access, the use of stem access codes would be revealed by effects of stem frequency on lexical decision time for the whole words (see Burani & Caramazza, 1987). Although the techniques used in psycholinguistics are generally well thought out, many of them can and have been criticised. The major problem is to make sure that the technique taps into the correct subsystem of the language processor. For instance, although it is logically possible to decide that a letter series is a word (lexical decision) without consulting its meaning (i.e. solely on the basis of contacting the word's access representation, being the equivalent of a dictionary entry), it may be impossible for the language user to make a decision at that point in processing. Or take another example. Although it is correct that having subjects pronounce a word (and measuring the time it takes to initiate a pronunciation) avoids the unnatural decision component of other tasks, it introduces the problem that subjects may bypass the lexical representation and generate the pronunciation on the basis of grapheme-to-phoneme rules.

As a result of problems such as these, some researchers think that the techniques for measuring aspects of mental events should be made more natural. Their point of view is that the best way to measure aspects of language processing is to set the machinery in operation in the most natural way possible. In practice this means that one should be reluctant to ask subjects to perform operations on language which they normally do not perform (e.g. make explicit decisions of a certain type). This critique has given rise to at least two alternative paradigms in psycholinguistic research: eye movement research in reading and research using electro-related potentials (ERP research). The important point to be made in the present context is that in both an attempt is made to correlate naturally occurring events (eye fixations and the electrical activity of the brain) with aspects of language processing. As is to be expected of a young field of research, psycholinguistics is still looking for the best research tools it can find, to get at those untangible entities and events in the mind of the language user.

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[*See also:* Statistics]

FIELDWORK

Gunter Senft

The Compact Oxford English Dictionary (1991 ed.) defines 'fieldwork' as "a comprehensive name to describe the practical side of research in archaeology, linguistics, the social sciences etc., carried out in the areas concerned, as distinguished from theoretical or laboratory investigation".

In the disciplines mentioned, but also in anthropology, in anthropological linguistics or ethnolinguistics, in dialectology, in sociolinguistics, in branches of linguistic pragmatics, in sociology, social psychology and in behavioral ethology, 'fieldwork' is the cover term for a complex of methodological approaches to data gathering. The term subsumes a variety of ways of collecting data 'in the field', such as elicitation, interviews, and participant observation where the observer interacts with the observed. Thus, fieldwork typically involves the presence of the researcher among what s/he has defined as her/his subject: society at large (whatever this may be), a group of people, a speech community, a restricted category of well-specified individuals

serving as 'consultants', etc. Ideally, the researcher is physically present in her/his field. There are certain cases, however, where either some technical apparatus, as e.g. recording equipment that is used for data gathering purposes or a trained local consultant who is collecting data for the researcher, may serve as substitutes for the scientist (thus making her/his presence indirect, so to speak). That the researcher has to be extremely careful in selecting this local consultant is a point which will be discussed below. As already indicated by the list of disciplines where fieldwork is done, it must be mentioned that fieldwork can also include forms of unobtrusive observation in which the observer, e.g. the archeologist, collects data without interaction with any kind of (animate) participants at all. Finally, it should be emphasized that this methodological approach is hardly ever used as the sole source of information for the scholar. It is mostly combined with other forms of data collection such as experimentation, archive research, corpus analysis, etc.

Disciplines such as archaeology, ethology, psychology, and sociology can certainly (and proudly) refer to their pioneers with respect to fieldwork done, and field-research methods used. However, by far the most, if not all, modern forms of fieldwork in these various disciplines owe much more to both cultural and social anthropology and anthropological linguistics with respect to the development of this complex of methods than to any other discipline within the social sciences (Goldschmidt 1976). Thus, besides the early demands for fieldwork or field research and participant observation as methods of data gathering in dialectology by researchers like Johann A. Schmeller (who referred to his 'field-trips' to remote valleys in the Alps as *Kundfahrten*, i.e. 'reconnaissance trips') in the first half of the 19th century, it was the pioneering fieldwork of anthropologists that promoted this method and approach especially within the social sciences. Probably the most important advocate of fieldwork was the anthropologist Franz Boas who undertook his first one-year-long scientific expedition to Baffin Island in 1883-84. All his students, including B.A.L. Kroeber, R.H. Lowie, P. Radin, and especially E. Sapir, did fieldwork themselves and established this method in American anthropology. The famous Cambridge Anthropological Expedition to Torres Straits, organized by W.H.R. Rivers and A.C. Haddon, the 'Südsee-Expedition der Hamburgischen Wissenschaftlichen Stiftung 1908-1909 im Bismarck-Archipel' (Expedition to the South Seas of the Hamburg Scientific Foundation in the Bismarck Archipelago in 1908-1909), the research on the Andaman Islands by A.R. Radcliffe-Brown, and especially the exemplary studies of the Trobriand Islanders' culture by Bronislaw Malinowski (see e.g. Malinowski 1922, 1936), but also scientists like M. Gusinde, P. Schebesta, O. Raum, and R. Thurnwald document a similar development with respect to the importance of fieldwork in British and German anthropology. The concept of fieldwork-based ethnography, developed in anthropology during the first half of our century, had strong influences on ethnomethodology (Garfinkel 1967) and the ethnography of speaking/communication framework in (socio-) linguistics (Gumperz & Hymes 1972; Hymes 1964, 1978).

To emphasize it once more, fieldwork in general is a *complex of methods* used for data gathering in the social sciences that subsumes and incorporates a broad variety of means for data collection — both in Western and in Non-Western societies (the latter of which are often referred to — from a somewhat ethnocentric point of view — as 'exotic' societies). It goes without saying that methods of fieldwork vary according to the specific disciplines; however, the target of fieldwork is more or less well-delimited in each case: to collect a set of 'natural' data, which then serve as the material for various kinds of analyses. Moreover, although the situations for, and of, fieldwork in the various disciplines are certainly different and do vary, the basic problems the researcher encounters and has to overcome seem to be quite similar to those problems anthropologists and anthropological linguists have to face and solve when they are doing fieldwork. Comparing this author's experiences doing sociologically and sociolinguistically oriented fieldwork with Italian, Spanish and German workers in Heidelberg (Heidelberger Forschungsprojekt 1977), social-psychologically, socio- and psycholinguistically oriented fieldwork

th German metalworkers in Kaiserslautern (Senft 1982), and human-ethologically and thropological-linguistically oriented fieldwork with Trobriand Islanders in Papua New Guinea enft 1986, 1992), differences become really marginal. Therefore, in what follows, fieldwork will discussed mainly from the anthropological and (anthropological-) linguistic point of view ıough this may result in some form of bias).

Anthropological and linguistic fieldwork requires first of all the researchers' participant ıservation in the (speech) community and in the culture they want to investigate. Linguists and ıthropologists can best study and describe a language, a language variety, a culture, or a bculture if they live within a city, a suburb, a village etc. where they can learn as much of the cial customs, the language and language use of the people as possible.

Especially for linguists, a good understanding of language use in its social context(s) turns out be extremely important because the social context(s) of language use directly affect(s) aspects language structure. The definition of their roles as participant observers is the result of ongoing teractive processes between the researchers and the communities in which they live and work. the researchers work in communities with relatively great cultural and linguistic distance to the ımmunities from which they come, the scientists have to pass a (more often than not difficult) itial phase of a first orientation in their new environment. The researchers' main aim must be get accepted — and somehow 'socialized' by their new communities. Especially in Non-Western ıltures it may help if researchers can show photographs to the members of the community with hich they want to live that document their personal (family) ties and aspects of their way of ing within their own (Western) culture and community. During this phase fieldworkers start to arn the language, try to live within, and understand, the new, the 'other', pattern of life, and ake first contacts with the people speaking the language and living (in) the culture they want ı investigate. As a rule of thumb, the greater the cultural and linguistic distance between the searchers and the communities under investigation, the longer the fieldwork should last. Where new language and culture must be learned, fieldwork should be done for at least a year; ideally, ıe researchers should stay in their respective fields for a longer period of time and also return ı their fields after periods of data analysis and phases of planning further research on successive eld trips.

After this first phase of orientation — which is necessarily open and rather 'unstructured' and ıus asks for the researchers' abilities to respond and react 'pragmatically' and spontaneously to ıeir new life situation — there usually comes a more exploratory, and also a more structured, hase where the researchers are now in a position to start their documentation of the respective ıltures and languages. It is in this phase that linguists may start with pattern elication and where nthropologists start with their routines of data gathering such as taking a census and recording enealogies. In the beginning of this phase, all researchers are almost completely dependent on ɔ-called 'main consultants' whom they should select very carefully. This selection of the main ɔnsultant(s) can be a rather delicate matter. There are societies where it is considered improper a consultant — and especially if the main consultant — is the opposite sex to the fieldworker. loreover, it can be extremely difficult for both the researcher and the consultant to change their ooperative relationship if it turns out that there are better candidates for the role of main onsultant than the one the researcher had chosen first. Before fieldworkers in such a situation ctually change their main consultant(s) they must know whether this switch will be regarded as n acceptable act in the community or whether it will be interpreted as social rejection or even rebuff. Good main or primary consultants should be interested in teaching the fieldworker omething about their language, language use and culture. They should be accepted within their wn community. They should be deeply rooted in their own language and culture, but at the same ime they should be so open-minded that they can cope with the misunderstandings caused by lashes between their own experience and the fielworkers' different social backgrounds.

To overcome these misunderstandings requires mental alertness, a communicative and also a somewhat extrovert personality, and a general feeling of mutual sympathy in both the fieldworker and the consultant. If there is a contact language in the area where the fieldwork is conducted, it is quite helpful if the main consultant also has a good command of this language (if the fieldworker does so, too, of course). However, researchers have to be very careful in not selecting someone as their main consultant who speaks this contact language, but otherwise has a rather 'marginal' status within her or his own community. The longer researchers live in their fields the less dependent they become on these first main consultants. The linguistic and/or anthropological information which they can then gather with the help of more and more consultants offers a somewhat more representative sample and data corpus for the scientists' specific research interests. However, fieldworkers should always keep in mind that their sex may open up or completely close down certain roads of special cultural information. It has been my own experience during my fieldwork on the Trobriand Islands that it is best to be in the field as a male and female team or even as a family. In this way, one can gather more information about the different 'worlds' of male and female, married and unmarried than one could hope for alone in the field.

The longer the researchers are in the field, the broader will, and can, be the variety of data gathering techniques employed to reach their research aims. Interview and elicitation techniques can become more and more sophisticated, it will be possible to get more natural data by documenting (on film and/or audio- and video-tape) everyday or ritualized social interaction patterns. To a certain extent, field research can now also become even 'experimental'. It is at this stage that researchers can rather highly structure their fieldwork in accordance with their respective research interests.

Data gathered in the framework of a primarily linguistic research project, however, should (ideally) always encompass as many different types of text as possible. In linguistics, elicited data certainly help to answer a number of specific questions. Elicitation methods are characterized by standardized formats of interaction between the researcher and the consultants: usually, the researcher asks a number of her/his consultants to answer standardized questions, to perform standardized tasks, or to solve standardized problems that are targeted at precise data types or themes. However, as Duranti (1981: 9 and 162ff) points out, elicitation sessions are speech events that as such influence the kind of language used. Therefore, the data collected should also include narratives about personal experiences, conversations of people in face-to-face interaction, legends, myths, songs, poems, nursery rhymes, and other forms of oral literature and verbal interaction. Thus, ethnographic methods, characterized by 'natural' and spontaneous interactions documented among the consultants and between the researcher and her/his consultants should always and ideally go hand in hand with these elicitation methods. Needless to say, these two methods constitute the two main poles around which fieldwork in pragmatics revolves. It is usually best to transcribe the gathered data in the field, first with the help of consultants, later under the control of consultants: even if it is possible to transcribe some texts alone back home in one's study, it is too often just too easy to miss something while transcribing like this.

All the data gathered during the researchers' fieldwork form the linguists' or anthropologists' corpus on which they rely for their description and analyses of the respective languages and cultures. It goes without saying that the fieldworkers' data collection which finally results in this corpus is always guided, if not governed, by theoretical considerations which clearly state what purposes the corpus should serve. However, fieldwork certainly is the area in the social sciences where researchers can experience the dialectics between theory and practice to its extremes. On the one hand, the practice of the field constantly forces researchers to reshape, criticize and reformulate their starting hypotheses and theories — on the other hand, it is these hypotheses and theories that help the fieldworker to not get 'drowned' in a 'sea' of sheer data.

It is exactly here that fieldwork as one of the main sources of data-collection in the social

sciences has to face criticism — especially with respect to its epistemological and methodological status (see e.g. Ellen 1984). In what follows I exaggerate 'arguments' to illustrate the extremes of the conflicting positions. For many fieldworkers criticism of their research comes especially from those theoreticians (fieldworkers often refer to them as 'armchair' anthropologists, linguists, or sociologists) who abhor the idea of wading through the morass of empiricism which may demolish their neat theories developed without contradiction and safeguarded by hardly vulnerable immunizing strategies. These theoreticians often denounce fieldworkers as being adventurers who flee their own culture (or laboratory or library) of which they have become estranged and then return from their romanticized 'field' with highly subjective data that claim scientific status though they can neither be falsified nor verified by others in a repeat study. Because some of the results the fieldworker reports are — necessarily — based on subjective impressions, this fact is used by those critics to question the use of fieldwork and all its results in general. They argue that fieldwork is an extremely personal experiment which cannot be reproduced and thus cannot claim having any kind of scientific status. Moreover, this subjectivity is said to leave the fieldworkers' theory formation in a 'mysterious twilight' (see e.g. Kohl 1979). However, most of these critics simply ignore the fact that fieldwork is not just one method, but — as emphasized above — a complex of methods researchers use for data collection purposes. Although fieldwork — in any discipline and in any field — is certainly a deeply personal experience, many of the specific methods can now be reproduced, falsified or verified. Moreover, by describing the conditions and situations in which fieldworkers gathered the data on which they base their analyses, they have no problems to come up with the methodological standards of their respective discipline. Many of the usually polemic discussions between fieldworkers on the more practical, empirical side and theoreticians on the more theory-dominated side are useless and void. Good theory needs practical proof, and good empirical research, good fieldwork is impossible without good theory.

By now, I assume, it is quite evident that fieldwork requires the researchers' familiarity with a broad variety of data collection techniques (see, e.g., Foley 1991; Mayntz et al. 1976; Ruoff 1973; Samarin 1967). Moreover, fieldworkers have to be familiar with systems for transcribing data and especially with the various machines (and at least some of their technical intricacies) that can be used for data documentation (like various kinds of tape recorders, film- or video cameras, microphones, etc., see Goodwin 1993).

However, the planning phases of much fieldwork also ask for the researchers' abilities to interact with bureaucratic institutions and their representatives to obtain research permits. Every field trip should be planned as carefully as possible — and researchers who plan fieldwork in foreign countries must first inquire whether or not these countries allow research without a special permit. It goes without saying that wherever a research permit is required, it must be requested — although the processing of this request may be the first test for the researcher's patience and perseverance. Most national linguistic, sociological, psychological, archeological, and anthropological associations can provide first useful information with respect to these questions. Contact with the respective researchers' national embassies or consulates in countries where they plan to do fieldwork is also extremely helpful. If fieldwork is planned in countries that can become problematic for the researchers' health — because of extreme climate zones or deseases like malaria — good medical information, all the necessary vaccinations, an excellent personal medicine kit for the field — especially for more remote areas — and some practical medical knowledge are a must (for basic information see e.g. Werner 1990).

Finally, fieldwork does not only ask for the researchers' physical fitness, it also requires a psychologically balanced and stable personality. The researchers' psychological stability is first tested — especially when alone in a rather remote field — when they have to deal with possibly very different local concepts of behavior, hygiene, time, privacy, etc. This initial phase of fieldwork

is often described as 'culture shock' — and every field researcher will experience some variety of it. Overcoming this culture shock requires a basic willingness to understand, and to learn something about, the 'other'; it requires patience, but also the researchers' marking of their unequivocal position with respect to the question of how far they are willing to immerse themselves into the participant part of their observation. On the one hand, there is a danger that fieldworkers remain too distanced and detached in their field and establish a subject-object relation between themselves as the observers and the 'others', the observed in their field. On the other hand, if the researchers identify themselves too much with the community under investigation — maybe as a strategy to overcome their culture shock — they are in danger of giving up the distance which is necessary for any scientific description that claims to be objective. In its extreme, this identification with the community under investigation results in a situation that is called 'going native'. A researcher 'gone native' is a kind of living oxymoron. As soon as researchers have gone native it is impossible for them to do any kind of sound research.

But even back home from the field, the researchers' psychology has to pass many trials, especially with respect to their ethical commitments to the communities with which they work. Here researchers are more often than not forced to make decisions that directly affect the personal rights of their consultants. It is difficult to suppress some extremely interesting bits of information that may provoke a vivid discussion in one's academic peer group because their publication may violate one's consultants' trust in the confidentiality of the information. If in doubt, consider the other side of the experience. Just imagine a group of Papuas with penile sheaths, bows and arrows, and netbags, of course, coming to your village, town or suburb to learn something about your language and customs and document as much interpersonal interaction as possible with video- and tape recorders. How would you welcome these researchers from a different 'world'? Would you care to talk to them and teach them your language? What would you tell these Papuan researchers? What would you allow them to tape-record and film? What would you tell them confidentially after a longer period of time? And would you like to read about this confidential information if you happen to find it printed in a scientific journal?

To conclude, fieldwork covers a broad variety of scientific methods for data gathering. However, fieldwork is not only a methodological approach to the resolution of a problem of scientific interest, but also — and maybe first of all — an approach to our fellow human beings.

For additional information about field research I would like to refer the interested reader to Agar (1980), Barley (1983), Boas (1911), Bohannan (1964), Bouquiaux & Thomas (eds.) (1976), Dixon (1984), Healey (ed.) (1975), Howell (1990), Jahoda et al. (1933), Lehmann (1976), Malinowski (1967), Nida (1949), Powdermaker (1966), Rubinstein (ed.) (1991), Sapir (1921), Wilkins (1992), Whyte (1943), and Wylie (1957).

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[*See also:* Elicitation; Ethnography; Interview]

INTERVIEW

Charles Briggs

1. Introduction

Interviewing constitutes one of the most fascinating and most poorly investigated realms of pragmatic inquiry. One reason for its importance to the field is its ubiquity as a means of obtaining information. Practitioners in a broad range of disciplines rely on interviews. The widespread use of interviewing by journalists, providers of social services, physicians, and employers as well as the emphasis that politicians and corporations place on surveys point to the central role that interviews play in creating the institutional structures of modern societies. It is thus remarkable that relatively little research has explored the pragmatic underpinnings and effects of interviewing. This hiatus reflects the common assumption that interviews are relatively simple, straightforward, and well understood. The limited number of works that investigate the pragmatics of interviewing in depth rather point to its discursive complexity and to crucial gaps in our understanding. Practitioners may also be reluctant to subject interviews to too profound a critique in view of their efficacy as means of imbuing social scientific discourses with authority.

2. The construction of interviews and questionnaires

The major mode of structuring interviews discursively is the recursive use of question-answer pairs, often with follow-up Q-A sequences (generally termed 'probes'). The participation framework of interviews is likewise organized around a central, asymmetrical opposition: the interviewer asks the questions, the respondent answers them, and the interviewer then signals when s/he considers the response adequate (especially by asking a new question). Formal or structured interviews are pragmatically distinct from informal, unstructured ones. The former involve the use of a predetermined set of questions, and their presentation by an interviewer is standardized as much as possible: questions are to be read as printed and presented in the same order. The standardization of responses may be maximized through the use of closed questions in which the interviewee must choose between preselected alternatives. In survey interviews, professional social scientists write questions that reflect their research interests and chosen methodology. The researchers then hire a staff of interviewers, who are generally not social scientists, instruct them in how they are to present questions, and assign lists of interviewees that

are produced by sampling techniques. Formal interviews are thus structured by an absent party, one who also controls rights to interpret the discourse, a fascinating sort of ventriloquism (see Haraway 1992). The use of written questionnaires that are completed by the respondent standardize the process of obtaining information by dispensing with a face-to-face interview.

Informal interviews are generally conducted by researchers themselves. While lists of questions are often prepared in advance, exact wordings and the order of presentation emerge in the course of the interview; as a result, the discourse produced in such settings is often structured more by the social interaction — as guided by the researcher's interests — than by discursive constraints imposed by predetermined questions. Since the range of possible responses is less constrained, respondents are often invited to use a wider range of discursive forms (such as narratives). Unstructured interviews are generally associated with qualitative research and structured interviews using closed questions with quantitative approaches; the work of Labov (1972a, 1972b) and other sociolinguists suggests, however, that open questions posed during relatively unstructured phases of an interview may also be analyzed quantitatively.

3. Research on interviewing

While sociologists have long taken the lead in focusing attention on the problematics of interviews (see König 1966; Hyman et al. 1954; Manyntz et al. 1969), much of this research has been limited to attempts to identify sources of 'bias' or 'distortion'. Such approaches assume that it is possible for the interviewer to constitute, at least in ideal terms, a means of obtaining the 'individual true value' — the 'real' or 'unbiased' view — of the respondent (see Brenner 1981). Practitioners have also concentrated their efforts on attempts to increase the 'reliability' and 'validity' of interview data. Reliability refers to the degree of invariance that is achieved when the same procedures are repeated, while validity points to the accuracy of a given technique in measuring the phenomena in question. As Hyman et al. (1954) argued in a classic study, standard interview techniques are oriented much more toward reliability than validity; focusing so intently on reducing inter-interviewer variation creates a strong force for methodological conservatism.

Two sociologists who specialize in pragmatic approaches to language, Aaron Cicourel and Allen Grimshaw, offered trenchant critiques in the 1960s and 1970s. Grimshaw (1969, 1969-1970) criticized language ideologies that underlie sociological research in that they picture speech as a transparent vehicle for extracting ideas or attitudes from the mind of a subject and transmitting them onto the printed page. He emphasized the need to envision the role of language in research not only as an obstacle in the investigation of non-linguistic phenomena but as a crucial source of information on social processes. Cicourel (1964, 1982a, 1986) conducted in-depth research on the pragmatics of interview discourse. In landmark but, unfortunately, less often cited work on the use of interviews in documenting fertility practices in Argentina, Cicourel (1974) tape-recorded interviews and conducted follow-up interviews with interviewers and interviewees. He thus demonstrated the indexical links between the form and functions of questions and responses and the social and communicative dynamics of the social interactions that constitute interviews. He has also documented the importance of intertextual relations with discourse that emerges in other contexts in shaping the structure and content of medical interviews (Cicourel 1982b, 1992).

Once the pragmatic complexity of interview data is recognized, the assumptions that underlie such notions as bias, distortion, reliability, and validity becomes apparent. Mishler (1986) argues that researchers commonly see interview data as behavior that can be analyzed using stimulus-response models associated with scientific experimentation. The pragmatic reductionism of received interview practices also spring from Western ideologies of language (see Kroskrity et al. 1992; Joseph & Taylor 1990) that treat verbal interaction as a transfer of referential content from one party to another, as if participants had no interests or communicative foci that interfere with their playing the roles of interviewer and respondent (see Back & Cross 1982; Clark & Schober