which item in the turn

is the repairable and what was wrong with the repairable, if anything. Thus, in example 10, although lecture is at first cut-off, the subsequent repair shows that it is not to be taken as the repairable. Rather, by the contrast of the articles a and the, the latter is shown to be the repairable.

10. We finally got a' hhh a roo:m today in- in the leh-
a lecture hall.

In addition the type of error that the was is also displayed: that is, the wrong choice of article. If, however, the repair shows that the cut-off item is the repairable, then it is said to be fairly certain that correction is involved. Finally, even if the repairable is not cut-off it may yet be shown to the recipient that it is being explicitly corrected. This is possible, say Drew and Wootton, if the repair segment repeats a certain amount of the context of the repairable (before and/or after the repairable), cf. example 15.

15. J: Yeh I guess it was the middle u- the end of September
I had it taken.

Repairs embedded in repetition may be organized to highlight that one word is being replaced by another. (...) J starts by reporting that she had something taken in the middle of (what turns out to be a month), and then corrects that to the end of (September). If 'middle' and 'end' had simply been juxtaposed .. (e.g. it was the middle end of September) the sense of the repairable's faultiness would be recognizably diminished.²⁴

Drew and Wotton also argue that even in what appear to be clear cases of self-correction, more than correction may be achieved. For instance, it may be displayed, at the very least, that e.g. accuracy of reference matters to the speaker at the moment. In some conversational contexts 'loose' reference is acceptable, but in others it cannot be tolerated. The correction of a 'loose' but otherwise adequate reference term by replacing it with a more uniquely identifying one can serve as a signal to the recipient that he recognizes the referential constraints of that

particular situation. So, not only does repair involve much more than the correction of an already produced item, but, even when correction is the primary goal, other interactional tasks may also be accomplished by means of the correction. Again, that such a secondary task is being performed depends, Drew and Wootton argue, on the recognition of the relation between repairable and repair, a relation which presumably is context bound. Consequently, they conclude that the continuing relevance of the repairable to the interactional work accomplished by an utterance leads one to see that both (a) the notion of repair as flawed performance and (b) the notion of repair as involving the deletion of the repairable from the utterance 'miss some of the interactional work which may be accomplished through managing talk ... with repair'.²⁵

2. Analysis

The previous pages have shown that the ethnomethodologist have made a variety of claims about discontinuity in conversational speech. For analytical purposes it is perhaps best to distinguish among these four types of claims: distributional; preferential; organizational; and functional.

a. Distributional statements

Many of the substantive statements made in the repair articles amount to claims about the distribution of particular phenomena in speech. An overview of their articles will reveal the ethnomethodologists' habit of qualifying their distributional claims with such adverbs of quantity as 'massively', 'regularly', 'overwhelmingly', 'normally', etc.

- a. Even casual inspection of talk in interaction finds self-correction vastly more common than other-correction.²⁶

- b. Most self-initiated repairs are initiated in the turn which contains the trouble source; and, of those, the vast majority are accomplished successfully within the same turn.(...) Those initiated in transition space and third turn also are overwhelmingly successful within the turn in which they are initiated. Most repairs initiated by any other party in next turn take multiple turns to get accomplished.²⁷
- c. Although trouble-source turns are often interrupted for initiation of repair, such interruptions are overwhelmingly self-interruptions by the speaker of the trouble-source turn for the self-initiation of repair and are rarely interruptions by other-initiation.²⁸
- d. ... other-initiations regularly are withheld a bit PAST the possible completion of trouble-source turn...²⁹
- e. Massively, for those repairables on which repair is initiated, same-turn and transition-space opportunities for self-initiation ARE TAKEN by speakers of the trouble source.³⁰
- f. In the vast majority of cases, however, the trouble-locating is compacted into the repair-candidate itself...³¹
- g. ... there is an overwhelming tendency for the correction to be initiated adjacent to, or very nearly adjacent to, the repairable.³²
- h. In cases where all items prior to the repairable are repeated ... this (is) generally is back as far as an item which could be turn initial...³³
- i. ... self initiated, same turn repair is by far the most common form of repair.³⁴
- j. Generally, but not invariably, the cut-off initiates repair on some already-produced element of the turn.³⁵
- k. The generally pre-positioned repair-initiators appear quite restricted in their distribution:... they occur just before the trouble-source.³⁶
- 1. Regularly a regressive try turns out to be the last on the same repairable.³⁷

The initiation of same turn repair takes one of a limited number of forms.³⁸

At first sight, these statements might appear fairly innocent. It might be assumed that all the person intent on verifying these statements had to do was to examine a large corpus of conversational speech. The sceptic could then determine claims are or are not accurate. However, things are not quite so straightforward. The truth of such claims is dependent upon implicit type-token relations between (a) the type of element (e.g. self-corrections) about which the claim is made and (b) particular instances of that element (e.g. a particular self-correction occurring in the data). Proof that X's 'massively' occur after Q's depends on our incontrovertably being able to identify particular X's and Q's in the data. Either what counts as a particular instance of an X (or a Q) must be self-evident, or some criterion must be provided so that we may decide if any particular piece of data does or does not exemplify an X (or a Q).

In none of the papers here considered do the authors ever provide identifying criteria for a particular type of conversational device about which they make distributional claims. Repair, self-correction, repair-initiators, turn-transition relevance places, etc., are not given any specific definitions, that is, definitions which might allow one to check up on the accuracy of the distributional claims.

This would pose no problem if the identification of these 'devices' was a task any layman could safely be entrusted with. If every instance of such a device were easily identifiable, although the formulation of an identifying criterion remained problematic, it might rightly appear 'niggling' to insist on the vagueness of the categorial type. For instance, even though it has proved quite impossible to formulate adequate criteria for identifying words, if it could be shown that native speakers never have any problem subdividing texts — written and

spoken — into their component words, then it would, at best, be unfair to insist on the impossibility of formulating adequate criteria as proof that words do not exist in the language. At the very least, one may always refer to the ability of native speakers to identify the word as a sufficient criterion with which to define it.

The ethnomethodologists do not make any such argument in defence of their distributional types. This is understandable since it is unlikely that native speakers would have an easy time identifying the repairs, repair-initiators, and turn-transition relevance places in a text, unless, that is, they were provided with a set of adequate identifying criteria.

Instead, a few lines of conversational data, supposedly exemplifying the element or operation under discussion, are usually provided in lieu of the missing criterion. We are not told what characteristics uniquely identify e.g. a repair, but many examples of repair are given. In this case one can only assume that it is being left up to the reader to derive the appropriate criteria for identifying further instances of the relevant device. It is arguable that for some of the devices analysed the reader should not have too much trouble in this respect. He should, for instance, be able to distinguish self-repair from other-repair without any difficulty. But the distinction between what, in one speaker's turn, counts as self-repair and what does not is not so easily established. For example, no mention is made in these articles of the simple repetition of elements. In example 13, the speaker repeats himself: kinda bitchin though to go out to go out to dinner on a bike. Is this to be counted as self-repair or not? On what grounds is this decision to be made? Perhaps one could argue that since there is no obvious 'trouble' — and hence no repairable — then, in one sense at least, there is no repair. But where is the 'trouble' and what is the repairable in a word search (e.g. example 8)?

8. I can have the garment left next door at the uh jewler's shop.

The lack of a vocalized 'trouble' does not seem criterially adequate for distinguishing repair from non-repair. On the other hand, the repetition of to go out, could be taken as a sufficient criterion for classifying the utterance as involving repair. But a word search such as example 8 does not always lead to repetition. Or perhaps it would be held that if the repetition alters part or all of the repeated segment then repair is involved. However, phoneticians have long argued that it is impossible ever to pronounce something twice in exactly the same way. So how much alteration is needed in the repetition for self-repair to have occurred? Or is change in intonation or some paralinguistic effect not to be a criterion for repair? (One can easily imagine examples where the change effected on the repeated segment is great indeed and, more importantly perhaps, potentially significant.) In any case, this troubling question is not even raised in the papers under consideration.^I

Another example of the difficulty created by the lack of identifying criteria for technical notions is provided in the SJS paper. The authors state that other-initiations are regularly withheld 'a bit past the possible completion of the trouble-source turn? However, it is not at all clear how one could judge the evidence for this claim (even if one disregards for the moment the difficulty of identifying possible turn completion points). For any particular piece of data to count as evidence for this claim, one would require a criterion for telling if the data does or does not include an instance of the withholding of other-initiation and that it is not just e.g. a late start of other-initiation due to some other factor. That is, to say that someone is withholding repair-initiation suggests an intention or at least an effort

to withhold. SJS does discuss this question but provides it with an answer that can hardly be regarded as satisfactory. The authors claim that their data includes instances where self-initiation occurs a moment or so past the completion of the trouble-source sentence.

16. A: That store has terra cotta floors ((pause)) not
terra cotta. Terrazzo.

The fact that, in such instances, 'other' (i.e. a/the hearer at the time the trouble was uttered) does not take the opportunity to initiate at the moment speaker completes his sentence is claimed to be evidence that other is withholding initiation. The support that they present for this claim is as follows. (a) Earlier work has convinced them that the turn-taking system obliges conversationalists to minimize the gap between turns. (b) So other should not allow such a gap after completion of the trouble-source sentence. (c) Nonetheless other does allow the gap to occur. (d) So he is to be seen as 'withholding to allow self-initiation'. That is to say, because other may be found in some instances not to act in accord with one purported set of rules (the turn-taking system), this means that he must be doing so in order to conform to a separate, and in this case contradictory, set of rules: those of the repair mechanism. That is, his delay in repair-initiation must be seen as in response to a rule of repair.

To be convinced by such reasoning one must first assume that every action of a conversationalist is in accord with a conversational rule and secondly that therefore all that is needed to prove that a conversationalist is acting in accord with one rule is to show that another one is being contravened by his actions. (The existence of the latter rule can presumably be proved by a similar reference to another rule, and so on ad infinitum). If such an assumption were to be accepted then

conversational rules would not only proliferate infinitely but every new hypothesis regarding the existence of a rule would be unfalsifiable. No exceptions could be found. Not to accept these assumptions would mean the collapse of the argument that the delay of other-initiation is evidence of the withholding of other-initiation. Whether other is withholding his repair-initiation or not remains an unanswerable question. Consequently, the distributional claim that 'other-initiations regularly are withheld a bit past the possible completion of trouble-source turn' is untestable. We can discover if they regularly occur past the possible completing of trouble-source turn (given that that point can be identified) but not that they are being withheld.

It appears that the variety of distributional claims made in the repair articles are not as straightforward as they might first seem. No one will ever have any difficulty supporting claims about the distribution of certain phenomena as long as he is the sole arbiter of what counts as a particular instance of that class of phenomena. And yet, in the papers under discussion, the authors do not provide any criteria for the identification of the classes of elements whose distribution they characterize.

Schegloff's paper presents another difficulty with distributional claims: what to do about exceptions. He claims that ' "uh" or a pause (...) is more likely to initiate repair on a next-due item; that is, it is generally "pre-positioned" ' ⁴⁰. However, he evidently finds that there are many exceptions to this distributional rule; that is, instances where repair is initiated by a filled or silent pause — supposedly indicating a trouble in the as-yet-unsaid remainder of the turn — but where what is corrected is an earlier part of the turn.

Schegloff seems to have two options open to him in this case:

either (1) admit that the distributional evidence does not therefore lead to the conclusion that pauses are forward-oriented repair initiators or (2) present a supplementary argument which 'explains away' the exceptions, i.e. an argument which gives reason why the exceptions should not be seen as such but rather as disguised confirmations of the distributional rule. Whether as an effort to save the symmetry of his model of repair-initiation or merely due to the laxity of his distributional categories Schegloff chooses the latter of these options. He 'explains away' the exceptions to the claimed distribution of forward-oriented pauses by arguing that, when they are followed by a correction of a previous part of the turn, what has in fact occurred is a 'repair-conversion'. The repair is said to be converted from a forward-oriented operation to a backward-oriented one.

One basis for this type of variation lies in the capacity for 'repair conversion'; that is, for a repair initiated for one type of trouble (e.g. a word is 'missing') to be recast and solved by repairing another (e.g. circumlocution to avoid the need for the 'missing' element).⁴¹

Essentially what Schegloff is arguing may be schematized as follows: (i) A's are followed by B's; (ii) X's are followed by Y's; (iii) however, in some cases a B may follow an X, but this is because in this instance the B is a transformed Y. A 'back door' clause such as (iii) may appear to explain away exceptions to distributional rules such as (i) and (ii), but it also fatally weakens the credibility of those rules by revealing how flexible are the criteria for identifying any particular piece of data.^{42, J}

b. Preferences

The ethnomethodologists do not rest on their distributional claims. Their characteristically informal statements of the distribution of

certain conversational operations or elements are taken only as a preliminary step in their analyses. Instead what appears to be the goal of their analyses is an explanation of why the noted distributions occur. This goal has been approached from various directions.

One way that has been adopted for explaining the distribution of certain elements or operations is to represent it as resulting from a preference (or system of preferences) to which conversational interactants are oriented. For instance, in the SJS paper the authors ask 'how is the preponderance (of self-correction over other-correction) produced?'. In answer they claim that the greater frequency of self-correction is due to an independent preference in conversation for self-correction over other-correction. The evidence for this is based on the following observations: (a) self-initiations 'massively' yield self-corrections; (b) other-initiations also overwhelmingly yield self-corrections; (c) 'when other-corrections do occur they are frequently modulated in form'.⁴³

The first two of these claims are essentially distributional. The third is also, but perhaps less obviously so. By 'modulated in form' SJS explains that other-corrections are 'down-graded' by uncertainty markers, as in examples 17 and 18 or performed jokingly as in example 19.

17. Ben: Listen to the pigeons

Bill: Quail, I think

Ben: Oh yeh?

Ben: No that's not quail, that's a pigeon. p..

18. L: But Y'know single beds are awfully thin to sleep on

E: Y'mean narrow?

L: They're awfully narrow yeah

19. Louise: We got a nice large table for her and her
 husband to demonstrate
 (...)

Roger: Not demonstrate, indulge hhheh hh

Thus (c) is also essentially a distributional claim, viz. that other-corrections are generally accompanied by a set of elements which are labelled 'uncertainty markers' or by laughter and other joking markers. In other words, in an effort to explain why a certain distribution occurs, they invoke the notion of preferences and then support the use of that notion by producing further distributional claims related to the original one which is being explained. The question that must be asked is: what role does the notion of preference have in the explanation?

Distributional facts are not conclusive evidence for claims about preferences. Distributional facts show, for instance, that a greater proportion of the U.K. population live in cities than in the country. This does not mean that British people prefer to live in cities. Such a preference might in fact exist in the U.K. But to argue that it does exist requires more than distributional evidence. Geo-political economic, psychological, or social arguments, as well as evidence from personal interviews, would be needed to justify the use of the term 'preference' in this context.

In other words, the objection here raised concerns the justification for linking, in the name of explanation, sets of distributional facts by means of the notion of 'preference'.

SJS appear to be prepared for such an objection.

We use the term 'preference' technically to refer not to motivations of the participants, but to sequence and turn-organizational features of conversation. For example, 'dispreferreds' are structurally delayed in turns and sequences, and are (or may be) preceded by other items; dispreferreds may be formed as preferreds. Cf. Pomerantz 1975.

The term 'preference' in this context does not refer to a speaker's psychological predisposition: instead it describes the systematic features of the design of turns in which certain alternative but non-equivalent actions are taken, as well as aspects of the sequential organization of such actions.⁴⁵

In the final analysis the ethnomethodologists claim not to be using the word 'preference' in the ordinary way, that is, involving reference to motivations and desires. But then, it must be asked, why choose to use that term at all? In the use which they claim the term seems only to refer to particular distributional characteristics: so any neologism could have done the same without the suspicious and misleading inference of a psychological state.

The invocation of preferences does not seem to help explain the occurrence of certain distributions in conversation. In fact, as the ethnomethodologists are quick to point out, the notion of preference amounts to nothing more than the distributional facts it is designed to explain. It adds only the misleading implications of a psychological state.

c. The Repair 'mechanism'

Another typical way of explaining the distribution of repair involves reference to an organizational mechanism. SJS speak of a repair mechanism which 'produces the observed over-all skewed distribution (of self-correction over other-correction)'.⁴⁶ They imply that this mechanism is a system of rules which operates on particular sequential environments.⁴⁷ Jefferson, as we have seen, postulates the existence of an error correction format. Schegloff suggests that same-turn repair may be seen either as a 'super-syntax which operates on whatever syntax ... organizes' or as 'a part of syntax proper, but a syntax reconstructed as a "syntax-for-conversation" '.⁴⁸

However, little if any evidence is given in these papers to justify such claims about an independent organizational mechanism (or syntax, or set of rules) governing repair. It is indeed difficult to imagine what sort of evidence there possibly could be. The 'facts' which we are in possession of — i.e. the corpus of transcribed conversations with its instances of repair — do not give direct evidence of any such mechanism. Furthermore, the type of evidence which is often invoked to justify the postulation of similar organizational mechanisms — e.g. the grammar of English — is not available for repair studies. That is, one does not encounter speakers referring in conversation to the repair mechanism, yet they are often found to refer to (what they take to be) the grammar and the rules of English. And, while one may test a speaker's intuitions regarding the 'output' of certain hypothetical syntactic mechanisms or grammars, an analogous test on the output of a repair mechanism is difficult to imagine.

Although the ethnomethodologists have not provided characterizations of the hypothetical repair mechanism nor given evidence for the existence of such a mechanism, they have nonetheless provided examples of the consequences of postulating such a mechanism. One such consequence is the neglect of the influence of possible 'outside forces' on the distribution of repair. Quote (c)⁴⁹ from SJS makes a distributional claim about self-initiations and other-initiations. The ordering of self-initiation and other-initiation relative to each other is said to be 'organizationally designed, i.e. the product of an organization that relates the positions to each other ...'.⁵⁰ A consequence of seeing the data in these terms is that one is led to ignore the possibility that the paucity of other-initiation of repair is (at least partly) due to the hearer's lack of cause to initiate repair unless he perceives a

'trouble' worth repairing. But there is very often no reason why the hearer should, or even could, perceive any trouble. For instance, if the speaker is not able to think of an appropriate term at a certain point in his turn he may initiate a word-search. The hearer could not possibly initiate such a repair. (Of course having seen that the speaker is having trouble finding the word he wants, the hearer can suggest the term to him. But this, according to SJS, is not initiating repair, but merely offering a candidate repair. The repair is initiated by the speaker's pause which signals to the hearer that he is embarking on a word-search or other forward-oriented repair.) Similarly, in many cases where the speaker has either said a word which was not the one he had intended to say or has changed his mind about a previously delivered word and wants to 'correct' it, the hearer cannot possibly initiate the repair. He cannot have any knowledge that an error has occurred. If he knows of no error or potential repairable, why should he consider initiating repair? These considerations clearly have nothing to do with a presumed organizational mechanism producing the positional ordering of self-initiation and other-initiation of repair, and yet the distributional results to which they contribute are taken as evidence for the existence of such a mechanism.

Indeed, SJS mentions an 'outside force' which might contribute to producing the low frequency of other-correction, also said to be the result of the repair mechanism. If the hearer knows that the speaker meant to say X, instead of what he did say, Y, then there is no need to initiate repair. He might as well continue the sequence: i.e. answer the question he has been asked, reply to the invitation, etc. In spite of the speaker's 'error', the communication has nonetheless been successful. In such a situation only the pedant should feel the need

to initiate a repair since at best he can hope to discover by means of the repair only what he already knows: viz. that the speaker meant to say X not Y.

Similarly, Jefferson assumes that if a speaker says 'thuh uh', then he must have rejected a consonant-initial noun (or adjective) just before producing it. Example 2 is explained in this way.

2. I turned onto /thuh/ uh left lane

Supposedly the speaker was about to say 'thuh right lane' but changed his mind about the adjective at the last moment, that is, after the production of the article. This explanation is adopted, presumably, because it conforms with the error-correction format — word₁ + hesitation marker + word₂ — providing that word₁ is taken to be the unuttered, but supposedly projected, 'right'.

What such an explanation fails to take into account is the possibility that the speaker may very well have produced the filled pause for any number of other reasons. Perhaps something caught his eye at just that moment. Perhaps he paused a moment just to check 'in his mind's eye' if it was indeed the right lane. Perhaps he paused to attract the hearer's eye contact. (Goodwin offers this explanation of filled pauses in mid-sentence position: cf. our discussion in Chapter Five). Other plausible reasons may be imagined.

In other words, to speak of repair as the product of an organizational mechanism is to force oneself to neglect the desires, purposes, and contextual factors which are involved in conversation and in the ordering of it. This neglect results from focussing the analysis exclusively on the formal distributional data and from trying to see that data as the product of one mechanism, one set of rules, or one grammar. A great variety of possible types of influence are easily considered as contributing

to the observable distribution of repair in conversation. Any account which explains that distribution in terms of an organizational mechanism will necessarily neglect such 'outside' influences.

d. Functional/semiological explanations

Drew and Wootton have approached the task of explaining the distributional facts of repair from a different angle. They do not attempt to account for the distribution of certain repair elements in terms of preferences, nor in terms of an organizational mechanism governing the distribution. Instead they ask a functional question: what different interactional tasks are achieved by the different repair formats? That is, given the variety of formal techniques by which one may repair a sentence, what is the difference between what the different techniques achieve?

In this Drew and Wootton appear to differ from their predecessors. In the SJS and Schegloff papers, the function of repair is not considered. Same turn repair, other-repair, 3rd turn repair, etc, were all presented as accomplishing the same task. This task appeared to be primarily formal: the replacement of one word by another, the deletion of a stretch of the pre-initiation turn in favour of a post-initiation segment, and so on. Similarly the 'explanation' given of different repair formats and repair positioning was also framed in formal, rather than functional, terms. For instance, self-repair was related to other-repair in terms of where each could occur relative to the other and not in terms of the differing functions that each could have by means of their positioning. Like the more extreme distributional linguists who aimed to account for the distribution of linguistic features in terms of formal relations, without reference to meaning, SJS and Schegloff attempted to provide an account

of repair without reference to its meaning or function.

Drew and Wootton do not appear to have rejected the findings of SJS and Schegloff but seem to have found that a different type of explanation is needed to account for the distribution of the componential elements of repair. The questions which their paper aims to answer are something like the following: why, when repairing, do speakers sometimes repeat prior elements of the turn and yet sometimes do not; and why do they initiate, locate, and correct in such a variety of ways?

Presumably it would have been possible to construct some sort of answer to these questions in terms of preferences or organizing mechanisms. Instead, Drew and Wootton return to a method of explanation which Jefferson had first adopted in her paper: "Error-correction as an interactional resource". She claims that in example 20 the speaker seeks to show that he was about to say 'thuh cop' but then corrected at the last moment to officer in deference to the speech situation (in court). Not only will this last minute correction supposedly convey the speaker's respect to the judge and jury, it will also serve to portray him as one who does not normally speak in that way (so the fact that he is now speaking in that way shows that he is trying to be respectful).

20. When thuh Ku- officer came up I ...

(He) can acknowledge the courtroom surround without prejudice to (his) identity as a regular guy by recognizing the relevance of the courtroom to his talk no sooner than 'just in time', and can seek to escape the consequences of the word to mark that identity by recognizing the relevance of the situation no later than just in time, by utilizing a conventional available device; i.e. the format for errors cancelled just prior to delivery.⁵¹

In a similar fashion Drew and Wootton offer a semiological (or functional) explanation of repair distribution. The different types of repair format function much like signs. Each indicates a different

type of interactional 'work' to be accomplished by the repair. They have different 'meanings' (in an extended sense of that term). The 'meaning' of a repair format is the type of syntactic, semantic, and/or interactional operation that it performs on the utterance or on certain parts of the utterance.

Through repairing their talk, speakers can display that they are engaging in a variety of activities.⁵²

A particular repair format may communicate to the hearer that the repairable is to be deleted from the turn and replaced by the 'repair'. Or it may signal that the repair is to be thought of as an alternative to the repairable but that this relation does not involve the deletion of either of them. A repair format could also serve to indicate, by the nature of the contrast between repairable and repair, the speaker's conscious adjustment to his surroundings and/or his interlocutors.

The speaker may instruct us, through the extent of repetition in the repair segment, as to the implications of the repair for the general character of the turn.⁵³

This is not to imply that every type of format has a fixed 'meaning' which it communicates regardless of context. Drew and Wootton specifically reject this position as regards self-correction.

There is nothing in the (repair) design to distinguish 'correction' from any other of the activities which may be accomplished through repair.⁵⁴

Instead the hearer is supposed to be able to determine the type of operation which the repair is performing on the utterance only by inspecting the contextualized relation between repair and repairable as well as the way in which this relation is presented by the repair components. So repair formats indicate the type of operation that is being performed on the utterance, but they do not do this independently of context. Repair is thus a contextually determined syntactic and semantic modification of

both the structure and the interactional significance of an utterance.

There is however an obstacle to obtaining any concrete analytical results on the basis of such an 'explanation' of repair. If one were to hold that each type of repair format uniquely indicated one type of operation to be performed on the utterance, then the same type of problem would arise as that which troubles any bi-planar analysis of communication.⁵⁵ In order to distinguish between different types of repair format, it would be necessary to be able to tell if a particular difference in format did or did not signal a parallel difference in repair operation. This would require the analyst to be able to identify what sort of operation is being performed on the utterance: i.e. whether the repair is to be seen as deleting and replacing the repairable, whether it is merely to be considered as an equivalent alternative, or whether it is to be seen as accomplishing some other interactional task. If this were possible, then he could identify the different types of repair format by means of a contrastive analysis carried out from the perspective of the different types of operations performed (much as in at least one version of orthodox phonology a contrastive analysis of sound features is carried out from the perspective of meaning differentiation).

The problem with such an analysis would lie in the inscrutability of the interactional operations performed by repair. Drew and Wootton mention a wide variety of such operations but never explain how one is to tell whether one of them is or is not being performed by any particular utterance. What is required for such an analysis to proceed is a criterion with which to identify types of repair operation. This is not provided; nor is the construction of one easily imagined. Drew and Wootton rely in their analysis on an intuitive grasp of such operations. They distinguish between examples which are corrections and those that

are some other form of repair without letting the reader in on the secret to making such a distinction. In this case, much hinges on the way they use the term 'correction', but this use is never fully explained.

On the other hand, if one were to adopt a more strictly contextualist view of repair operations — a view which Drew and Wootton adopt in principle but not always in practice — it is difficult to see how any functional analysis of repair formats could proceed. If there are no hard-and-fast relations between form and function — that is, if how a particular repair format functions in a particular situation may only be determined for just that particular situation — then a discussion of the functional role of particular elements of the repair format would seem to have no basis. For instance, if a format involving repetition of elements both before and after the repairable (as in hijxlm...ijklm) functioned in one way in this context, differently in another, etc., then it would not be possible to state the functional role of the 'types' pre- and post-repetition. Furthermore if this problem is combined with the difficulty, suggested in the last paragraph, of identifying repair function, then the functional or semiological explanation of the distributional facts of repair would appear doomed.

3. Conclusion

Four types of claims have been examined: distributional, preferential, organizational, and functional/semiological. The distributional claims were found unstable due to the lack of criteria with which to identify the elements whose distribution is characterized. It was argued that

the claim about the influence of preferences amounted to an amalgamation of distributional claims, with the unjustified — but supposedly unwanted — implication of psychological states thrown in. In as much as the explanation in terms of preferences reduces to a restatement of the distributional claims, then that explanation is open to the same objections about criteria of identification as are the original distributional claims. The postulation of an organizational mechanism or system of rules governing repair was found not only to be unjustified — in that the only evidence for it lay in the questionable claims about distribution — but was also found to lead to undesirable consequences: the neglect of common sensical, non-organizational accounts of the data. Finally, the semiological 'explanation' of the distribution of repair, viz. in terms of the work achieved by the different repair formats, was shown to be based on questionable assumptions regarding the types of 'work' accomplished by repairs. Without a criterion with which to identify the purportedly different interactional tasks, such an account is left only with the distribution of different formats as evidence for its explanation; and yet the characterization of that distribution remains problematic.

Thus it would appear that what is wrong with the ethnomethodological theories of repair is not simply to be put down to the types of explanation which they propose for the data. It is also due to their descriptive characterization of the data. For it is on the basis of this preliminary characterization that the ethnomethodologists build their distributional claims; and it is on top of these claims that they construct their 'explanations' and theories. Although we are provided with statements about the distributional frequency of self-repairs, self-initiations, trouble-locators, other-corrections, ... and so on, we are never told e.g. what a trouble is, or what does and what does not count as a self-repair.

This criticism is not merely a complaint to the effect that no precise definitions are ever given. What is more crucial is that it is unclear how one could have an organizational mechanism or a system of preferences if the elements they are supposed to operate on are not specifiable. In what sense does the category 'repair' (or any other of the named categories) exist at all? Consequently, although the nature of repair has been explained by a variety of methods — in terms of preferences, systems, and functions — no clear picture has emerged.

While the notion of repair as a 'self-righting mechanism' is concerned with the speaker's activity in response to the occurrence of discontinuities in speech, a similar notion is required to refer to the hearer's response to discontinuity. Some researchers have argued that, when he hears an utterance containing a discontinuity, the hearer unconsciously edits the utterance to an acceptable form (i.e. without discontinuities). The notion of editing, and of the hearer's response to discontinuity, is the subject of the next chapter.

NOTES

Chapter Four

1. Garfinkel and Sacks, "On formal structures of practical actions" in J. McKinney and E. Tiryakian (eds.) Theoretical Sociology, 1970.
2. Atkinson and Drew, Order in Court, 1979, p.37.
3. Sacks, Schegloff and Jefferson, "A simplest systematics for the organization of turn taking in conversation", in Schenkein, J. (ed.) Studies in the Organization of Conversational Interaction, 1978, pp.9-10.
4. Sacks, Schegloff and Jefferson, "A simplest systematics for the organization of turn taking in conversation", in Schenkein, J. (ed.) Studies in the Organization of Conversational Interaction, 1978.
5. cf. Jefferson, "Side sequences", in D. Sudnow (ed.) Studies in Social Interaction, 1972; "A case of precision timing in ordinary conversation", Semiotica, 9,1, 1973; and "Error correction as an interactional resource", Language in Society, III,2, 1974.
6. Jefferson, "Error correction as an interactional resource", Language in Society, III,2, 1974.
7. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.381.
8. The references to the examples used in this chapter are as follows:
 1. Jefferson, "Error correction as an interactional resource", Language in Society, III,2, 1974, p.185.
 2. Jefferson, "Error correction as an interactional resource", Language in Society, III,2, 1974, p.184.
 3. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.363.
 4. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.364.
 5. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.372.
 6. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.366.
 7. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July, 1979, p.9.

8. Jefferson, "Error correction as an interactional resource", Language in Society, III,2, 1974, p.183.
 9. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.273.
 10. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.278.
 11. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.278.
 12. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.279.
 13. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Gibon (ed.) Discourse and Syntax, 1979, p.279.
 14. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July, 1979, p.9.
 15. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July, 1979, p.10.
 16. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.376.
 17. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.378.
 18. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.378.
 19. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.378.
 20. Jefferson, "Error correction as an interactional resource", Language and Speech, 17, 1974, p.193.
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9. For an explanation of the ethnomethodologist's transcription system see the preface to Schenkein, Studies in the Organization of Conversational Interaction, 1978.
 10. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.362.
 11. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.369.
 12. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.369.

13. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.373.
14. cf. Chapter One, Section 4.
15. e.g. Example 4 might be re-written as:
 Ken: Is Al here today?
 Dan: Yeah
 Roger: He is? No, he was!
 In this example, 'other' (i.e. Roger) initiates, locates and corrects the repairable is.
16. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.266.
17. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.268.
18. cf. for example, Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, fn.1.
19. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, pp.273-4.
20. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.273.
21. This widely circulated paper was originally delivered before the SSRC conference on 'Practical Reasoning and Discourse Processes', at St.Hugh's College, Oxford, on 7 July 1979.
22. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.9.
23. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.14.
24. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.14.
25. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.21.
26. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.362.
27. Schegloff, Jefferson and Sacks, "The preference-for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.369.

28. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.373.
29. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.374.
30. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.376.
31. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.376.
32. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.1.
33. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.15.
34. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.268.
35. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.273.
36. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.275.
37. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.275.
38. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.272.
39. cf. similar criticism of ethnomethodological analyses of conversation in Coulthard and Brazil, Exchange Structure, 1979, section 1.1.
40. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.273.
41. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.273.
42. A similar form of argument is used by Jefferson in her analysis of 'thee + uh' versus 'thuh + uh'.
43. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.378.

44. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language 53(2), 1977, p.362. Pomerantz, Second Assessments: A study of some features of agreements/disagreements. Unpublished Ph.D. dissertation School of Social Science, Univ. of California, Irvine, 1975, which is the central text for the ethnomethodological theory of conversational preferences.
45. Atkinson and Drew, Order in Court, 1979, p.59.
46. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.362.
47. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.362, fn.5.
48. Schegloff, "The relevance of repair to syntax-for-conversation", in T. Givon (ed.) Discourse and Syntax, 1979, p.262.
49. See quotes listed at the beginning of section 2a of this chapter.
50. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977, p.373.
51. Jefferson, "Error correction as an interactional resource", Language in Society, III,2, 1974, p.194.
52. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.14.
53. Drew and Wootton, "Self-repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.12.
54. Drew and Wootton, "Self- repair and self-correction", unpublished paper delivered before SSRC Conference on "Practical Reasoning and Discourse Processes", Oxford, July 1979, p.16.
55. cf. for instance, a discussion of the problems arising for bi-planar stylistic analysis in Taylor, Linguistic Theory and Structural Stylistics, 1981, Chapter 6.

CHAPTER FIVE

Utterances, Sentences, and Editing

There are many accounts, some verified, others not, of the verbal exploits of the former Warden of New College, Dr. William A. Spooner. One such account concerns a sermon delivered by Spooner before the congregation at the University church of St. Mary. Supposedly, Spooner spoke at length on the combination of Christian piety and intellectual acumen present in the writings of Aristotle. The congregation listened in silence. At the end of the sermon, Spooner began to descend the stairs from the pulpit but then stopped halfway down. He turned and climbed up the stairs. When he had reached the top, he once again addressed the congregation with words to this effect:

"In the sermon which I have just delivered,
where I said Aristotle, I meant Augustine."

Anyone who has carried out a grammatical analysis of a lengthy corpus of transcribed conversational speech will know that before such an analysis may be done, the data must be 'idealized'. The transcribed speech must be modified so that the utterances take the appearance of sentences such as those typically encountered in written texts.

For instance, should the analyst come upon a typical stretch of speech like the following, he would not begin the analysis before making certain alterations to the text.

- (1) It's just a subject which is - . sort of of . basically well-known to all mathematicians and is a tool which . one uses whenever required and is - at the [ha:] . at the is the logical foundation of quite a lot of . quite a lot of things (LUND, S.1.6, 849-855)₁

To idealize this transcribed speech, the analyst will delete those parts of the transcription which keep the utterance from looking like a standard sentence. For instance the passage and is at the [ha:] at the is the logical foundation would never appear in a written text. So, the analyst would probably idealize this passage to the form "and is the logical foundation".

It is important to see why the analyst would decide to leave out part of the phenomena which he is analysing. A contributory reason is that the unidealized passage cited above is simply not grammatical according to conventional standards. The sequence and is at the [ha:] at the is the logical foundation could not form part of what any grammarian would call a standard grammatical sentence of English. This is true regardless of the theoretical 'school' to which the grammarian belongs. Indeed, one part of the sequence, [ha:], could not be taken as a token of any word-form of contemporary English. Thus, to leave such a sequence unchanged in an utterance would render that utterance grammatically un-analysable. There is no need to go into the details of the various methods of grammatical analysis in order to defend this claim. Since

the utterance (a) contains a form which does not approximate to any English word-form, and (b) incorporates sequences which could not form a part of any English sentence, it is clear that any grammatical analysis which takes the sentence and the word as basic elements would not find the above sequence acceptable. One needs only to point to the odd combinations such as article plus verb (the is) and article plus non-word (the [ha:]) to confirm such a conclusion. Grammatical analyses are not designed to parse speech sequences which are not sentences of English.

In his introduction to linguistics, Charles Hockett argues for editing as a pre-requisite to grammatical analysis.² He produces a transcribed sample of real conversational speech. This sample, similar to the passage from the Lund corpus given above, includes a fair amount of both filled and silent pauses, repeats, false starts and other features characterized by Hockett's expression "hemming and hawing". Following this sample, he offers an edited version of the same transcription. The edited version, from which the features of 'hemming and hawing' are deleted, is said to convey "much the same meaning" as the unedited version. Indeed, Hockett argues, "this edited version is implicit within the original". It is such edited versions which Hockett claims to be the phenomena deserving of the linguist's attentions.

Recent research suggests that much can be learned about a person through a close examination of his unedited speech. The particular ways in which he hems and haws, varies the register of his voice, changes his tone quality, and so on, are revealing both of his basic personality and of his momentary emotional orientation. But since (if our assumption is correct) phenomena of these sorts are not manifestations of the speaker's linguistic habits, it is proper to ignore them in the study of language, basing that study exclusively on edited speech.³

However, it should not be concluded that the grammarian idealizes his data for the sole purpose of rendering ungrammatical utterances into grammatical sentences. He also must idealize sequences which, if taken

without alteration, could pass as grammatical strings. Furthermore, he will not necessarily render all ungrammatical sequences into grammatical form.

To take the latter point first, it may be seen that ungrammatical utterances such as (2) would not merit idealization in the same way as (1)

(2) There~~was~~ three girls on the front

(invented)

Most analysts will leave ungrammatical utterances like (2) intact in their analyses and will simply register the fact that the utterance is ungrammatical. Few grammarians would feel justified to correct data such as (2) to a more acceptable form. This would be deemed a falsification of the data, a claim not applicable to the 'idealization' under discussion. So it is not merely the fact that (1) is ungrammatical that leads the analyst to idealize it.

If one considers examples (3) and (4), it appears that ungrammaticality is not a necessary criterion for the grammarian to feel that idealization is required.

(3) I don't think. I mean he's very organized
(S.1.8, 914)⁴

(4) I'd paint for I'd paint forever if I had the time
(S.1.8, 853)

In all probability, examples (3) and (4) would be idealized to (3a) and (4a) by the grammarian.

(3a) I mean he's very organized.

(4a) I'd paint forever if I had the time.

However, as they stand, (3) and (4) are not ungrammatical. If (3) was not altered, it could be analysed as the two sentences (3b) and (3c).

(3b) I don't think.

(3c) I mean he's very organized.

Similarly, the utterance transcribed as (4) could be analysed as the two grammatical sentences (4b) and (4c).

(4b) I'd paint four.

(4c) I'd paint forever if I had the time.

Thus, it appears that there are criteria other than ungrammaticality which may lead the analyst to alter a transcribed utterance into an idealized sentence-form amenable to grammatical analysis. Although the interest of such a study is apparent, we will not pause here to investigate the criteria used by the analyst in his activity of idealization. For our present purposes, we need only conclude informally that grammarians will idealize transcribed utterances into sentence form in order that their analysandum (a) is a grammatical sentence of English (unless the utterance is actually held to be ungrammatical), and (b) conforms to what the grammarian feels the speaker 'meant to say'. Although this formulation will not be considered further, the remainder of this chapter should shed some light on its principles.^K

* * *

. . . utterances never consist of sentences, but of one or more segments of speech (or written text) which can be put into correspondence with the sentence generated by the grammar. J. Lyons (1969) 5

* * *

1. The Target Sentence Hypothesis

In his article "The Production of Speech" John Laver argues that discontinuities such as those in example (1) are not noticed by the hearer in perceiving speech. Much like the analyst who deletes parts of a transcription in order to idealize it and thus make it amenable to grammatical analysis, the hearer is thought by Laver to 'edit' what he hears.

The conscious perception of speech in some sense regularizes and idealizes the actual data of speech.⁶

Bernd Voss adopts this argument in his paper on hesitation phenomena and perceptual errors. He suggests that the hearer's "idealization process is similar to the one operative in reducing speech to writing."

He continues,

The hypothesis used to explain this observation is well known. If we assume that the perception of speech is not primarily a process of identifying the segments of the speech chain but rather one of matching the listener's (re)construction with the incoming acoustic information (Fry, 1970), there is no need for the listener to reconstruct the speaker's performance difficulties (such as hesitation or slips) for his perception process.⁷

In other words, the listener is assumed to have, 'in store', the units of his language and the possible sentential sequences of such units. So, when he hears an utterance, he will attempt to take it as an instance of one of the stored sentences. Furthermore, during his decoding of an incoming utterance, he is thought to form hypotheses as to what sentence the utterance is a token of. If he can then take the remainder of the utterance as conforming to the predicted sentence, he will do so.

In this case, 'performance difficulties' of the type - the logical foundation of quite a lot of . quite a lot of things - would be unconsciously edited by the hearer in order that he may take the whole utterance (1) as an instance of the grammatical sentence (5).

- (5) It's just a subject which is basically well-known to all mathematicians and is a tool which one uses whenever required and is the logical foundation of quite a lot of things.

That is, the hearer is thought to edit incoming utterances in much the same way as the grammarian who edits speech in reducing it to analysable, written form. By doing so, both hearer and analyst are able to assign a structural description to the sentence assumed to underlie the utterance.

If we adopt this assumption (i.e. what will here be called the 'target' sentence hypothesis': that, like grammatical analysts, hearers have to edit incoming speech in order to analyse it in terms of its sentential grammatical structure) then it would appear that knowledge of how to edit utterances into sentences is part of a competent speaker/hearer's linguistic knowledge. (It is best, for the moment, to leave aside whether this knowledge is part of his linguistic competence or part of his knowledge of performance rules.) That is, most hearers and transcriptionists can be assumed to know that and is - at the [ha:] . at the is the logical foundation of is not to be edited to e.g. "and at the logical foundation of ..." or "and is at the logical foundation of", etc. If we assume that hearers implicitly edit utterances into sentence form then we must also assume that, if all hearers of the same utterance edit it to the same sentence form, there are shared rules or procedures which guide their editing. Consequently, it becomes of interest what these rules or procedures are.

In this context, the research on the perceptual errors made by non-native speakers of English takes on a relative importance. In the aforementioned article "Hesitation phenomenon and perceptual errors", Voss claims that his experiments show that non-native speakers/hearers of English have difficulties with the task of editing performance utterances into 'target' competence sentences. Sometimes they do not realize that editing should be done; sometimes they edit improperly; and sometimes they edit where no editing is required. Referring to the task of 'reconstructing' incoming utterances into appropriate sentence form, Voss claims

This, however is a task which although usually no problem for the native speaker is typically more difficult for the non-native speaker. If the perception of speech is determined by the three variables of content information, linguistic

information and acoustic information (...), then the non-native speaker because of his imperfect command of the language (i.e. deficient generative system = linguistic information) is less likely to make accurate linguistic predictions in his reconstruction attempts. He will therefore have to depend more heavily on the acoustic information. This, however, is problematic in that some hesitation phenomena such as filled pause or repetition are acoustically identical with, or at least similar to, unstressed forms or parts of words. The non-native speaker will find it difficult to know in each case whether a given stretch of acoustic information is part of the speaker's performance that can be disregarded, or whether, if the reconstruction is to be correct, it needs to be accounted for.⁸

In an experiment performed in 1975, Voss asked 22 German students to transcribe a passage of spontaneous English speech. All the students were training to be teachers of English and had undergone 5 to 7 years of instruction in English. The experiment was based on the aforementioned assumption that speech perception consists of a matching between a predicted competence sentence and the incoming acoustic information. The students were asked to transcribe the passage "as accurately as possible - in the sense of providing an orthographic rendering".⁹ It was assumed that the errors in the transcript - identified by a comparison of the students' transcriptions with a transcription made and checked by a native English speaker - should reveal the general causes of perceptual difficulties for the non-native speakers. In addition,

No instructions were given how to handle hesitations. The assumption was that they would be idealized out. This turned out on the whole to be true ...¹⁰

Voss found that 32.6% of the errors made in the non-native transcriptions were "likely to have originated in a misinterpretation of hesitation features".¹¹ He groups the hesitation-linked errors into two types. The first type consists of cases where the subject has taken a repeat or a filled pause to be an instance of a word or

a part of a word. For instance, the utterance and you you can - ring off a a great string was interpreted by one subject "and you can ring offer a great string". What happened here appears to be that the hearer took the utterance of off followed by the repeated indefinite article a a as an instance not of a repeat but of a non-hesitation-interrupted sequence consisting of the two words "offer" and "a". That is, the first a was taken as a second syllable attached to off to make the word "offer". Similarly, a great string of erm of activities was mistaken by one subject for "a great string of an activities". Here a filled pause is taken for the article "an". This is not surprising since some filled pauses and indefinite articles sound identical.^L

The second type of error attributable to unfamiliarity with hesitation phenomena in English consists of those errors when the subjects mistook a word or part of a word for an instance of a repeat or a filled pause. Two cases are cited. In the first, the subject took the utterance one of the problems -erm- of people who work ... for "one of the problems people who work ...". Here, the preposition of appears to have been mistaken for a filled pause and so was idealized-out of the subject's transcript. In the second instance, the utterance ... such as - boat building was transcribed as "such as building". Voss suggests that the subject here took building to be a correction of boat since both begin with the same consonant. Thus the subject is supposed to have interpreted the utterance in the same way that one might (correctly) interpret the utterance I work at the top of a large boat building on the 37th floor as "I work at the top of a large building on the 37th floor".

In his own terms, what Voss' data seems to indicate is that the non-native hearers often did not know the signals given by the English

speakers regarding when a section of the speaker's utterance needed to be edited or how it needed to be edited in order that it might match an idealized competence sentence. Sometimes they edited where no editing was required, and sometimes they did not recognize cases where editing was called for.^M And yet, native speakers do not appear to have these difficulties.

Consequently, if we accept Voss' reasoning it seems possible to conclude that there are procedures which guide the hearer in establishing which competence sentence (or 'target sentence') a discontinuous utterance is to be matched with. The non-natives in Voss' test appeared to lack the linguistic knowledge required for the operation of these procedures. Furthermore, because lack of this knowledge seems to have kept the non-native hearers from performing as competently as a native hearer of English, this knowledge can, in some sense at least, be rightfully termed part of the 'knowledge of English'. An investigation of a significant part of this knowledge will serve as the topic for this chapter.

It should be remembered that the notion of such editing procedures, as well as of the signs and rules which may be assumed to guide them, presupposes the relevance of 'target sentences' conceived as the competence sentences which hearers supposedly derive from utterances in their interpretations of speech performance. From another perspective, these target sentences may be thought of as the abstract sentences underlying the speaker's performance. They represent the sentence forms which the speaker is thought to intend to convey and which, if he is to decode the speaker's verbal output correctly, the hearer must (at least implicitly) recognize at some stage in his decoding. If it is not assumed that hearers, like grammarians, must idealize their incoming data in this way, then there is no need of the notions of

editing procedures and target sentences. In this case there would be required an alternative account of how hearers can interpret such apparently confused sequences, such as example (1) from the Lund Corpus.

* * *

Speakers co-operate with listeners by presenting their message in a form made to ease the listener's task of decoding. Ideally the act of speaking serves the communication of meaning. Goldman-Eisler (1969)¹²

For heuristic purposes, we may distinguish two phases in editing. The hearer must recognize when editing is required, and he must then edit appropriately. Consequently, we may imagine that the speaker somehow signals in his performance when and how the hearer should edit the speech produced. Our concern will be primarily with the rules governing how the hearer should edit, since their formulation raises some perplexing problems for a sentence-based notion of syntax. If an alternative conception of syntax replaces that based on the competence sentence, the question of how a speaker signals when editing is required should disappear. Nevertheless, we will have frequent recourse throughout our discussion of editing rules to related questions of how the need for such rules, at any moment in performance, may be signalled.

* * *

2. Patterns of Ligation

The assumption that there are rules governing editing may be seen to be supported by Sieb Nooteboom's evidence on the distributional features of certain aspects of discontinuity. In a report¹³ originally given to the 12th International Congress of Linguists (1977), he recounts what he found in an examination of Meringer's 1908 corpus of speech

errors (in German) and the self-corrections it contains.* For example, he found that although speakers in the Meringer corpus are sometimes seen to discontinue their production in the middle of the word in which the to-be-corrected error occurs (and this they only did about 20% of the time) not once did they discontinue production in the middle of a word subsequent to the one to be corrected. So although speakers would sometimes produce something like A, the majority produced B or C (mainly B) and never D.

A - And so we had to w- run home as fast as possible.

B - And so we had to walk - run home as fast as possible.

C - And so we had to walk home as-run home as fast as possible.

D - And so we had to walk home as fa- run home as fast as possible.

Nooteboom takes the results as evidence for a pressure on the speaker to respect 'linguistic orthodoxy', that is, to complete whatever programming unit is being executed when the error is detected. Thus if the error in producing walk above is detected while the speaker is producing the word fast, the speaker is compelled to finish the production of fast before discontinuing and beginning the correction of walk by run. This pressure, however, is seen to be less effective if the word in production is the error-word itself.^N

* The fact that Meringer's corpus is in German might be thought to weaken the relevance of Nooteboom's paper to a study of editing in English. However, the conclusions drawn from Nooteboom's work will be minimal. The intention is only to show that the speaker's activity in discontinuing and repairing his utterance may be thought of as an aid to the hearer's editing task. This is the implication of the suggestion put forward by Goldman-Eisler and quoted above. Nooteboom's conclusions show that the target sentence hypothesis is commonly applied not only to the activity of the hearer but also to that of the speaker. The fact that these conclusions are based upon examination of a German corpus is therefore of little importance.

Nooteboom's findings may be taken as evidence of the speaker's role in helping the hearer edit. (For purposes of clarity, we will refer to the activity performed by the hearer in deriving a target sentence from a discontinuous utterance as 'editing'. This is to be distinguished from the elements in the speaker's production which signal how the editing is to be done. The latter we will term 'ligation'. We have avoided here the ethnomethodologist's term 'repair' since it connotes a certain theoretical perspective which is not that of Nooteboom. The notions are, however, similar. In example B above, the speaker 'ligates' the part of the utterance after the discontinuity, i.e. the 'post-discontinuity', to the 'pre-discontinuity' by means of correcting walk by run. The hearer 'edits' B to "And so we had to run home as fast as possible". Nooteboom's study, it will be noted, concerns how speakers ligate.) We may imagine the following ligation rule as capturing the significant aspects of this part of Nooteboom's findings.

Ligation Rule 1: if the speaker detects an error in a word already produced and if that error is deemed to require correction, the speaker should discontinue production at the next word boundary.

The phrasing of ligation rule 1 is intended to leave unclear the status of the position of an error detected in a word during its production. Thus the possibility of interrupted error-words, is allowed.

In turn such a role may be considered to help the hearer in his task of editing. If a word is discontinued in mid-production, the hearer may assume that it is the word to be corrected by what follows the discontinuity. If there is no cut-off word when a discontinuity in the utterance appears, the hearer must then turn to other cues to determine what the post-discontinuity sequence will correct, if anything. The question of what these other cues might be will be considered later in this chapter.

Nooteboom also looks at where the speaker begins his correction after having discontinued. He found that, for the correction of phonological errors (i.e. mispronunciations of the type we had to ralk all the way home), the speaker nearly always (approximately 90%) re-started after the discontinuity with the error word itself. Thus A was highly favoured over B.

A - So we had to ralk all the - walk all the way home.

B - So we had to ralk all the - had to walk all the way home.

This was not found to conform with lexical errors, in which case speakers were much more prone to go further 'back' into the pre-error utterance to begin their correction. Still, 58% of lexical corrections began with the error word itself.

58%: C - So we had to walk all the - run all the way home.

42%: D - So we had to walk all the - to run all the way home. ^o

Nooteboom argues

To explain this difference (between phonological and lexical error corrections) we have to assume that the mental strategy taking care of the detection and correction of overt speech errors differentiates between lexical and phonological errors. If the error is classed as phonological, there is no reason to go further back in the utterance, as the linguistic domain of phonological form is the word. If, however, the error is lexical, this might have induced changes in other parts of the phrase ... (...). Apparently the speaker often prefers not to take chances, and would rather repeat the whole relevant phrase than to perform an extra check on the syntactic and semantic appropriateness of the phrase after insertion of the correct lexical item. In this way, he betrays that, for example, combinations of articles and/or prepositions and nouns and combinations of adverbs and adjectives or verbs, are operationally functioning as the domains of lexical insertion in the detection and correction of speech errors.¹⁴

It is interesting that, whether correcting a phonological or a lexical error, Nooteboom found that speakers will almost always begin

the correction at the beginning of a word. Instances like the following in which the speaker begins a correction in the middle of the error-word (but note, at a morphemic boundary) were found to be extremely rare (3% of cases).

Example: Das Wirtheis - haus heisst. ^P

It is possible to interpret the rarity of such cases as resulting, again, from the speaker's aim, in ligating, to aid the hearer's editing task. That is, speakers do not begin a post-discontinuity sequence in the middle of a lexical item so as to avoid possible ambiguities that might arise. For instance, in the following example, there are three different ways that the post-discontinuity could be taken to correct the pre-discontinuity. The invented utterance is (6), and (6 a-c) are possible target sentences.

(6) That's the first port which I have longed ...st which
I really wanted.

(6a) That's the first which I really wanted.

(6b) That's the first post which I really wanted.

(6c) That's the first port which I have lost which I really
wanted.

By beginning the post-discontinuity with the sound /st/, the speaker of example (6) would leave the hearer unclear whether /st/ was to be considered the repeated ending of first, the corrected ending of port, or the corrected ending of longed. However, if the speaker had begun the discontinuity by uttering the corrected (or repeated) word in full (i.e. 'first', 'post', or 'lost') this ambiguity would not have arisen. The avoidance of such ambiguity may therefore account for the extreme rarity of post-discontinuities beginning in word-medial position.

In his conclusion Nooteboom reports on a further finding. Whenever some words prior to the corrected word were repeated, these words were always 'syntactically coherent' with the error-word. That is, A was the regular occurrence, not B.

A - And so we had to walk - had to run all the way home.

B - And so we had to walk - and so run all the way home.

When an error is discovered while checking the phonological orthodoxy of word forms, the command to stop speaking will be followed by a command to go back to the word that went wrong, to correct its phonological form, and to start speaking again with that word. When a lexical error is detected while checking the syntactic and semantic appropriateness of the phrase, the command to stop will be followed either by a command to delete the wrongly selected word, to fill in the correct one and to restart the original program with the new word, or, in nearly half the cases, to delete the phrase the wrongly selected word belonged to make a new phrase, and start speaking this phrase.¹⁵

Thus, we may envisage the following possible ligation rule as capturing the two choices Nooteboom deems the speaker to have when correcting a lexical error. Here, A_1 and A_2 are meant to stand for two elements of the same phrase: say, the article and noun of a noun phrase. The same applies to other letters and their sub-scripts. B_x is the word corrected by B_y .

Ligation rule 2: $A_1 A_2 B_1 B_2 B_x C_1 \dots \begin{cases} B_y C_1 C_2 \\ B_1 B_2 B_y C_1 C_2 \end{cases}$

As Nooteboom suggests, the speaker may decide which of these two options he chooses on the basis of his aim to facilitate the hearer's editing. Note that, under either option, the hearer will edit to the same target sentence, viz. $A_1 A_2 B_1 B_2 B_y C_1 C_2$. Thus, the choice for the speaker does not concern which target sentence he wants the hearer to derive from his utterance. It should also be remarked that the following

theoretically possible combinations are not produced by speakers.

(i) * $A_1 A_2 B_1 B_2 B_y C_1 C_2 \dots A_2 B_y C_2$

(ii) * $A_1 A_2 B_1 B_2 B_x C_1 C_2 \dots B_y C_2$

That is, as Nooteboom claims, if there is repetition of certain of the elements which were produced before or after the error-word in the prediscontinuity sequence, then elements repeated must be continuous with the error-word. So, in (i), A_2 may not be repeated unless, to establish continuity with the error-word, B_1 and B_2 are also repeated to give: $\dots A_2 B_1 B_2 B_y \dots$. Similarly, C_2 may not be repeated unless C_1 is also repeated so that the sequence is continuous from the corrected error-word. It is possible to interpret these restrictions as conforming to a general principle underlying ligation that discontinuity should be kept to a minimum in order that the hearer may apply his editing rules with the minimum of uncertainty. To understand this restriction on the speaker's ligation process better, we will therefore need to turn to an examination of the hearer's editing process.

* * *

3. Editing rules; principles and problems

What form could editing rules have? (The following exposition is partly derived from the hypotheses William Labov presented in a paper read at SUNY, Binghamton, New York in 1975, entitled "On the Grammaticality of Every-day Speech").

We need first of all to account for breaks in the continuity of speech where the speaker resumes after the discontinuity (post-discontinuity) without any correction or deletion of the 'pre-discontinuity'.

It might be thought that the provision of a rule for such cases is unnecessary. Nothing appears to require editing as the speaker has merely paused mid-utterance and then gone on, as in the following two examples:

(7) I will have to visit . the major resources - in the United States . and [ə] several in Europe (S.2.1, 52).

(8) They've got priority on vice-chancellors and [ə] . English schoolteachers (S.2.1, 72).

However, it must be remembered that a fully explicit grammar would generate not only sequences of words but also their phonetic representation including intonational pattern. Since rhythmic and intonational patterns can sometimes be used for syntactic (and semantic) purposes, it is reasonable to assume that 'target sentences' would have fixed phonetic patterns specified for them. Should an utterance not match one of these patterns but show certain differences in rhythmic structure, those differences would have to be edited out in order for the hearer to derive the intended 'target sentence'. For instance, it has often been pointed out that the pause can have syntactic consequences.

(9) I gave John . our local policeman a cup of tea.
(invented)

In (9) the micro-pause can have the function of signalling that the phrase our local policeman is in apposition to John that is, John and our local policeman will be taken to have the same references. However, the pause might alternatively be taken as signalling that our local policeman is a correction on John. In this case, assuming that John and our local policeman refer to different individuals, the speaker would be seen to correct himself; it was not John to whom he gave the cup of tea but the local policeman. Thus, depending on whether the

pause was thought by the hearer to be an intentional part of the 'target sentence' or not, (9) would be given a different meaning. This means that, in editing an utterance into a target sentence, it is necessary to edit out all pauses which are not intended to play a functional role in the target sentence itself.

The above argument concerns silent or unfilled pauses. For filled pauses the case for editing is even stronger, since - as Voss' data on non-native perception of filled pauses show - it is possible to take the sound of a filled pause as an intentional part of the underlying target sentence. For instance, in (8), unless the filled pause is edited out of the target sentence the latter might look like the following

- (10) They've got priority on vice-chancellors and a
English schoolteachers.

Of course, (10) is not grammatical; in which case it could not be matched by a sentence of the competence grammar and given an interpretation. However, although ungrammatical on its own, (10) could be seen as an unfinished sentence. A possible completion is envisaged in (11).

- (11) They've got priority on vice-chancellors and a
English schoolteacher's fund has agreed with them.

That is, it is quite possible that the filled pause could be taken as the indefinite article of the noun phrase 'a English schoolteacher's X'. In fact, if we examine the context of this utterance in the LUND corpus, we find that this interpretation is entirely possible.

they've got priority on vice-chancellors and [ə] .
English schoolteachers . but I did [†] (Laughs) I did
talk to Doctor Power - and his suggestion was to come
straight to you (S.2.1, 72-75).

On examination, it will be seen that the filled pause [ə] can indeed be taken as an indefinite article beginning a subsequently unfinished

noun phrase of the form 'a English schoolteacher's X'. If this interpretation is adopted by the hearer, then the subsequent discontinuity following schoolteachers would be taken as initiating a correction and a deletion of the unfinished noun phrase. The possible target sentence thereby resulting would be as follows:

- (12) They've got priority on vice-chancellors but I did talk to Doctor Power and his suggestion was to come straight to you.

It is not difficult to find further utterances in which filled pauses suggest a similar ambiguity.^Q

- (13) I'm wearing my [ə] . white shirt (S.2.5, 359).

Given the similarity of the filled pause /ə/ to the indefinite article written as a, it is quite possible that the former might be taken as an instance of the latter and so as a correction of the determiner my. That is, the target sentence might be taken to be (3a).

- (13a) I'm wearing a white shirt.

If [ə] is not taken as an indefinite article with the ligatory function of signalling an editing correction of my, then it could be seen simply as a filled pause to be deleted from the target sentence, as in (3b)

- (13b) I'm wearing my white shirt.

In such cases, the hearer's decision is obviously not clear-cut. As it is, the speaker has not provided any clues to help the hearer in this editing choice, although he could have done so by repeating more of the pre-discontinuity context, perhaps even re-starting the utterance.

To conclude then, it appears that, if it is assumed that utterances must be edited into appropriate target sentence form in order to be understood by hearers, then both silent and filled pauses must be deleted in editing. The rule which is needed to effect this type of editing could be something like the following. Labov calls this

a 'cohesion rule'.¹⁶

$$\frac{\text{utterance}}{\text{ABC... (P) ... DEF}} \longrightarrow \frac{\text{target sentence}}{\text{ABCDEF}}$$

That is, delete all pauses from the utterance when editing into target sentence form.

Note that the above formulated cohesion rule makes no distinction between pauses required by the intonation and/or syntactic structure and those not required. Yet the hearer would have to know when to make this distinction, just as he would have to know which pauses were 'phonemic'. Only the non-phonemic pause possesses a possible editing function. It is therefore assumed that the hearer will somehow - by inspecting the speaker's intonation, gestures, the context of utterance, etc. - be able to recognize what type of pause a silence is.

We next turn our attention to repetition.

(14) Unfortunately they . they probably had it on twice
(S.2.5, 141).

(15) I presume that Worth hasn't got any more money .
because [w] when I asked him last week he said no ,
(S.2.5, 284):

If it can be recognized that repetition has occurred, the hearer has the simple task of deleting the repeated item or items. It is important, however, that it is the earlier version, i.e. pre-discontinuity version, of the item(s) that is deleted. The post-discontinuity version might have some alteration of the accentual or pitch structure which could have a role in the utterance's interpretation. Thus, the latest version of a repeated item is the one to be retained.^R

A possible rule for dealing with repetitions may be envisaged. Labov calls this a 'stammering rule'.

$$\text{ABC}_1(\dots)(\text{FP})\text{C}_2\text{D} \longrightarrow \text{ABC}_2\text{D}$$

Once again, it is not dealt with here how the hearer is able to recognize

that a repetition has occurred. Presumably there are cases where two items on either side of a discontinuity are instances of the same form but are not to be taken as repetition.

(16) I think that ... that man ought to be unburdened.
(invented)

Example (16) can be interpreted either as saying that a designated man ought to be unburdened or that (generic) man ought to be unburdened. The interpretation chosen depends on whether the second that is taken as a repetition of the first. (It is possible that the second 'that' would be accented if the former interpretation were intended, but not necessary.)^S

It will be noted that a very large class of repetitions repeat from the very beginning of the utterance concerned.

(17) I I I once made a dark brew (S.1.7, 1132).

(18) I'm not I'm not interested (S.1.8, 74), ^T

It is arguable that these ought to be kept separate from repetitions such as (14)-(16) in which only a part of the pre-discontinuity utterance is repeated but not all of it. The basis of this distinction lies in the fact that the post-discontinuity utterance in (14)-(16) is 'syntactically dependent' upon the pre-discontinuity utterance. In (17) and (18), what comes after the discontinuity is in fact syntactically independent. That is, the target sentences derivable from (17) and (18) would not include any of the items from the pre-discontinuity utterance. This would not be the case with examples (14) through (16).

This does not mean, however, that at no point does the hearer have to 'process' the pre-discontinuity sequences in (17) and (18). On the contrary, he has to have examined them and interpreted them at least to the point where he can determine that they do not contain

any items which must be retained in the target sentence. That is, recognition of the syntactic independence of the post-discontinuity is dependent on a prior analysis of the pre-discontinuity. On these grounds, it is probably best that a theory of editing does not distinguish 'false starts' such as (17) and (18) as outside the domain of editing rules.

* * *

The editing rules which apply to repetitions may also be applied to insertions.

(19) If he doesn't use -- wanta use his intelligence (Labov, 12.1).

(20) And I worked with them -- with some of them -- at least 25 years (Labov, 12.4).

Insertions may be seen as based on the repetition format but with one or more 'new' items added to the repeated sequence.^U The new items may be added either before the repeated sequence, as in (19), or in the midst of the repeated sequence, as in (20). Like repeats, only those items in the pre-discontinuity which are repeated in the post-discontinuity have to be deleted in the editing process. Thus, the following insertion rule may be envisaged.

ABCD(B)XCDE $\longrightarrow$ ABXCDE

* * *

At first glance it may seem no more difficult to formulate editing rules to deal with self-corrections than it is to devise such rules for the ellipsis of pauses, for repetitions, and for insertions. This is not, however, strictly true.

It is perhaps best to start off with the simplest aspect of correction editing and then to move on to the difficulties. It seems evident, given the target sentence hypothesis, that a post-discontinuity

involving a correction is to be linked, via editing, with the pre-discontinuity in the same way as is the case with repetitions and insertions.

(21) I've watched him for a long ... longer than you have. [✓]
(overheard)

The target sentence for (21) would appear to be (21b).

(21b) I've watched him for longer than you have.

Thus it would appear that (21) is edited into (21b) in much the same way as (22) into (22b).

(22) You always seem to ... seem to look tired.

(22b) You always seem to look tired.

How might we envisage the general editing rule applicable to (21) and (22)? A first suggestion might be that the hearer simply compares utterances (21) and (22) with a mental list of all the possible competence sentences of English. (21b) and (22b) would certainly be among the latter. How is the hearer to know that (21b) and (22b) are the appropriate competence sentences to take as the targets for (21) and (22)? There are other possible selections, such as those listed under (21c) and (22c).

(21a) I've watched longer than you have.

I've watched him longer than you have.

I've longer than you have.

(22c) You always look tired.

You seem to look tired.

You always seem to look.

Given that hearers do use editing rules, they must have a method by which they can tell which of the possible target competence sentences are the appropriate ones. Merely comparing the discontinuous utterance with the list of possible output sentences of the grammar does not solve that problem.

Another possible suggestion is that all of the utterance — both pre- and post-discontinuity—is preserved except for the element(s) in the pre-discontinuity which are repeated in the post-discontinuity. This would produce an acceptable result for (22) but would leave us with the ungrammatical (21d) as the target for (21).

(21d) I've watched him for a longer than you have.

Although this method of editing avoids the error underlying the (21c) and (22c) targets — the error of deleting more than is minimally required to be deleted from the original utterance — it may result in the deletion of less than is required, as in (21d). Target sentences are supposed to be possible outputs of the grammar for the reason that it is thought to be on the basis of his knowledge of this grammar that the hearer is able to understand English utterances. If the editing process does not result in a target which is grammatical, it should not be interpretable.

Thus, it appears that what is needed is a set of editing rules which prevents the hearer from deleting more than is required yet leads him to delete as much as is necessary to arrive at an interpretable, grammatical target sentence. In this case, we might perhaps take (21d) to represent an intermediate stage in the editing process. The final step would be to edit (21d) to (21b), i.e. the intended target sentence. How could this be done? In order to know how to edit (21d) — or, for that matter, (21) itself — to (21b) the hearer would have to be aware that a needed to be deleted. But how could he know that? It would seem that the hearer would already have to have determined that it is (21b) which is the intended target. Consequently, the editing would serve no purpose since the hearer was apparently already aware of the intended target. Going through the motions of deriving that target sentence when, in fact, he was already aware of it would be a pointless

exercise. If we are to make sense of the notion of editing rules, knowledge of which is shared by all hearers, we must imagine that the hearer is not aware in advance what the output of the editing process will be. He must be thought to apply a simple set of principles to the discontinuous utterance. This then results in his discovery of the intended target sentence. In order to resolve the dilemma presented by the two editing procedures suggested so far, the hearer would have to know in advance the intended target sentence.

It is evident that in order to derive the target sentences (21b) and (22b), an editing process is required which deletes certain units from the pre-discontinuity.

(x) - I've watched him for ~~a~~ ~~long~~ ... longer than you have

(y) - You always ~~seem to~~ ... seem to look tired

We may imagine the hearer as deleting units from the pre-discontinuity in order to be able to take the post-discontinuity as a continuation from the pre-discontinuity: i.e. for (21), to take longer than you have as a continuation from I've watched him for. The relevant question is: which principles guide the hearer in his choice of elements to delete? Why should he not delete as in (21e)?

(21e) I've watched ~~him~~ for ~~a~~ ~~long~~ ... longer than you have

The result is a competence sentence ("I've watched for longer than you have") but is, nonetheless, not appropriate. In (21e) the hearer deletes too much of the pre-discontinuity. The deletion of him is in excess. He also deletes unsystematically. That is, there is no apparent principle guiding his choice of him, a, and long.

It is important that the editing process be systematic. If not, it would be possible for different hearers of the same utterance to arrive at different target sentences for that utterance, such as (21b)

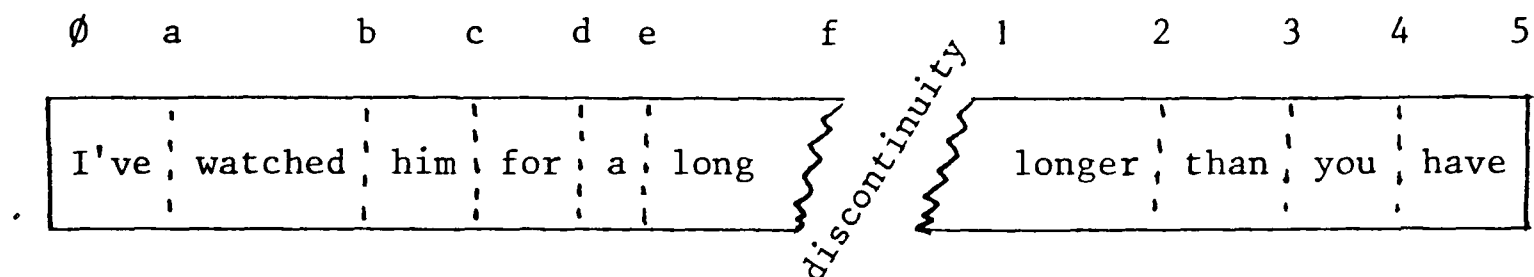
and (21e). The end result would be differing interpretations. And yet, we do know from observing conversation that hearers do not seem to experience difficulty in interpreting discontinuous utterances. They do so effortlessly, quickly, and in apparent agreement with other hearers. Disputes rarely, if ever, arise. Thus, if editing is a rule-governed process - an assumption the current discussion takes for granted - the relevant rules must be known to all users of the language in question. Different hearers must edit an utterance in the same way in order to preserve shared interpretations.

In deriving (21b) and (22b) from (21) and (22), it would seem that a principle which the hearer might use is as follows: delete 'from right to left' in the pre-discontinuity until the post-discontinuity may be taken as a continuation from the post-discontinuity. The examples (x) and (y) confirm to this principle. The principle, 'from right to left' ensures a systematic order in the editing while the proviso, 'until the post-discontinuity may be taken as a continuation from the pre-discontinuity', is needed to ensure that the deletion be the minimum necessary to arrive at a continuous competence sentence as target.

That is, to give an informal description of the editing process, we may imagine that elements of the pre-discontinuity are deleted - starting with the last word and working back one by one to the beginning of the utterance - until the remainder of the pre-discontinuity followed by the post-discontinuity approximate a satisfactory target sentence. Analogously, the utterance can be thought of as a section of magnetic tape,



We may then imagine that the post-discontinuity is pushed back over the end of the pre-discontinuity until a grammatical sentence, formed by the splicing of the two parts of tape, is arrived at.



So, first point (1) in the post-discontinuity is joined with (f) of the pre-discontinuity, as if only Labov's cohesion rule need be applied. But, the resultant string - "I've watched him for a long longer than you have" - is obviously not a grammatical sequence. Consequently the post-discontinuity must be pushed back further into the pre-discontinuity. If point (1) is made then to coincide with (e) in the pre-discontinuity, the resulting string - "I've watched him for a longer than you have" is still ungrammatical. Not until Point (1) - (note: always the beginning of the post-discontinuity) - is joined to (d) does the grammatical sentence (21b) finally appear.

This process may similarly be thought of in terms of deletion. That is, first the last word in the pre-discontinuity string is deleted, but the resultant combination of the remainder of the pre- and post-discontinuity is ungrammatical. Only when the last two words of the pre-discontinuity are deleted - i.e. (d)-(f) - do we arrive at the grammatical sentence formed by (0)-(d) followed by (1)-(5).

It is important to note that the process deletes words of the pre-discontinuity in reverse order of their production and then stops as soon as a grammatical sequence appears. For instance, if this were not the case and (b) to (f) were deleted first, a grammatical sentence (23) would appear.

(23) I've watched longer than you have.

Similarly, deletion of (c) through (f) would produce a grammatical sentence:

(24) I've watched him longer than you have.

Although there is hardly if any difference in meaning between (24) and (21b) the principle that deletion proceeds one-by-one in reverse order of production (what will be called the 'minimalness principle') must be retained so that grammatical but unsatisfactory sequences like (23) are not derived by hearers.

Similarly, with the repetition ligation of example (20) it may be seen that editing deletion must not only be word by word, but must also apply beginning from the point of discontinuity and then proceed 'back' into the utterance, in reverse order of its production. For if editing was carried out on (20) in a left-to-right order the hearer would derive such inappropriate sentences as (20a) and would never arrive at the correct target sentence (20b).

(20) I think it's about time we had a (expletive) in
[ʒi: əm] in the survey (S.2.5, 733).

(20a) We had a (expletive) in [ʒi: əm] in the survey.

(20b) I think it's about time we had a (expletive) in the survey.

We have seen enough cases of the editing of different types of ligation that the principles of editing are becoming clear. Remembering all the while that the notion of editing presupposes that hearers must derive target competence sentences from discontinuous utterances such as (21), (22), and (20) in order to understand them, we may formulate the following hypothesis as a preliminary description of the 'minimal deletion rule' underlying all forms of editing.

minimal deletion rule

- (1) if a break in the continuity of speech is perceived, take the post-discontinuity as a continuation of the pre-discontinuity utterance, but
- (2) if (1) is inapplicable due to its creation of an ungrammatical or otherwise unacceptable sequence, then delete only as much of the end of the pre-discontinuity as is needed to make (1) applicable, insuring that the order of deletion is the reverse order of the production of the pre-discontinuity.

This minimal deletion hypothesis is meant to apply equally to editing involving pauses, repetitions, insertions, and corrections.

The reason for formulating such a rule is, in this case, heuristic. That is, it will be easier to form a clearer view of editing if a preliminary rule of editing is first proposed and then the data is inspected for instances of exception to the formulated rule. In this way, the proposed rule will organize our investigation of the characteristics of editing.

A question naturally arises concerning the criteria, expressed in part 2 of the rule, which must be met if part 1 is to be deemed inapplicable, and thus deletion begun. According to the rule, the hearer is only to begin deleting words from the end of the pre-discontinuity if it is the case that the post-discontinuity may not be taken as a continuation of the pre-discontinuity because an 'ungrammatical or otherwise unacceptable sequence' would result. These criteria are supposed to be applied after each new deletion until an acceptable and grammatical sequence is arrived at. Although there are important questions that might be asked concerning (a) when in the hearer's processing of an incoming utterance, these criteria are to be applied, and (b) what determines if, at any one

moment, a sequence is ungrammatical or unacceptable, these questions will not be raised here. The purpose here is not to investigate how it is that hearers know that editing must be done, nor, for that matter, how speakers signal the need for editing to the hearer. Our concern, instead, is with the question of how that editing may be carried out, given that hearers know when it must be carried out and that a competence sentence is the target of the editing. If it can be shown that there is no way that many utterances could be plausibly edited to target sentences, then the questions of (a) when such editing should be done and (b) how the need for it is signalled automatically lapse.

An examination of the minimal deletion rule will reveal that it is based on (at least) four principles. The first, which may be called the 'minimalness principle', holds that only as much is deleted of the pre-discontinuity as is needed for the criteria of grammaticality and acceptability of the resultant sequence to apply. A second principle may be identified, the 'continuity principle'. This principle states that when all the deletion required (if any) is completed, the post-discontinuity must be taken as a continuation from the (remainder of the) pre-discontinuity. These two - the (remainder of the) pre-discontinuity and the post-discontinuity - will form a conjoined, continuous whole which approximates the relevant target sentence. The third principle, the 'deletion principle', implies that whatever is deleted in the editing process does not form a part of the target sentence. Furthermore, since the target sentence hypothesis holds that the hearer's interpretation of an utterance proceeds by relating that utterance to a target competence sentence and then deriving the meaning which the grammar assigns to that competence sentence, it may be seen that whatever is deleted from the

utterance during deletion is also thereby deleted from consideration in the hearer's interpretation of the utterance. Finally, a fourth principle, the 'explicitness principle' refers to the assumption, important to the notion of editing, that the speaker explicitly produces and in their proper order all the elements of the eventual target sentence. He also produces more than what goes into the target sentence. That is, he produces the target sentence plus what is deleted from the utterance during the process of editing. This means that the hearer does not have to invent any parts of the target sentence but merely derives it from the utterance produced. Each one of these four principles will be shown to raise significant problems.

There may be sentences which result from the minimal deletion rule which do not qualify as the desired target sentences for the utterances concerned.

(25) It was sort of the ... uh certain speech patterns (Labov, 9.3)

(26) I saw Mary at the market and she ... at the hairdressers and she looked quite ill. (invented)

(27) He's in the Yiddish [dipa:] department of Yiddish literature (S.1.2, 741).

In these examples there is nothing to prohibit the inappropriate application of part 1 of the minimal deletion rule. The outputs of this, presented in (25a), (26a) and (27a), are, at first glance, perfectly satisfactory target sentences.

(25a) It was sort of the certain speech patterns.

(26a) I saw Mary at the market and at the hairdressers and she looked quite ill.

(27a) He's in the Yiddish department of Yiddish literature.

However, it may well be that, in ligating, the speakers of these utterances meant to signal that (25b), (26b) and (27b) were the

intended target sentences.

(25b) It was sort of certain speech patterns.

(26b) I saw Mary at the hairdressers and she looked quite ill.

(27b) He's in the department of Yiddish literature.

But if the hearer respects the minimalness principle of his editing he cannot derive (25b), (26b) and (27b) from (25), (26) and (27) since they require more than the minimum of editing needed to arrive at a target sentence. In order to derive (25b), (26b) and (27b) the hearer will have to overrule the minimalness principle.

A similar situation may be seen with the following example.

(28) It was during the fifty-six sixty-four (S.1.2, 1089).

From the context in which this utterance is embedded, it is apparent that the speaker means to say here "It was during sixty-four": i.e. it was during the year 1964. Similarly, it appears from the context that the hearer understands that the speaker means 'during 1964'. But, if we suppose the hearer to be following a minimalness principle, he should not have derived a target sentence which could be so interpreted.

A strict application of the minimalness principle to (28) would merely result in the deletion of the pause between fifty and six. There is no reason - except for that provided by the context of the utterance - why the speaker cannot be thought to be saying that the event referred to was 'during the fifty-six sixty-four'. To make sense of such an utterance we need only imagine that "the 5664" refers to some event - e.g. the annual meeting of a futurist society. Similarly, if the hearer edits only the from the utterance, a next-to-minimal deletion, the target sentence - "It was during 5664" - makes perfect sense if taken within the context of a discussion of a science fiction novel about the 57th century. Or if the hearer deletes both the and

fifty, a less minimal edit, the sentence would make perfect sense within a discussion of the middle ages.

The point, here, is that the minimalness principle will sometimes have to be overruled by the criteria governing the acceptability of the resultant sentence.^W Furthermore, these criteria may be seen to involve contextual or situational considerations. That is, the utterance, as produced, is a grammatical sentence of English and could be acceptable given the appropriate context-of-utterance. Therefore, the question of what the minimum amount is that can be deleted in order to edit the utterance into an appropriate sentence will always be dependent on the situation in which the utterance is embedded. This also applies to examples (25), (26) and (27) as well as all utterances where the question of editing is thought to arise. In this case, it seems reasonable to say that the minimal deletion rule requires that the hearer already knows what is being said - having guessed this from the context - before he can decide how to edit an utterance to a form from which he may derive its meaning. So, presumably if the situation does not provide enough contextual clues - e.g. as to the century being talked about in (28) - the hearer will not be able to tell what sentence a speaker's utterance is an instance of. In this way, the hearer's interpretation of an utterance may be seen to come in at a very early stage in the process assumed by the target sentence hypothesis.

So, although it is evident that the minimalness principle must sometimes be overruled, it is possible to put down the reasons for this overruling to the criterion of acceptability expressed in part 2 of the minimal deletion rule. In this sense, then, the rule can be retained without adjustment, given that the criterion of acceptability is sufficiently broad to include quite detailed and advanced knowledge of context.

The examples of problems raised by the continuity and deletion principles are not specific to either of these principles.

- (29) It was just such a nightmare - I mean this whole system of being invited somewhere for lunch and then for dinner - and overnight . and breakfast (S.1.3, 221).
- (30) We could all slant atheistic slogans . slant . chant
(overheard in conversation).
- (31) The interview was - it was all right (S.1.3, 305).
- (32) The chaplains that we want just aren't ... I mean ... the headmaster wouldn't invite them in. (Labov, 1975)
- (33) She said it's the basic truth about men . is that men . like to be with other men (S.1.3, 755).

The continuity principle holds that, after all deletion has been carried out in the edit, then the post-discontinuity is to be read as a continuation from the end of the (remainder of the) pre-discontinuity. So, in (34), after six months later is deleted, three months later is to be read as a continuation from the end of and then they rang up about, giving the target sentence (34a).

- (34) and then they rang up about six months later three months later (S.1.3, 134).

(34a) and then they rang up about three months later.

However, with an example such as (30), it appears that such a process will never produce the intended target sentence (30a).

- (30a) We could all chant atheistic slogans.

In the production of (30) the speaker makes the error of transposing the word-initial cluster of slogans to the front of chant, replacing its own initial cluster /tʃ/. He notices this error and draws attention to it by repeating it. Then he produces the correct form chant. If we maintain the continuity principle, so that the post-discontinuity is to be taken as continuing from the end of the remainder of the pre-discontinuity, then (30b) is the resultant sentence.

- (30b) We could all chant.

Note that atheistic slogans is deleted from (30) in the derivation of (30b) because of the continuity principle. If the post-discontinuity, chant, is taken as continuing on from the remainder of the pre-discontinuity, we could all, then atheistic slogans, deleted in the editing, must be absent from the target sentence. This is the meaning of the point stated in the deletion principle.

Although (30b) is a perfectly adequate sentence, it is quite obviously neither what the speaker means by his utterance nor what any ordinary hearer would take him to mean. Few would disagree that (30a) conveys the appropriate meaning of (30), and yet, according to the deletion principle, the information encoded in atheistic slogans cannot be conveyed by a target sentence which is derived in accord with the continuity principle.

Given the laxity of the grammaticality and acceptability criteria, it could be argued that (30b) would not be accepted by the hearer. In this case, no target sentence would be derived from (30) and, consequently, no information conveyed. The ease of interpreting (30) then stands in need of explanation.

If we reflect on example (29) - repeated below - it appears that the only appropriate target sentence for it is (29a). However (29a) cannot be derived from the minimal deletion rule.

(29) It was just such a nightmare - I mean this whole system of being invited somewhere for lunch and then for dinner - and overnight . and breakfast (S.1.3,221).

(29a) This whole system of being invited somewhere for lunch and then for dinner and overnight and breakfast was just such a nightmare.

What follows I mean in (29) is apparently a specification of the subject of the sentence, which in the pre-discontinuity is filled by the pronoun it. Because of the continuity and deletion principles there is no acceptable target sentence which could be produced by taking

This whole system ... and breakfast as a correction of the subject it.

A similar situation applies to (31) and (32). In each of these examples the editing would result in the whole of the pre-discontinuity being deleted, so that the target sentences (31a) and (32a) would result.

(31a) It was all right.

(32a) The headmaster wouldn't invite them in.

However, in these target sentences it is unclear what the pronouns are substitutes for since The interview and The chaplains that we want have been deleted through editing. Although no context is provided for (32), the speech preceding (31) does not provide the hearer with the noun phrase - the interview - which it is a substitute for.

Since (31) and (32) do appear, on reflection, perfectly easy to interpret, the conclusion must be drawn that the deletion principle does not always hold. That is, it does appear to be the case that some words which are deleted from an utterance, in the process of editing it into a sentence, continue to have a semantic/syntactic relevance to the interpretation of the derived target sentence. Although, due to the continuity principle, phrases such as the chaplains and the interview are not part of the relevant target sentences - from which the interpretations of those utterances are derived - they nonetheless continue to influence the hearer's interpretation.

Example (33) provides another problem for the principles of continuity and deletion.

(33) She said it's the basic truth about men . is that men .
like to be with other men (S.1.3, 755).

Here, the post-discontinuity - viz. is that men like ... men - cannot be taken as continuing from any part of the pre-discontinuity unless its is deleted from the pre-discontinuity. That is, (33a) appears to be the appropriate target sentence.

- (33a) She said the basic truth about men is that men like to be with other men.

According to the minimal deletion rule, the order of deletion is the reverse order of the production beginning at the end of the pre-discontinuity. This means, that in order to delete its, the editing would previously have to have deleted the basic truth about men. If this occurs, then the resultant target sentence, (33b) is not grammatical. (In addition, the information provided by the basic truth about would be lost.)

- (33b) She said is that men like to be with other men.

Yet, in spite of the fact that the appropriate target sentence, (33a), cannot be derived from (33), it seems quite certain that any hearer would understand (33) in the same way that (33a) would be understood. The interpretation of (33), although it is uneditable, poses no problems.^X

The problem posed by (33) is much the same as that posed by (34).

- (34) and there was [ə m] - a lecturer in German - a lecturer in philosophy --- [ə m]. the president . of the college -- the treasurer of the college -- and another --- bitchy . iceberg of a woman)S.1.3, 456).

The explicitness principle holds that the speaker explicitly produces, and in their proper order, all the elements of the target sentence in his utterance. All the hearer has to do is delete superfluous elements from the utterance. No additional corrections, re-orderings, or insertions are required of the hearer in order to edit the utterance into the appropriate target sentence. If we take one of the discontinuities to occur in (34) after a lecturer in German, it may be seen that what occurs in the post-discontinuity renders the utterance ungrammatical due to the lack of agreement between the copula was and its plural complement. That is, before the discontinuity the copula was in agreement with the single complement, a lecturer in German,

but lost that agreement due to the addition of further complements after the discontinuity. To derive an appropriate target sentence was would have to be edited to 'were'. But such an editing would contravene the explicitness principle since 'were' is never produced by the speaker.

The same problem arises with example (35)

- (35) the Society is obviously not in the dark as much as is
the audience about ... the participants themselves about
what is to be said. (overheard in conversation).

In this example, the correction of the audience by the participants results in the lack of agreement between the singular verb is and its post posed subject. That is, the sequence 'as much as is the participants' is ungrammatical. To derive a grammatical sentence from (35), the hearer would have to make a correction not explicitly carried out in the speaker's ligated utterance, although perhaps implicitly presupposed. He would have to correct as is to 'as are'. If the explicitness principle is not contravened in this way, no grammatical target sentence would be produced. Consequently, according to the target sentence hypothesis, the hearer would not be able to interpret (35).^y

It would, of course, be perverse to hold that example (35) was uninterpretable. Speakers commonly make errors of this kind without ever correcting them, and yet there is no evidence that such errors ever provide an obstacle to the success of the communicative act.

In view of the ordinariness of such errors, one might want to argue that the fact that hearers are able to interpret utterances with such errors shows that the editing process does not have to produce a perfect, error-free competence sentence. That is, as our intuitions would have it, we are able to understand utterances that do not conform exactly to the letter of grammaticality. So, there is no reason to hold that, in editing, the hearer must derive perfectly grammatical target sentences.

But then the following question arises: if interpretation is not blocked by the lack of competence sentence form in (35), why should this not also hold for the other examples given above (e.g. (1), (7), (8), (19), (20), (21), (33) ...)? Is editing to competence form really necessary for the interpretation of such examples? At the core of the question here raised is another question: what is the relation between grammaticality and communication?

* * *

4. 'Grammatical incoherence' and communication

In this light, the argument put forth by K. Brown in his paper, "Grammatical Incoherence"¹⁷ is of considerable interest. He uses the expression 'grammatical incoherence' to characterize utterances like the following:

(36) How long do you suppose a life of a fur has?

He points out that grammatical incoherence is not a version of the 'tongue-slip'. The latter generally involves a distortion of word form and is, as Boomer and Laver claim,¹⁸ "almost always detected, not always consciously, by the speaker, and corrected." Grammatical incoherence, on the other hand, characteristically passes uncorrected by the speaker and undetected by the hearer.¹⁹ Brown guesses that there is a different mental monitoring for word formation than for surface sentence structure, and that the former, being stricter than the latter, accounts for the greater detection and correction of tongue-slips. Furthermore, Brown claims grammatically incoherent speech does not appear to hinder successful communication

Finally, I would hypothesise that typically it is not at variance with the speaker's cognitive intention. I would suggest that it is this last hypothesis coupled with the fact that grammatically incoherent speech does not involve distortion of word form that accounts for the fact that it passes uncorrected and does not apparently impede communication.²⁰

The major theme of Brown's argument seems to be that grammatical incoherence provides as useful a 'window on the mind' as do speech errors and hesitation phenomena. That is, they offer the analyst evidence on the working of verbal mental planning and monitoring processes. He isolates a number of different types of grammatical incoherence which are claimed to stem from different functions of the verbal planning process. Among these he discusses two types of blending, cognitive and process, and five further types of incoherence to which he gives the names: topicalization incoherence, co-ordination incoherence, process incoherence, referential incoherence, and selection incoherence. These are illustrated in the examples following.²

- (37) It's about the police I'd like to talk about
(topicalization incoherence).
- (38) That is a suggestion for which I am all (process incoherence).
- (39) A loss of wages about the overtime + about thirty five
pounds a week (co-ordination incoherence).
- (40) You also realize of course that apart from denying + certain
people who can't actually go to matches + the right to see
it + and enjoy it + you are also denying smaller teams +
the chance to get the money + that television eh + brings
to the game. (referential incoherence).
- (41) I feel that the press eh eh in a lot of cases give us a
bad publicity (selection incoherence).

For our present purposes, what is of interest in Brown's argument is not so much the different types of planning strategies which he sees as evidenced by the various types of blends and incoherence, but rather his defence of the claim that these syntactic anomalies do not interfere with the success of the speech acts of which they are a part.

Brown claims that incoherence passes uncorrected and probably undetected by both speaker and hearer. He presents a hypothesis accounting for this in his discussion of example (36), here repeated.

(36) How long do you suppose a life of a fur has?

Having first characterized the production of a fur as a cognitive blend, based on the model of a hair (the speaker is holding up one hair from the fur of an animal), Brown proposes that such a surface structure might also be the result of the blending of more general grammatical processes underlying the related structures (36a), (36b) and (36c).

(36a) How long a life does a hair have?

(36b) How long a life has a hair?

(36c) How long is the life of a hair?

Brown argues against accounting for (36) in terms of a blending of the surface structures of (36a-c). This would be inappropriate because the mechanism which would be required to regulate such a blending would have to be very complex. It would be quite absurd to suppose that a speaker could not manage his speech production mechanisms well enough to produce a grammatically well-formed sentence yet could manage a complex mechanism required to blend three intricate surface structures into an ungrammatical output.

Instead Brown argues

It is perhaps less complex to hypothesise that what is involved is a set of abstract grammatical processes. The processes involved I have represented as 'interrogative inversion' (shared by 36a and 36b); DO support (shared by 36a and the interpolated do you suppose), the formation of the correct form of have (shared in different ways by 36a, 36b); the formation of constituents (shown by 36a, 36b, and 36c). Suppose that some monitoring function is satisfied, if the appropriate processes have been carried out - and in a sense, as you can see, they have: we have DO support (but only once in do you suppose rather than twice), we have interrogative inversion, we have a form of HAVE, and we have the interpolation of do you suppose at a place that is consistent with some appropriate constituent break.²¹

The point here seems to be that utterance (36) fulfills a variety of syntactic requirements, viz. those needed for the purpose of communicating

the intended message. To fill these requirements a number of operations have to be carried out: DO support, inversion, constituent structuring, etc. These could be carried out in different ways; but as long as they are individually performed in some way, it does not matter that the ways chosen for each particular operation do not form a grammatically coherent fit with the ways chosen for the other operations. To illustrate, let us imagine that four types of operation (A,B,C,D) must be performed in order to achieve the desired effect X. For each of these types of operation, there are three different ways of accomplishing them. These are represented by subscripts: So we have $A_1, A_2, A_3; B_1, B_2, B_3 \dots D_3$. Four operations must be chosen, one from each letter set. If, for instance, A_2, B_1, C_1 , and D_3 are chosen, X is achieved. The same result applies to A_3, B_2, C_2 , and D_1 , and so on for all the possible permutations. However, imagine now that there is more to performing these operations than simply achieving X. Throughout history, certain conventional methods have been established for achieving X. In an imaginary community, at a particular moment, it may be 'the done thing' to achieve X by choosing subscripts in their proper numerical order: e.g. A_1, B_2, C_3 , and D_4 . According to this convention, although another combination - say, A_3, B_1, C_3, D_1 - may achieve X, it does not do so by the socially accepted method.

This illustration appears to capture what is at issue in Brown's grammatically incoherent, but communicationally successful, utterances: example (36). It may be that communication is achieved by (36) because each of the types of grammatical processes required for that utterance are indeed performed. However, the choice of variants within those types does not result in a combination which could conventionally be called 'grammatical'. Consequently, (36) appears, on paper at least, to be

grammatically incoherent. No editor would allow it into the pages of his publications. Many teachers and linguists would notice it in the speech of others and - if pedantic - would correct it. What is of interest is that the question of the grammaticality of (36) appears to be separate from that of its communicational efficacy.

Thus, it does not seem unreasonable to conclude that hearers - attempting to interpret a speaker's utterance - and speakers - monitoring their speech for its communicational efficacy - will be undisturbed by such 'grammatical incoherence'. They may indeed not even notice it. If successful communication is their primary concern, there is no reason why they should. Should they add to their monitoring a requirement that the speech be grammatical - and this is quite common, say, in a 'public speaking' context - then they might well notice such incoherence. But in ordinary conversational situations - i.e. those situations in which language is mainly used - they will have no need to.

Another example: if I am playing in a trial for the real tennis team, I will do more than simply try my best to beat my trial opponent. I will also try to impress the selectors with my stroke play, my service, my continuous awareness of the game's possibilities, my use of the winning openings and so on. I must do this if I am to convince the selectors not only that I can win on my home court against a familiar opponent but that my game is so solid that it should adapt well to playing against opponents of differing styles on a variety of courts. Just beating my trial opponent is necessary but not sufficient if I am to convince my judges of my all-round tennis ability.

However, if the same opponent and I are playing in the court, with a large bet riding on the result, all that matters then is that I win. Who care whether I do so with rather unorthodox shots and relying on my knowledge of the peculiarities of bounce and spin on this particular

court? In order to win the bet it is sufficient for me simply to defeat the opponent, any way I can. The extra requirement of impressing selectors is absent in this situation. Only the minimum requirement of winning matters.

It is possible to conceive of ordinary conversational interaction analogously. In everyday conversation what matters most, it could be argued, is that communication is achieved, to the mutual satisfaction of the parties concerned. As long as this criterion is fulfilled (or, as long as the interactants have no reason to believe that it has not been fulfilled), the question of the grammaticality of the utterances produced is irrelevant. The standard of grammaticality which is applied to written language (and which many grammarians take to underlie language competence: for speaking or for writing) does not apply to everyday conversational speech. If this conclusion is accepted, then it may be seen that, in an ordinary conversational setting, the presence of a certain amount of 'grammatical incoherence', of discontinuity, of repair, of 'hemming and hawing', etc., is both natural and untroublesome. In the same way, it is to be expected, when no selectors are present, that my style of play should be sacrificed to the achievement of the more important goal, in this context, of winning. It would be artificial to measure my style of play, in that context, against the selectors' conventional notion of 'ideal style', since that notion is irrelevant to the accomplishment of what, in that context, is the task at hand. The same conclusion might be drawn regarding the application of written language standards of grammaticality to conversational speech.

To summarize the conclusions drawn from this reflection on Brown's data: it appears that two questions may be separated in the analysis of (at least) conversational speech: the question of communicational efficacy and the question of grammaticality. It is one thing to speak

effectively, another to speak in conformity to the conventional standards which apply to the particular speech situation. This does not mean that 'speaking grammatically' will never overlap with 'speaking effectively', but only that satisfaction of the requirement for one does not imply satisfaction of the requirements for another. With example (36) - as well as examples (7), (8), (13), (25), (29), (33), ... - we have seen that one does not have to speak grammatically in order to speak successfully within a particular communicational context. Similarly, we may imagine someone who speaks perfectly grammatical sentences, 'worthy of prose', yet who fails to communicate in a situation because his utterances do not fit the communicational needs of the moment.

David Crystal makes a related point in his paper "Neglected grammatical factors in conversational English".²² He shows that some regular features of conversational English - specific features of clausal arrangement and adverbial usage - contravene orthodox grammatical notions of the sentence and its constituent parts. He reflects on the relevance of the typical invented examples, which one finds in theoretical accounts of language, to the linguistic analysis of conversational speech.

Because all such sentences - or at least most of them - are speakable, it is easy to imagine that there is no problem - that the grammar of informal domestic conversation is basically a reflection of that of the written language, with a few additional conventions such as ellipsis, intonation, and emphatic word order, and a few omissions, such as the structures characteristic of the more formal and literary modes of expression. The argument of the present paper, on the contrary, is that the linguistic organization of this variety of English has been fundamentally misconceived, due partly to the absence of data, partly to the uncritical application of traditional paradigms of enquiry.²³

At one point in his paper, Crystal examines a typical conversational passage which would rebuff any attempt to analyse it in terms of sentences. The lines are identified by letters in order to facilitate discussion.

- (a) I'm very suspicious of the press generally
- (b) and I can tell you
- (c) because - not only I mean that's one case
- (d) that you've given
- (e) but also in their reporting of erm affairs foreign affairs -
- (f) because - living in Cyprus
- (g) I've seen - quite a number of historical events you know

Few problems arise in attempting to understand this passage, but, as in Brown's examples, they do arise if one tries to explain how we are able to understand it in terms of our implicit grasp of a supposedly underlying sentence structure.

From the context, it is plain that the reason for (a) is given in (c-e). The reason for (b), i.e. why the speaker is an authority, is given in (f-g). What we have, therefore, is a structure of the following type.

Main Clause A + Main Clause B + Subordinate

Clause A + Subordinate Clause B

and it is this kind of pattern which is here referred to as 'intercalated'. The situation is however more complex than this. (e), from a semantic viewpoint, relates to both sentences: it is half of the reason for (a), along with (c-d), but it also provides the new theme which is the link with (f-g). Syntactically, (e) has no main verb, and there is thus some motivation for seeing this as a complex adverbial, linked (via the because of (f) to (g)). Because of such complications, we are once again faced with an unclear analysis in terms of sentence structure.²⁴

We are not, however, faced with an uninterpretable passage of conversation.

The same applies to the following passage from the LUND Corpus.

- (42) Really unless there's something wrong with the candidate
from their college . why she shouldn't get it . can you
- this makes sense to me sort of loyalty to their own
(S.1.3, 266).

Neither of these passages can be transformed into competence sentences unless an implausible amount of editing and 'filling in' of ellipses is attributed to the transformational process. In view of the fact that there is little difficulty in understanding what the speakers are trying

to say in these passages, it would therefore seem that grammaticality is far from being a necessary criterion in the achievement of communicational efficacy.

Crystal's argument solely concerns the division of conversational passages into recognizable sentential units. On this account he recommends that the clause, rather than the sentence, be taken as the unit "in terms of which (conversational) material is most conveniently organized".²⁵ While it is true that such a move would allow a more coherent description of many conversational passages, still examples such as (36), (36), and (38), repeated below, suggest that even the orthodox, written-language notion of the clause is sometimes inapplicable to communicationally efficacious speech.

(36) How long do you suppose a life of a fur has?

(37) It's about the police I'd like to talk about.

(38) That is a suggestion for which I am all.

It might still be argued that another possibility is left open in this discussion. Perhaps speakers aim to speak both effectively and grammatically. That is, although it might be admitted that grammatically perfect utterances can be communicational failures (in certain contexts) and that ungrammatical utterances can be communicational successes, this does not mean that speakers do not aim to utter grammatical and continuous utterances as a partial means to communicating effectively. After all, it seems intuitively obvious that if you fail to meet certain minimal standards of grammaticality (or, of continuity), you will, ipso facto, fail to communicate. So, it seems reasonable to assume that speakers try to communicate by means of using grammatically well-formed and continuously produced utterances.

Such an argument would suggest that grammaticality and communicational

efficacy are inextricably linked. The way to communicate effectively is - as much as possible - to 'speak in prose' (given that you keep your utterances on the topic of the discussion). And, when you fall from this ideal, you must allow hearers to repair the faults in your product: i.e. you must ligate in ways that facilitate editing. Only in this way can your utterances be related to the shared linguistic code. Indeed Crystal has argued in another publication

We may certainly be imprecise or change our minds in informal conversation but it is necessary to do this grammatically.²⁶

However, as the evidence from Brown, Crystal himself, and passage after passage of the LUND corpus shows, many of the utterances speakers produce cannot, without the hearer invoking ad hoc procedures, be edited into grammatical target sentences; and yet they still are communicatively efficacious. This must raise doubts about the above argument. If hearers do not require grammaticality in order to understand utterance, and if speakers fail time and time again to speak grammatically, it seems just as reasonable to suppose that grammaticality is not a relevant criterion for the speaker. Instead, as Brown argues, the speaker may aim to accomplish certain grammatical processes as a means to communicate; but these do not have to result in a competence sentence in order to achieve the goal of communication. There are a number of ways to accomplish these processes, each conjunction of which does not necessarily result in an utterance which the grammarian would be willing to call a grammatical sentence.

Furthermore, it is arguable that there are certain characteristics inherent to conversation which make a sentence-based notion of grammar inapplicable. Indeed, these characteristics may offer a partial explanation of why conversational utterances could not look like

written sentences, without taking away from conversation some of its central functions. An examination of a paper by C. Goodwin will serve to illustrate these features.

5. The utterance as an interactive construct

In his article "The Interactive Construction of a Sentence in Natural Conversation",²⁷ Charles Goodwin argues that the structure of sentences cannot be adequately understood without analysing the situations in which they are interactively constructed. The structure that a particular sentence has is, Goodwin argues, given to it not just by the speaker who utters it but also by the hearers and potential hearers to which it is addressed.

... in traditional linguistics it has been assumed that the analysis of sentences can be performed upon examples isolated from such an interactive process. In opposition to such a view it will be argued here that sentences in natural conversation emerge as the products of a process of interaction between speaker and hearer and that they mutually construct the turn at talk.²⁸

Goodwin's claim is that the speaker of an utterance will alter the meaning of that utterance as well as the structure of that utterance in the course of its production. This may be done for reasons of recipient-design, viz. "to maintain its appropriateness to its recipient of the moment." And since it is possible for a sentence's principal recipient to change, once or more than once, during the production of an utterance, it is also possible that the utterance will change shape and function to fit its changing recipients (for Goodwin 'recipient' appears to refer to the hearer at whom the speaker is looking at any one moment). Furthermore, the shape of an utterance can be used to accomplish interactive tasks during its production. Goodwin here is particularly interested in the negotiation of gaze between interactants and holds that the emerging shape of the utterance can be used as a tool

to negotiate gaze.

Goodwin's argument is made by reference to a passage from an actual conversation between two couples at dinner. The passage was videotaped and is transcribed by Goodwin, as follows:

- J: . , , ... [Don, , [Don_____
1. John: I gave, I gave up smoking cigarettes : : . =
- D: . . [John_____
2. Don : = Yeah,
- J: . . . [Beth_____ . . . [Ann_____
3. John: 1 - uh: one - one week ago t'day. acshilly
- B: [
- A: [Beth, . . . [John_____

(Goodwin uses special transcription conventions to indicate the orientation of the gaze of parties to the conversation. In the example above the speech following "John:" is uttered by John, husband of Beth. The line above John's speech, prefaced by "J:", indicates the movement of John's gaze during his utterance. Where there is a line beneath another interactant's name, e.g. "Don_____" immediately above "cigarettes : : .", the implication is that John is looking directly at the named interactant. Dots serve to signal the movement of gaze towards a recipient, while commas signal a movement away. So in the first line of John's utterance John starts to bring his gaze towards Don, hesitates during the utterance of the first "gave", moves again towards Don and reaches him, but only for a moment at "up". He again fixes on Don during most of "cigarettes". In the second line his gaze rests on his wife, Beth for a while and then moves to Ann. The lines beneath John's utterance indicate how the other interactants are orienting their gaze. Don begins gazing at John in the

middle of John's utterance, at "smoking". Beth never looks at John. Ann, in the second line, gazes at Beth for a moment before turning to John near the end of his utterance. Note that at the end, John and Ann are gazing at each other. (Further elaborations on this transcription system may be found in Goodwin, 1978).

Goodwin claims that there are two rules organizing gaze during conversational settings such as these.

Rule 1: The gaze of a speaker should locate the party being gazed at as an addressee of his utterance.²⁹

Rule 2: When a speaker gazes at a recipient he should make eye contact with that recipient.³⁰

An inspection of the passage above will reveal that these rules are not always adhered to. It is true that when John is looking at Don - and so, according to Rule 1, locating Don as his addressee - Don returns that gaze, thus fulfilling Rule 2. Still John appears to change addressees in line 2 to his wife, Beth. So, given Rule 2, we should expect Beth to return John's gaze since he has picked her out as the recipient of the second part of his utterance. But she does not do so. Goodwin never tells us what Beth is doing, at this moment (is she able to return John's gaze? Or is she, say, carving a particularly tough slice of meat?) We do know, however, that her actions do not satisfy the requirements of Rule 2. John then turns to Ann, hoping finally to achieve a state of mutual gaze. This is eventually accomplished, Goodwin surmises, by John's adding the 'utterance-lengthener' actually to the end of his utterance, thereby allowing Ann the time to turn her gaze from Beth to John in time to achieve mutual gaze as well as the satisfaction of Rule 2, before the end of John's utterance.

Goodwin argues that the construction of John's utterance responds to and even plays an active part in the management of these gaze manoeuvres.

The ways in which this is done are discussed below under a, b, c, d, and e.

- a - By restarting his utterance (I gave, I gave) John requests the gaze of his recipient, Don.

A speaker can request the gaze of a recipient by providing a phrasal break, such as a restart or a pause, in his utterance. After such a phrasal break nongazing recipients regularly bring their gaze to the speaker.³¹

John then moves his gaze to his wife, Beth.

- b - By producing the phrasal break, 1 - uh:, John requests the gaze of his new recipient, Beth. In addition, the length of this break, managed by the time spent on the filled pause [ə], is said to give John just the amount of time needed to move his gaze from Don to Beth "so that the next section of the sentence begins just as the gaze of the speaker reaches his new recipient."³²

This suggests that the speaker has the ability to precisely control events even within the production of a single phonetic unit (the filled pause: TJT) in order to accomplish social tasks posed in the construction of the turn.³³

- c - John discontinues again - one - one as a request for Beth to bring her gaze to him, but still with no success.
- d - At the beginning of the second line John turns his gaze to his wife, Beth. At this moment, John corrects his projected utterance, 1 - uh: one - one week ago Goodwin claims that John corrects his planned utterance, something like "last week" or "last Monday", to one week ago because he finds himself addressing a 'knowing recipient', i.e. his wife. That is, Beth is thought to be well aware that John gave up smoking last week. So, there is no reason to inform her of this fact. Thus to continue the projected utterance,

begun when addressing an unknowing recipient (Don), would be inappropriate now that a knowing recipient (Beth) is being addressed. Consequently, John corrects to one week ago, with 'discovery intonation', on one to provide his new recipient with the new information that that day is the first week's anniversary of his giving up smoking. In other words, in response to a switch from an unknowing to a knowing recipient, the speaker alters his planned utterance in mid-course so as to adapt his on-going speech to, what Sacks calls, the 'general rule of conversation': viz. "one should not tell one's co-participants what one takes it they already know."³⁴ Orientation to this rule, says Goodwin, affects John's on-going construction of his utterance.

e - The 'utterance-lengthener', acshilly, as already mentioned, is added to the utterance in order to give Ann time to turn her gaze to John before his utterance ends. In addition, by adding this form

the discovery of the anniversary is transformed into a report about it. Rather than being asked to recognize the anniversary the recipient is told that in fact the event being marked by it did occur a week ago. The addition of "acshilly" thus again reconstructs the emerging meaning of John's sentence so that once more it becomes appropriate to its recipient of the moment.³⁵

Thus, Goodwin proposes at least five ways in which one utterance in a natural conversation is reconstructed during its production in order to respond to certain of the non-verbal features and background features of the interaction. This is not the place to discuss the evidence for each of his conclusions. Rather the point of this consideration of Goodwin's argument is that there are a number of potential interactional reasons why utterances should not be produced as ideally delivered competence sentences. The reasons considered here are also quite distinct from

those considered in the chapter on pausology, in which discontinuities were seen as errors in cognitive processing performance. The discontinuities and (incoherences) here discussed would have to qualify as tools in the speaker's attempt to communicate within the relevant interactional setting. They allow him both to make his speech an integrated part of that interaction and to contribute to the continuously developing management of that communicative interaction.

In other words, if we take the speaker's aim to be the ideal production of competence sentences which encode the meaning he wants to convey, then the discontinuities and the incoherence which richly characterize natural conversational speech must be taken as errors, for which the extra device of editing rules is required in order to splice the utterances back into sentence form. However, if instead the speaker's goal is conceived to be to contribute intentionally and effectively to the developing communication situation, then these discontinuities and incoherences may be seen as useful tools for the accomplishment of that goal.

This would mean standing on its head the orthodox view of the relation between the sentences we find in print and the utterances we find in speech. That is, the evidence on discontinuities and incoherence suggests that speech is not an error-prone form of producing sentences of the type seen in print. It would be more plausible to conceive of written language as different from spoken language because additional conventional restrictions are applied to writing. Such a formulation would not necessarily amount to a claim that written language 'equals' spoken language plus other conventions, but rather that both are drawn from our linguistic competence (a competence not describable in terms of sentence-generating rules) with the addition, for written language,

of a secondary set of filtering rules not applicable to the more informal varieties of conversational speech.

In view of his interesting remarks on the interactive construction of utterances, Goodwin's footnote, quoted below, is quite surprising. However, it also reveals the influence of the scriptist bias in the orthodox notion of syntax. Commenting on the T-G distinction between utterances and sentences, Goodwin argues

such a distinction may be useful analytically. For example, in the following the word 'put' occurs twice in the stream of speech but only once in a unit on another level of organization necessary for properly understanding that stream of speech:

Clacia: He pu:t uhm, (0.7) Tch! put crabmeat on th' bo::dum.

The processes being examined in this paper change, in Lyon's (1969) terminology, both the utterance and the underlying sentence abstractable from it.³⁶

We have noted that Goodwin sees the production of speech as a dynamic process with changes in structure and meaning occurring mid-course and with the use of discontinuity and intonation as tools with which to alter the utterance during the course of its production. He does not appear, then to see speech as the delivery of a competence sentence, marred only by production errors.

However, he does appear to take the interpretation of speech as based on the decoding of the competence sentences supposedly underlying that speech. Speech, he seems to say, is full of what, from a competence grammar point of view, would be termed errors, re-starts, phrasal breaks, etc.; and this is due to the interactive dynamic way in which it is constructed. Nevertheless, in order to understand speech characterized by these performance features, the hearer must somehow ignore, delete, and re-structure parts of the speaker's utterance. This will enable him to derive, from the chaos of interactively constructed utterances,

error-free sentences of the type described in orthodox syntactic studies. Thus, in the competence sentence derived from the above example, the word "put" only occurs once, the other one in the utterance having somehow been deleted by the hearer in the early stages of his interpretation processes. Furthermore, it is, says Goodwin, because the hearer can derive a sentence from an utterance that the hearer is able to understand it properly. By implication, utterances from which no competence sentences are derivable are therefore not understandable. In this sense, then, Goodwin's adopts the target-sentence hypothesis.

But what, one wants to ask, is the competence sentence derivable from the speech act involving John, Don, Beth and Ann? Is it merely the following sentence?

(43) I gave up smoking cigarettes one week ago today actually. Such a conclusion would appear to miss completely the dynamic development of the meaning as the utterance was produced, and, by settling on what might be deemed to be its meaning for Ann, at the end of the utterance, ignores the meaning it might have had for Don and Ann (and even Beth) at different points in the utterance.

Similarly, if - in the example he gives in his footnote - "put" only occurs once (in the underlying sentence by which the utterance is interpreted), then it must also be true that "I gave" and "one" only occur once in the derived competence sentence. But, Goodwin says that the repetition of "I gave" functions as a request for the hearer to bring his gaze to the speaker. However, if the sequence "I gave" only occurs once on the level of organisation necessary for properly understanding" (Goodwin, FN3), then, as far as the hearer is concerned in his attempt to understand what is being said, no repeat and so no gaze-request has occurred. And yet the fact that Don does at this moment

start to bring his gaze to John is at least minimal evidence that such a request has in fact occurred. The same could be said of the phrasal breaks "1 - uh:" and "one - one", which Goodwin sees as (unsuccessful) requests for Beth's gaze.

In fact, Goodwin's claim, quoted above, that the speaker can "construct events within the production of a single phonetic unit in order to accomplish social tasks" seems to be contradicted by his later claim that hearers understand utterances by means of the abstraction of sentences from them. For, in such abstractions, the actions performed by the speaker in order to accomplish social tasks, will be deleted. Since competence sentences are held not to contain restarts, filled pauses, silent pauses, self-corrections, etc., they cannot therefore be held to use such phenomena for the purpose of accomplishing interactional tasks. If Goodwin wants to hold that utterances can perform a variety of interactional tasks during their production, then he cannot at the same time hold that utterances are only understood by means of an abstraction to an 'underlying' sentence-form empty of the very phenomena which, he holds, accomplish these tasks.

Furthermore, we have already noted a number of conversational passages from which it seems impossible to derive underlying competence sentences. In other examples, although it is possible to imagine plausible underlying sentences, it seems that no set of rules which could account for the derivation of sentences from those examples could apply generally to speech without deleting features of syntactic, and interactional importance to the speech concerned. If producing an utterance is, as Goodwin argues, a dynamic, alterable, and negotiable interactional task, there is every reason to suppose that understanding an utterance has the same characteristics. To neglect these characteristics

is to ignore a crucial dimension of verbal activity: the dimension of time.

6. Summary of the critique of editing rules

The work of B. Voss shows that knowledge of the function of filled pauses, repeats, and other forms of ligation is part of the speaker/hearer's knowledge of his language. Although based on a very different sort of evidence and relying on different assumptions, the same conclusion may be drawn from Goodwin's work. In addition, we may conclude from Nooteboom's study of ligation in German that the features of ligation — the speaker's contribution to the cohesion of an utterance across a discontinuity — appear to have a certain regularity. It may, however, be that this regularity is best explained in statistical terms rather than in reference to a hypothetical rule system governing ligation. Furthermore, it does not seem plausible to take the 'syntax' of ligation to be constructed for the purpose of signalling to hearers how to edit discontinuous performance utterances into continuous, grammatical competence sentences. This conclusion is based on a consideration of the principles which would be required for the formulation of practical editing rules.

The hypothesis that hearer's edit discontinuous utterances into target sentences fails primarily because of the wide variety of ways in which speakers construct their utterances and ligate within those utterances. Because of the principles of continuity, deletion, and explicitness, it does not seem possible to set up a system of editing rules which can avoid the production of unacceptable (for one reason or another) target sentences. These target sentences will be unacceptable — in the case of the deletion and continuity principles — because they fail to convey some part of the semantic or interactional content which,

intuitively, the original utterances themselves convey. Or, in the case of the explicitness principle, the resultant target sentences may be ungrammatical due to the inability of the edit to alter features of the pre-discontinuity which are not explicitly corrected, deleted, or replaced by something in the post-discontinuity. Because the argument for editing is based on a claim that, in order to understand an utterance, hearers need to abstract from that utterance an underlying grammatical sentence, the inability of editing rules to retain in a target sentence all that the original utterance itself conveys therefore amounts to significant failure.

At this point, the proponent of the target sentence hypothesis (and the editing rules which it implies) may object that all that has been shown is that the principles of continuity, deletion, and explicitness are at fault. There is no reason, he might argue, not to retain the notions of target sentences and editing without, at the same time, retaining the aforementioned regulative principles. Why could we not assume that hearers do edit utterances into sentence form on the basis of rather more complex editing rules than those which the principles of continuity, deletion, and explicitness will allow?

There is a major stumbling block to such an argument. It is obliged either to rely on the notion that hearers can understand as-yet-unedited utterances or to accept that there is no way for hearers to determine what the correct edit of a particular utterance is. Consider again the following examples.

(36) How long do you suppose a life of a fur has?

(42) Really unless there's something wrong with the candidate from their college . why she shouldn't get it . can you - this makes sense to me sort of loyalty to their own (S.1.3, 266).

We might imagine that the target sentence to be derived from (36) is (36d).

(36d) How long a life do you suppose an animal's hair has?

There are, of course, other target sentence possibilities. Some of these are suggested by Brown in examples (36a) to (36c) above. But in any case, the hearer would already have to have understood 'what the speaker was getting at' before he could decide how to edit (36) appropriately. If he did not have such a prior understanding and if the restrictive principles of minimalness, continuity, deletion, and explicitness did not apply, there is no end to the number and nature of the permutations which could be derived via editing. A consideration of (42) makes this point even more forcefully since it is not clear what competence sentence, or competence sentences, would be(an) appropriate target(s), let alone how the hearer would know how to edit to that (those) sentence(s).

In other words, there is no consist^ent editing strategy which could enable different hearers to derive the same target sentences from the typical sort of utterances produced by speakers. Without the constraints of the minimalness, continuity, deletion, and explicitness principles, editing would amount to an ad hoc procedure. The hearer would have to choose an appropriate editing strategy according to the characteristics of each new utterance. This would mean, first of all, that different hearers might edit the same utterance in different ways. The possibility that they might arrive at different interpretations would then arise. There would be no way of knowing what the correct edit was at any one time. Secondly, if we accept that editing is different for each utterance produced, then we must face the problem of explaining how the hearer could decide which strategy is appropriate for a given utterance. The hearer would have to have formed some preliminary decoding of the utterance in order to decide what sort of editing

strategy would be appropriate. But, if the hearer has already succeeded in decoding the utterance, what need is there of editing? The question then arises, how did he arrive at the preliminary, pre-editing interpretation?

Consequently, the notion of editing rules requires either (a) that the hearer have advance knowledge of the speaker's formal or semantic intention in speaking (in which case editing would be superfluous) or (b) that hearers could never know if they had arrived at the speaker's intended target sentence or only at one of many possible targets derivable from the speaker's utterance. In the latter case, editing would prove to be an obstacle rather than an aid to successful communication. By making editing rules more complex than the suggested minimal deletion rule, the dilemmas encountered by the application of those rules would themselves become more complex.

In addition, we have the evidence presented by K. Brown, as well as that provided by any transcription of informal conversational speech. It is shown that hearers are able to interpret - or at least appear to be able to interpret - utterances which are 'grammatically incoherent' in much the same way as are unedited, discontinuous utterances. If hearers can understand such utterances, the need for editing and for the complex mechanisms which editing rules would require, disappears.

7. The parallel case of ellipsis

In Chapter Six of The Language Myth Roy Harris argues that linguistics is in need of radical re-orientation. He blames what he sees as the failure of the modern linguistic orthodoxy on its exclusive concern with the abstract language-system(s) purported to underlie language activity. The result of this concern is that the creative renewal which Harris takes to be the foundation of the individual, contextualized

speech act is ignored. And yet, he argues, it is 'the total speech act in the total speech situation' which linguistics should focus on. Orthodox linguistic theory assigns individual- and situation-specific creativity to the realm of the 'non-linguistic'. In so doing, it establishes the fundamental distinction between the language-system and the individual speech act (competence and performance, langue and parole, etc.).

There is no question of simply 'adding on' some appendix to the apparatus of modern linguistics, in order to deal with the phenomena which are central to the renewal of language. For the paradigm of modern linguistics makes it impossible to acknowledge them as language phenomena at all. The sterility of modern linguistic orthodoxy is precisely that it relegates the essential features and conditions of language to the realm of the non-linguistic. It treats the renewal of language as a mere accident of the communicational process.³⁷

Harris argues that, instead, linguistics should focus its attention on "the individual (speech) act in its communicational setting".³⁸ Heretofore the orthodox linguist has always contrived to put a 'safe distance' between theories of the language system and the observable facts of language-use which stand as flagrant refutations of those theories. Harris claims that the linguist falls back on two standard doctrines in order to avoid "the need to take the evidence of speech acts seriously". These doctrines

can cover all cases of apparent discrepancy between linguistic behaviour and postulated language rules. One is the doctrine of ellipsis, which allows speakers not to mean what they actually say, but to mean something else which they could have said if only they had taken the trouble to express themselves in a fuller or more explicit form. The other doctrine is one which distinguishes between linguistic knowledge on the one hand and contextual knowledge, pragmatic knowledge, knowledge of the world, knowledge of history, knowledge of cricket, etc., on the other.³⁹

Speakers often produce turns-at-talk which could not qualify as orthodox English sentences. They say things like (a) "next week", (b) "why?", (c) "the red", and (d) "certainly". The doctrine of ellipsis holds that such utterances leave much that is unsaid but not, however, unimplied. That is, the examples listed are assumed to be elliptical versions of something like the following (a1) I am leaving next week; (b1) Why do you ask me when I am leaving?; (c1) I want you to give me the red zonker, not the blue zonker; (d1) Certainly I will accompany you in your car down to the railroad station to meet your husband who is arriving on the next express train from Ho-Chi-Minh City. The doctrine of ellipsis supports the view that it is sentences like (a1) - (d1) which the speakers of (a) - (d) have in mind but decline to produce. Instead they utter the more stylistically acceptable elliptical versions. The latter are supposedly derived, by means of a fixed set of ellipsis rules, from the underlying, sentential versions. In parallel fashion, the hearers of (a)-(d) are thought to work back through the ellipsis transformations to derive (a1)-(d1) from the actual utterances. Grammars, the orthodoxy holds, only generate complete sentences. However, speakers do not always produce complete sentences. Consequently, since it is thought to be by their shared knowledge of a grammar that speaker and hearer are able to communicate, an account of the communicational success of utterances as (a)-(d) must take them to be derivative versions of sentences generated by a shared grammar. Furthermore, their method of derivation - e.g. by rules of ellipsis - must also be presumed to be shared. This is necessary in order to discount the possibility that different hearers might derive different sentences - and so, different interpretations - from the same elliptical utterance.

In addition, without the doctrine of ellipsis, the task of devising a grammar of English would become infinitely more complex than it already is. That is, if a proposed grammar of English was obliged to generate outputs such as (a)-(d)—each of which, given the appropriate conversational context, is an acceptably complete utterance—the grammarian might abandon any hope of reducing the rules of that grammar to a well-defined system. Indeed, it seems that virtually any single word of English could be a complete utterance in the appropriate context. Consequently, in order to keep the task of constructing and using a grammar within the realm of the conceivable, the orthodox linguist exiles from his grammar all possible utterances which do not approximate fully explicit, written language form. The occurrence of forms such as (a)-(d) must then be explained by recourse to extra-grammatical rules of ellipsis linking the outputs of grammars with the outputs of speakers.

In the context of this chapter the doctrine of ellipsis strikes a familiar chord. In fact, the doctrine of ellipsis needs to be complemented by a doctrine of editing. That is, while the linguist makes use of the notion of ellipsis to avoid the awkward problems presented by utterances which need elements added to them — so that they may be seen as derivations from grammatical sentences — the linguist also needs the notion of editing in order that he may deal with those utterances which, for the same reason, need elements subtracted from them. Indeed, we may group the notions of ellipsis and editing under a more general notion. This doctrine, which may be baptized the 'E-doctrine' (eediting and ellipsis), holds the strategic role of explaining why it is that the utterances which we standardly encounter in ordinary conversation do not take the form of the sentences which are standardly generated by orthodox grammars. The performance utterances,

so the doctrine holds, either are missing words which may be taken as implied, or they incorporate a surfeit of unintended deletable words.

The problem raised in this chapter is that there is no set of editing rules which can derive the appropriate, grammatical sentences from every type of, nevertheless, interpretable utterance. The interpretability of the uneditable utterances thus stands in need of explanation. Or, if it is held that no explanation is needed, then the same conclusion should be applicable to those utterances for which editing rules can be made to fit.

Harris employs a similar argument regarding ellipsis. If the hearer of a supposedly elliptical utterance is thought, for the purpose of interpreting it, to derive its underlying sentential form, then he will need rules to guide him from elliptical utterance to complete sentence. But there will always be an infinite set of sentences to which an elliptical utterance may be linked.

Utterances like 'Mary', or 'It's mine' may apparently represent, on different occasions, indefinitely many sentences of English. But then in order to determine which sentence of language L is actually represented by (any arbitrary utterance) X in any given case, it becomes necessary for the rational speaker-hearer to have available some workable decision procedure for determining the relationship between X and the inventory of sentences of L. This amounts to saying that he already needs to know what the utterance means in order to find out which sentence it represents. But then finding out which sentence it represents is either impossible or superfluous, or both.⁴⁰

On this argument, then, it appears that the orthodox position cannot account for the type of things people say and understand in ordinary conversation. Even with the twofold 'E-doctrine' the abstract language-system which is the focus of the linguist's attention cannot be brought into determinate contact with actually occurring speech acts.

In addition, if the meaning of purportedly discontinuous or

elliptical utterances is thought only to be discoverable by means of first deriving the competence sentences underlying them, then much of the communicational potential of utterance form is ignored. To say that example (a) above and (a1) mean the same (as well as (b) and (b1), (c) and (c1), etc.) because they share the same underlying grammatical form is to miss the possible difference in the communicational relevance of the members of each pair. The same point may be drawn from Goodwin's paper, viz. that what may be seen as a deletable discontinuity in an utterance may nevertheless have an important communicative function. A 'false start', for example, may serve to draw a potential listener's attention. The E-doctrine cannot help but neglect the potential significance of those features of behaviour which are not explicable in terms of particular aspects of the hypothetical underlying system of sentences.

In one sense, the purpose of the E-doctrine is to bring the linguistic model into contact with the realities of spoken performance. Without such a doctrine, it would at best be unclear how speakers and hearers could make use of that model for the purposes of ordinary daily discourse. The linguistic model describes language competence in terms of ideally complete and continuous sentences. The native speaker/hearer whose linguistic equipment consisted solely of such a model would find himself unable to engage in conversation with other native speakers. Quite simply, he would be unable to decode their utterances. Expressed in other terms, his 'theory of language' could not cope with the empirical facts of language behaviour. Editing and ellipsis rules, then, form crucial struts in the linguist's explanatory bridge between theory and data.

This is why the refutation of the E-doctrine holds such important

consequences for linguistic theory. If the bridge connecting theory and data collapses, the model proposed by linguistic theory becomes isolated and, consequently, loses explanatory power. And yet, it is difficult to see what is left of the E-doctrine when it is shown that no editing or ellipsis rules can be devised which, without advance knowledge of the speaker's meaning, can unambiguously derive appropriate underlying sentences from the type of utterances regularly produced in ordinary conversation. This leaves the native speaker/hearer conspicuously unable to use the abstract knowledge of sentence-grammar which he is claimed to have internalized.

On reflection, it may be seen that the target sentence hypothesis - the notion that our understanding of an utterance is based on our awareness of the competence sentence abstractable from it - has its source in a confusion of communicability with grammaticality. In order to understand an utterance, it is thought, the hearer must see it as a token of some sentence which may be generated by the rules of the grammar. An ungrammatical sequence - i.e. a sequence which could not be generated by the grammar - must either be editable to grammatical form or be uninterpretable. If we assume that understanding is based on knowledge of a language, its grammar, and the sentences generated by that grammar, then, so the story goes, communicability equals grammaticality. This chapter has been concerned to expose the dilemmas that arise when these assumptions are brought to bear on the data of actual utterances of conversational speech.

* * *

8. Conclusion

If on the other hand, we choose to avoid confusion, to separate communicability from grammaticality, what are we left with? Most certainly, a different view of syntax will be required. The question 'how must a language-user structure his output (spoken or written) in order to make it grammatical?' must be distinguished from the crucially different question 'how must he structure his output in order to make it communicative?' Both questions, it may be seen, demand contextually determined answers. How one answers the first depends on whether the output is spoken or written, telegraphed or shouted, used in an advertisement or in an instruction manual. The answer to the first also depends - as well we all know from letters to the newspaper and arguments with our friends - on who has asked and who is answering the question. And, finally, it is important to realize that in many situations - when the overriding concern is not grammar but communication - the first question is simply irrelevant, or, irrelevant until someone decides to make it explicitly relevant.

The same context-bound character must be attributed to answers to the second question. Whether an utterance is or is not a communicational success depends on who uttered it and on who they were speaking to. It also depends on what the interactants' criteria are for success at that particular moment and place. If a speaker inaudibly mumbles something and obtains the glass of wine he desired, this may be counted, in some situations, by some people, as a communicational success. Others may call it luck. But the point is that the question of the communicational efficacy of an utterance is not decidable once-and-for-all by fixed criteria. The criteria change with interaction, and the same criteria can be applied in different ways. In this case, the answer to the

question 'how must the language-user structure his output in order to be communicative?' is not formulable in terms of abstract rules which generate all and only the set of communicative utterances. For what is communicative at one moment, for one person, may not be at a later moment, for a different person, and vice versa.

At the very least, the attempt to answer the latter question may be informed by a consideration of discontinuity in speech. The evidence that discontinuity provides teaches us that the question of how a communicative utterance must be structured is not answers in terms of a specification of what the final output must look like. An examination of the utterances people actually produce leads to the conclusion that the final output can look like almost anything, i.e. that no fixed limits may be drawn as to what utterances must be like in order to be communicative. Instead, the sort of answer which can be of interest is one which is concerned not with the output of utterances but with the processes by which a contextually adaptable output may be produced. That is, the question of syntactic structure and communicability becomes more fruitful if transformed to the following: 'what resources can the speaker draw on to make his utterance cohere?'

A syntactic investigation along the lines of this question would not be concerned with the specification of the characteristics of possible output strings. Rather, it would focus on how the speaker can make disparate elements, uttered in sequence and across time, signal their dependence on each other. This would seem to be a primary function of (what we might call) 'syntax' in speech: viz. to give a sequentially uttered string of words more than its sequential arrangement. The function of syntax, under this view, is to establish non-sequential, structural relations between sequentially ordered elements.

This is certainly not the place to begin the exposition of a new theory of syntax. In any case, there are a variety of ways that the requirements on such a syntax might be filled. Brown's suggestion of syntactic processes underlying the construction of an utterance is one possibility. Another is the structural syntax devised by the school of linguists led by Jean Gagnepain.⁴¹ Others are conceivable; what is important for our present purposes is the understanding that such a syntax would not view speech as the production of pre-ordered strings of elements: i.e. sentences. The problem with analysing behaviour in terms of the production of fixed strings of elements arises when one considers the huge number of strings produced which, under such a view, are incorrectly formed. This is the problem that a sentence-based syntax encounters with discontinuity and 'grammatical incoherence'. The postulation of editing rules, in this uncharitable light, may thus be seen as an attempt to patch up sentence-based syntax just at the point where it connects with reality: that is, at the point where the well-formed strings generated by the grammar are compared with the utterances produced and understood by real people in communicative interaction. But no amount of patchwork can bring together two such vastly incommensurate domains.

According to the view of syntax proposed here, it is how syntactic relations are signalled which we 'know' as speaker/hearers of the language. We do not know an infinite set of sentences to which any interpretable utterance must conform. Conceived in this way, the notion of syntax accords better with the evidence on the interactional construction and dynamic function of utterances as well as with the 'non-grammatical' (i.e. non-written-style) utterances which are, nevertheless, perfectly understandable. Also, to bring the argument

full circle, it gives us a clearer idea of what it is that non-natives do not know about (e.g.) English speech. Foreign learners of English learn primarily from written texts or from tape-recorded utterances which conform to the canons of written style. When they finally come face to face with English as spoken by the native speakers in ordinary conversational settings they find as Voss showed, that they have yet to learn all there is to know about the syntax of English. They find themselves in the analogous position of someone who has learned to 'drive' in a laboratory-simulated car and, when first out on the road, finds the reality of driving distinctly different from the ideal conditions in the laboratory. The style of English performance which the learner learns prohibits many of the important syntactic features which native English speakers encounter every day. It is no wonder that, on his first visit to an English speaking community, the non-native finds his knowledge lacking.

NOTES

Chapter Five

1. 'LUND' refers to A Corpus of Conversational English, edited by Svartvik and Quirk, 1979.
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3. Hockett, A Course in Modern Linguistics, 1958, pp.143-4.
4. All references in this form refer to specific tone units in the LUND corpus, cited in footnote 1.
5. Lyons, Introduction to Theoretical Linguistics, 1969, p.176.
6. Laver, "The production of speech", in J. Lyons (ed.) New Horizons in Linguistics, 1970, p.73.
7. Voss, "Hesitation phenomena as sources of perceptual errors for non-native speakers", Language and Speech, 22, 1979, p.130. The Fry article is printed in Lyons, 1970.
8. Voss, "Hesitation phenomena as sources of perceptual errors for non-native speakers", Language and Speech, 22, 1979, p.130.
9. Voss, "Hesitation phenomena as sources of perceptual errors for non-native speakers", Language and Speech, 22, 1979, p.131.
10. Voss, "Hesitation phenomena as sources of perceptual errors for non-native speakers", Language and Speech, 22, 1979, p.132.
11. Voss, "Hesitation phenomena as sources of perceptual errors for non-native speakers", Language and Speech, 22, 1979, p.136.
12. Goldman-Eisler, Psycholinguistics, 1968, p.13.
13. Nooteboom, "Speaking and unspeaking: detection and correction of phonological and lexical errors in spontaneous speech", in V. Fromkin (ed.) Errors in Linguistic Performance, 1980.
14. Nooteboom, "Speaking and unspeaking: detection and correction of phonological and lexical errors in spontaneous speech", in V. Fromkin (ed.) Errors in Linguistic Performance, 1980, p.94.
15. Nooteboom, "Speaking and unspeaking: detection and correction of phonological and lexical errors in spontaneous speech", in V. Fromkin (ed.) Errors in Linguistic Performance, 1980, p.95.
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17. Brown, "Grammatical Incoherence", in H. Deckert and M. Raupach (eds.) Temporal Variables in Speech, 1980.

18. Brown, "Grammatical Incoherence", in H. Deckert and M. Raupach (eds.) Temporal Variables in Speech, 1980, p.28, quoting from Boomer and Laver, "Slips of the Tongue", British Journal of Disorders of Communication, 3, 1968.
19. Brown, "Grammatical Incoherence", in H. Deckert and M. Raupach (eds.) Temporal Variables in Speech, 1980, p.27.
20. Brown, "Grammatical Incoherence", in H. Deckert and M. Raupach (eds.) Temporal Variables in Speech, 1980, p.28.
21. Brown, "Grammatical Incoherence", in H. Deckert and M. Raupach (eds.) Temporal Variables in Speech, 1980, p.30.
22. Crystal, "Neglected grammatical factors in conversational English", in S. Greenbaum, G. Leech and J. Svartvik (eds.) Studies in English Linguistics, 1980.
23. Crystal, "Neglected grammatical factors in conversational English", in S. Greenbaum, G. Leech and J. Svartvik (eds.) Studies in English Linguistics, 1980, p.166.
24. Crystal, "Neglected grammatical factors in conversational English", in S. Greenbaum, G. Leech and J. Svartvik (eds.) Studies in English Linguistics, 1980, p.158.
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33. Goodwin, "The interactive construction of a sentence in natural conversation", in Everyday Language, Psathas, G. (ed.) 1978, p.108.
34. Sacks, "On some puns: with some intimations", in Report of the 23rd Annual Round Table Meeting of Linguistics and Language Studies, R. Shuy (ed.) 1973, p.139.

35. Goodwin, "The interactive construction of a sentence in natural conversation", in Everyday Language, Psathas, G. (ed.) 1978, p.111.
36. Goodwin, "The interactive construction of a sentence in natural conversation", in Everyday Language, Psathas, G. (ed.) 1978, p.98, fn.3. The Lyons reference is to Lyons, Introduction to Theoretical Linguistics, 1969, p.176.
37. Harris, The Language Myth, 1981, p. 166.
38. Harris, The Language Myth, 1981, p. 167.
39. Harris, The Language Myth, 1981, p. 152.
40. Harris, The Language Myth, 1981, pp. 203-4.
41. Cf. Gagnepain, Le Vouloir Dire, forthcoming.

CHAPTER SIX

The Concept of Discontinuity

Jourdain: Et comme l'on parle, qu'est-ce que c'est donc
que cela?

Maître de Philosophie: De la prose.

Jourdain: Quoi, quand je dis, Nicole, apportez-moi mes
pantoufles, et me donnez mon bonnet de nuit;
c'est de la prose?

Maître de Philosophie: Oui, Monsieur.

Jourdain: Par ma foi, il y a plus de quarante ans que
je dis de la prose, sans que j'en susse rien;
et je vous suis le plus obligé du monde de
m'avoir appris cela.

Molière, Le Bourgeois Gentilhomme, Acte II, Scene IV.

What is the source of the concepts of discontinuity and continuity?

Why are discontinuities identified as errors of speech performance?

The preceding chapters have shown that certain speech phenomena - e.g. silent and filled pauses, repeated units, etc. - are commonly thought to be errors in the production of speech. They disrupt the continuity of a speaker's utterance. In turn, special tactics on the part of both speaker and hearer are thought required in order that the discontinuities do not provide obstacles to the success of the communication process. Repair mechanism and editing rules are instances of such strategies. They are concerned with 'patching up' the damage presumed done by errors of discontinuity. But, to return to the question left suspended in Chapter One, why is it that discontinuities are taken to be performance errors? The answer lies, as will now be argued, in a pervasive written language bias underlying our ordinary, as well as technical, conceptions of language.

1. Discontinuity in the rhetorical tradition

There is, in fact, a long history to the practice of taking discontinuities as errors in speech performance of a special type. The type of speech performance in question is what is usually called 'public speaking': i.e. lectures, sermons, formal talks, toasts, and the like. Many rhetoricians have proscribed discontinuities from the properly delivered speech.

The hurried man will stammer, the nervous man will flounder in his talk. The unready will "hum" and "haw" until he becomes a bore to his audience. So there are many things to avoid and some to gain.¹

The above is taken from a Victorian handbook entitled Speeches and Toasts: How to Make and Propose Them published by Ward, Lock, and Company.

In this manual the reader is given examples of speeches appropriate

for a variety of occasions: patriotic toasts, temperance meetings, christenings, regatta dinners, etc. It is urged that the would-be public speaker should focus on certain essential qualities: "clearness of utterance, deliberation of manner, and repose of mien".

Most of the types of discontinuity we have discussed were seen as blemishes to speech delivery.

It is interesting that perfect continuity of speech was likened to reading out a written text aloud. Indeed, some rhetoricians (although not all) instructed public speakers - whether novices or veterans - to write down and memorize the written text of a speech before its actual performance. For instance, Lord Brougham, a famous orator of the early 19th century (whose speeches were published in four volumes in 1838) recommend the memorization of a written text as the essential key to attaining the 'art of easy speaking'. Of course, discontinuity, a common feature of ordinary conversation as well as of the untrained speaker's delivery, would be expunged from speeches consisting of a practised delivery of a written text. As shown in the experiments of Goldman-Eisler,² even the most common of the types of discontinuity, i.e. the silent pause, tends to disappear as verbal performances are repeated. To attain perfect continuous oratory, Lord Brougham argues, a speech must first be put into written form and then memorized word-for-word. There could be no better recipe for excising discontinuities from verbal delivery.

It is laborious, no doubt, and it is more difficult beyond comparison than speaking off hand; but it is necessary to perfect oratory, and at any rate it is necessary to acquire the habit of correct diction. But I go farther, and say even to the end of a man's life he must prepare word for word most of his finer passages.³

In The Lay Preacher's Handbook⁴ C.O. Eldridge recommends to the beginning preacher that the filled pause be avoided in sermons. Indeed, he appears to prefer a short silent pause rather than the occurrence of other types of disruptive discontinuities.

Open your mouth and let the words come out; but let them be real words, and not inarticulate mumblings. If you cannot think of a word, don't "hum" and "ha", and calmly pause a moment, and then, if you cannot find the right word, use the best you can.⁵

It is not surprising that Lord Brougham, like other rhetoricians, urged the public speaker first to express his thoughts in written prose and then to deliver his speech as a memorized production of that written text. Indeed, much of the history of rhetoric is imbued with the notion that the ideal form of language is writing. The more one's speaking style approximates that of a tasteful writing style, the better. This did not apply only to formal speaking situations. When speaking differed from writing, no matter what the circumstance, communication suffered. For instance, speaking is envisaged in the Ward, Lock, and Co. manual as 'pronouncing the letters'. In the section entitled 'Common Errors in Speaking', the authors warn speakers to pronounce words 'as they are written'.

. . . did any one ever see an "h-less" speaker who was sensible of his error? We have heard a fluent speaker, and one quite equal to writing his speech correctly, say "'ee 'eld up 'is 'at," "'ee put 'is 'and to 'is 'ead," quite unconscious that he was infringing any grammatical rule; and yet had he written the sentences quoted he would have put them correctly, with aspirates in the proper places.(...) The letter "H" is one of the most sinned against in England, particularly in London.⁶

The sin of the Londoner is to have formed speech habits which deviate from those represented in the ideal form: i.e. written texts.

Indeed, this written language bias to the study of speech (alternatively 'scriptist bias' or 'scriptism' following Harris, 1980 and 1981)

is not a modern development. As Robins points out in A Short History of Linguistics scriptism formed the basis of early Greek phonetics.

Foreign words were written as best they could be with the Greek letters, but no scholarly concern was evinced over alien sounds or alien sound systems. The descriptive framework for Greek phonetics was the Greek alphabet, and statements took the form of accounts of the pronunciation of the letters in it.(...) More seriously, an improper analogy was accepted between the relation of discrete letters to a text and that of allegedly discrete sounds to a spoken utterance. This fallacy was not challenged, and it appears explicitly in Priscian at the end of the classical period, writing on Latin: 'Just as atoms come together and produce every corporeal thing, so likewise do speech sounds compose articulate speech as if it were some bodily entity'.⁷

It is at least doubtful that Priscian's atomic analogy would occur to anyone not acquainted with an alphabet.

It is possible, then, to take the rhetorician's belief in the ideal of written language as stemming from a traditional and still widely accepted view of the relations between speech and writing. The Greek phonetician simply took speech to be sequentially constructed on the model of written language. The rhetorician, on the other hand, advised that speakers perform in a manner resembling, as much as possible, written language style. This advice is a natural development from the belief that speaking is 'pronouncing the letters'. If this claim about speaking is assumed true, then to speak well is to speak as if reading out loud.

The use of expressions of quality and modality in describing the rhetorician's notion of the relations between speech and writing reminds us that the rhetorician's brief was not to describe but to pre-(and pro-) scribe. The rhetorician was an acknowledged authority on language use. From this position of experience and authority he gave advice to the inexperienced on how they should use language. If his proclamations were at all descriptive, they were only so in the sense that he

believed himself to be describing his own practise. That is, Lord Brougham, Eldridge, and the authors of the Ward, Lock, and Co. manual were not in the business of investigating the nature of speaking. Rather, their intentions were to give advice to speakers on how to improve their performance. Advice is not the equivalent of description. The tennis professional might recommend, for instance: "If you want to hit a good topspin lob, try to peel the cover off the ball with your racquet without actually coming into contact with the ball". While this may be good advice, in that it encourages the player to form a useful mental image of the ideal lob, it most certainly does not constitute a satisfactory description of what the best players do when lobbing. In recent years at least, no player at Wimbledon has succeeded in peeling the cover off of a tennis ball.

The point being made is that the rhetorician had a purpose in assimilating speech to writing. His aim was not to discover the truth about speech but to instruct speakers on how to develop a persuasive and pleasing delivery. This must not be ignored when considering the rhetorician's views on discontinuity. The rhetorician was concerned to proscribe discontinuities in public speaking. He made no claims about the nature or causes of discontinuity. This point will be recalled in our consideration of the scriptist basis of the modern language theorist's notions of discontinuity and the 'ideal delivery'.

2. The ideal delivery

In their psycholinguistics textbook, Psychology and Language, Herbert and Eve Clark include a series of chapters on the speech production process. Fundamental to their outline of speech production is the notion of the 'ideal delivery'. They make a distinction between the (mental)

activity of planning what one is going to say and the activity of executing that plan in speaking.

In speaking, people take already formulated plans and execute them. But not all goes well in everyday speech. In practice they have fundamental problems. First, they have not always formulated their plans fully before they begin their execution. For this reason they often speak in fits and starts and make a variety of speech errors.⁸

Among these 'speech errors', Clark and Clark include "hesitations, corrections, uhs, and many other indications that speakers are interrupting the execution to do further planning".

For there to be a speech "error" there must be a "correct" way of executing a sentence, and this will be called the ideal delivery. When people know what they want to say and say it fluently, they are making an ideal delivery. Actors saying their lines, except when making deliberate errors, come close to the ideal delivery, and so do practised readers and orators. For theories of speech production the ideal delivery is of central importance. They all assume that people strive for the ideal delivery, and every deviation points to something that has gone wrong in planning or execution.⁹

The notion of 'speech errors' then is contrasted with its opposite, the error-free or 'ideal' delivery.

What might first be noted here is that this opposition is needed in order that such discontinuities as hesitations and filled pauses may be called speech 'errors'. For we do not normally say of an ordinary conversationalist that he has made an error when he pauses in speaking or when he slips an 'um' into his flow of speech. The concept of error might be more usually applied to a speaker who corrects himself in the course of his utterance, as in the following example.

I wanted Jonathan to . . . Michael to fix my bike for me.

It might commonly be said of the speaker of the above utterance that he had 'made an error' in saying Jonathan. However, in order that such

phenomena be grouped with hesitations and filled pauses, which are not ordinarily termed 'errors', they must be seen to share some characteristic. In the case of Clark and Clark, this characteristic is taken to be a shared disruption in speech delivery so that that delivery may no longer be deemed 'ideal'. Both hesitations and phenomena such as the utterance of Jonathan may thus be grouped together as 'errors' in relation to the ideal delivery in which neither of them would occur.

As an example of typical spontaneous speech, Clark and Clark cite a genuine example referred to in a paper by Maclay and Osgood.¹⁰

As far as I know, no one yet has done the / in a way obvious now and interesting problem of [pause] doing a / in a sense a structural frequency study of the alternative [pause] syntactical [uh] / in a given language, say, like English, the alternative [uh] possible structures, and how / what their hierarchical [pause] probability of occurrence structure is.

The speech, of which the above is a fairly liberal transcription, is said by Clark and Clark to be replete with deviations from the ideal delivery. Such deviations here include the marked silent pauses, the 'filled' pauses (i.e. [uh]), and the various 'false starts'. The latter (marked in the text by a /) occur, according to the authors, when "the speaker has backed up and begun again".

Although Clark and Clark do not say what an ideal delivery of this passage might have been, we may, for present purposes, assume that at the very least there would have been no pauses or false starts. In this case, we may imagine a transcription looking something like the following:

As far as I know no one has yet done the in a way obvious now and interesting problem of in a sense a structural frequency study of, in a given language, say, like English, the alternative possible structures, and what their hierarchical probability of occurrence structure is.

The assumption is that it is such a pause- and false start- free passage that the speaker was attempting to produce. For, according to the authors, ordinary speakers constantly strive for an ideal delivery. However, somewhere within the planning process or in the execution of the plan things went wrong. As a result the speaker strayed from the ideal delivery.

It must first be pointed out that there is no immediate evidence for this assumption. There is nothing in the unedited transcript which indicates that the speaker was indeed trying to produce an ideal delivery. We merely have a version of what he did in fact produce and no evidence that he was trying to produce something else like the edited passage.

In other words, the claim that speakers strive for an ideal delivery is not made on observational or experimental grounds. Although speakers may, in fact, sometimes produce what Clark and Clark might be willing to call an ideal delivery, there is no evidence to the effect that such a delivery is what speakers are trying for even at times when what they produce is far from such an ideal.

On the other hand, nor is the authors' claim made on prescriptive grounds. Whereas the rhetoricians discussed above hold that speakers (at least in certain contexts) should strive for an ideal delivery, Clark and Clark claim that ordinary speakers do strive for an ideal delivery. When the rhetoricians give their reasons why public speakers should try to avoid 'hemming' and 'hawing', they are giving advice. But when Clark and Clark, in the following quote, say why speakers should try for an ideal delivery, they take themselves to be explaining, not advising. Their aim is not prescription but explanation and description. The following passage then is to be seen as a reply to a question with the sense of: 'why should it be the case, given that it

is the case, that speakers try for an ideal delivery? If Clark and Clark had written a handbook on public speaking rather than a textbook on psycholinguistics, this passage would acquire a different force.

Why should speakers try for an ideal delivery?
 One reason is to make themselves better understood.
 Speech with breaks that are not at constituent
 boundaries is difficult to understand. In the
 ideal delivery, all breaks will be at grammatical
 junctures between sentences or major clauses, where
 they will help, not hinder. Another less obvious
 reason is that people who speak fluently are very
 likely judged cleverer, abler, and more effective
 than people without the same fluency. Moreover,
 it isn't considered polite to speak unless one has
 something definite to say, and every hesitation,
uh, and false start adds to the impression that
 one does not have something definite to say. So
 it is only natural, in most circumstances, to strive
 for the ideal delivery.¹¹

Speakers are thought try for an ideal delivery because the latter is supposedly more effective in communication. It facilitates the communication of meaning. It also leads to a better impression of the speaker being formed in the hearer's mind. In other words, Clark and Clark claim that the ideal delivery is an aid to the fulfillment of the semantic and social functions of speech. On the assumption that, in the production of speech acts, speakers do try to fulfill these functions, then, according to Clark and Clark, it is reasonable to assume that they should try to produce an ideal delivery.

However, this conclusion depends on the claim that deviations from the ideal delivery do in fact constitute an obstacle to effective communication. In order to prove such a claim it would have to be shown that deviations from the ideal delivery do indeed obstruct understanding and lead to poor impressions being formed of speakers. It would also have to be shown in addition that speakers are aware of these consequences and, on these grounds, strive to produce the ideal

delivery. Furthermore, the burden of proof is on the investigators who, like Clark and Clark, claim that the ideal delivery is a real goal of speakers. For one merely has to observe people in ordinary conversation to discover that deviations from the ideal delivery are the norm. Even approximations to ideally fluent deliveries are rare exceptions. On what grounds, then, should we suppose that, in speaking, communicators actively strive to do something that they only very infrequently accomplish? Ordinary discontinuous conversational speech seems to work perfectly well for the communicational tasks of everyday conversational interaction. Discontinuous speech appears to raise no obstacle to the success of communication. So the proponent of the theory of the ideal delivery is asking us to believe not only that speakers constantly try to do what they almost never do but also that, if they fail to succeed, it makes no manifest difference to the success of the speech act. Perhaps deviations from the ideal do make some sort of hidden difference in the minds of the hearers, but, given the observational evidence of successful speech acts incorporating discontinuities, the burden for proof for such a claim is on the claimant. Clark and Clark offer no such proof.

3. The influence of scriptism

It is indicative of the influence writing has on our conceptualization of speech in particular and on language in general that an example of the ideal delivery is said by Clark and Clark¹² to be the speech produced by a practised reader reading aloud. It is often an aid to the investigator who has chosen the task of explaining some feature of speech to do so by drawing an analogy with writing. The influence of this analogy is understandable, granted the assistance it provides in the task of explanation. Written language has a temporal permanence which speech lacks.

It may easily be reproduced and transmitted to hundreds of recipients at hundreds of distant locations. It is more open to detailed observational scrutiny than is speech. When we have doubts about our knowledge of language we turn to the two traditional authorities, both of which are written texts: the dictionary and the grammar book. Even linguistics, through the practice of which our more technical metalinguistic concepts are formed, is 'done' in written form. It is not surprising that the influence of written categories can be seen throughout the study of spoken language. The Greeks' notion of 'pronouncing the letters' is one result of the influence of scriptism. Another is 'the Londoner's sin', discussed above, of dropping his 'H' when speaking.

Consequently, it is understandable that, in finding ways to talk about speech, we are led to borrow the concepts that are easily applied in talking about writing. It is no coincidence, on this account, that speech is conceived of as being the utterance of a sequence of 'phonemes', much like writing is the production of a string of letters. Similarly, we see speech as consisting of strings of separate units, viz. words, since modern writing is easily divisible - due to the grapheme [space] - in that way. It requires no effort, then, to push the analogy further to conceive of speech as consisting of spoken sentences or utterances, just as writing is broken into sentences.

Scriptism, i.e. the influence of writing on the conceptualization of speech, is pervasive in language-study. Clark and Clark, however, have extended its domain by considering (from a descriptive rather than a prescriptive point of view) ideal speech to be that which approximates the practised reading of a written text. Indeed, the articulatory execution of a planned utterance is held to be the reading off of a mental text. Scriptism tends to push the difference between spoken

language and written language farther and farther out to the periphery of the communicative act. The essential features of speech and writing are assumed to be the same.

However, the fact that speech is regularly conceptualized in terms of the concepts of written language does not mean that the difference between the two should necessarily be errors or deviations on the part of speech, and yet it is essentially because they are thought to be errors that Clark and Clark believe discontinuities to be avoided by speakers. However, that which is different about speech does not have to be thought of as somehow incorrect. There is no intonation, for instance, in written language, but it is not usually thought, on that account, to be an incorrect feature of speech.

We may then reasonably ask what Clark and Clark's grounds are for claiming that certain features of spoken language, including discontinuity, constitute deviations from an ideal. Why is continuity regarded as ideal in reading aloud? What is at issue in this question is how the notion of the 'correctness' of written language, as compared to speech, is made to apply.

There is, of course, no a priori reason why, of speech and writing, the latter should be thought the more correct. It is possible to argue that speech and writing are simply two differing, but equivalent, styles or registers of language-use. This is the position defended by Lyons in Language and Linguistics. Lyons holds that

A distinction must be drawn between language-signals and the medium in which the signals are realized.¹³

The language-system is held to be independent of the media (i.e. speaking, writing, hand signals, etc.) in which the 'language-signals' (i.e. utterances, paragraphs, sequences of hand movements, etc.) of that language

are realized. Consequently, instances of language in two different media, e.g. speech and writing, may be thought to differ only stylistically rather than essentially. It does not have to be that one is taken to be the correct ideal of which the other is only a faulty imitation. On this account, there would be no apparent reason for the ordinary conversationalist to try to speak in a style imitative of reading out loud.

Having seen that the contrary assumption, expounded by Lyons, appears, at least, to be plausible, we may return to the question of the grounds for Clark and Clark's belief that ideal speech delivery is that which approximates the practised reading aloud of a written text. It is tempting to turn to the rhetorical tradition for elucidation of this question. However, no answer to our question will be found there. In the rhetorical tradition it is also simply taken as an unsupported assumption that the ideal form of language-use is writing. A plausible reason why this assumption was accepted might be seen to lie in the social value attached to literacy. He who spoke as if reading from a written text would thereby demonstrate a high degree of literacy - a prestige accomplishment, generally associated with being intelligent, well-educated, and therefore of a reasonably elevated class. If on only those grounds alone his speech would acquire a desired air of authority. That is, continuous speech would be regarded as the counterpart of the kind of fluency in reading which demonstrates literacy.

Even so, these speculations will not lead us to the reasons behind Clark and Clark's scriptist assumption. For, as was discussed above, the rhetoricians were concerned to advise would be public speakers. They were not describing what everyday conversationalists actually do. It is a privilege allowed the teacher to draw on a variety of well-

and not so well-founded arguments if the end result is that the student acquires the taught skill. Thus the rhetorician was not obliged to show cause for his scriptist assumptions. If rhetorical advice based on those assumptions led to students speaking in the desired manner, then the theoretical basis for those assumptions is irrelevant. This is not, however, the case with the explanatory psycholinguistic theories of Clark and Clark. The nature of their enterprise demands that their assumptions regarding the ideal form of speech production be well-grounded. If it were discovered that Clark and Clark's scriptist assumptions were, in fact, based on a wholesale adoption of the advice of 19th century rhetoric, this would lend no support to their case. We would still have to look deeper for the source of their belief in the correctness of the continuity instanced by reading out loud.

Indeed, it is noteworthy that much the opposite assumption was expressed by Saussure in his discussions of the primacy of speech. Although the question of continuity was not in fact at issue, Saussure argued that speech was the ideal form of language and that writing consisted of a (frequently infelicitous) attempt to imitate speech.

In his Cours de linguistique générale, Saussure took the primacy of speech to be a fundamental principle of linguistics and criticized earlier grammarians such as Bopp and Grimm for thinking that "a language is inseparable from its alphabet".¹⁴ For Saussure, the study of speech was the only rational basis for linguistics.

Langue et écriture sont deux systèmes de signes distincts; l'unique raison d'être du second est de représenter le premier; l'objet linguistique n'est pas défini par la combinaison du mot écrit et du mot parlé; ce dernier constitue à lui seul cet objet.¹⁵

That is, the spoken word is taken to be the only legitimate object of linguistic analysis. Although, at the time, such a position was

revolutionary, today it represents a central pillar of orthodoxy. Linguistic textbooks constantly inform their readers that, in the study of language, speech takes priority over writing.

Given the Saussurian doctrine of the primacy of speech, Clark and Clark's assumptions regarding the 'correctness' of writing stands as a puzzling development of modern linguistic thought. In order to understand this development it is perhaps best to examine its common-sense foundations.

We may first remark that the concept of correctness is more normally applied to writing than to speech. When people write they have the time and the freedom to stop and alter what they have written. This is not possible, at least not in the same way, with speaking. Once said there is no 'un-saying' in the way that one may erase or scratch out a previous bit of writing. With this facility in writing comes the habitual process of altering one's written product until it is thought to satisfy the conventions of correct or grammatical prose. When we are taught to write we are taught not simply to transcribe what we might otherwise speak but to alter that transcription until it conforms to certain standards or rules. It is essential that this is not possible with speech. Once something is said, it cannot be unsaid and corrected in order to meet a standard of grammaticality.

Consequently, with writing it is possible to conform ideally to abstract standards. Sentences may be rewritten and drafts may be re-edited, even thrown away, until the writer is satisfied that all deviations from grammaticality are purged. In speaking, the dominant priority is to advance the interaction according to one's aims, but, in writing, there is a priority of comparable status to that of communicational efficacy: viz. to communicate according to the style laid down

by conventions of grammar and usage. Our teachers in school will correct our written output until we so conform. They will not, however, correct all our deviations in speech.

There is a fundamental principle underlying this distinction and one which, owing to scriptism, has often been ignored in linguistic studies. Speech takes place within the confines of the temporal dimension, while writing is able, to a certain extent, to overcome the limits of temporality. The teacher may say 'Don't say 'ain't'!', but her admonishment, no matter how strong, will be powerless to correct what the child has already produced. By the time that the teacher is able to utter her command to the child, the latter's utterance of "ain't" belongs to history. Nothing can change that. Of course it is true that the teacher's words may provoke the child to repeat his utterance with the shameful "ain't" replaced by an acceptable "isn't". But in a very important sense all the child has really done is to utter another sentence. Two utterances have now been produced by the child, not a single corrected one.

However, when the teacher points at the child's paper and says "Don't say 'ain't'!", the child may quickly erase the offending letters and actually replace them with "isn't". Or if he is writing in non-erasable ink, he may go so far as to throw his text away - under the approving eye of the teacher - and write the whole sentence over again with "isn't" instead of "ain't". Stretches of speech cannot be erased or thrown away as stretches of writing can be. Writing has the (albeit fundamentally illusory) ability to reach back into what has happened and change it.

Thus, written language may be changed until it conforms to some ideal of correctness imposed by the society, by our 'linguistic betters', or by our teachers. In any case, it is possible for the trained and

attentive writer to produce ideal sentence after ideal sentence, in a way that is much less feasible in spontaneous speech. It is not surprising, then, that the notion of the ideal language-signal (like that of grammaticality) is wedded to that of the perfectly correct written sentence. A correct written sentence is thought to be a transparently perfect instantiation of the abstract language-signal. In other words, the process of correcting writing is thought to be not as a means to satisfy certain supplementary conventions, applicable mainly to writing, but as a means to realize the language-signal without error. The grammatical written sentence is simply the ideal, abstract language-signal made real.

As a result of this scriptism an important distinction is lost: the distinction (discussed in the last chapter) between communicational efficacy and grammaticality. In taking the ideal language-signal to be an abstract version of the grammatical written sentence, the standard of communicational efficacy is blended with the standard of grammaticality. The important part of communication, the part with which linguistics concerns itself, is taken to be the methods or rules by which we verbalize grammatically. The difference between communicating effectively - which the use of "ain't" does - and communicating in accord with the standards of grammatical correctness - which "ain't" does not do - is thereby ignored. Speaking is thought simply to involve the manipulation of grammatical knowledge (in the sense of rules generating the grammatical sentences of a language); and deviations from grammaticality in speech are therefore thought to be errors. They are 'errors' then, because they do not conform to the restrictions which are imposed on our use of language when we are taught to write in school.

The result is that the notion of the abstract language-signal comes

to be calqued onto the notion of the ideal written form. The language-signal, which is abstract and not, in principle, realizable, is thus thought to be a mental version of the correct written sentence. "What is the point", argues the orthodox linguist, "of correcting writing except to get it to reproduce the ideal? That is what the writer aims for in correcting his writing". In this way, the concept of the abstract language-signal, underlying effective communication in a language, is perceived from the vantage point of writing and of our ability to correct, erase, and edit writing to conventional form.

If the language-signal is thought to be like the correct written sentence, then every difference between spoken utterances and written sentences which cannot be attributed to a constitutive difference in media (such as intonation) may thereby be seen as a deviation, on the part of speaking, from the ideal. Consequently, the (non-essential differences which speaking manifests in comparison to writing are conceptualized in terms of errors, as failures to produce the ideal language-signals. Furthermore, since this is what writers strive to do - viz. produce ideal forms - it is assumed that this is also what speakers typically try to do; but they fail. The differences between speech and writing are not therefore thought of in terms of stylistic differences. In both cases the language-producer is thought to be trying to do the same thing: i.e. produce instances of ideal language-signals; but in writing success is achieved almost invariably, in speaking only rarely.

4. 'Speech errors' and communication

It is reasonable to add a further dimension to this argument. From the perspective of modern linguistics, the key to effective communication is thought to lie in the instantiation of ideal language-signals.

Deviations from that ideal are therefore thought to raise obstacles to expression and understanding. On this argument, those who express themselves orally in a style closest to that of written style are the most effective communicators. Discontinuity and other similar features of spoken performance are not taken to be useful aspects of communication.

In The Language Myth Harris argues that many of the dilemmas of modern linguistics may be seen to be engendered by what he calls the telementational and fixed-code fallacies. Words, according to the telementational fallacy, "are symbols devised by man for transferring thoughts from one mind to another. Speech is a form of telementation".¹⁶ The fixed-code fallacy holds that speech is able to transfer thoughts from speaker to hearer because men

agree upon some fixed set of correlations between ideas and verbal symbols, in order to provide themselves with a viable system for exchanging thoughts. Languages are, precisely, systems of this kind. (...)

In brief, the model of linguistic communication offered is as follows. Individuals are able to exchange their thoughts by means of words because - and insofar as - they have come to understand and to adhere to a fixed public plan for doing so. The plan is based on recurrent instantiation of invariant items belonging to a set known to all members of the community. These items are the 'sentences' of the community's language.¹⁷

Thus, an utterance which deviates somehow in form from one of these ideal sentences is 'in error'. Furthermore, such an error is an obstacle to communication since communication is based upon the instantiation of ideal sentences. An utterance with performance errors such as discontinuities will not be in the hearer's fixed-codebook, i.e. it will not be part of his knowledge of his language. Consequently, he will not be able to decode the thought that prompted the speaker to utter it. Communication will break down over such a discontinuity. The study of linguistic competence, conceived of as the study of the

internalized knowledge which enables inter-personal communication to take place, will therefore disregard discontinuities as instances of performance errors.

In other words, it is not simply that discontinuities are conceptualized as errors of grammar - that is, as failures to instantiate the form of ideal language-signals. The orthodox model of linguistic communication, as described by Harris, promotes the view that discontinuities are also errors of communication. Because utterances incorporating discontinuities do not approximate ideal (i.e. written-language) sentence form, they are thought to provide an obstacle to the smooth transfer of thoughts between speaker and hearer. This transfer depends on a pre-established correlation between verbal signals (in this case, competence sentences) and thoughts. A discontinuous utterance does not unambiguously pick out the sentence which it is an instance of and so does not clearly indicate the thought it encodes. Discontinuities, then, interfere with what should ideally be the instantaneous transfer of thoughts. Thus, the need arises for the type of repair and editing rules studied in Chapters Four and Five.

Given this background - in common-sense as well as in theoretical assumptions - it is understandable that Clark and Clark should adopt a concept of discontinuity features which classifies those features as errors of speech performance. To accept discontinuities as bona fide features of speech delivery would be to deny the scriptist foundations on which are based our everyday notions of correct language-use and our theoretical notions of verbal communication. The concept of discontinuity is a derivative of these authoritative notions.

5. General Conclusion

The scriptist conception of speech has had a vast influence, not only in discontinuity studies. Still, the effects of scriptism are

most easily illustrated in the ways language researchers have conceptualized the nature and appropriate study of discontinuities in speech. A cursory inspection of the theories discussed in these chapters reveals that this written language bias has influenced investigations into the causes of, management of, and consequences of discontinuity.

For example, given the scriptist conception of speaking, the following question naturally arises: if successful communication, prima facie, may be taken to be every speaker's aim, why do they produce obstacles to communication, such as discontinuities? It should be noted that this question only arises because discontinuities are considered to be obstacles to communication. Similar questions are never tabled concerning the occurrence of, say, the ^{speech sound} /p/ or the fall-rise pitch movement, neither of which is considered to be a hindrance to communicational efficacy. Indeed, the occurrence of pauses at locations where a writer would put a comma or a full stop also does not attract the inquiring attentions of researchers. It seems to be because the occurrence of discontinuities compares so unfavourably with the scriptist-based notion continuity, embodied in Clark and Clark's 'ideal delivery', that the question arises concerning their causes.

In addition, it is significant that those who have taken an interest in answering that question are not linguists, but psychologists, sociologists, anthropologists, etc. The scriptist conception exiles discontinuities to the non- or extra-linguistic. Because they do not occur in writing they are not thought to be features determined by the speaker/hearer's abstract knowledge of his language. Their occurrence must then stem from a different source. Consequently, non-linguistic disciplines are called upon for assistance.

The psychologists, for instance, have attempted to answer the

question about the occurrence of discontinuities by referring to the extra-linguistic properties of mental processing, emotional states, and face-to-face interaction. This involves treating discontinuities as akin to features of 'noise' in information systems. The question then becomes: what is the source of the interference producing that 'noise'? Is it anxiety? Or planning? Other answers are possible. It is interesting that in many cases the psychologist's interest in discontinuity arises because of what are taken to be more central psychological concerns. Thus, their interest is not drawn by the question of the occurrence of discontinuities as such, but by the possibility that discontinuities are the result of more general human psychological factors: e.g. anxiety, planning, etc. In this sense, discontinuities are used as evidence for more general theories about the human mind. To continue the analogy, the 'noise' is taken to be nothing but the manifestation of certain processes of interference. It is primarily the latter which is of interest to the psychologist. Whatever the merits of this enterprise, it may be seen to exclude from the start a perspective taking discontinuities as bona fide features of speech. Such a perspective would be seen to be like taking radio static as a feature of the broadcaster's intended programme.

Again, the same written language bias may be discovered at the bottom of the psychologist's view of discontinuity. The literate person knows that it is possible to lose one's place in a text while reading out loud, or to have difficulty in deciphering the next word in his handwriting, or suddenly to have doubts about how a sentence continues over the page. These 'difficulties with the text' - which are not the result of linguistic features of the sentences making up that text - are typical sources of pauses when reading aloud. As the psychologists

Clark and Clark point out the closest version to the ideal delivery is the performance of the practised reader reading out loud. So it may simply be assumed that analogous difficulties with a^a hypothesized 'mental text' - i.e. the utterance plan - must be the main source of pauses in spontaneous speech. The scriptist bias likens speech to reading out loud and so draws attention to those features of speaking which should not occur in a properly performed reading of a text.

Scriptism does not only give rise to questions concerning the causes of discontinuity while at the same time, pre-determining the type of answers which those questions may receive. It also influences questions regarding the consequences of discontinuity, for speakers and for hearers. Here a typical question is the following: given that discontinuities do occur, how is communication between speaker and hearer still possible? Again, for a non-scriptist perspective on speech communication, within which discontinuities were treated as bona fide features of speech, such questions would not arise. There would no more be the need to ask how communication is possible in spite of discontinuities than there would be to ask why communication did not break down over the occurrence of vowels and consonants. For such questions to arise, discontinuities must be taken to be errors. This explains why, in the theories of repair and editing, mention is frequently made of 'error-correction',¹⁸ of 'self-righting mechanisms',¹⁹ and indeed of 'editing' itself, a notion which does not conceal its written language connotations. What the student of repair can be seen to find of interest are those parts of utterances which do not conform to the canons of written style. It is noteworthy that, for example, ethnomethodological studies of conversation, even those not concerned with repair, only focus on instances of speech which manifest noticeable differences with written style. In essence, the theories of repair and editing

accept that discontinuities are errors and so focus on the ways available to speakers and to hearers to fix ('repair') the possible damage to communication.

The notion of repair concerns how, from the ethnomethodological perspective, speakers may be seen^{to} repair the damage caused by discontinuities such as inappropriately uttered words, word searches, faulty articulation, and so on. Editing, on the other hand, refers to the hearer's ability to extract an underlying sentence from the apparent chaos of an utterance marred by discontinuities and subsequent efforts to repair. Editing and repair both presuppose that communication is based on the speaker's choice of a sentence which encodes his intended meaning. This choice is then transmitted to the hearer who, in turn, decodes the intended meaning from the sentence. The theories of repair and editing explain how it is that discontinuities do not block the transmission of the chosen sentence and its meaning from speaker to hearer.

It is expectable that, as with the explanations of the causes of discontinuity, the theories of repair and editing - dealing with the consequences of discontinuity - postulate the operation of extra-linguistic forces. The mechanism of repair, like the rules of editing, are proposed as features of the organization of conversational discourse. They are not taken to be sub-components of the language in which the conversation is spoken but rather of extra-linguistic systems which speaker/hearers supposedly draw upon in order to be able to use their knowledge of the language in conversation. So it is not surprising that those who have taken an interest in the study of the consequences of discontinuity, like those who study the causes of discontinuity, are not from the field of ('core') linguistics proper. Rather, they are sociolinguists, or ethnomethodologists, or anthropologists.

This brief survey reveals how pervasive is the influence of scriptism in the study of discontinuity. Without the notion of discontinuity as an error in speech - a notion brought about by the scriptist perspective - there would be no special reason to search for the causes of discontinuities. Nor would any question arise concerning the speaker's and hearer's management of the communicational obstacles created by discontinuities. Indeed, without scriptism, the notion of discontinuity might well prove to be incoherent. It is, after all, only by comparison with the notion of continuity that it is possible to identify a set of phenomena as instances of discontinuity. Discontinuity is taken to include precisely those features of a speaker's performance which would not occur in the continuous spoken performance of a practised reader reading out loud. The practical identification of discontinuity, like its theoretical definition, is dependent upon its opposition to a fundamentally written language notion of continuity. Should these assumptions underlying the notion of continuity be withdrawn, it is not at all clear on what basis the important dialectical opposition between continuity and discontinuity could be justified.

Does this then mean that, providing that the scriptist view of speech is abandoned, there is no sense in studying such things as pauses, repeats, hesitations, and the like? It does not. The result of the scriptist bias is that all such features of speech are treated as communicational errors. It groups together actual occurrences of such features as concrete instances of the same abstract formal type: viz. discontinuity. This, in turn predetermines the ways in which their causes, their functions, and their sequential arrangement in speech are studied. Consequently, little attention is paid to the analysis of individual instances of (e.g.) pauses, and the specific causes and

functions of them in particular interactional contexts. Scriptism leads, as the preceding chapters have shown, to the neglect of alternative case-by-case explanations of the role of 'discontinuity' features in favour of explanatory models which (a) take them as unintended errors and (b) assume that their sequential and interactional organization is designed to minimize their supposedly disruptive effect on communication. It is this strategy of neglect which is proven to be at fault when, on the contrary, the evidence discussed in these pages reveals (i) that 'discontinuities' can be intentional features of a speaker's performance, (ii) that they are only disruptive from the blinkered perspective of the scriptist conception of the communicative act, and (iii) that they may serve interactional aims in speech which, due to scriptism, have too often been ignored.

The abandonment of the scriptist bias would mean that the speech features which have here been called 'discontinuities' would have to be accorded the same theoretical, as well as ontological, status as that accorded to any other 'common-or-garden' feature of the speech act. There would be no justification in separating the study of 'discontinuities' from e.g. the study of intonation or syntax. Nor would there be any specific justification to group what have been called 'discontinuities' as instances of the same type. Any recognizable difference in (sound) substance - e.g. between two filled pauses - would have to be considered as of potential interactional significance. It is to be hoped that there would be room for such a radically different approach to the study of 'discontinuities' in a revitalized investigation of conversational behaviour.

NOTES

Chapter Six

1. Ward, Lock & Co., (no author listed), Speeches and Toasts: how to make and propose them, n.d., pp.9-10.
2. e.g. Goldman-Eisler, "A comparative study of two hesitation phenomena", Language and Speech, 4, 1961.
3. Lord Brougham, quoted in Ward, Lock & Co., Speeches and Toasts: how to make and propose them, n.d., pp.10-11.
4. Eldridge, The Lay Preacher's Handbook: first steps in homiletics, 1894.
5. Eldridge, The Lay Preacher's Handbook: first steps in homiletics, 1894.
6. Ward, Lock & Co. Speeches and Toasts: how to make and propose them, n.d., pp.12-13.
7. Robins, A Short History of Linguistics, 1967, p.23.
8. Clark and Clark, Psychology and Language, 1977, p.259.
9. Clark and Clark, Psychology and Language, 1977, p.261.
10. Maclay and Osgood, "Hesitation phenomena in spontaneous English speech", Word, 15, 1959, p.25.
11. Clark and Clark, Psychology and Language, 1977, pp.261-2.
12. Clark and Clark, Psychology and Language, 1977, p.261.
13. Lyons, Language and Linguistics, 1981, p.11.
14. Saussure, Cours de linguistique générale (ed.) T. de Mauro, 1978, p.46.
15. Saussure, Cours de linguistique générale (ed.) T. de Mauro, 1978, p.45.
16. Harris, The Language Myth, 1981, p. 9.
17. Harris, The Language Myth, 1981, p.10.
18. Jefferson, "Side sequences", in D. Sudnow (ed.) Studies in Social Interaction, 1972; "A case of precision timing in ordinary conversation", Semiotica, 9,1, 1973; and "Error correction as an interactional resource", Language in Society, III,2, 1974.
19. Schegloff, Jefferson and Sacks, "The preference for self-correction in the organization of repair in conversation", Language, 53(2), 1977.

APPENDIX

I.

The following appendix has an important if not an essential functional role in this thesis. Its main task is comparative. The main body of the appendix, section III, consists of a selection of passages from the Lund Corpus of English Conversation. They are intended to serve as comparative evidence for the examples discussed throughout the main body of the thesis. This comparison is effected by section II of the appendix in which evidence discussed in the chapters is related to a selection of the passages from the corpus. This comparative exercise serves to provide a broader empirical basis for the theoretical discussions in the chapters.

The Lund corpus is available in two forms. There is an 890 page book of transcripts from the corpus published by CWK Gleerup, Lund, Sweden. Fifty conversations are included, each consisting of approximately 5000 words. These transcripts are also coded onto magnetic computer tape (ASC II code). The computer tape is distributed by the International Computer Archive of Modern English at the University of Bergen, Norway.

It is from the computerized version of the corpus that the following passages have been selected. This selection was carried out by means of a concordance program specially written by the author for this appendix. The program reproduced below, is written within the parameters of the Oxford Concordance Program devised at the Oxford University Computing Service by S. Hockey and I. Marriott. Funding for the development of the program - both the general version and the variant developed for the compilation of this appendix - was provided by the U.K. Social Science Research Council. The general version of the program is explained in the Oxford Concordance Program: Users' Manual, published in 1980 by the Oxford University Computing Service, 13 Banbury Road, Oxford.

The passages selected below were chosen because of their inclusion of silent pauses. This decision was based on the following two points. First, silent pauses manifestly are by far the most common of discontinuities, and when other discontinuities occur it is usual for a silent pause to accompany them. Second, after repeated programming attempts, it was concluded that the concordance program – in its present version at least – could not reliably identify any other type of discontinuity, whether it be filled pause, false start, repeat, insertion, repair, or other. While it was always possible to find e.g. some false starts, any program which was able to do that also, at the same time, found an even greater number of passages which did not include what discontinuity theorists call 'false starts' while omitting other possible 'false starts'. This also applied to the other discontinuity types, including filled pauses. For instance, the nature of the transcriptions are such that filled pauses are not the only units transcribed phonetically between square brackets. It was not possible to write a program which could separate out the genuine filled pauses from the non-genuine. The problem presented by computer identification of discontinuity types surely raises interesting questions for those researchers who aim to develop computer models of language processing. Unfortunately, these are not questions for which there is space to discuss here.

The selected corpus of passages in section III is the result of applying the concordance program to three of the conversations in the Lund collection. These are identified in the Lund book as conversations S.1.1, S.1.4, and S.1.5. In this appendix, these are called File 1, File 4, and File 5. The first, File 1 or S.1.1, was recorded in 1964 and involves two male speakers, both academics, aged about 44 and 60. This conversation was recorded surreptitiously as were both the other

conversations included here. File 4 was recorded in 1969 and consists of a conversation between two male academics both about 48 years old. File 5, which was recorded in 1967, is the recording of a multi-party conversation involving three female secretaries, aged 21, 35, and 21 (speakers A, C, and D respectively) and a female academic, age 25.

It is possible that a larger corpus of passages, selected from a wider range of conversational situations and involving a greater variety of speakers would have added empirical weight to this appendix. But, how large a corpus would be required? And how wide should the range of situations and speakers be? Answers to these questions depend on the use to which the corpus is put. In the present instance, there is no intention to present a broad empirical survey and description of the distribution of discontinuity types in respect to speaker and situational variability. Such a study is, unfortunately, beyond the limits of a thesis which is focussed on the analysis of the theoretical problems raised by discontinuities in conversational speech. While no corpus would, in principle, be large enough for a complete empirical investigation of discontinuities (since there would always be speakers and situations not included), nevertheless the length and breadth of the corpus included here seems adequate for the comparative purpose for which it is intended. There are many instances in the corpus of most of the discontinuity types discussed in the thesis, including a good number of interesting contextual varieties. These are discussed in section II. To reiterate, the aim of this corpus is not to provide evidence to prove or disprove any hypotheses about the distributional characteristics of discontinuity. Rather, its purpose is to provide a comparative empirical background to the examples discussed in the main body of the text. While it was necessary in the text to use those examples which the theorists themselves based their analyses on, it is also instructive

to compare those examples with similar instances taken from a 'neutral' source.

The passages listed in the corpus have undergone a small amount of editing. It was not possible to get the concordance program to recognise sentence boundaries. So, in order to provide a more readable text, it was often necessary to delete parts of the beginning and end of the passages output by the program. In every case these were beginning or end sentence-fragments. In addition, since speakers would frequently discontinue more than once in an utterance, there was a good deal of duplication and overlap in the passages. There appeared no reason to include any given passage more than once. As a result, some passages appear longer than others. This is the effect produced by running two or more continuous selections into one in order to avoid unnecessary duplication. In each case, however, the relevant utterances are included without deletion or alteration with the exception of the shortening and splicing just described. The result may be compared with the Lund text to verify the inconsequential nature of the editing.

The prosodic coding used here is reproduced from the computer output. It is based on the prosodic system described in D. Crystal's Prosodic Systems and Intonation in English (1969). This system has become a fairly popular descriptive framework for prosodic features in spontaneous conversation and is indeed the framework chosen by Randolph Quirk's Survey of English Usage. Nevertheless, it may not be immediately apparent how the code used by the computer output, reproduced here, is to be matched with the various descriptive features of the prosodic system. In this case, the following explanation should clarify the meaning of the important prosodic symbols. Further explanation should be sought in Crystal 1969.

	yes	= simultaneous talk
	(laughs)	= contextual comment
	((yes))	= incomprehensible words or questionable transcription
tone unit	{ f	= end of tone unit
	{ ^yes	= onset
	{ {yes}	= subordinate tone unit
nucleus	{ y\es	= pitch fall on nucleus
	{ y/es	= rise
	{ y=es	= level
	{ y\ /es	= (rise) fall - rise
	{ y/ \es	= (fall) rise - fall
	{ y\es y/es	= fall - plus - rise
	{ y/es y\es	= rise - plus - fall
booster	{ -yes	= continuance
	{ :yes	= higher than preceding syllable
	{ !yes	= higher than preceding pitch - prominent syllable
	{ !!yes	= very high
stress	{ 'yes	= normal stress
	{ "yes	= heavy stress
pause	{ yes.yes	= brief pause (of one light syllable)
	{ yes - yes	= unit pause (of one stress unit or 'foot')
phonetics	{ @	= schwa
	{ ?	= glottal stop

The concordance program used to compile the corpus is as follows:

*INPUT

REFERENCES 9 to 12 = T, 18 to 23 = S.

TEXT 28 to 78.

*WORDS

ALPHABET "A=a B=b C=c D=d E=e F=f G=g H=h I=i

J=j K=k L=l M=m N=n O=o P=p Q=q R=r S=s T=t

U=u V=v W=w X=x Y=y Z=z.-".

PUNCTUATION "".

PADDING "*+^_` () [] / \ :: : " ! - :=

{ } ` ' @ 1 2 3 4 5 6 7 8 9 0 ; ? " .

*ACTION

DO CONCORDANCE AND STATISTICS

PICK WORDS ". - " .

REFERENCES T = 4, S = 2.

MAXIMUM CONTEXT 150 LETTERS WHOLE WORDS.

*FORMAT

LAYOUT LINES 2 BELOW CONTEXTS

CONTEXT SIZE 3 AND LEFT ALIGNED AND INDENT 3.

*GO

II.

- A Repeats occur throughout the Lund corpus, either at the beginning of utterances or at utterance-medial positions.
- For instances of utterance-initial repeats, see, e.g. extracts: F1, 143; F1, 176; F1, 177; F1, 238; F4, 663; F4, 769; F1, 681.
- For utterance-medial repeats see, e.g. extracts: F1, 124; F1, 177 (near end); F4, 252 (but does the change from 'thee' to 'thuh' bar this from being a repeat? See also, in this regard, F4, 316); F4, 357; F4, 759; F5, 164; F5, 217.
- B False starts of all three types (retraced, non-retraced, and type c) abound in the corpus.
- Non-retraced false starts may be found, e.g. in extracts F1, 265; F1, 463; F4, 317 ("I found ... say, you've got the ..."); F4, 557; F4, 586; F5, 125.
- Retraced false starts occur, e.g. in extracts F1, 11 ("make sure that ..."); F1, 41; F1, 315; F4, 103; F4, 243; F4, 600; F5, 574.
- Type c false starts (i.e. repeat of the utterance from its beginning until the replacement of an earlier error-word) occur, e.g. in extracts F1, 681; F1, 741 ("so it'll ... so anybody who"); F4, 81; F5, 221; F5, 380 ("You may get your ... you may not quite"); F5, 418; F5, 491.
- "(The distinction between retraced and non-retraced FS) is made on the basis of whether or not the speaker backed up in an attempt to correct one of the words he had already used." (Maclay and Osgood, 1959, p.309)
- C There are a variety of filled pauses in the corpus. They mostly take the form of /@/ or /@m/, although /m/ and /æ/ do occur. There are no instances of Maclay and Osgood's /E/ and /r/.
- Some instances of /@/: F1, 11; F1, 41; F1, 50; F1, 177; F1, 229; F1, 265; F1, 411; F1, 750; F4, 279; F5, 574; F5, 854.
- Some instances of /@m/: F1, 41; F1, 78; F1, 83; F1, 287; F1, 656; F1, 1037; F5, 100; F5, 164; F5, 342.
- Instances of /æ/: F1, 468; F1, 611; F4, 928.
- Instances of /m/: F4, 691; F4, 889.
- D Some instances of possibly ambiguous filled pauses occur in the following extracts: F1, 686 (where "/@/noun" could be seen to correct "the following"); F1, 733 (where "/@/" could be taken as an article preceding "preference"); F1, 958 (two instances: "/@h/precis" and "/@/mix-up").
- E Each extract in the corpus includes an unfilled pause. Whether or not they are taken to be 'abnormal hesitations' is not decidable solely by reference to the transcripts. The transcription does not, in general, mark 'non-phonemic lengthening of phonemes', so it is not possible to say if these are common in the transcribed conversations. It does appear however that one such 'drawl' is indicated in File 4, extract 944; "where I [ai]".

F For some of the many examples of repeats and false starts which it would at best, seem odd to classify in terms of the words they follow, see e.g. extracts F1, 124 (ought ought ...) (also "of of their arrival"); F1, 177 ("now [i] if if ..."); F1, 296 ("to to teach"); F1, 570 ("see him at the at the viva"); F1, 733 ("the the similarities"); F1, 741 ("FS from "so it'll" to "so anybody who") (also repeat of "a a niche") (also FS correcting "a niche" to "a ready made niche". If this False Start were classified according to the word which preceded it that word would be the pronoun "it" which appears irrelevant to the speaker's reason for discontinuing).

G There is no way to determine whether a particular filled pause does or does not have a floor-holding function. However, by inspecting the sequential location of a filled pause, it is possible to say that some have a greater relative probability of functioning as floor-holders than others.

Filled pauses with a more obvious floor-holding function: F1, 124; F1, 142 (here-the utterance is possibly complete, so the FP holds the floor); F1, 167; F4, 279 (after "I took out bits" the utterance is possibly complete); F5, 491; F5, 854.

Filled pauses which, due to the sequential context, probably do not have a floor-holding function: F1, 78 ("saying that [@m] the thing is now ...". Here the floor is already retained by the syntactic incompleteness of the speaker's utterance. This also applies to the following extracts): F1, 172; F1, 468; F4, 33; F4, 252; F5, 177; F5, 563; F5, 746.

H For instances of types A, B, C and D, see the following extracts:

A - F1, 1026;

B - F1, 229; F1, 325; F4, 103; F4, 612; F5, 678;

C - F1, 111; F1, 167; F1, 315; F4, 293;

D - F4, 66; F4, 1138; F5, 574; F4, 647 ("he [s@] he said");
F4, 971; F4, 1180.

It is noticeable that type A, in which the speaker recycles to the beginning of a constituent in order to correct a later item in the constituent, occurs rarely in the corpus. Maclay and Osgood say that it occurs three times more often than type B (where the speaker does not recycle to the beginning of the constituent). However, type B occurs frequently in the corpus.

I Listed below are some extracts including sequences for which it is indeterminate whether they would be called self-repairs. In some cases, there are instances of juxtaposed items, the second of which might be taken as a repair on the first. The ethnomethodologists do not provide criteria on which to base a decision. Other extracts include repetition of sequential elements - even what might be called stuttering. (In some of the repeats, there is a change of intonation in the second item of the pair.) There are no guidelines for determining whether these are or are not self-repairs. Finally, there are extracts where a given utterance is not completed. The question then arises whether the following utterance should be taken as a repair of the first.

For questionable repairs see, e.g. extracts:

- F1, 124 ("I suspected always that X, that Y ...". Is Y a repair on X?).
- F1, 143 ("I've got a. I've got about a week").
- F1, 176 (The speaker's first utterance is not fully completed. It is possible, however, to take it as complete by implication, especially if the speaker is thumbing through the pages of a diary. Does the following utterance constitute a repair on the first, incomplete one?).
- F1, 177 (In the sequence "four five six seven about the eighth" is each number a repair on the preceding one?)
- F1, 345 (Here, the repeat of "in" is marked by an additional pitch-fall. Does this mean that the second "in" is a repair on the first? Or that the pitch movement on the second is a repair on the pitch movement of the first? Or neither?)
- F1, 455 (Is "packet" a repair on "some"?)
- F1, 553 (Does the infinitive clause following "you know" repair the earlier infinitive clause?)
- F1, 711 (Does the sequential placing of the grammatical examples "it's like five pounds; it's worth five pounds; it's for five pounds" mean that the second is a repair on the first and the third on the second? This shows that the question of whether something is a self-repair may not be answered in formal terms. Rather, one has to know in advance what the speaker means in speaking to know if he has self-repaired. But, if understanding a speaker does not require knowing if he has self-repaired, then why is it important at all to the hearer to recognize when a speaker repairs?)
- F1, 758 (Does the topic shift after "if you" function as a repair on what the speaker has already said?)
- F1, 837 (Is "Virginia Woolf" a repair on "George Eliot"?)
- F1, 1059 (Since only B's second utterance is recognizably complete, is it a repair on the first?)

Other similar problems for the identification of repairs may be found in Files 4 and 5.

- J Given a generous interpretation of Schegloff's notion of backward- and forward-oriented repair-initiators, it appears from the Lund corpus that filled pauses may function as either. In the following extracts, there occur filled pauses which appear to be backward- rather than forward-oriented. In this case, they would have to come under the application of Schegloff's proposed notion of "repair-conversion".

Backward-oriented filled pauses:

- F1, 41 (/@/ initiates a replacement of the preceding "I" by "some of our people")
- F1, 315 ("I've only got five years to /@@/ eight years to go". It is difficult to see how this could be construed as a repair-conversion.)
- F1, 750 ("this is one of the things that -/@/ one of the many things ...")
- F5, 342 ("when I first came Beryl Martin. /@m/ Mervyn was the ...")

There are many instances of filled pauses which would be more easily classified as forward-oriented.

- K It is possible to find, in the Lund corpus, further instances of utterances which are not necessarily ungrammatical, but which the grammatical analyst would want to edit before analysis.

For example, see extracts :F1, 111 (Does "until" replace "to", or could the latter be an instance of "too", thus requiring no editing?) F1, 176 (could this be construed as "let's have a look at Di?" To conclude that this utterance needs editing, one first needs to decide that /at dai/ is improperly formed. But it does not have to be taken so.) F1, 315 (This could be "I've only got five years too!", followed by two filled pauses, followed by "Eight years to go anyway!", a locution which could easily stand on its own.) F4, 184 ("Each tilted the other in. In favour of me coming was the fact that ..."): F4, 971 ("There's a tall young man. I was going to ask you. There's a poor little fellow who's ..." How is one to determine whether the speaker is correcting himself here or not?)

Further examples of indeterminacies of this sort may be found under appendix note I.

- L See appendix note D.

- M See extracts listed under appendix note K.

- N For comparable instances of Nooteboom's discontinuity locations see the following extracts:

Nooteboom's type A: F1, 41; F1, 325; F4, 81; F4, 103; F4, 243; F4, 252; F4, 600; F5, 678.
type B: F1, 111; F4, 66.
type C: F1, 229; F1, 315; F4, 293; F4, 922.
type D: F1, 1026.

In contrast to Nooteboom's German findings, the English speakers of Files 1, 4 and 5 tend more towards type A discontinuities. Nooteboom found no type D in his German text, but speaker A in File 1 produces one in the Lund corpus. The extracts studied, however, amount to too small a corpus on which to base any definite conclusions..

- O For comparable instances of Nooteboom's re-start locations, see the following corpus extracts. A and B refer to phonological errors, C and D to lexical errors.

type A: F1, 325; F4, 81; F4, 103; F4, 243; F4, 252; F4, 600.
type B: F1, 1048 (if /i/ following the filled pause is taken as a phonological error: perhaps a mispronounced "each"? This is doubtful.)
type C: F1, 41; F1, 111; F1, 315; F4, 293; F4, 1138; F5, 678.
type D: F1, 229; F1, 1026; F4, 66; F4, 264.

In agreement with Nooteboom's German findings, very few (if any) type B re-starts were found. The other types also give totals in agreement with Nooteboom's statistics.

- P See File 4, extract 84. Here /z/ is added to the word "picture" after a pause, thereby making it a plural form. This is not, however, quite the same as Nootboom's German example.
- It is possible to take the sequence in F4, 1214 ("he's [@] pokes fun ...") as restarting in the middle of the form "he's", to give "he pokes fun". This would then be similar to Nootboom's example. However, this analysis is questionable.
- Otherwise, Nootboom's claim that word-medial re-starts are extremely-rare in German would be in agreement with our English corpus.
- Q See extracts listed under appendix note D.
- R See, for example, the following extracts: F5, 164 (change of accent on "used to"); F5, 380 ("quite" vs. "^quite"); F4, 317 ("thee" switched to "thuh"); F1, 345 (repeated "in" very different); F1, 681 (the final "only" is altered. Indeed, the speaker draws attention to the change in pitch.)
- S See extract F1, 686 where the repeated "like" is not to be edited.
- T See the utterance-initial repeats listed under appendix note A.
- U For some examples of 'insertions', see extracts: F1, 741; F1, 750; F4, 264; F4, 859; F5, 177; F5, 1097.
- V Self-corrections occur throughout the corpus. This category is essentially equivalent to Maclay and Osgood's 'retraced false starts'. A selection of these are listed in appendix note B.
- W There are a number of extracts from the Lund corpus where, to derive what would be an appropriate target sentence, the minimalness principle must be overruled. (See, for example, the extracts listed under appendix note K.) A selection of these extracts are listed here:
- F1, 11: A minimal deletion would simply edit out "your" in B's utterance to give "what you do is to make sure that your own candidate is that. There's something that your own candidate ..."
 - F1, 741: The following is a minimal edit: "So anybody who is looking for a niche to fit a ready made niche in English grammar."
 - F1, 1048: The minimal deletion would here give "He was saying for example that. These questions three and four didn't ..."
 - F1, 1059: Here one would need only delete "which" from the end of speaker B's first utterance to render both of his utterances grammatically complete sentences. But knowledge of context would result in this minimal deletion being unacceptable.
 - F4, 647: A minimal deletion would give "He thinks it's scandalous that we don't. He said I'm amazed that dons should ...". This is not, however, the contextually appropriate edit, even though the two sentences are grammatical.

- X For further problems caused by the continuity and deletion principles, see e.g. the following extracts:
- F1, 41: The target would be "I wondered whether it tends to be a comparative graphology paper". This would leave no reference for the pronoun "it", since "the graphology paper" has been deleted.
 - F1, 177: In order to arrive at a grammatical target, the initial clause beginning with "if" through "the twenty-ninth and /@/" would have to be deleted. But it seems that the hearer does understand that he is to get the papers to B by the 29th of June. So, the information provided by the initial if-clause is not, in fact, deleted.
 - F1, 287: Here, the deletion of the first "whether X ..." clause by the subsequent "whether Y ..." clause would leave no pronominal reference for the pronoun "he" in "whether he wants a medieval ...".
 - F4, 434: It would be an infringement of the continuity and deletion principles to place "a copy of" where it would belong in an appropriate target sentence, viz. "That's a copy of Velasquez' Pope Innocent the Fourth". This extract is similar to the "slant chant" example on page 215.
 - F4, 647: The minimal deletion rule would edit this to "He said I'm amazed that dons should still be ... summer term". This target would thereby delete the relevant fact that the person spoken of thinks that "it is rather scandalous" that the dons, etc. Yet, intuitively, it does not seem that that information is deleted from a proper understanding of the utterance.
 - F4, 1082: The derived target sentence would here be "I had a seminar in which people hadn't read the play". This would, however, delete the relevant information that they hadn't read the play "because of sessionals".
- Y For extracts, in which the explicitness principle needs to be overruled, see the following:
- F1, 455: The appropriate target for the latter part of this extract would be "I've got a packet here of ten for half a crown". However, the speaker never utters the article "a" (or any other article) to precede "packet". In order to edit to an appropriate target sentence, the hearer would have to add a word not explicitly produced by the speaker.
 - F1, 921: In this extract, the speaker switches from indirect speech ("They talk about the ...") to direct speech ("By all means ...") without correcting the verb "they talk about" to the more appropriate "they say".
 - F5, 967: The speaker never explicitly produces the noun suggested by "can't set foot beyond a certain, you know?". This does not, however, make the utterance uninterpretable.
- Z There are many utterances in the corpus which are 'grammatically incoherent', in the sense Brown gives to this expression. Many of these are listed under appendix notes W, X and Y. It is worth pointing out, in addition, the following extracts:

- F1, 271 ("if this is also come ...")
- F1, 681: The phrase which comes before "but it seems to me" does not fit into the syntactic structure of this utterance if taken as a token of a grammatical sentence.
- F1, 855: Grammatically incoherent clause linkage.
- F1, 904: "Much more to where these people want to read" is grammatically incoherent.
- F1, 932: If not grammatically incoherent, this utterance is at least grammatically very awkward. The speaker appears to change from a "why not X" structure to a "let's Y" structure mid-way through the utterance.
- F4, 252: There is no preposition before "a month's time".
- F4, 296: Even editing rules cannot correct "I then simply extracted the satirical passages which don't in themselves good".
- F4, 647: This extract also raises problems for the deletion and continuity principles: see appendix note X.
- F4, 663
- F4, 691: The following component part of this extract is 'grammatically incoherent': "I was just groaning last night to say I'll have to do one".
- F4, 1214: of "His satires often is directed ..."
- F4, 1223: The sequence "pays anyway lip service" might well be labelled as 'grammatically incoherent'.
- F5, 646: "I mean I'm sack them" is ungrammatical.
- F5, 1132: There is no main 'then' clause to combine with the "if I take you round, etc." clause. The structure is:
I think + if X + because Y. This is perhaps a perfectly ordinary conversational locution, but that does not diminish the fact that it does not approximate a grammatical competence sentence.

III.

File 1

11.

- A. well . " m/\ay* I _askf ^what goes !\into that paper
n/owf be^cause I !have to adv=isef . ((a)) ^couple of people
who are !d\oing [dhi: @]
- B. well ^what you :d\of ^is to - - ^this is sort of be:tween
the :tw\o of _usf ^what you :d\of is to ^make sure that
your 'own . !c\andidate is . . ^that your . there's
^something that your :own candidate can :h\andlef - -

41.

- A. be^cause you see ((I)) - [@] ^some of our p\oplef (. clears
throat) ((who)) are d\oing LEsf - - [@:m] ^have to consider
which !{p\aper} . to "d\of . and I ^wondered . whether .
[dhi] . gra:phology p/aperf . is in ^fact . [@:m] .
^whether it !tends to be a com:p\arative gra_phology paper f

50.

- B. wellf ^you give* them the :l\ot {^you s=eef}*.* ^that's
the **p/ointf** ((and)) ^make sure that there's :s\omethingf
[@:] ^fairly :cl\osely rel/atedf
- A. *^[=m]f* **^[=m]f**
- B. ((to ^what they've st\udiedf

71.

- A. [@:m] - - De^laney's the Ca:n\adian . st/udent {re^m/emberf} f
^last y/earf
- B. ^[mh/m]f

74.

- A. [@ :] he ^should have had his . dissertation \ / inf ((at the))
 be ^ginning of M \ / ayf . ((but)) the ^damn thing ((hasn't))
 c / omef -

78.

- A. [@ :] I ^did get a !p \ ostcard fr / om himf - - ^saying that
 [@ : m] the !thing is now :r / eadyf . and that he will ^send
 it by the :end . of :J \ unef . ^that's what he !s \ aysf

83.

- A. now . !A he may not . send it . quite as soon as . :th / atf
 and ^=Bf it ^may take a hell of a long time to !c \ omef .
 ^if he !puts it into the :diplomatic b \ agf ^as !wh \ at's
 his _namef . Mickey ^C \ / ohn _didf . ^then ((it's)) not so
 b \ adf

91.

- B. I ^wouldn't want it before the :end of June : \ anyhow R / eynardf
 be ^cause I'm !going to Madr \ idf . on the ^t \ / enthf and ^coming
 back on the twenty-n / inthf

111.

- B. I'm h \ oppingf to ^get into Sp = ainf . from a ^bout the
 - !twenty- . - : \ eighth of / Augustf ((to)) un ^til about the
 !twentieth or :something of that kind of Sept \ emberf

113.

- B. ^[adh@w] a ^part from :th = atf . I'll be at ^h \ \ omef and
 al" ^though I'll be doing CS / C _stufff and ^that kind of
 th = ingf ^I can always 'put it on one *s / idef

124.

- A. [@:] you ^see the !\ /other _manf Ch\omleyf ^ought .
 ^ought . ^ought !\alsof to have . ^got his in on t/imef
 and I sus" ^p\ectedf ^\ /alwaysf that De^laney would be
 l/atef . that ^Chomley would be on t/imef and that ^this
 would . produce a nice " !st\aggingf of . of their
 ar^rival on your !d\eskf

141.

- A. I'm ^not going to have my summers . bugged up in *this
 kind of !w\ay

143.

- B. it'll be !p\erfectly all r/ight _Reynardf I've ^got a .
 I've ^got about a w\eekef . of ^fairly hard w=orkf [@]
 ^after the :fourth of Jul\yf . this ^CS\C _stufff ^you
 s/eeef

154.

- B. I'll be ^doing [dhi] Burgos a:w\ardf from ^[dhi:] !tw\ /entiethf
 to the ^twenty-!f\ifthf . but [@] ^really I've got about .
 :thr\ee w/eekef " ^less than th/atf of ^hardish :w=orkf
 ^spread over those two m\onthsf ^you *s/eeef*

167.

- A. ^w\ellf . [@:m . dhi dhi] the " ^{ \other thing } \is you
 s/eeef that . ^if . you !h\aven't _got _timef to ^mark ((a))
 paper by about . [dhi] . " ^/any p\aperf by a^bout the
 !middle of Jul\ /yf - - then it's ^not worth . !w\orrying
 a _bout itf . un^til . the :end of [dhi] summer . va!c\ationf

172.

- A. then it's ^not worth . !w\orrying a_bout itf . un til . the :end of [dhi] summer . va!c\ationf be^c=ausef . [[:m] . the ^last meeting of ((the)) Council Com:mission is about the middle of Jul/yf

176.

- B. ^let's have a look ((at [dai])) ^I'm !back on the twenty-n\inth {^R\eynardf}f

177.

- B. ^now [i] if if !these papers come . by the :twenty-ninth of J\unef and you ^send them through to m\ef . ^in L/oughtonf - ^then . be!tween ([[[:]]) the !twenty-n/inthf and [[:] ^let me s\eeef - we're ^having this meeting of :CSC assistant((s)) on the !fourth of Jul/yf which is a ^S\aturdayf - I'll ^have about !half a day's work to look at someodd scripts before th/enf . and ^then I :shan't get !\any _scripts {from the as^s\istantsf}f be^fore about let me s\eeef ^f=ourf ^f=ivef ^s=evenf a^bout the !\eighthf - ^so . I shall have . r\oughtlyf from the "twenty-ninth of J\unef . to the ^eighth of Jul/yf . on ^which I can . [[:] I can ^spend the !wh\ole of th/at _timef on ^those two p/apersf . if they ^happen to *c\omef

211.

- B. [[:] ^I shall g=etf [[:] ^scripts from :ten assistant ex:\aminersf which will ^mean . a !couple of days' w\orkf

220.

- ^I'll ^I'll get them through . qu\icklyf ^once I've had them

229.

- A. [@:m] our ex^ternal ex=aminerf . ^as well as our:s/elves
of _coursef . [@:] ^would be !free during t\erm-_timef -
be ^freed "by the time term's \endedf -
- B. [@:m] you're ^very kind old S/amf - -

238.

- A. [@:m] . ((now)) ^what was the \other thing {I ^wanted to
:=ask _youf}f . ^[i] ^is . ^is it 'this y\earf that [@:]
^N\ightingale _goesf

265.

- A. I [@] you ^come to m\/ention itf it re"^m\ember that you
_didf - [@] but I ^heard it . [@] mentioned by somebody
/\elsef - I ^think !W=attf - I'm ^not s/uref

271.

- A. ^if !this is :\also c/omef ^from [@] - N/ightingale . {or
^y/ouf}f . then it's ^not so b/adf

287.

- A. do you ^happen to know whether . [@:m] . :Sparrow has got
an :image . of the man he w/antsf ^whether he wants a
medieval !l/iterature _manf or - - ^wh\atf

296.

- B. I mean ^my i:dea would :be to - to teach !l\/anguagf and
^hire somebody to do the "!l\iterature {^sort of th\ingf}f

310.

- B. ^there's [@] . !L\oganf . and ^probably !T\illmanf ^you s/ee

315.

- B. ((^I m=eanf)) . one ^can't . w/\orry a_bout itf I've ^only
got :five years to [@ @] ^/\eight years to _go {^/\anywayf}f

325.

- B. ^Joe has [go] . g/ot it of _coursef

345.

- B. ^w\ellf I should ^think . that there'd be a :b\etter chancef
of ap^pointing the :reader in - :^in the _placef ^I
supp/osef

348.

- B. on the^o\ther hand I supp/osef . if ^Roy P/eel _went for
itf ^they couldn't do anything :else !!b\ut ap_ppointf

364.

- B. I ^mean a !man coming from well say from . :Batley to
:Br\ightonf ((well then)) there'd ^be a !r\eason probablyf
((for)) . ^wanting to make !th\at ch/angef *^w\ouldn't theref

372.

- A. ^look at . !Z\immerman for ex_amplef . from . ^T=urnwickf .
^to - Lord W/ardenf . to P^Pf

395.

- A. ^which I sup:p=osef - ^one could . repres/\entf - ^quite
!r\easonablyf - - [[:]] one could ^say was :one of the .
:main . positive ad!v\antagesf

411.

B. I'm ^on the top ((of the)) !r\angef and [[:] ^what - I've got !three or four years moreof . ex/amingf which ^makes *a*

A. *^[\m]f*

B. spot of m/oney _for mef

440.

B. if ^I catch the !one twenty-:\eight {from Vic^t\/oriaf}f ^that *gets me in at about . !half past tw\of*

448.

B. ((let me)) ^tell you a st\oryf - - - a ^girl went into a a :chemist's sh=opf and ^asked f=orf . ^contra!c\eptive _tabletsf - -

451.

B. ((so he said)) ^well I've got . all k/\indsf and . ^all pr/\icesf ^what do you !w\antf

455.

B. ((she said well)) ^what have you !!g\otf ((and he said)) I've ^got some :h/eref - ^packet here of . :t/enf for ^half a cr/ownf

457.

B. ((^oh well are)) . are they ^all !!r\ightf ((he said)) well ((he said) I supp/ose _sof ((I mean)) do you ^guaran"!t\ee themf

463.

- B. he said ^oh n/\of I ^don't guaran"!t\ee themf I mean -
 ((you)) ^can't do :th\atf - - ((she said)) ^w\ellf .
 ((I [d ai])) they're ^no good to :me if you :can't
 guaran!t\ee themf ((^not r/eallyf))

468.

- B. I sup^pose ((as)) you :say you've :got them all !pr\icesf
 I sup^pose if I [@] 'get more ex!p\ensive onesf ^they'll
 be . !!s\afterf he said ^w\ellf ^one would ex!pect th/atf
 he said I've ^got some :here [ae:] at ten for a !!p\oundf

493.

- B. ^((he said well you can)) do what you !l\ike a_boutitf but .
 if you ^use these according to di:r\ectionsf I can
 ^guaran!tee they're !!s\afef - - th\ank youf r\ightf
 I'll h\ave themf

508.

- B. ((he said))^oh _nof you ^don't take them in a glass of
 ":w\aterf - you should "wait until the man is just
 "r\eadyf and ^th=enf . you ^take out one of these
 :t/abletsf and . you ^place the t\abletf . [@] be^tw\eenf
 your kn/eesf and ^h\old it th/eref

546.

- B. I'm ^going to B=urgosf . ^W=ednesdayf . ^w\eekef . ^y\esf
 the ^t\enthf

553.

- B. [a:] ^this is to . :run their coordinating mach\ine(ry))f -
 ^you kn/owf ^to . establish the :st\andardsf and ^all th\at
 _kind of _thingf

558.

- A. have you !m\et our man Yoolet y/etf - *((the ^one who's
a !student for the .))* 'dipl\/omaf
- B. *[@:] ^n\of ((^n\of))*

570.

- A. ^now that* he's finished his examin:\/ationsf you ^kn/owf
^you'll be s\eeing himf at ^[dhi] at ^[dhi] . v/\ivaf .
^on W/\ednesdayf

577.

- A. because ^if . ^if he . d/\oesn't _workfin ^close .
collabor:ation with y=ouf and - - and ^try to get - your
ex!p\eriencef - - he's going to go badly . at !s\eaf

584.

- B. I ^think they're* :grooming him for - - :quite a are:sp\onsible
j/obf at . ma^dr/idf

606.

- A. this is an^other of your "!b\awdinesses I'll b/etf - [@h@:] - -
- B. it's ^not worth . the tr\oublef - - how do you \analysef -
w/\orthf

611.

- B. this is [@:] ((the)) :little . :fellow (([ae] in))the
department of sty:listics at . :B\urgosf . ((^[b\oadi]f)) .
and he ^wasn't :quite . _certain how to :t\acklef .^worth
the !tr\oublef in ^that sort of ((collo:c\ation))

631.

- B. ^one would say for example it's :not worth !r\readingf .
 ((which is similar to)) it is ^not
- A. *((^\\ahf ^y\\esf ^y\\esf ^y\\esf))*
- B. worth . [!dh\\i:] . {r\\readingf}f

646.

- A. well ^what I :mean /isf . that ""w\\/orth {is sui
 g\\enerisf}f -

656.

- A. you ^s/eef . [m] . you ^can't s=ayf . that . ^w/orthf .
 ^is . adject\\ivalf - ^r/ightf
- B. ^n/of -
- A. be^cause . you :can't . say !v\\ery . {in ^fr=ont ofitf}f .
 ^or . [m] - ((it'd ^mean one would have to ext=endf))
 and ^say it's very worthwh\\ilef ((^w/ouldn't itf))

672.

- A. [m - - m] ^we ^we h/ave _itf ^in ^in the !P\\urviewf as
 a as a ^closed ! system w/ordf I'm ^pretty s/uref - -
 [m:] ^y=epf . we ^h/avef

676.

- A. ^and . !we
- B. [m]^h\\mf
- A. would . :certainly pre:liminarily . :pattern it with
 prepo:s\\itionsf

681.

- A. the [^][ouw] the [^]only . the [^]only !\other . possib/ilityf .
 [[:m] [^]w\ellf . not the [^]\only other possib/ilityf . but
 it [^]seems to me that you've got :two . _two th=ingsf
 [[:m] . that you can . [^]d\o with th/isf

686.

- A. you can [^]either say that it'd :like l/ikef . and . [[: [^]some
 of the other . [[:] !ps\eudo-prepos/itionsf - - [[:m]
 ((that)) it's [^]not . {l/ike} . m/ef ((it's)) [^]not like his
 w/ife f . ((it's)) [^]not worth the tr/oublef *. [^]not* worth .
 f/ivepencef
- B. [^][mh\m]f*
- A. . [[: [^]where . the !prepositional phrase :worth 'plus /Nf .
[^]has always !N as a ":v\alue t/ermf - from . the [^]system of
 v/alue _termsf *-* [^]/\orf [^]you can s/ayf . that [^]w/orthf -
[^]is . to be :treated as
- B. *[m][^][h/m]f*
- A. !not with [dhi] following . [[: . n/ounf . [^]but ((has)) to
 be !treated as a concatenation with the verb to :b\ef as if
[^]some kind of verbal !gr\oupf *. in*
- B. *[m][^][h/m]f*
- A. [^]which c=asef . [^]you . [[:] . :cluster it with . the :other
 con!c\atenativesf . [^]like . say . to be !\ablef - - - [[:
 - [^]=orf . to be a[^]b\out {to [^]d\o _somethingf}f or [^]something
 of !th \is _kindf - where you get [[: a :type of . of verbal
 !gr\oupf

711.

- A. ⁱ/ef . [@ :] you can ^{s=se}ef . [@] . it's ^worth five
 p/oundsf - - as being ^{l=i}kef . it's ^like five
 p/oundsf . it's ^with five p/oundsf it's ^for five
 p/oundsf

724.

- A. "^or^ref . you can ^{s/ee} _itf as ^being . like . it
 !v\alues - - { ^five p\oundsf }f ⁱt w\orths { ^five
 poundsf }f ⁱt is w\orth five p/oundsf . ⁱt . " !m\easures
 { ^five p\oundsf }f . ^{r/ight}ef ^where - the !verb to " !be
 plus w=orthf - ⁱs to be :treated as . [@ : m . @]
 a /unitf . ^phrasal !v\erbf

733.

- A. but my ^own . [@ : m - - @ :] !pr\eferencef - I ^think would
 be for - ex!ploring [dhi . dhi] . :similarities to
 :prepositional :phr\/asef . ^rather than . !similarities
 *((5 to 6 sylls :v\erbsf)

741.

- A. ^that's the ap:pr\oach I_thinkf very ^d\efinitelyf but
 you ^s=eef it ^is sui :g\enerisf ^so ((it'll) ^so .
 !anybody who is :l\ooking {^f=orf}f [:@:m] . a a ^niche to
 !fit it a ready-made :n\ichef . in English ^grammar to
 !fit it . /intof . is sort of ^begging for the !m\oonf - -
 ((you ^s/eef))

750.

- A. this is :one of the :things that - [:@] ^one of the "!m\any
 _thingsf - [:@] in ^English str/ucturef which is - [:@:m]
 - - - an ^item in a closed !s\ystemf .

758.

- A. [:@:] ^how many people have :y\ou _gotf ((^for [dhi])) you
 ^know ((if you)) ^incidentally :we haven't !s\een each
 otherf ^since that [:@:m] . pe:culiar :meeting with
 [:dhi: - @:m] - - l\ecturersf . re^m/emberf

786.

- B. he's got a good m/\indf but - ^I !thought he was :going to
 :talk us into :having to do another com:plete set of .
 :set !b=ooksf for ^that !bloody phil!\ology *p/aperf

812.

- A. he a^m\uses m/ef ^I I ^don't I ^don't "f\ind my_selff
 ^getting . ^getting as as as !\irritatedf I'm ^more am\used
 you kn/owf

818.

- A. ^but - he "!certainly has a . :hell of a high o:p\/inion
 of him_selff

828.

- B. well ^n\ow {you ^s=eef}f . ^these people for ":y\earsf .
[a:] as a ^matter of fact [@] :I rather :burst /\outf -
[@:h] - ^H\art you _seef is ^doing the :sch\ools examin/ationf

837.

- B. and he was ^b\oastingf about ^all this :stuff they'd been
:\usingf of ""L\awrencef and . ^George \Eliotf Vir^ginia
W\oolff and ^that kind of :th/ingf

850.

- B. ((he was ^talking)) about the :high literary !c\ontentf you
^s/eef of ^[dhi:] . compre!h\ension _questionf

852.

- B. I said ^what the hell is the :good of :th\isf for ^all these .
"!sc\ientistsf and ^engineers and !th\at kind of thingf

855.

- B. ""why can't we have :something . on econ!!\omicsf or
^p\oliticsf . [@:] ^wouldn't !\advertisingf ^be !just as g\oodf
or or^t\esting these _peoplef with [@] ^lexical !s\etsf and
se^mantic f\ieldsf as the ^stuff that you're getting !npwf

868.

- B. and they're ^even talking about

A. *^[=m f*

- B. :setting a combined language and !l/\literature _paper {at at
^P _sectionf}f . ((in^stead of)) [dhi:] . :paper n/\owf

- B. ^[dhi] re!pórt on :English ex/aming {^as you kn/owf}f
 ^talked about . [dhi] :best wr\/itingf ^of . !different
 :k\indsf

878.

- B. but the "only thing !th\ey can _think _off ((as)) the ^best
 writing of :different :k\/indsf . ^=isf . *to ex"^tend* the
 :number of
- A. *((^that's s=of))*
- B. *-* :literary !\authorsf

885.

- B. he ^said !w\ellf . he ^said /I _don't {^m\ind youf
 ^s\ettingf}f . ^stuff on !p\oliticsf - [@] ^anything you
 l/ikef so ^long as it's :well !wr\ittenf

904.

- B. it's ^just as good as setting *Virginia :W/ooolff* .and
 ^much more . ((to))
- A. *^[=m]f . ^[=m]f*
- B. ((:where)) *-* these :people **:want to :r\/eadf;

911.

- B. and [@] ^I'm ^I'm de":t\erminedf to get ^that sort of st=ufff
 ""^into {the ^compreh\ension {^qu\estionsf}f}f . ^rather
 than :all this :high-faluting :l\/iterature_stufff

918.

- B. be cause the scientists : don't "w ant that sort of _stufff
- A. [=m]f . [fa:] far !further from* . from the :students
 ex:p eriencef and s o onf

921.

- B. I ^mean ((they)) . ((they)) talk about (([@ @ dh@:])) . by
 ""^all means en:courage the :scientist to !r\ead this sort
 of _stuff . but that's ^very different matter from
 com":pelling themf . to ^deal *with* :that sort of ma:t\/erialf
 at

921. contd.

- A. $\text{**}[=m]f^*$
- B. $\text{^what is an } \{im"!p\ortant\} \text{ examin!!\ationf}$

932.

- B. $\text{^well n=owf . } "^\text{why/y } \{\text{n/otf}\}f . [i @] "^\text{if you _want this literature st/ufff . } ^\text{why n/otf } ^\text{f/or the compreh\ensionf } ^\text{and for the l\iteraturef } ^\text{even if you l/ikef } ^\text{or } ['@] \text{ for the !compreh\ension qu/estionf } ^\text{even if you /likef } ^((f))\text{or . } [dh i @] \text{ the !pre\ecis qu/estionf } ^\text{let's have . a new !p\atternf . } ^\text{let's have a piece of !straightforward stuff of :this k/indf}$

944.

- B. $\text{^let's have it in the !same position :every t/imef . } ^\text{so that your !scientist will know that :he's goingfor !question /onef . } ^\text{l/iterature _personf will } ^\text{go for question !tw\of and . } ^\text{they " !don't have to :read the two *!passages* and de!c\idef}$
- A. $\text{*}((^\text{qu\itef}))^*$

958.

- B. $\text{I've } ^\text{also I :th/inkf } ^\text{managed to } \{\text{g\et _them}\} \text{ at !l\astf } ^\text{out of } [dhi @h] . :precis !p\araphrase [@] \{\text{m\ix-_upf}\}f . \text{ because in } ^\text{Br\ighton you s/eef } ^\text{we !ask them for a :s\ummaryf . } ^\text{using your !own words as far as !p\ossible f}$

975.

- A. $\text{^they penalise them!!s\elvesf } ^.*$
- B. $\text{*}^\text{y=sf}^*$
- A. $\text{be}^\text{cause they run out of !!w\ordsf be}^\text{cause !then it it sends them :chasing aw/ayf . to } ^\text{find !quite . mere:tr\icious . } [:@:m] \{\text{s\ubstitutes for _wordsf}\}f$

983.

- B. and ^very /oftenf you ^get a :student 'who . :probably
!d\oes understand the p/assagef . but be^cause he feels he
!m\ustn't use the *words of* the p/assagef **-* ^gives
- A. *((^qu =itef))* **((^y=esf))*
- B. you the im:pression that he *:d\esn't under:standitf* -
be^cause he's :used words which !aren't so :g/\oodf

997.

- B. ^I've been cam!p\aigning for th/atf for ^several !y\ears
n/owf and in ^\any _case {I ^m\eanf}f . ^why !!sh\ould we
{^test the two things tog\etherf}f

1014

- B. you(('ll)) ^get an :\/essayf . *((a)) ^s\ummaryf and a
^compreh\ensionf*

1016.

- A. ^but [dhi] !compreh\ension *([[pe:]])^ [dhi: @] !paper
tw\of will
- B. *^y=eahf*
- A. ^now be :merely ((a)) :summary and *:compreh\ensionf*
- B. *^y=esf* ^y=esF . ^y=esf

1033.

- A. [@:] ^this ((is)) ex:actly in :line with [dhi @:m i] .
:English Language ex:amining [@m] . re:p\ortf ** ((that))
you ^cut those !\outf

1037.

- B. ^I've been campaigning for this !ever !since !C/\anning was .
{^m\oderatorf}f ^ten !y/\ears a_gof . ^and [@] !there ag=ainf .
^how do you get \on {with ^this fellow H\artf}f

1047.

- A. ^J\oe ^J\oe thinkgs that ((of him {t\oof}f . at ^t=imesf))

1048.

- B. but [@: @]* . [@:] ^he was :saying for ex:\amplef that [@: i]
 ((that)) ^these questions :three and :f\ourf ^didn't make any
 !d\ifferencef ^r\eallyf to the re^sult of the examin!!\ationf

1059.

- B. if ^you take a sta!tistical an:alysis of the :people who
 :p\assf you'lll ^find that it !is 'this qu\estionf . *((which))*
- A. *on ^which* they are . ^y\esf
- B. they're " ^p\assingf on * ((that))* !qu\estionf
- A. *^y=esf*

1067.

- B. it's ^fifteen marks out of forty-!f\ive {^R eynardf}f *.*
 ^which is a 'very different pro!!p\ortionf
- A. *^y=esf*

1071.

- A. and ^while those questions surv/ivef . ex^aminers . " !bunch
 their m/arks . {^on [dhi] . /essayf} {((and)) ^compreh/ensionf}f

1073.

- A. ((they)) ^give ((this)) sort of . ^you kn/owf . ^half-w/\ay
 {^m\arkf}f

1081.

- A. and you ((can)) ^get your !spr\ead of m/arksf . " ^just from
 that al\onef

File 4

33.

- A. ^then he [a:h] . 'sent a :m\essagef . ((^by)) 'Stanley
'Johnson 'saying ((can I)) 'come at f\ourf

47.

- B. yes ^Tim's *(('having 'that . :I kn/owf* but))
A. **y\eahf is ^this ((b\oiling))f*
B. he ^couldn't de'cide **:whether to or :n\otf

66.

- B. but I "s\aidf . ^p\ersonallyf I'm ^s\orry I haven't
repl/iedf but I ^would . I'm "g\oing tof be^cause I
would !like to c/\omef

72.

- B. ^then he 'said well !don't b/otherf [fa@] ^((now)) . ^now
you've "t\old mef you ^kn/owf you ^just !c\omef

81.

- A. ^[o] is ^this the ^this ((is)) the '[t@:] I sup^pose 'this
is the [k@mpl] - com:plete *!ch\oicef

84.

- A. - ^stick an in:initial l\abel {^on the b\ackf}f of ^any
-!p\icture . -[z]f . you * f\ancyf★
B. *you* ^f\ancyf

98

- B. these are 'just !b\orrowedf ((in ^this that)) ^he . ^he's
'cottoned /onf ^being eff/icientf to^the ^fact that the
!Stoke will !l\end 'picturesf"^fr\eeef . you ^see

103.

- A. in[^]stead of [p] 'cluttering 'up their \own *'placef . we
[^]might ((jolly)) well !\use themf*
- B. [^]that's r/ightf ((4 sylls)) .*

134.

- B. ((we)) used to [^]have that in the !w\arf
- A. I [^]had it in !c\offeef . [^]\earlierf*

159.

- A. we [^]spent a'bout an :h/our and a h\alff [^]just ar_ranging
 for . _us to _take "one oc!c\asional 'studentf

184.

- A. [^]each 'tilted the \otherf in in [f] in [^]favour of me !c\omingf
 was the [^]f\actf [^]that there was this !complic/ationf of [dhi:] .
[^]afternoon s\essionf

206.

- A. so - [@] . it [^]looks _very !g\oodf I [^]think 'what you've .
 'quoted in ((:th\at)) 'thingf

229.

- B. I [^]mean it's [@] . [^]so much (telephone starts ringing) b\etterf

243.

- A. I've [^]d\one m/inef and [@] I . [^][bro:] [^]thought I !brought
 it !d\ownf

248.

- A. ^c\oursef ^what you w\antedf was "tw\o th/ingsf ^one was an im!m\ediate /onef of of . se^lections for :th=isf - to^go
 . *into the b\ookf*
- B. * th\at's 'rightf*

252.

- B. and ^then in a !month's *t/imef the ^whole ((l\ot))f . you ^kn/owf*
- A. *^but . a !month's t/imef you ^want* . [dhi] the !wh\ole
 [s] r/angef from ^which se'lections *^[\m]f* **can ((be m\ade))f**

264.

- A. well you ^see !I was al:l\/owedf . ^only [t@] - 'rather only
 "tw\o 'hundred 'lines of{Ari^st\ophanesf}f I ^th/inkf . and
 ^three 'hundred !S\eneca

279.

- B. ((so)) I ^th=oughtf well ^I'll !just 'take The Cl\oudsf .
- A. "^[=m]f .
- B. ^ and [dhi] I t/ook 'out b\itsf - [dhi:] ^which [???] were/ ^{!s\o} se/ectedf
 that the ^r\aderf ^((even)) !{r\eadinf} " !just 'those b\itsf would
 ^st\ill 'getf *the* ^out'line . of the pl\ayf
- A. *^[=m]f*

293.

- A. he's ^{n\ot 'theref} as a :dr\amatist n/owf he's ^there as a
 ! s\atiristf so ^I ((had to)) !did it a:g\ainf

296.

- A. I ^th\enf . ^simply ex:tracted the sa!tirical !p\assagesf .
 which ""d\on'tf . [?] ^in
- B. **^[=m]f*
- A. **them!s\elves** _((good)) f but ^that
- B. **^[\\m]f**
- A. 'doesn't !m\atterf * d\oes itf;-*;
- B. *^not a b\itf . ^n\of*

307.

- B. ^n\of* ^cos [?] !any'body with :any s\/ensef would ^read the
 pl\ay in the -'trans -l/ationf ^w\ouldn't theyf

317.

- A. I I ^spent the :week!\end 'at itf and ^I !f=oundf . that
 ^[tr@] ^s\ayf you've ^got [dhi:] . the :gh\ost 'thingf; .;
 ^which is \interestingf **for** ^st/udentsf

350.

- A. so* it's ^awfully _t\emptingf to ^put **!tw\o of /eachf**
 to ""sh\owf
- B. **I ^kn\owf**
- A. *that ^this* . ^would be an !interesting comparisof
- B. ^qu\itef

357.

- A. but ^since I'm only all\owedf ^three 'hundred l/inesf -
- B. **^[\\m]f**
- A. **^I was** [@] ^I was [@:m] . re^duced just to 'putting
 ":one of !\eachf

434.

B. do you^recognize th/is~~f~~ . ^that's . Ve!l\asquez' f ^Pope
!Innocent the !F\ourthf . a ^c/opy 'of~~f~~ -

A. ^are these *c/opiesf

449.

B. ((it's a sort of)) . sort of ^semi-'cubist ,!f\antasyf

461.

B. I've " ^g\ot one or 'two 'paintings 'up {^in my r/oomf}f as .
you *^kn\owf

491.

A. !when he wr\ote to me {^f\irstf}f he said ^as you 'may rem\emberf
which of ^course I d\idn'tf *.* ^in 'my r\oomf

B. *^[\m]f*

A. I've ^got some _p\ictures on the W/alls *from [dh@s]f*

B. *^[\m]f*

517.

- A. ^what's th\isf . ^J/ones . sch\olarshipf
- B. it ^looks 'quite :n\ice 'actuallyf

539.

- B. ^B\anks the/is 'isf - ^=Ermyntude f - ^Ermyntude B\anksf
a^bout 'nineteen !tw\enty you 'seef they've ^had th\atf .
^forty y/\ears app/arentlyf

557.

- B. . there's* ^very l\ittlef ^I ((think it would be a)) :m=uchf -
- - per^haps he's got m/oref and ^bringing them !\upf
- A. . ^well l\ookf . shall ^I _take !th\isf

574.

- A. ^n/\of because ^I was . !g\oing to 'sayf that I ^think it
'fits in 'better with 'what you've:g\otf . *as ^far as I
rememberf

586.

- A. *you* ^say 'this is . ^who/o is *'thisf Ve^l/asquez* [@]
- B. *it's a " c\opyf* of a Ve^l\asquez {^th\atf}f .

600.

- B. I've ^s\eenf .^you ^kn/owf [f@u] ^coloured repro:d\uctions
of the o_riginalf in ^\art 'booksf

612.

- A. ^wh/o is itf
- B. Pope ^Innocent the F\ourthf - ((a)) ^[p@] ^seventeenth 'century
p\opef . ((of)) ^some . ^s\ome 'ilk or /otherf

616.

- B. I ^don't know 'Leslie's !v\iewsf and I ^s\aid to _him you _knowf ^one of the th/ings that df . ((it)) ^seems to 'me it would 'be con!v\ /enientf if we ((could)) ^all . if we ^could you kn/owf ^[set] ""m\ore or _lessf a^gree tog/ether as ((to)) ^when we 'stop l/\ecturing this 'termf

627.

- B. cos*
- A. *^if* we could _all ((what))
- B. . a^gr eef as ((to)) ^when we !stop . !l\ecturingf ^I s/aidf be^cause in !pr\evious y/earsf . [@m] you ^kn/owf ^I've been ""{v\ery} emb\arrassed ((by)) the f/actfthat ^though I'm 'not . ex:\ /amingf ^none the l=essf . ^teaching 'seems to :come to an /endf . you ^kn/owf ^after a _bout !six !w\eeksf which ^suits me very ""!w\ellf

647.

- B. and he ^thinks it's 'rather 'scandalous that we !d\on'tf . (a:] ^h\e [s@] ^h\e 'saidf ^I'm a!!m\azedf that ^dons should !st\ill bef - [@m] - pre^paring their 'lectures at the 'end of the 'summer t\ /ermf

657.

- A. you ^mean !he's . 'he's [@?] what ^scanda_liz((ed)) ((1 s /y11)) that we're pre!!p\aring 'lecturesf

663.

- B. n\of ^he(('s)) ^w\ellf ^he's sur!!pr\isedf that ^we're pre!p\ /aring 'themf but ""((faintly)) _sc\ /an*dalizedf* that [?] we ^are . [@m - ?]
- A. *((^wh\yf))*
- B. you ^kn/owf that we ^do in "f\actf as ^though it's a !communal ""!l\ine on 'thisf we ^do . !seize the ch\ancef of ^stopping l\ecturesf

677.

- B. ^he 'seemed to as:s\umef that ^people . !got [@m] !all their 'lectures tog\etherf like ^getting a 'bag of mat/erialf ^so ((that)) by the !time they 'got to the s/ummerfthere was ^no 'more !w\ork to 'dof which ^isn't {"m\y 'system} at *:a\lf - it ^never "!c\ould* bef

691.

- B. I ^know when !\I did 'onef . ^I was !just going to [m m] "agr\oaningf ^last !n\ightf ((to ^say)) I'll "!h\ave to d/o onef and I ^suddenly rem/emberedf I ^had e!leven ((till)) \one fr/eef

702.

- B. I ex^pect it's the . :kind of ma!t\erial f he ^{g\ets ((it with))}

704.

- B. ((*it's ^either)) "f\actual. {^d\etailed 'stufff}f*
 A. *((2 sylls))* . ((^in his :n\otesf)) or ^all ((in adv\ance))f
 B. - it's ^not 'like a :lecture on Ch/aucerf

715.

- B. ^he said you kn/owf that . [ko:p] *^how will 'students j\ustifyf ^staying in L\ondonf ^spending-m\oney you _seef if they ^don't get *t\oughtf

735.

- B. ^I seef ^that's a!bout wh=atf ^forty [f] . ^over 'fifty per :c\entf - ^\aren't 'theyf

752.

- A. I ^don't 'think :G\illianf ^or \/Ingeborgf . ^are !\on the 'board this y/earf -

759.

A. well "G\illianf could [k] could^carry /on you 'seef . ^giving those !l\ecturesf

769.

A. !I* . ^I'm d amnedf if ^\I'm going on to ^what [sh] ^I !th=inkf .
^eitherf it's . "either (('there's)) :th isf - ^where \is
itf ^th\is /onef - . "th\eref

794.

A. but - do ^you 'think 'that /isf or ^is it !t\oo . {^w\ildf}f

807.

A. ^\ohf it'll ^sh\owf that [@] . you ^kn/owf - ^I shall
*(!f\eel itf)) ^up to a p\ointf

816.

A. **^what** have you 'put on *'your . ((th\ing))f*

B. *well I've ^just* 'put my in:\itials on the b/ack as^{'he}/'saysf

836.

B. and ^so the !others [@m . z] the ^others _sort of "f=eelf
that ^things !won't go !\on much 'longerf

840.

A. well they "really 'haven't ": any ^{r\eason _to)f be^cause
I 'mean *.* :finalists 'are

B. *^[=m]f*

A. :[f/ain] and ^they 'actually "d\o 'finish (('thenf))

859.

A. ((but ^I . but ^I [th@])) I ^don't 'think ":\I'm 'going
to go /on with itf

860.

B. ^are you !doing _two or \one . _paper this _yearf

A. . ^only :\onef

889.

B. ((this)) ^P=ollyf . you ^know !that g=irlf whom I've [?] I [m?] I [m m] pre^sented . ((a)) :rather abs\ /urd re'port in a 'wayf ((that)) ^genuinely :repre'sented what !I f\eltf I ^((said)) she *!might* f/ailf

909.

A. I ^know 'who 'she :\isf but ^she's !((been)) 'rather [@] - e!l\usive . as 'far as 'I'm conc/ernedf ^so I !don't 'really !kn\ow herf

917.

A. ^I 'think :\our ((grannyf - ^gave us)) [@] . an e:lectric k/ettlef and ^bring it to c/ollegef and with ^that ((*m\ i)kf . [@]^and ^and s/ugarf)) - ^why !n\otf

922.

B. ^two 'years a'go _n=owf . ^three* . ^three 'years ag\of I ^brought !cups and s/aucersf and a ^coffee and t/ea 'potf and I ^haven't 'done :anything \elsef

928.

A. but ^have you _got a !k\ettlef

B. . ^well [ae] . I ^what !I would /usef is [@] ^one of _those _little [? @m] , - 'solid !f\uel j/obsf

937.

B. and you ^just 'have a {c\ube} and :l\ight *itf it's m\/eths
'reallyf but it's ^far* m ore than . well it ^lasts

A. * y\esf ^does it l\astf*

A. 'quite a t\/imef we've ^had it on :p\icnicsf and [@m] ^when
we've been c\ampingf

944.

A. ^w\ellf I ^think there 'is a *((pl/acef where ^I [ai]))* .^I

B. *((we've ^even 'got a !st\oreroomf))*

A. can 'get a 'cheap !k\/ettlef

971.

B. there's a ((^tall 'young I was ^going to \ask youf)) there's
a ^poor little f/ellowf who's .^trying to 'get through
f/inalsf ((for)) the ^third "t\/imef and ^he w\antsf to .
^he ^Joe N\easden'sf ^very 'kindly been :h\/elping 'himf

995.

B. he's ^taking 'finals for the :third t\imef this s\ummerf

A. ^and [w] !he . !knows \Josephf .

B. ^well !I intro:d\uced himf

999.

B. and ^\I asked _Josephf ^could 'Joseph :fix 'up 'someone to
:d/o itf - you ^kn/owf . be ^p\/aidf and ^give him a 'bit
of !t\eachingf as he ^seemed to be !absolutely ((:j\ammed))
'upf in ^Old !\Englishf

1006.

B. in "f\act 'Joseph with"out being ":\/askedf ^did it him"!s\elff
you ^kn=owf - ^fr\eeff

A. *((^y/eahf))*

B. ^which was ex'tremely !n\ice of himf

1062.

B. ^so I 'said . ((at [?])) the !end of !last ((!y\ear))f
((^Graham [æ] 'Williamson)) was /illf and . ^she 'said he
[w] :y\esf he ^won t re!c\overf

1082.

B. ^\I had a 'seminar tod/ayf ((in which)) . ^people !hadn' t
'read the ((!st\uff))f be^cause of !s\essionalsf ^hadn' t !read
the !pl\ayf ^so we 'had to "!spin it \outf

1138.

B. ^n/of it ^came on its /ownf be^cause there !wasn't . it's
^not t\ime yetf for the ^next 'meeting of the *f\acultyf

1141.

A. ^so I pre!sume it 'is for :any'body in the :facultyof !\artsf .
^even if I 'haven't . been !\askedf ^I could !g\of

1154.

A. [@ f?] the ^p\oint _isf ^I !still s\ayf - ^that [@m] - at
!least the 'students :in this 'classical b/ackgroundf ^will
((have)) !read "!s\ome . *S/enecaf* ^in the o!r\iginal f

1154. contd.

B. *^{qu}\itef* - ^[/\m]f

A. [^{@:}] which [^]gives then an i_dea of ((a)) "[!]s\amplinf
^d\oesn t itf -

B. *^{abso}*!l\utelyf

1174.

A. it [^]is . 'three h\undred to a s/eminarf . and I [^]think*
!you had m/\oref for *(([^]2 sylls in the "s\eminarsf 3 sylls))*

1180.

B. [^]I !d\idf [^]I [s] [^]I had [[@]]* [^]I had !two * [[!]pei] ((\eachf))*
[^]seven

A. *(([^]\ohf))*

B. f\iftyf

1194.

A. [^]so I ve se!lected :three or 'four 'passages :fr\omThe
Cl/oudsf *-[^]which 'are sa!t\iricalf

B. *[^]y\esf*

A. +[^]that's+ /allf . and [^]quite am\using

1203.

B. [^]what do you th\inkf do you [^]think [[@]] for the [s] 'two
'fifty from \Aeschylusf [^]I could . !take perhaps the 'Orest:\eiaf
or do you [^]think I should 'take an :\early 'playf [^]like the
*Pro:m\ethe'usf

1214.

A. I was [^]reading The Cl\oudsf ★and★ [[@] . ^{@?}] [^]Ari'stophanes
al:though

B. *[^][\ m] f*

1214. contd.

- A. he's [ə] . ;pokes f\un at th/ingsf is . was ^very
con!s\ervativef .
- B. " ^v\ery { ^y\eahf }f . * ^[\m]f*
- A. ^and *his* !s\ /atiresf ((^ \oftenf)) ^is di!rected in 'this
par'ticular **w/ayf**

1223.

- A. ^but [ə] he !\is cons/ervativef . ^and !pays 'anyway !l\ip
'servicef to the ^g\ods f in *the ^pl\ayf*
- B. * ^[\m]f*

112.

- B. they're ^by their st\udentsf and by their ^t\eachers .
((actually))f ^in the p\icture cl/ubf which ^I bel\ong
_tof - the ^Stoke t\eachersf and some se^lected st\udentsf
" ^l\end 'picturesf to the ^cl/ubf ^which !we " !h\iref . we
^p\ay

167.

- A. but ^h\e 'got 'rid of ((him)) { ^very :sm\artlyf }f
*- * but I ^wasn't [bi]
- B. * ^y /\esf*
- A. 'able to dis:cuss at !l\eisuref

194.

- A. but in " ^f\ /actf [ə] ^when 'I'd !missed s\ /eeing ((it))f
*- * ^there was !no _point
- B. * ^[\m]f*
- A. in in in ((syll)) !d\oing itf

374.

- A. and ^\onef . and ""^one p\assagef I ^found in the :Trojan
!W\omenf
- B. * ^[\m] f*
- A. ((was)) was ^\actuallyf a ""^v\ery v/aguef ^rather s/ubtlef
- [[:] ^revel_ation of !ch\aracterf - *^through* d\ialoguef
- B. *^[\m]@*
- A. which ^isn't !\often ((with)) 'Senecaf
- B. not in the S/enecaf ^n\of

385.

- A. so ""^what I've d=onef is ^simply ch=oo[s] f - ^fairly :sh\ortish
'bitsf *-* that ^/I
- B. *^ [=m] f*
- A. {^th/inkf}f are ^repre'sentative of the _main - f\eaturesf
of ^{S\eneca`s} -
- B. *((^y\esf))*
- A. *:style*

410.

- B. I'll ^have to _take ""!l\onger *((4 sylls))* 'stretchesf
^ pr\obablyf you ^know 'like [[:] 'Ajax's _speech before d\eathf
and ^things of !that k/ind - ^and [[:] or E!lectra's 'speech to
the \urnf

440.

- B. ^th\at's a c/opyf [[:] ^that's 'only a :Stoke 'student has :made
a 'copy of the !p\ainting whichf the ^painting's in Ma!dr\idf -
^I !th\inkf it's ^not in L\ondonf

505.

B. ^this is - !David S\imonsf . ^St\oke {^y\esf}f ^gives his
addr/essf - but ^no d\atef

526.

A. you ^((could)) !put - - - ^if :I was 'going to h/ave 'onef
I ^think _I'd _have !th\isf ^just for !f\unf

605.

B. I ^{kn\ow} it "is 'Innocent the :F\ourthf I'm ^s\uref
- be^cause I ((you ^kn=owf)) I've ""^seen the 'portrait in
:l\ectures on Ve_lasquezf *

829.

B. ^course an!other 'factor in ((disagr/eementf ^\isn't itf))
is the ^fact that a _th\irdf of the ^wh\ole {of the de^p\artmentf}
disap^pear in (([dh@mi])) the be!ginning of M\ayf - ^does af'fect
the !\other 'studentsf it ^doesn't "!\n\eed 'tof but it ^does
in f\actf

File 5

38.

B. *and* ^ they dis'cussed what ((a))

A. *^[=m]f*

B. :w\ord wasf and - what's a s\entencef

A. **^[=m]f**

B. that's *^ev*en m\ore 'difficultf .

A. *^y=eahf* ^y=eahf

B. - and ^s\o onf . and " ^then I :also 'went to some !p\ost'
'graduate 'onesf which were ^m\ore 'interestingf -

A. ^y\eaf

B. which he ^had for [dhi:] - di:p1\o'maf - the ^m=ain 'peoplef

60.

B. ((then)) you can ^pick 'up 'all the j\argonf and

A. ^y\eaf - -

B. and then sort of ^get the 'hang of 'what they're :t\alking
a'boutf

72.

B. ^or ^th\en '[dhe] ((he ^found)) 'last y/earf that that ^five
fif:t\een onef - ^cl\ashedf with ^they ^some of them 'had
phon:\etics or s/omethingf

A. (([mhm]))

B. - and he ^said w\ellf - ^what a'bout 'half 'past s\evenf

93.

A. I ^can't 'come 'Monday or !T=uesday eveningf cos- I ^s\/ingf

100.

- A. ^[əw]what in 'fact did you :d\of if you ^hadn't 'done/- 'English since - - *\0'level*f
- B. *^maths and* ph\ysicsf
- A. ((they're ^both)) ana:l\yticf
- B. ^[m]f

108.

- B. but I've ^always be'lieved in 'having a 'sort of you kn/owf - - - ^\eveningsf for ^doing one's !h\obbiesf .
- A. ^y\esf - ^w=ellf - I ^like to 'do 'more than \onef - - ^ th\ingf ((in my ^l/ife))f

116.

- B. ((well)) ^I can't st\and 'thatf
- ?. ^[\m]f - - -
- B. and ^Hart you've 'got to 'stand \up tof ((^h\aven't youf))
- A. ^y\esf

125.

- B. he's ^always ^I've never 'had I :don't sup'pose :y\ou've had any tr/ouble 'Jane ((have you))f
- C. ^well !n\of - well ^his 'manner ap:p\ealed to mef

140.

- B. she used to ^come in 'Late in the m\orningf and he'd say good ""^after'noonf ^B/erylf - which ^used to 'make her !terribly w\orriedf

164.

- B. ^no he "used to ex'pect [ə:m] ":used to ex'pect 'Grace to be 'there when she was :m\eant to be th/eref

177.

- A. I ^must s\ayf ^I rang 'up on :Th\ursdayf be^cause I !had a
- l\etterf an of f\icial 'letterf ^\ages a'gof ^from
*[@:] -** !Miss "!B\akerf
- C. **((4 to 5 sylls))** ^saying 'come at !ten o'!cl\ockf

209.

- B. ^what he !d\oesn t r/ealizef is that ^not \everybody elsef can
^work 'quite as 'hard as !h\e 'canf +- .+ and you ^j\ust have
to _sayf
- A. +^y\eaf+
- B. w\ellf . I'm [s] I'm ^s\inging to'night or **s/omethinf

217.

- A. ^oh well 'I've !already !t\old himf - cos he ^tried to 'get
me 'in in ^f\actf . ^I was !{sl\ightly} an:n\oyedf but
^actually it !{r\ather} . am\used mef . but [@m] .
^he s\aidf ^oh 'well you kn/ow If ^might get - [@m] -
!t\erriblyf . - you ^kn/ow I'mf - I'm 'just :hanging /on
'nowf

238.

- A. . ^and !s/of - ^then I 'found that - 'I'd been \ordered
{by h\imf}f to ^come and do some w\orkf be^cause there was
such a !r\ush onf

342.

- B. " ^that was the w\ay [@m]f - ^when I f\irst c/amef ^B\erylf
^M\artinf . [@:m] ^M\ervyn wasf the . sort of ^mathema'tician
cum :pr\ogrammer you 'seef and ^I 'started 'working with h\imf
and ^his 'wife was \Englishf - and ^sh\e went _through some
_first stage an_alyses{w\ith mef}f

359.

A. ^what are you d\oingf* ^you're ; !ch\ecking themf

380.

B. and ^then you !m\ay get y/ourf ^you kn/owf you ^may 'not 'quite
 . 'think . ^quite a'gree with 'their :s\ystemf

393.

C. ^well I'm *'glad it* 'wasn't 'just _m\ef be^cause there was
 !some pec\uliarf - [[:] con^vention about !h\yphensf which
 ^just

B. ^seemed quite *\arbitraryf*

C. *it was* ^absolutely il:l\ogicalf

418.

A. I mean ^are you . {\all} 'members of a re'search .
 :pr\objectf - or ^just a gr\oupf I mean ^is . M\arilynf .
 [[: @:] as^sistant ![le @:] I mean ^is she a !l/ecturerf

491.

B. ((and)) there are ^four [a:] there are " ^thr\ee 'answers they
 can 'givef ^either it's ac:c/eptablef it's ^not acc/eptablef

A. ^[=m]f - -

B. it's ^m\arginalf . ^or you kn/owf it's ^somewhere be":tw\ /eenf

509.

B. and they ^write 'something 'like 'I en!t\ /irelyf ^dot 'dot
 d\otf - and the ^student 'has to com:plete the s\entencef

A. -^[\m]f -

B. ^well with en"!t\ /irelyf they'll nearly 'all 'write agr\ee
 with 'youf

A. ^y\esf

513.

B. and en[^]tirely and a'gree *-* 'to tog\etherf

A. *[^][=m]f* [^][=m]f col[^]l\ate or s/omething it's 'calledf

536.

B. and [^]then they *'sort of* they [^]score them \upf in a [^]certain
w\ayf and they'll say [^]have

A. *[^]y\esf* **[^]y\esf**

B. they . [ə] - - [^]have they d\onef what they were [^]t\old tof
and if 'not wh\y 'notf

557.

B. [^]but you'll be a!m\azed{[^]\actuallyf}f if you [^]go to :some of
these s\eminarsf the [^]things that :people - !!s\ayf - which
they re[^]gard as gram!m\aticalf

563.

B. [^]\I 'once [ə] h/eardf [^]somebody who'd - [^]r\ead 'Englishf
[^]seven y/ears ago f had been [^]teaching 'English in sch/oolf
and is [^]now 'doing a ; 'M_/A {or - [^]s/omethingf}f

A. [^][/m]f [^][/m]f

B. - and "[^]sh\e pro'duced a s/entencef which [^]{I 'thought} didn' t
!m\eanf . [^]what she . th/ought it *'meantf*

574.

A. [^]I m\ust sayf it [^]((syll)) 'obviously :\is a m/atterf of
[^]seeing 'whether . one :gets - - one's suf[^]ficiently :\/interested
in a 'thingf and one [^]d\oesn t 'get 'bogged d/ownf . [^]in [ə:]
- - [^]\oh the _sort of - - - [^]rou!t\ine ((of the 1 syll))f

591.

B. and at[^]the end of 'three y/earsf they [^]had to pro'duce *-*
- what:ever they were 'going to

A. *[^][/m]f*

B. prod\ucef

632.

- B. ^Gr\acef when - ^she was :l/\eavingf sort of ^s\aidf ^well
- ^he 'ought to 'make him'self a !t\ime'tablef

635.

- B. and we ^said that the 'survey's n\everf ^going to be !f\inishedf
be^cause he 'ought to be - at 'this stage n\owf and we're
" ^n\otf

639.

- B. ((and I think)) ^she just 'sort of 'worked a'way 'rather *-
- 'feeling ((!thr\ough))f
- A. *^[\m] f*

646.

- B. but you ^shouldn't be 'influenced by :m\ef I ^mean I'm *-
((!s\ack themf))*
- A. *((^unaff\ectedf))*

656.

- A. I mean one's ^n\everf . I mean you're ^making -
concl\usionsf which you can ^never :really :v\erifyf because
((^you can)) . by the ^time you ve f\inishedf it will have
^changed !\anywayf

678,

- A. ^oh I s\eeff ^oh y\esf ^no I don't - - [e] I ((don't))
- - ^g\ave up 'trying to 'do m/athsf

686

- A. ^if I 'put a . "M\arvel{^t\inf}f in his ^wastepaper b/asketf
^will he 'think it - 'unaes!th\eticf

746.

- C. ^ and there's !Edward W/ilsonf . and - - ^wh\at's his 'namef
 . ^ Bl/ackf ^Freddy Bl/ackf - ^Bl=ack_stonef . ^n\of ^M\ichael
 'Blackstonef - who ^deals with ['dhi: @:m] - 'student applic\ationsf

753.

- C. they ^are 'really 'part of the 'main 'English gr\oupf
 A. ^y\eahf
 C. but ^only sort of 'come in h/eref . to dis^cuss . 'applic\ationsf
 where they *- in* ^th\is dep/artmentf - but [@:] you've
 A. *^ [=m] f*
 C. ^met the 'main 'body of . !Hart's *((p\eople))f*
 A. *^y\esf*

794.

- C. I ^don't think you've met :Nelly 'Cartwright up:st\airsf
 A. ^n\of
 C. ^I won't ![pri: - @_m] - - - wh\at's the 'wordf - ^pre-per!su\ade
 youf but [@:] - - she's not of the most :h\elpful var/ietyf

814.

- C. but ^Nelly :had been the :j\uniorf -
 A. ^I s\eeff
 C. and is^[s@:] . ^certainly !nowhere n\ear /old enoughf -
 A. ^\I 'seef
 C. ^for the "!p\ostf but . be^cause of :various - - !c\ircum_stancesf
 she ^somehow . 'worked her way !\inf

846.

- C. a ^f\ewf [@:] the ^only [?@:] oc:c\asionsf that [@] that I
 ^w\ent up'stairsf were . for ^\instancef if - ^Hart was
 !ch\anginf his ^time of "!l\ecturef or ^something like th\isf

854.

- C. I would ^g\o upf and ^put a 'notice on the b/\oardf **. **
and ^duly in'form !N\ellyf
- A. **^[=m]f**
- C. to ^t\ell 'people f *- * ^th\is 'sort of 'thingf **but** [a]
a^p\art from th/atf
- A. *^[=m]f . ^[=m]f* **^[=m]f**
- C. there's ^very 'little - - [a:m] - - co'oper:\ationf

910.

- C. ^what 'makes you 'do 'secret\arial 'workf - as ^w\ellf
((if you . ^want to 2 sylls))

914.

- A. ^\ohf be^cause I . be^cause I 'got a - I ^got a . a ^not'very
'good de!gr\eeef . so I - I ^got a degr/eef which I ^w\ouldn't
have 'got supp/ortedf if I ^did re!s\earchf

927.

- A. I'd ^had these inst/uctions to ^get a !j\obf so *I* ^didn't
:[i:] - I mean I ^didn't par:ticularly w\ant to 'teachf but
- C. *^[mh\m]f*
- A. I ^did th\inkf ((that)) I must . ^do !something about
((!finding !work)) with!in the next !y\earf
- C. ^[\m]f + ^[mh/m]f*
- A. ^rather than :sitting ab/outf *- * ^spending another ":tw\o
'years for ex/amplef be^coming an aca!d\emicf

939.

- A. and then I ^happily :sat in :c\ollege ((for .)) {^two
y\earsf}f - which was ^v\eryf [a:m] - - - ^well it's !v\ery
{p\leasantf}f

952.

- C. he - *he* ^doesn't 'like 'secretaries to be
'merelys\ecretariesf

955.

- C. they ^can't 'join 'in ['dhi:] - 'more - !c\ollege s/idef

967.

- C. you can ^hold the "top - - ad'ministrative 'job inc\ollegef
and if you ^haven't 'got a degr/eef - .

- A. *((^r\eallyf))*

- C. *you just* ^can't 'set :foot be'ond a 'certain . you ^kn/owf
but ^if you've 'got a degr/eef ^this is !m\agicf

995.

- A. so ^when you come 'somewhere like h\eref you ^want to be 'sort
of . !r\ecognizedf

1015.

- A. ((I ^must say if)) one !!wants to 'be . ^have a .suc!cess .
a suc"^\essful 'jobf - and to ^be suc!cessful in 'what:ever
{f\ield} :one \entersf

1092.

- A. it's [[:m]]

- C. it's [[:m]]

- A. ^w\ere you in f=actf - ^doing the 'sort of :secretary 'tour of
the de:p\artmentf

- C. ^[\m] f

- C. ((and [[:]])) - the :m\aths de'partmentf which is ^quite a
big de:p\artmentf

1097.

- C. I I'd ^rather 'go [sh] - ^into 'sheer : \adminf
 A. ^y\eahf
 C. and ^n\ot just bef . somebody's s\ecretary if p/ossiblef

1132.

- C. I ^think if I :just sort of - :take you r\oundfand ^sh\ow
 you where [dhɪ] *- * {central s\ervices}** and** so on : \aref
 because ^H\art {as you ^kn\owf}f
 A. **^y\eahf* **^y\esf**
 C. ^is a ^man who . 'knows !\everythingf

1165.

- A. ((well I ^kn\ow that y/ouf [m])) [@:m] but ^y\ou of c/oursef
 ^have 'people 'in and 'out 'all the *t\imef*
 D. **^y\esf* ^y\esf
 A. ^so that at - at the m\omentf I sup^p\osef I'm pre^pared to go
 'down and have !l\unchf
 D. ^y\esf
 A. ^\if it's 'not ((too)) exp/ensivef

1178.

- A. I'm ^\ /alsof [t] " ^r\easonably /anxiousf to ^bump !\into
 'peoplef but per^haps one :just . 'sort of - :h\ /olds on 'thatf

BIBLIOGRAPHICAL REFERENCES

There are innumerable articles and reports on topics more or less related to that of discontinuity. A large sampling of these are listed in A Selected Bibliography on Temporal Variables in Speech, edited by G. Appel, H. Dechert, and M. Raupach and published in 1980 published in 1980 by Gunter Narr Verlag, Tübingen. Over one thousand entries are listed and cross-classified according to sub-topics. There is, however, some material not included in the Tübingen bibliography. For instance, none of the ethnomethodological studies of repair are included. Still, it is the best available resource for discontinuity studies.

For this thesis, there would be little point in attempting either to duplicate or supplement the Tübingen bibliography. In this case, only works actually referred to in the text of the thesis have been listed here.

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