ethnography will also be discussed in the respective chapter (see 5.22). With regard to the analysis of data, a number of developments may be discerned. In general, we may distinguish between procedures that use coding and develop categories, either in the tradition of Anselm Strauss (see 5.13) or of content analysis (see 5.12), from those that are in the hermeneutic tradition (see 5.16, 5.18, 5.20, 5.21). These procedures stand alone and may – with certain differences – be applied to all types of data.

In the case of interview studies we may ask how the available analytical procedures may be used for the data obtained. Concrete proposals have been made for semi-structured interviews (see 5.10) and narrative-biographical interviews (see 5.11), the former being more strongly oriented to a coding-categorizing procedure and the latter more to hermeneutic understanding.

The use of computers in the analysis of qualitative data (see 5.14) has become more and more widespread, and at present they are used particularly frequently in coding types of analysis.

In German-speaking countries there is a growing differentiation among hermeneutic methods (see 3.5): from objective and sociological hermeneutics, hermeneutic theoretical sociology (see 5.16) has developed. Out of conversation analysis (see 5.17) has come genre analysis (see 5.18). In this latter case, the term 'communicative genres' again comprises a broader understanding of data. Similarly, out of conversation analysis has grown discourse analysis (see 5.19), which has attracted particular attention in Anglo-American psychology.

The role played by the presentation of results (see 5.22) and procedures in qualitative research has recently been treated as a decisive step, in particular in Anglo-American discussion in the field of ethnography. This has led at the very least to growing awareness of the importance of modes of presentation of the results. Ultimately the making of discoveries in empirical science is often not only the result of a consistent and rule-governed application of methods. The art of interpretation (see 5.21) sometimes also involves the use of chance and openness to the unexpected as well as methodologically transparent and controlled theoretical speculation.

In general terms, the individual chapters contained in Part 5 of the *Companion* are ordered following the steps a researcher runs through in the process of doing research – from entry into the field, to data collection and transcription and ultimately to analysis and writing. On the other hand, there are an increasing number of integrated methods – for example, ethnography, film analysis or genre analysis – that cannot be unambiguously assigned either to collection or to analysis. In cases such as these, the collection and interpretation of data will be treated together in a single chapter.

Part 5A

Entering the Field

5.1 Ways into the Field and their Variants

Stephan Wolff

1	Field access – terminology and objectives	195
2	The way into the field and its thematization in the social sciences	196
3	Structural problems of access	197

1 FIELD ACCESS – TERMINOLOGY AND OBJECTIVES

It would be an error in dealing with the 'way into the field' to think in terms of a fixed boundary, the crossing of which provides the researcher with an open and unrestricted view of the interior of the field. For that reason, in what follows we shall speak not of 'entry' but 'access' to the field. This term not only makes more prominent the activity or process quality of the event in question, it also succeeds in avoiding a strict inside–outside distinction. By 'research field' we understand here naturally occurring social fields of action, as opposed to artificial situational arrangements deliberately engineered for research purposes.

Research fields may be public places, groups, social milieux ('scenes') but also organizations or tribal groups. For each of these research fields there are, from the researcher's point of view, *two fundamental questions*.

1 How can the researcher succeed in making contact with the chosen research field and in stimulating the informants to cooperate? If research is to become in any sense a social event, the involved representatives of the field should be ready, of their own volition, not only to take account of unfamiliar *demands*, which might include:

- making available time for conversations;
- partially giving up control of physical space;
- enduring embarrassment;
- facing up to communicative pressures (such as those that arise in narrative interviews);
- limiting one's own communicative needs (if they are subordinate to a semi-structured regime); and
- accepting the questioning of what has always been taken for granted;

But also *display a wide range of their own activities*, such as:

- putting themselves in the researcher's position (in order to be able to provide data interesting to him or her);
- informing the researcher about situational relevancies;

- smoothing the researcher's path and suggesting competent interview partners;
- answering questions they have never put to themselves, the meaning of which is initially obscure;
- trusting the researcher without guarantees;
- explaining to themselves and others what the researcher and the project are aiming at; and
- signalling that they are not disturbed, even though they know they are under scrutiny, and so on.

2 How can the researcher *position himself or herself* in respect of the field so as to secure the factual, temporal and social conditions to carry out appropriately the planned research, or at least not significantly inhibit relevant freedom of action?

There are no patent recipes as to how a way into the field should be sought and found. Furthermore, it is not wise either to invoke the illusion that everything can be planned or to complain about the unpredictability of the situation. It would also be a mistake to trivialize the question of access as a technical or psychological ground-clearing problem, with the real research beginning after it has been dealt with. For this reason one should look upon (and set up) the way into the field as a task that is never completed and which must be handled cooperatively, that is jointly with the intended 'objects' of the research. A preoccupation with the way into the field serves not only methodological or research-pragmatic purposes, it also yields insights into structures and sequences in the research as a social event, and into the field of action that is under investigation. The trial paths, detours and false trails that researchers often complain about and feel to be burdensome, and even the failed attempts at gaining access – which are normally carefully suppressed – all then become 'critical events', the analysis of which opens up chances of making discoveries.

2 THE WAY INTO THE FIELD AND ITS THEMATIZATION IN THE SOCIAL SCIENCES

Classic descriptions of access read like epics of heroism in which, after a phase of struggles and irritation, the researcher ultimate attains the 'Heart of Darkness' (*à la* Joseph Conrad) to which he or she aspires. The decisive moment of arrival in the field often takes on a particular stylization: perhaps an abrupt (positive) change in relations with the 'natives', an overpowering emotional feeling of arrival or a sudden revelation, as with the overturning of familiar ways of looking at things. This also defines simultaneously the precise moment at which the actual research can begin (particularly elegantly expressed in Geertz 1972; see 2.6).

The problem of access is first discussed as a *problem of the researcher* who desires access and who has to cope, in the process, with the resistance of the field, but also with his or her own psychic defence mechanisms in the face of uncertainties and irritations associated with access to a particular situation (cf. Lindner 1981). Proven interactive and/or psychological strategies are looked for that will put the investigator in a position to recognize and effectively neutralize all such problems (as an example of a collection of such recommendations, cf. Gans 1982). The posthumous (1967) publication of the research diary of Malinowski, the celebrated pioneer of participant observation (see 5.5), dealing with his time with the Trobriand people, led to a permanent lowering of expectations in respect of how far the field-researcher could substantiate 'being-there', and made it unavoidable that the problems of access should henceforth always be more thoroughly confronted – and not only in ethnology.

As a next step, the problem of access is reformulated as a *problem of relations*. Here the relation to *key informants* and their particular characteristics is brought into the foreground (Casagrande 1960).

Many of these key informants have achieved real fame in ethnographic research: among these we find 'Doc', who not only provided William F. Whyte with contacts in the Street Corner Society, but also accompanied him as a sort of coach; 'Don Juan', who apparently initiated Carlos Castaneda in the teachings of Yaqui magic; or Ogotomméli, who functioned as Marcel Griaule's wise conversation partner amongst the Dogon.

A strikingly large number of these key persons occupy the position of social outsiders in their own community, for example because of earlier intensive experience with outsiders or frequently also because of some particular personal quirk of fate. The problems for field access that might be found in the relation to such 'marginal men' was either treated as a transient problem

(which would solve itself when the research took on greater depth) or used positively by referring to the particular sensitivity of such persons and their competence as observers.

One further difficulty on the way into the field results from the fact that the *delimitation* of research fields in the areas of both ethnology and sociology seems increasingly questionable. For a long time field researchers proceeded on the basis that they could relate to isolated social entities on the periphery of (world) society or to clearly defined urban scenes, groups and organizations; that is, that the field to which they sought access could not only be identified without difficulty, but also be reconstructed as an independent cultural context. If we look at the true interrelations between these only apparently isolated entities and their environment we see, however, that it is impossible to draw unambiguous borders between them. The ethnographic view is also blurred by the fact that field research has increasingly to do with its *own* culture, and with phenomena that tend to be situated in the area of *social normality* (see 3.8). The growing *lack of strangeness*, together with an increased awareness of the way in which ethnographic exoticism functions, makes the use of the strategy of 'methodological alienation' (Hirschauer and Amann 1997), which is typical of ethnographic studies, appear to be simultaneously both acceptable *and* questionable. From this there develops an awareness of the fact that with the autonomy and identifiability of a culture we are dealing with a *constructive research activity*. The issue to which one seeks access is only constituted as such in the course of the research – and this already begins with the classifications of groups to be investigated (cf. Moerman 1974) that are incorporated in the scientific questions. If one relates this consideration to the idea of ethnographic authority (Clifford 1983), then it may be concluded that to a certain extent researchers are *seeking access to their own fiction.*

The discussion of access takes on a new quality if – on the basis of their social status and social capital – one considers more powerful groups (such as supervisory boards, the nobility or senior doctors; cf. Hertz and Imber 1997; Saffir et al. 1980) and organizations as research fields, that is to say 'objects' who want and are able actively to control access to their domains. This is particularly true of *organizations* (see 3.11), which are increasingly becoming the principal point of address for requests for access. They have at their disposal a wide range of practices to keep curious third parties at a distance, to generate information about themselves, to influence it and control its utilization. Even those organizations that are more open to research needs rarely fail to set up obstacles to access or at least to develop access routines. The investigator, therefore, has not only to persuade informal 'gatekeepers', but also to follow official channels – in an extreme case extending to a highly official contract management via a research access monitoring agency set up precisely for this purpose. Many such agencies and procedural routes actually function as research *preventers.*

The question of access takes on a new dimension in view of the fact that the objects of an investigation increasingly have *prior knowledge* about social research, and are sometimes even equipped with sociological models and education. Occasionally this may contribute to greater understanding and receptiveness towards the research. Such knowledge may also, however, be 'utilized paradoxically', that is, to *resist* attempts at access, resulting in extreme cases in the researcher's being sued on the grounds of inadequate research standards (see 6.3).

In view of developments of this sort, the classical *model of the invisible field researcher*, slipping unnoticed into the field as if invisible under a magic coat and conducting observations totally unnoticed, is no longer plausible even as an ideal. The fear of reactivity, which sees the interactive aspects of field access only as disturbing variables that have to be neutralized, is increasingly giving way to the view that such effects should essentially be evaluated as evidence of the 'naturalness' of an investigation, and that they should be reflected and, in certain cases, even used as sources of information. Field access must be viewed, analysed and designed as an *independent* social phenomenon. If this is done, a range of *fundamental work problems* become clear which all parties (and not only the investigators) have to deal with on their common path into and in the field, regardless of whether or not they make this an explicit issue.

3 STRUCTURAL PROBLEMS OF ACCESS

The field as a social system

Like every outsider, the researcher, from the point of view of the field, is initially *a person*

without history who can only adapt with difficulty to the categories that are normal there and whose loyalty remains dubious. If there arise further fundamental difficulties of understanding at the verbal and non-verbal level (vocabulary, communication style, behavioural types), then it will rarely be possible to integrate this outsider and his or her request *without difficulty* into the normal communicative contexts. In order to answer the question of whether one should 'let in' the outsider, what is decisive is if and how his or her person and request can be identified as 'acceptable' or can be made 'acceptable'. In the process of access, what is crucial, therefore, is not the attempt to achieve a cognitive and social *placement* of the researcher and the request, but simultaneously to establish the experience, dramatization and establishment of a *boundary* between the particular social unit and its environment.

As a rule the social *placement* takes place in two stages: first, the basic acceptability is tested. This is to do with the question of whether the recognizable features of the person (gender, age, ethnic group) and his or her request, together with aspects of the organizational world to which the researcher belongs, are compatible with local world-views, interests and events. It is only at the second stage that the allocation or agreement of particular participant roles is agreed (cf. Lau and Wolff 1983). This two-stage process is mostly described in the literature with reference to an opposition between 'getting in' (physically) and 'getting on' (in terms of social access), although what is overlooked is that 'getting in' already implies some social placement.

In the process of field access the field constitutes itself and is simultaneously experienced by both the actors and the observers as *a social unit*, that is, as a communicative context distinct from its environment, where participants are distinguished from outsiders. In the case of organizations, participation requires the taking on of a membership role and the acceptance of the expectations that are attached to it. One of the implications of this fundamental importance of the *maintenance of boundaries* and the *membership role* is that the researcher *qua researcher* cannot become a member of the organization in question. This would only be conceivable if he or she is given a particular functional status for particular purposes within the organization ('our researcher', perhaps in the role of a consultant or legitimizing authority) or when the researcher personally resolves the difference between him- or herself and the field ('going native').

One form of breaking down distance that is complementary to 'going native' consists of *undercover research*, where the researcher sets up access to a harmless membership role, but can no longer appear as a researcher. Apart from ethical and political objections (see **6.1**), what is against this form of access is that the necessary adaptations that ensue can limit considerably not only the social form but also the quality of data collection.

Much the same is true in the case of access to *simple social systems* (Luhmann 1972). Even when this is a matter of events in public and apparently freely accessible settings, the way in which the presence of the researcher is set up can become a problem. Simply looking in passing at a social encounter may constitute a problem of access, that is to say, a social situation that demands attention. At that moment when those present become mutually aware of each other's presence and address each other, a simple social system inevitably comes into being, even if this is only of short duration. Accordingly, under conditions of mutual perceptibility, a mere absence of involvement is not sufficient to avoid causing disturbance. What is required is rather a socially acceptable form of making oneself invisible, in the active development of which both the observer and those observed can have a share, perhaps by controlling their facial expressions, gestures, spatial locations and so on. Goffman (1971) points out that outsiders, in order to retain this status, would need to display 'polite indifference'.

If this cannot be achieved, the integrity of the person in question, and sometimes even of the interaction system concerned, becomes problematic. The disturbance has to be considered as a question of access in an *independent* interaction system and worked on there – perhaps by means of allocating an acceptable observer role. Many social settings have institutionalized observer roles that may be adopted by researchers for the purpose of making undercover observations.

Classic examples may be found in investigations of deviant 'scenes', such as in the investigation of the porno-scene around Times Square in New York by Karp (1980), or Humphreys's (1970) controversial observations of homosexual 'toilet-dealings'. As with undercover observation in organizations a number of quite delicate circumstances come to light here, where there is always a risk of loss of contact or discovery, where there is an acute need for information control and impression management, and where there are few opportunities for direct communication.

Dealing with 'gatekeepers'

To take care of their border relationships organizations and many groups have 'gatekeepers' (or 'stranger-handlers') of their own (cf. Agar 1996). Astute dealing with such gatekeepers therefore takes on strategic importance within the process of gaining access. Of course, in individual cases it is not always possible to say definitely who it is who has to agree to a request for access or whose agreement actually counts. In respect of organizations as research fields the following rules of thumb may be formulated (cf. Morrill et al. 1999).

In comparatively monocratic organizations it is only the agreement of the senior management that counts, whereas in decentralized organizations there may be a variety of addresses that have to be contacted. It is difficult to decide upon an orientation if there is a high degree of politicization of decision-making policy within an organization. Then the researcher has to seek the agreement of a coalition of decision-makers, and in the worst case of a number of mutually hostile coalitions. Here experience teaches that there are sensitive phases for attempts at access: one should therefore reckon with difficulties, for example, if there has just been a change of management, if the organization is just recovering from a scandal, or if fellow-researchers have recently been there. In organizations with a range of loosely coupled power centres ambiguous situations may arise in which the researcher finds him- or herself between two stools, and where, conversely, no one can rightly say what is valid and who one should refer to. In particular, in cases of high political dynamics in the field of investigation, the question of identifying the current gatekeepers remains a task of constant importance (for a classic example cf. Gouldner 1954). In cases of doubt, it is advisable to follow official channels.

'Immune reactions'

The fields in question react to attempts at access, as far as possible, by relying on familiar and tested patterns for neutralizing disturbance and dealing with unpleasant or unusual requests. The following are some of the strategies that may be found in the relevant *repertoire of organizations*.

- *Pass upstairs*: the request is first passed to a higher level with a request for examination.
- *Cross-question*: the researcher is repeatedly asked for new presentations of the research goal and procedures.
- *Wait and see*: the matter is referred for resubmission, because experience shows that many enquiries sort themselves out.
- *Make an offer*: the request is basically accepted, but the organization offers its own data or agrees to a mode of collection that was not originally foreseen.
- *Allocate*: times, roles and research opportunities are provided which the organization, from its own standpoint, considers suitable and appropriate.
- *Incorporate*: the organization makes the research and the results into an affair of its own, and attempts to integrate the researcher into organizational matters or disputes with other organizations, or to give him or her some kind of indirect task.

Because researchers, for their part, can and do adjust to such strategies, there are in practice many types of interaction effect that result from this. The specific dynamic of these derives from the degree of unity in the particular field in the face of research endeavours and the transparency of the research intentions (Hornsby-Smith 1993). Particularly difficult constellations may arise in the case of 'upward' research, in the sense that elites contrast their high visibility with a high degree of inaccessibility, and also because it is part of the social status of such people to control their accessibility and to set up a functioning management of their (non-) availability.

Ambivalence

There is a *notorious ambivalence* on the part of the researcher which corresponds to these immune reactions. This quite often takes the form of aggressively expressed fantasies of omnipotence or inferiority in respect of the field. A researcher then oscillates between a feeling

of irritation at not being given more than bare facades and a conviction that he or she basically understands the field and its problems better than the informants. What is characteristic of the state of mind of many researchers on their way into the field is the almost unavoidable idea that behind the facades there is a 'true' but maliciously concealed reality. The field research situation – apparently so open – encourages a pervasive *willingness to suspect motives* or the notorious suspicion that the real show is being played out 'behind the scenes'. Interestingly enough, this feeling is repeatedly encountered in *all* participants, that is, not only the informants. Practically all ethnographers, at some point in their careers, must have been looked upon as some kind of spy.

Secrecy and confidentiality

Lee (1993), referring to such ambivalences, describes access to the field as an exercise in the 'politics of distrust'. How this politics of distrust is carried out depends to a large extent on how secrecy and confidentiality are viewed.

Alois Hahn (1997) provides some useful clarifications on this subject: if there is regulation of access to knowledge or to the content of conscious awareness that is not yet shared knowledge ('How wonderful that nobody knows my name is Rumpelstiltskin'), then we are dealing with secrecy, or the attempt to remove information from a particular piece of information. A more relevant case for field research is that of *confidentiality*. Here there is a restriction on the number of people permitted to talk and write about a particular subject, or who – as hearers or readers – may share in the knowledge of a particular communication. Confidentiality relates to what has already been shared, in the sense that, within a group of people who are in communication with each other, something is subsequently and expressly declared to be a secret, with the result that the group is thereby closed to the outside. For this reason confidentiality may truly be used as a productive mechanism, *for the purpose of creating an identity for the particular field and its members*. But it may also happen that a guarantee of confidentiality *precedes* a particular communication, which means that the information in question is only being passed on under an explicit 'cloak of silence'. In this case the sociological meaning of the secret lies less in the closing of the communicative frontiers of a social group or association than in the selective opening to third parties (as with institutionalized confessional or Hippocratic secrets, or journalistic protection of sources of information) of a communicative opportunity that would otherwise not exist.

Becoming aware of these distinctions allows one not only to avoid hasty subjectivist interpretations and rationalizations of motives (that is, not confusing secrecy with confidentiality), but also to gain a perspective on the paradox of the 'risk' to the researcher that can derive from *initiation* into the secrets of a particular field. In so far as a secret constitutes a difference from the uninitiated, anyone who has been initiated into secrets is in a quandary. Gaining information about insiders can become a problem for the researcher, which confronts him or her with the alternatives of betrayal or self-censure. Goffman (1989: 129) is therefore quite justified in warning the researcher against believing that it is a sign of really being 'in' if one is admitted, without asking, into strategic secrets.

Structural opacity

One remarkable feature of the process of access that has regularly been reported is the fact that informants rarely ask about the content of the research project or what is said about it in the various papers and introductory talks. From the viewpoint of the field, the researcher must succeed, in the manner of his or her presentation, in giving proof that:

- the research project is *serious*;
- the relevant institutions and groups are not threatened with any *harm*;
- one can, within certain limits, *rely* on the researcher's willingness to cooperate, on their solidarity and discretion;
- the researcher will only *disturb* normal daily business in an acceptably limited way;
- one will again *be rid of* the researcher in the foreseeable future.

These questions are not susceptible to any direct testing by the informants; even the researcher has no final answer to them! It is not so much the weight of the research goal or the elaborate nature of the methodological arsenal – the content aspects – but rather the appropriateness of the presentation, the credible signalling of a reputable organizational environment, the nature of the personal approach, or the willingness to accept annoyances and sensitivities pointed out by the field which therefore prove to be the decisive indicators of the acceptability of a request and of the researcher as a person (cf. Lau and Wolff 1983). Apart from this, experienced gatekeepers believe that presentations that appear to be scientifically neutral are produced, polished and beautified, that is to say, 'non-impartial presentations' – which corresponds to their own handling of information (for instance, if they have to produce or read annual reports, job advertisements or applications). Frequently additional information is only requested in order to be able to draw conclusions from possible gaps in particular presentations (cf. Feldman and March 1981).

In the sense that they do not deal with these ambiguities, strict codes of ethics and radical demands for 'informed consent' imply an unrealistic picture of research practice. The process of field access can, in fact, only be set in motion when any possible demands have been met: work can therefore begin in spite of any remaining lack of clarity. This type of work consensus implies a *situation-related dialectic of honesty*, in the light of which the rule of thumb formulated by Taylor and Bogdan (1984), 'be trustful, but vague and imprecise', seems a sensible recommendation. In contrast, any attempt, from whatever viewpoint, to provide complete transparency (such as handing over full research applications) or to insist upon it (for instance, by requiring information about every detail of a research proposal) is a guaranteed way of *not* getting a piece of research off the ground.

Field research as an independent action system

The goal of access work consists only to a limited extent of removing the distance between the researcher and the field, or the differences of interest, information and perspective of the two parties. It would appear to be of at least equal importance to recognize these mutual differences as resources for the epistemological process, to cultivate them and even exploit them. For the researcher this means above all that there is a need always to remain aware of the difference between participation

and observation. What is helpful here are agreements about:

- the allocation of an acceptable observer role (such as that of some practitioners);
- the possibility of a temporary withdrawal from the field ('short-term ethnography');
- the researcher's asking 'naive' questions about matters that are actually self-evident.

Informants must not only be prepared to agree to these 'alienations', which appear at times to be quite artificial (Hirschauer and Amann 1997). They should also be capable of accepting that what they take for granted may be 'questionable'. To be able to engage in any kind of conversation with one another, both parties will feel that it is necessary, to a certain extent, to distance themselves consciously from their social and cognitive reference system. Both parties are moving in a border area between their respective reference cultures. Through their working association they constitute, for a particular period of time, a *hybrid system*, the existence and feasibility of which depends not least on the maintenance of these differences.

Often it is the informants who gamble with the recognition and maintenance of differences. Excessively well-adjusted researchers are commonly faced with as much scepticism as those who announce their solidarity or make helpful suggestions without being asked. Conversely, caution is also advised if there is an over-enthusiastic reception by the field, because this may often relate to secret hopes and expectations in advance of what the researcher may wish to set up.

Within this hybrid system there will also develop particular role-relationships, time horizons, forms of communication, rationality criteria and obligations, and these may again have important consequences for the discovery potential of the project: this may influence, for example, the situational acceptability of particular methods (interviews – yes; observation – no), or the problem of what topics are legitimate subjects for questioning, what events the researcher can participate in, and where limits have to be respected. Experienced field researchers will orient themselves according to the options that arise in the framework of this action-system and, in the light of the particular practical circumstances, will reformulate their questions and the steps in their investigation

accordingly. What seems sensible is a *progressive field-access strategy*, which begins with relatively diffuse questioning and does not insist upon immediate use of the most demanding collection procedures. With this kind of strategy what is, at least initially, in the foreground is not the accomplishment of the research plan but the *securing and setting up of an appropriate* situational context for the research *process*.

The researcher can offer nothing to the field

Field research relationships are fragile entities. Participants tend to come together by chance, they are linked by only a brief history, and a common future seems unlikely. They embark on a complex process of cooperation, for which there are almost no routines and whose development cannot be foreseen in any detail. Both parties have to adjust to one another, with no proper bases and certainties for trust. In view of this kind of constellation, it may well be understandable if researchers seek to buy their way in with problematic announcements or even promises concerning the expected uses of the project for the field that is under investigation.

This sort of bargaining model, however, not only implies an unacceptable simplification of the relationship between science and the field. It also represents, in view of the triviality of what the researcher is actually able to offer, a form of *bragging*. Behind this the true value for the field is in most cases limited to a short-term interruption of the daily boredom, an opportunity to bring one's cares and complaints to someone, or the chance of doing a good piece of work. Only rarely are representatives of the field willing and able to do something with the results of an investigation. If gatekeepers really do offer the researcher the role of an evaluator, critic or consultant, and therefore require achievements in return, then caution is advised, not only because this may require more than the researcher's competence can deliver. What is more problematic is that this may lead to the *diffusion of roles* between the participants, and above all the self-limitations and compromises for which the researcher must be prepared in this kind of situation, to balance out the different expectations of representatives of the field and the interests of the researcher (see 3.12, 6.3). What conflicts with this insight, however, is the uncertainty of the researcher about whether he or she will really be taken seriously by the field if he of she is unable to display a modicum of expertise.

In view of the temptations, expectations and fantasies that swamp the researcher in the access situation, he or she can easily run the risk of being or wishing to appear too wise too quickly. To counter this risk, it is advisable to exaggerate one's naivety, not only to the field but also to oneself, to be able to exploit methodologically, and for as long as possible, the researcher's (real or imagined) ignorance.

FURTHER READING

Johnson, J. M. (1975) *Doing Field Research*. New York: Free Press.

Morrill, C., Buller, D. B., Buller, M. K. and Larley, L. L. (1999) 'Toward an Organizational Perspective on Identifying and Managing Formal Gatekeepers', *Qualitative Sociology*, 22: 51–72.

Saffir, W. B., Stebbins, R. A. and Turowetz, A. (eds) (1980) *Fieldwork Experience – Qualitative Approaches to Social Research*. New York: St Martins Press.

Part 5B

Collecting Verbal Data

5.2 Qualitative Interviews: An Overview

Christel Hopf

1 Introduction 203
2 Variants of qualitative interviews 204
3 Selected questions on the conduct of qualitative interviews 207

1 INTRODUCTION

In social research, qualitative interviews – semi-standardized or open interviews – are very widely used. In the context of quantitative research projects they are used predominantly in the preparation of standardized data collection and the development of data collection tools. In qualitative research there are many more opportunities for their use. In the first place qualitative interviews play an important role in ethnographic research projects based on participant observation (see 5.5). One of their uses here is the imparting of expert knowledge about the research field in question, the recording and analysis of the informants' subjective perspective, or the collection of data relating to their biography (see 3.6). More usual – at least in Germany – are qualitative interviews in research projects for which they are the main empirical base. These include: projects in the area of biographical research, studies on gender-related questions (see 3.10), studies of the social and political orientations of different population groups, or studies of access to professions and of professional socialization.

Compared to other research procedures in the social sciences, qualitative interviews are particularly closely related to the approaches of interpretative sociology. Because of the possibility of enquiring openly about situational meanings or motives for action, or collecting everyday theories and self-interpretations in a differentiated and open way, and also because of the possibility of discursive understanding through interpretations, open or semi-standardized interviews provide important opportunities for an empirical application of action- theory ideas in sociology and psychology. Together with the establishment of qualitative procedures in social research, this has repeatedly been emphasized as a particular achievement of qualitative interviews – compared to the more restricted possibilities of standardized questioning – frequently relying on the theoretical traditions of phenomenological sociology (see 3.1). Reference has also been made to Max Weber's idea of an interpretative sociology or to the traditions of symbolic interactionism (see 3.3). Nevertheless, the link between interpretative sociology and qualitative interviews is not obligatory, as can be explained, for example,