

Chapter 2

Empirical Methods

The development of psycholinguistic methods over the past 50 years reveals a clear tendency: a shift away from methods involving the collection and analysis of uncontrolled or genuine data toward experimentation (Dietrich, 2002, p. 14; our translation).

Chapter Prospectus

Chapter 2, *Empirical Methods*, is a corollary to Chapter 1 insofar as the methods of research characteristic of mainstream psycholinguistics have been largely limited to controlled, artificial, laboratory-based experimentation and quantitative analyses of findings. Field observational research involving spontaneous spoken discourse in naturalistic communicative settings has been radically neglected, and the relevance of qualitative analyses has correspondingly been downplayed.

Homo Loquens et Audiens

We have suggested at the end of Chapter 1 that modern mainstream psycholinguistics and a psychology of language use may be, in their present incarnations, irreconcilable. The rationale given for such a statement has been the basic principles, goals, and epistemology of the two approaches to language use. Currently, mainstream psycholinguistics concentrates on the language system itself, as instantiated in verbal expressions of cognitive processes. But it is our contention that a genuine psychology of language use must begin with a concentration on the people whose speaking and listening are being investigated in meaningful communicative contexts.

The last phrases of the foregoing paragraph have a subtle importance that is frequently overlooked. In the present book, we set out to engage not *homo linguisticus*, but *homo loquens et audiens*, or, as Herrmann (1985, p. 41; our translation) has expressed it, not “the *human being as language processor*,” but

“the *human being who also understands language and also speaks.*” It should be noted that Herrmann has specifically credited both Bloomfield (1933) and Vygotsky (1934/1962) for these insights (see also O’Connell, 1988, p. 54). The point to be made here is that human beings speak and listen *only occasionally*, not – as a function of their nature – uninterruptedly. Hörmann (1981, p. 191) has stated it as follows: “Speaking must be seen as the continuation of action by alternative means.” Such an intermittent recourse to the use of language is thus part of a comprehensive pursuit of intelligibility and relevance in the ambient world. As Bühler (1934/1982, p. 158) put it many years ago, as explication of his concept of the “*empraktische Gebrauch von Sprachzeichen*” (“*empractical use of language signs*” [Bühler, 1990, p. 179]):

Islands of language emerge from within the sea of silent but unequivocal communication at the places where a differentiation, a *diacrisis*, a decision between several possibilities has to be made, and easily can be made by interspersing a word. (p. 176)

Bühler (p. 176) has provided as an example a railroad passenger’s one-word request for a transfer. The ellipsis suffices precisely because it is embedded in a rich context, which carries the burden of meaning that is carried by words in a written text. In other words, the social context involved in this speaker/listener exchange is amplified by a nonverbal context: the very presence of the speaker on a train addressing the conductor – whose response is most likely to be nonverbal. A literary example of such language usage is to be found in a novel by Philip Roth (1959; cited in Page, 1988, p. 8):

There was not much dinner conversation; eating was heavy and methodical and serious, and it would be just as well to record all that was said in one swoop, rather than indicate the sentences lost in the passing of food, the words gurgled into mouthfuls, the syntax chopped and forgotten in heapings, spillings, and gorgings.

In Chapter 1, we have mentioned Graumann’s (1987) critique of the individualism of cognitive social psychology. More relevant in a methodological context is his claim that a psychology of language use must be subsumed under the broad rationale of social psychology. We too are convinced that the proper local habitat for a psychology of language use must be social psychology rather than cognitive psychology. However, we wish to emphasize as well that social psychology itself must be somehow compatible with the psychological understanding of the individual. Research on the social aspects of language use must never disregard the principles that formulate the dynamic activity of the individual speaker or listener, precisely because the social activities of the individual must be built upon the capacities and properties of the individual psyche. Hence, research and theory regarding spontaneous spoken discourse must be integrated with an all-embracing, comprehensive, general psychology of the individual. In other words, speaking and listening, meaning and understanding, can be properly contextualized, investigated, and understood only within the larger ambit of man’s psychological, social, and cultural life. Short of that context, both research and theory gravitate back to the protective cove of

an isolated language system once again. In fact, Graumann (2006) has harbored a fear that an American version of social psychology is still inadequate to deal with language use. He has argued that American social psychology traces its heritage back all the way to Floyd H. Allport's (1924) emphasis on the individual. Or, as Graumann (2006, p. 56 f.; our translation) has put it: "What we call psychology seems to be inseparably associated with the concept of the individual." Hence, he has preferred the designation "cultural psychology" rather than social psychology as the local habitat for a psychology of language use.

Transmittal of Data by Transcribers

The social and general venues of a psychology of language use specifically influence the type of question that can legitimately be asked in research as well as the applicable methods available for answering the questions. Since spontaneous spoken discourse is essentially ephemeral, research depends on the availability of accurate transcripts. But such availability depends in turn upon the intermediate step of transcribing, which is itself an example of a very specific type of language use and can be problematic. In a study of transcription, O'Connell and Kowal (1994, p. 129) have analyzed transcripts made by transcribers in various settings. One such transcript was made by a native speaker of and teacher of German. The transcript had been prepared for an American colleague, a non-native speaker of German, who was also a teacher of German. The transcript was intended for use in his German classes in America. Without advertent to or informing the researchers of her decision, the transcriber corrected errors and redundant repetitions as she transcribed:

Her expressed, self-instructed purpose was to produce a transcription of correct German for language instruction, and she described her procedure as involving deletion of much that was colloquially repetitious. Interestingly enough, her transcriptional principles violated the original instructions given to her by the American professor of German who had collected the data.

In this instance, the original purpose of the data collection – access to genuinely colloquial German for American university students – was actually violated and prevented by the well intentioned transcriber. In doing so, she made her transcripts excellent examples of what we have referred to as the written language bias (Linell, 1982, 2005). Her adherence to the principle that spoken discourse must be transferred to paper as well-formed sentences dramatically changed her principles of transcription from those she had been instructed to use. But she was unaware that she was complying with such a bias. Hence, her performance in this setting is evidence that the written language bias is not at all limited to the discipline of linguistics or the broader area of language and communication sciences; it affects literate people as such, most of whom have had little or no contact with linguistics, and it becomes criterial for their

judgments of the proper usage of language on paper (for a similar observation in anthropological fieldwork, see Urban, 1996, p. 27ff.).

The example serves also to bring us closer to the topic of this chapter, the empirical methods that characterize mainstream psycholinguistics on the one hand and a genuine psychology of language use on the other.

The Experimental Method

Psycholinguistics has its historical roots in both the experimental psychology (general psychology or in German *Allgemeine Psychologie*) of the nineteenth century and the behaviorism of the twentieth century. Experimental psychology and the behaviorist tradition both set a pattern of laboratory research in which careful control of extraneous variables and meticulous, systematic variation of relevant variables are essential. This tradition has led to a severe limitation of research to what could be thus subjected to laboratory conditions. In its most objectionably extreme form, it has led to the limitation of investigation to what can be most easily engaged rather than what is of the most relevance to human life. Ebbinghaus (1885/1992) has provided the prototypical example of this extreme: The very concept of rote learning as well as Ebbinghaus's use of nonsense trigrams to elicit such learning totally disregard, respectively, the nature of human learning and the meaning to be found in the most nonsensical of such materials. And despite Skinner's unsuccessful effort to return to the paradise of *Walden two* (1948), behaviorism's reduction of human learning to conditioning has also ended in a thoroughgoing irrelevance of his laboratory research to genuine language use. More recently, Foppa (1994, p. 147) has noted critically that one reason for the neglect of dialogue by psychologists "has certainly to do with the discipline's almost obsessive fixation on the experimental paradigm and with its belief that only by experimental methods can scientifically valid results be obtained."

Fellow Travelers of Psycholinguistics

What were added to these traditions by the psycholinguists of the mid-twentieth century included transformational grammar and a concomitant dependence on linguistics for experimental hypotheses and for underlying principles, along with an emphasis on information transfer and the computer instantiation of such as a model of human communication and the psychological processing of language. And all these problems contributed to a *Zeitgeist* that was very much associated with Noam Chomsky's and several other key researchers' charismatic leadership and visibility. Prominent among these researchers were George A. Miller and many of his colleagues and students at the Harvard Center for Cognitive Studies in Cambridge, Massachusetts.

An Example of Experimental Psycholinguistics

Allow us to analyze as exemplary of these various influences on methodology a well known experiment that has been cited in the literature many times since its publication, namely Levelt's (1981, 1989) linearization research, a conceptually simple experiment in which the subject describes aloud for audio recording a visually presented network of lines and colored nodes. As experimental laboratory research, it is clearly traditional. It is also in a tradition that Levelt (1989, p.1) himself has referred to as a cognitive/informational point of view that considers "the speaker as a highly complex information processor." Furthermore, the tradition is one in which the language system of the speaker, his or her "cognitive skill," rather than the speaker as such, is of interest: "a reasoned dissection of the system into subsystems, or processing components."

But methodologically, of even greater significance is the artificiality of the experiment itself. After the instructions, no further questions are entertained. Since the experimenter who is addressed by the experimental subject has already seen the visual materials being described, and since the "next subject" for whom the experimental subject is putatively articulating a description does not exist, there is no genuine recipient of the communication – it is all a fabrication that the subject is asked to accept and go along with. Nor is there any clear intention evident on the part of the speaker, any reason for him or her to speak, other than the request of the experimenter that he or she do so. Furthermore, without knowledge of the real purpose of the experiment, the experimental subject has no way of knowing how to address effectively the apocryphal "next subject." In other words, in a normal engagement of two people in spontaneous spoken discourse, the speaker would know *why* he or she is telling something to a listener and *how* the listener is expected or intended to use the description; and the listener in turn would give some kind of feedback, verbal or nonverbal. This gradual increment to the listener's knowledge should dictate radical differences in the speaker's description. The point may be further clarified by an example: If one is standing in Times Square, New York City, the question "How do I get to Central Park?" is clear enough; but if one is standing on the deck of a cruise ship in the North Sea, the same question would likely elicit some counter questions, perhaps along with laughter: "Starting from where?"; "By foot?"; "In this kind of weather?"

It should be carefully noted that all the experimental details described above are thought of in the experimental tradition precisely as proof that careful controls have been established. Levelt (1981, p. 309) has felt that it was sufficient to instruct the experimental subjects "to describe the figure in such a way as to enable the next subject to correctly draw it on the basis of the tape-recorded description." O'Connell (1992, p. 60 f.) has raised a number of specific objections to this experimental setting:

An experimental subject now begins to speak. Do we know what his or her intention is in speaking? No, we do not. We know only the demand characteristics of the instructions. To make this clear, let us assume that, for every successful description on the part

of a “next subject,” \$1,000 is to be shared evenly between the subject and the next subject. How would the subject best optimize this business enterprise? A prudent subject would clearly wish to know what “on the basis of the tape-recorded description” means. Does it mean that the next subject will have to execute a correct drawing: (1) *during* the playing of the tape-recording; (2) after only *one* playing of the tape-recording; (3) by some time *deadline*; (4) some combination of the above; or (5) with no limiting conditions? Of course, we all know realistically that there is no “next subject.”

The reason this little scenario seems so strange is not at all that the questions are absurd, but that the trivialization of speaking in such experiments is so common. If intention is really important, then we must be concerned about why a person speaks – to what end. Speaking because an experimenter has asked me to is indeed an intention, but not an exclusive nor an adequately determinative one. Intention is, in fact, simply neglected in the linearization experiment.

Finally, quite in keeping with the neglect of intentionality, the visual materials are patently trivial and meaningless, not even a puzzle that might be intellectually challenging to the experimental subject.

This is, of course, not the way human beings use language, except, as stated, under the most unusually artificial and demanding conditions. In addition, if indeed such a tape-recording were actually to be used by a “next subject” listener, the prosody of the spoken description would become of primary importance, simply as a transmitter of clarity and precision. But Levelt’s failure to analyze prosodic variables indicates that not even he considered the experiment realistically relevant to the pragmatic application described in the instructions. All these elements of artificiality become very evident if one begins to imagine how the experiment would change if there really were a “next subject” whose successful redrawing of the visual materials would actually yield a substantial monetary reward or would have some other impact on the outcome of a conversation or on the completion of some practical enterprise beyond the conversation.

And yet, this research has been seriously mined for evidence on how speakers edit their speaking. Suffice it to say that the generalizability of such evidence cannot go beyond the artificial conditions of the experiment itself. A realistic situation would have made the description and its attendant editing serious elements of a genuinely communicative situation that was expected to make some difference in the life of the “next subject.” Such an expectation would grant the whole procedure a truly legitimate intentionality. Unfortunately, one would then have to expect that the description and editing would both be quite different from the results reported and interpreted so generally for this experiment.

The Demand for Continuity in Speaking

It is our conviction that the ultimate rational for Levelt’s experiment is his implicit theoretical demand for continuity. It is obvious that repairs and editing do indeed occur in spontaneous spoken discourse. A problem arises, however,

when they are made into an evidential base for the continuity required by the ideal-speaker theory. Taylor (1997, p. 61 f.) has rejected this inference regarding the need for continuity:

Without the notion of discontinuity as an error in speech – a notion rhetorically derived from scriptist premises – 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, however, has been taken to include precisely those features of a speaker's performance which would not occur in the continuous spoken performance of a practice 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.

Scriptism thus appears to be Taylor's rendition of Linell's (1982, 2005) written language bias. The fact that there can be no such thing as a pure case of continuity or fluency in human speaking makes Taylor's argument the more cogent. There is no reason to penalize what Heinrich von Kleist (1806/undated, p. 975; our translation) has referred to as "the gradual working out of one's thoughts in the process of speaking." This gradual process – with all its stops and starts – is the only way human beings can possibly engage in spontaneous spoken discourse. Chafe (1980b, p. 171) has another way of referring to discontinuity in speaking. He has quoted William James (1891/1981, p. 243): "Like a bird's life, it seems to be made of an alternation of flights and perchings." In passing, one might note, as the quotation clearly indicates, that James's famous continuous stream of consciousness applies only to thought, not to speech: Speech is of its very nature *intermittent*.

Methodology in Psycholinguistic Textbooks: The Relationship of Data and Theory

Another way of looking at the problems of method and methodology in mainstream psycholinguistics is to investigate how these two concepts are used in standard textbooks. In both the classic textbook (Clark & Clark, 1977) and in more recent textbooks (e.g., Carroll, 2004, 2007; [Cutler, 2005 has no subject index]; Field, 2003; Harley, 2001, and see 2008; Tartter, 1998), the concepts simply do not appear in the subject index. To judge from Harley's subtitle *From Data to Theory*, one starts with data and ends with a theory. How then are we to know what data to collect and what they mean if we have no proto-theory? In his chapter on language production, Harley (2001, p. 351) does use the terms method and methodology: "The methodology behind speech error analysis is a

simple one. The most common method is to collect a large corpus of errors by recording as many as possible.” But, such a methodology is only reasonably applicable along with at least a preliminary definition of error, based on an at least implicit preliminary theory. Additionally, Harley’s description does not actually fit with what is done empirically in the collection of speech errors. In fact, there are instead antecedent expectations based on a theory, and the theory is that of continuity in the ideal speaker’s speech production. What Harley, and the textbook writers generally, do is to present the evidence collected in investigations such as these speech error ones and to incorporate them into the current theorizing. By no means does the research begin, as claimed, simply with the collection of a maximum number of speech errors.

By and large, the textbook writers have bought into the *Zeitgeist*. There is a certain implicit common-sense certainty about scriptism or the written language bias. The well-formed sentence is a joy to work with, and the controlled experiment is king. The consequent neglect of field-observational research – where genuinely intentional language use is to be found in abundance – has been a great loss to a psychology of language use. The virtual ban on qualitative analytic methods as an adjunct to quantitative methods and to inferential statistics has also been disappointing. In short, the methods available to mainstream psycholinguistics have been curtailed by an underlying bias toward the literate, written mode, and toward the traditional approaches of the laboratory. A number of recent handbooks have provided further details regarding the investigation of language use: Ball, Perkins, Müller, and Howard (2008), Graesser, Hernsbacher, and Goldman (2003), Schifffrin, Tannen, and Hamilton (2003), and Traxler and Gernsbacher (2006).

Monologism

Because of its overarching relevance for mainstream psycholinguistics, it is important to segregate *monologism* as a major determinant of method and methodology. Monologism is simply the concentration on monologue as the principal source of empirical material for research on language use (see O’Connell & Kowal, 2003), along with an implicit generalization of findings to dialogue. It has indeed been dominantly characteristic of mainstream psycholinguistics from its beginnings in the mid-twentieth century.

There are certainly monological components in human language use. Shakespeare’s Hamlet and Joyce’s Molly Bloom both give their soliloquies. But both are somehow surreptitiously engaging in dialogue: Their discourse is intended by Shakespeare and Joyce, respectively, for the audience. One can argue that language use is in principle dialogical, and that even the most private monologues have an element of dialogical otherness and distancing from self as a self-dialogue. In fact, such is precisely the rationale for the establishment in 2006 of the *International Journal for Dialogical Science*. In any event, the vast majority of

speaking, writing, listening, and reading transpires among people who are somehow present to one another – either physically or only mentally; all of these genres of language use are dialogical, not monological. Hence, it is no less than astounding that the perennial and overwhelming bias of mainstream psycholinguistics has been monologue. In his current German psycholinguistics textbook, Dietrich (2002, p. 140; our translation) has acknowledged this bias as regards research on the phase of conceptualization in speech production: “All the findings have as their source observations of monological spontaneous oral language production. They do not tell us anything about the dynamic of communicative activities.”

To be sure, as with the written language bias, so too with monologism, the bias preceded the mid-twentieth century. Lazarus (1879/1986) was ignored in the late nineteenth century when he tried to introduce the study of conversation (*Über Gespräche*) into psychology. Instead, words (e.g., Cattell, 1886) and consonant–vowel–consonant trigrams (Ebbinghaus, 1885/1992) held sway in the laboratory setting. One can readily acknowledge, however, that “monologicistic psycholinguistics has indeed engaged dialogue, but precisely monologicistically, by simply concentrating on an individual language user as the entity of analysis” (O’Connell & Kowal, 2003, p. 195). Or as Foppa (1994, p. 148) has expressed it in his own critique of mainstream psycholinguistics: All social phenomena are explained therein “on the basis of one’s knowledge of the processes in the participating individual members. In other words, there is no other ‘entity’ of analysis than the individual person.”

A Monologicistic Approach to Dialogue

Part of the problem for mainstream psycholinguistics is the fact that it is indeed “extremely difficult to have any experimental control over normal conversation and this makes it difficult to investigate dialogues in a rigorous way” (Garrod & Pickering, 1999, p. 10). But the rationale given by Garrod and Pickering for this difficulty is not convincing: Rigorous experimental control is not the only way to gather scientific data; this is simply a traditional bias of experimental psychology inherited from the last century. In addition, as we have already mentioned in Chapter 1, there is an underlying bias against dialogue on the part of Garrod and Pickering: “The language of dialogue is disorderly compared to the straightforward grammatical sentences of monologue” (p. 10). These authors seem to have confused disorderliness with complexity – a complexity based on organizing principles far different from and far beyond the grammatical organization evidenced in unrealistic monological sentences. And so, they have concluded with their own methodological desideratum: a controlled syntactic well-formedness in dialogue. As for methods genuinely applicable to conversation and dialogue in general, the fact of the matter is that “genuine dialogue is never a simulated experimental task” (O’Connell & Kowal, 2003, p. 200). The collocation of monologism and dialogism within mainstream psycholinguistics

is reminiscent of Rex Harrison's famous line in the film version of *My fair lady*: "Why can't a woman be like a man?" (Warner & Cukor, 1964/1986). Indeed, why can't a dialogue be like a monologue?

Turn-Taking

The reader may find this section somewhat more fine-grained than the other sections of this chapter. One need not seek far afield for the reason: Turn-taking comes close to being the core concept of dialogue; it is in turn-taking that the interactive process between interlocutors actually takes place and is made observable. Hence, it is of supreme importance that the monologistic well-formedness principle becomes an issue once again as we confront the analysis of turn-taking methodology. Two assumptions must be rejected: (1) that the criterion for the success of a conversation is to be found in "the smooth interchange of speaking turns" (Cutler & Pearson, 1986, p. 139) and (2) that, insofar as conversation is "organized around establishing consensus" (Garrod, 1999, p. 392; see also Clark & Brennan, 1991), "the fundamental goal of dialogue" (p. 393) is consensus. At a more operationalized level of turn-taking, Wilson and Wilson (2005, p. 966) have recently provided "a mechanistic account of how timing is coordinated between conversational partners." The sleeper here is simply that they have used data that, according to the original researchers themselves (Wilson & Zimmerman, 1986, p. 384), contained "substantial measurement error," included 25% between-speaker silences that were "not between-turn silences," and disregarded overlaps and both successful and unsuccessful interruptions on the grounds that they did not occur at "transition-relevance places" (p. 379). These conversations were also elicited dialogues, about which Taylor and Cameron (1987, p. 52) have commented: "There is no guarantee that data obtained in this way is representative of talk produced in non-experimental contexts." The fact that smooth transitions (with or without a pause) sometimes account for less than 50% of the turns in dialogue (see, e.g., Suleiman, O'Connell, & Kowal, 2002, p. 277) makes the turn data of Wilson and Wilson (2005) even more problematic. And the fact that the "*projectability*" (Wilson & Zimmerman, 1986, p. 379) claimed for such transition-relevance places has been challenged as an unreasonable and unrealistic psychological expectation (see O'Connell, Kowal, & Kaltenbacher, 1990) also goes unmentioned. And so, the population of turns in their experimental corpus is not validly represented and not legitimately analyzed in Wilson and Wilson's research.

Methodological Individualism

The monologism characteristic of mainstream psycholinguistics is essentially asocial, and such concentration on the individual is indeed methodological individualism (see Foppa, 1994, p. 148; Clark, 1985, p. 179). It is gratifying to

read in Pickering and Garrod (2005, p. 85): “The study of dialogue provides a radically different conception of psycholinguistics from the traditional study of language comprehension and language production in isolation.” And yet, they still conceptualize dialogue in the traditional mode “as a largely automatic process of alignment between interlocutors.” And once again, for Pickering and Garrod, this alignment of “situation models” (p. 87) constitutes the criterion for the successful execution of a dialogue. In reality, however, there is nothing at all automatic about whatever alignment occurs in the course of genuine conversations. In fact, we ourselves have found the assumption of an automatic process of alignment as the finality of dialogue to be an ever present temptation in our own research.

The Need for Normalization of Data

Another challenge to the appropriate analysis of data has to do with counts of various response measures. Biber, Conrad, and Reppen (1998, p. 263) have emphasized the importance of “normalization” for the sake of comparability of counts. In other words, the comparability of counts of response measures from one corpus to another depends on normalization whenever the corpora are of varying lengths (written or spoken, in syllables) or durations (spoken, in seconds). For example, Lakoff (2001) wanted to compare George W. Bush and Al Gore on the use of plural second-person pronominals in their speeches. She was unable to make any exact comparisons even within her own database because she failed to normalize her data. It should be noted that mainstream psycholinguistics does not generally violate this requirement of normalization of data; however, the same cannot be said for all the sciences that deal with language use. The danger arises particularly when corpora from field observational research are to be analyzed.

Access to Corpora of Spontaneous Spoken Discourse

Taylor and Cameron (1987, p. 15) have claimed: “It is a relatively straightforward task to collect conversational data, and that data lends itself to statistical analysis particularly well.” Would that it were true! The Scylla and Charybdis of surreptitious audio recordings on the one hand and in-your-face microphones on the other are omnipresent. The former is unethical and the latter distorts the data. This is the basic rationale for our own turning to media discourse, since it is of the best acoustic quality, is in the public domain, is characteristically about nontrivial matters, and is spoken by articulate, intelligent, public figures. One must, however, be alert to the danger of over-generalizing media discourse to other, more informal types of spontaneous spoken discourse.

Recordings of media discourse still pose challenges of transcription: The more vigorous and spontaneous (and correlatively, the more interesting) they become, the more difficult they can become to transcribe. Very rapidly enunciated particles, spoken in sub-second durations, very softly, and overlapping another interlocutor's speech, can at the same time be of great importance. Examples of methodological difficulties with transcription of rapid spoken discourse can be found in Redder and Ehlich (1994; for a critical review, see Kucharczik, 1996) and in Schegloff (2007; see also our Chapter 16). Both publications provide the audio recordings used for their transcripts. Re-analyses of samples from these audio recordings have suggested that the published audio/transcript correspondence is deficient, partly because the original audio recordings are of poor quality. Additionally, the use of an interlocutor's entire body as a communicative tool lends nuances and sometimes even negates what is being articulated verbally. Hardly a straightforward task! Nonetheless, this is where the action is.

Use of Transcripts Prepared by Others

Finally, an important methodological problem can be pinpointed in Clark and Fox Tree's (2002) research on using *uh* and *um* in spontaneous speaking. They have made use of the London-Lund corpus (Svartvik & Quirk, 1980, p. 74) to hypothesize that *uh* and *um* are "conventional English words" (Clark & Fox Tree, 2002, p. 73) with basic meanings that signify a speaker's intention to initiate, respectively, a minor or major delay, i.e., to introduce a shorter or longer pause. The problem arises insofar as Clark and Fox Tree did not measure the physical duration of pauses after *uh* and *um* in the corpus, but relied on the professional coders in Svartvik and Quirk's analyses who perceptually identified duration units. Or as Clark and Fox Tree (p. 81) have put it: "So it is ultimately the *perception* of pause length and prolongation that we are studying here." O'Connell and Kowal (2000) and Spinos, O'Connell, and Kowal (2002) have physically measured the durations of pauses following both *uh* and *um* in samples of the London-Lund corpus and found no evidence for the correctness of Clark and Fox Tree's hypotheses. In addition, O'Connell and Kowal (2005b) have replicated Clark and Fox Tree's investigation of pause durations after *uh* and *um* with a corpus of media interviews by Hillary Clinton and found the same negative results. Because of a multitude of both false-positive and false-negative identifications, perceptually identified silent pauses do not accurately reflect the actual physical occurrence thereof (these issues are discussed in greater detail in our Chapter 13).

There is quite another way in which transcripts prepared by others are made use of in research and teaching. Transcripts from published research projects are frequently reproduced in textbooks and in further research publications for purposes of comparison. O'Connell and Kowal (2000) compared 41 original transcript excerpts with the derivative reproduced transcript excerpts and found an extraordinarily high rate of change in the reproduced excerpts. Kitzinger

(1998) has claimed that such an unusually high rate is the result of carelessness on the part of researchers. We are of the opinion that it is a consequence of the very high density of transcript notations, which puts undue pressure on the ones responsible for the reproduction of the transcript excerpts (e.g., printers and copy editors). What is perhaps even more surprising – and disconcerting – is the fact that many of the notations in the original or reproduced transcripts prove to be totally superfluous, insofar as only a few of the notations are actually used for analyses that are carried out on the transcripts. There is a very simple solution to this problem: It is for researchers to make use of only those notations that are intended to enter into their analyses of the transcripts. Transcribing everything from a spoken passage is impossible; transcribing everything one can find to transcribe is not at all scientific, but smacks instead of an effort to appear scientific with an abundance of impressive notations (for a more detailed discussion see our Chapter 10).

Back to Issues of Control

We began this chapter with an epigraph in which Dietrich (2002, p. 14; our translation) has described modern mainstream psycholinguistics as characterized by *experimental controls*. We wish to call attention here to a much broader sense in which the language use that we ourselves have subsumed under the category of spontaneous spoken discourse is controlled – not experimentally, but *situationally*. Our empirical work in recent decades has concentrated upon media discourse and, in particular, political interviews. It should be noted that this genre of human discourse constitutes only a tiny corner of the spontaneous spoken discourse that itself constitutes the vast majority of human spoken discourse. Furthermore, political media discourse, like all media discourse, has a number of built-in external *controls* that limit its scope and style. Basically, it is a setting that *demand*s speaking; one is not free to arrange an interview and then sit there while others do all the speaking and the manner of speaking is also limited: Shouting or whispering into a live microphone can be quite counterproductive; good conduct – e.g., the avoidance of vulgarity and obscenity if one is so inclined – is required; time limits are stringently constraining, even to the point where interviewers must interrupt interviewees in order to stay within time limitations; no allowance is made for extraneous forms of behavior such as catching a snack or greeting a passer-by in the studio; the agreement is to a dialogical encounter, but to a very peculiar one, in which the interlocutors speak with one another for the sake of a third party, a media audience, and the format is generally question and answer. All of this is in sharp contrast with what we have already discussed above under the heading of Bühler's (1990, p. 179) "*empractical* use of language signs" and what a recent German-language dissertation has referred to as "Knappes Sprechen" (Baldauf, 2002, p. 1) – incidental, intermittent speaking: It is highly situational, elliptical, deictic, concrete, and often quite idiosyncratic.

A typical example can be heard in the commentaries and reactions of someone watching TV, e.g., *wow, huh, good for her, yeah, I thought so, high time.*

The danger for ourselves as researchers is to forget that we are occupying this tiny corner, whereas the vast amount of spontaneous spoken discourse is quite other than political media interviews. At the same time, it would be a serious mistake to think that any genre of spontaneous spoken discourse exists that is *without* controlling factors. There is always some concrete situation in which an utterance must be embedded, and the specifications of that situation are always such as to control, in one way or another, what is essentially spontaneous spoken discourse.

To return for a moment to the concept of *spontaneity* in this context, one could well argue that the controls listed above exclude any sense in which genuine spontaneity is exhibited by political interviews. The point is well taken, but the stark contrast between reading aloud or reproductive speaking on the one hand and productive speaking on the other must be maintained: Political interviews clearly fall into the latter category (see Kowal, 1991). Furthermore, as we will see in our empirical chapters (11–18), the relative differences between the literacy of interviewers (who do indeed typically make use of written notes) and the orality of interviewees is quite notable. In any event, spontaneity must be conceptualized as a broad-ranging continuum rather than as a narrow-band distribution of qualities.

Concluding Remarks

At mid-twentieth century, the new mentalists were convinced that the sterility of behaviorism was about to be overcome, and that a whole new world of psychological breakthroughs was at hand. Costall's (1991, p. 163; cited in Linell, 1998, p. 58, footnote 8) comment on such triumphalism is telling:

Cognitive psychologists have perhaps been too busy congratulating themselves on not being behaviourists to notice that they themselves treat people as machines (Skinner 1974: 110; Morris 1991). The mechanistic scheme, and computer metaphors in particular, lead us to regard the problem of cognition as nothing other than the internalized re-presentation of the environment.

In the same footnote, Linell (1998, p. 58) has referred back to his own earlier comments, in which “Linell (1979) points out that Chomskyan mentalism is not incompatible with a behaviorism that builds upon internal mediating variables.” Such is the psycholinguistic methodological legacy out of which a viable psychology of language use must be formulated anew, i.e., with an emphasis on a *genuinely psychological* study of language use. We are well aware that the present volume can only call attention to some of the directions needed to construct a psychology of language use that will prove to be a comprehensive investigation of oral communication with one another – spontaneous spoken discourse. The edifice is yet to be constructed.

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