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Interviewing scientists

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Abstract

With this article, we want to open a discussion about a methodological problem central to many empirical science studies, but which has received far too little attention, namely informed interviewing. We have shown that two positions can be distinguished in the general methodology of ethnographic studies: naïve observation and informed observation. The latter approach is applied in two forms: native observation, i.e. as sociological observation conducted by scientists who previously belonged to the field under study; and sociological observation by sociologists who acquire scientific knowledge that is relevant in the field under study. Our own application of the principle of informed observation must take the latter form. We applied the idea of informed observation to qualitative interviews, which should be conducted in a scientifically well informed manner for three reasons: the epistemic conditions of action must be included in sociological explanations; the research questions must be operationalised; and qualitative interviewing requires a depth that cannot be achieved without being familiar with the interviewee's world. In our informed interviewing, we encountered three basic tasks: preparing and suggesting a language for communication (an ad hoc - pidgin that is understood by both the interviewer and the scientist), preparing the interview by acquiring scientific knowledge, and negotiating a level of scientific understanding in the introductory sequence of the interview. We make suggestions how to solve these tasks, and discuss limitations of the approach of informed interviewing.

Contents

1 Do we need to understand science?

With this article, we want to trigger a discussion about a methodological problem and its practical consequences for interviewing scientists.[1] This problem is: To what extent do we have to understand scientists' work *scientifically* in order to explain their behaviour *sociologically*? It specifies a fundamental sociological insight for science studies. If we need to acquire an "interpretive understanding of social action" in order to achieve "a causal explanation of its course and consequences" (Weber [1922] 1978: 4), than we routinely face the task of getting acquainted with the life-world under study – be it a youth subculture, a firm, a community, or a scientific field.

While the task of acquiring such an understanding is the same for all subjects we want to study, these subjects differ in how knowledge about them can be obtained. There are many social settings that are 'self-explanatory' in that all knowledge that is needed to be a competent member can be found in the social setting itself. People enter these social settings without any specific prior knowledge, and acquire all the knowledge a member of that setting is supposed to have by endogenous learning. Sociologists entering such a setting are in the same situation, which means that they can acquire as much knowledge as any other prospective member of the culture.

The sciences (and other professions, some occupations etc.) are different because being a competent member of the culture requires an extended systematic prior training, a training the sociological observer usually cannot undergo. This puts the observer at a disadvantage that cannot be overcome: Because exogenous learning is necessary, sociological observers will not usually be able to perform the typical activities of the studied culture. Length of stay in the field can significantly reduce the gap between a member's and an observer's knowledge. However, the gap cannot be closed within the culture.[2]

The problem of limited understanding is aggravated when a study requires the understanding of not one scientific culture (as usually is the case in ethnographic studies), but several of them simultaneously. This is a problem we have faced in our own comparative studies of institutional influences on the production of scientific knowledge, which use qualitative interviews as their main method. The comparative approach severely limits the time one can spend for understanding the fields.

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[1] This paper has benefited from critical comments by Martin Meister, Jörg Strübing, and Lucy Suchman, neither of whom will probably agree with the use we have made of what we have learned from them.

[2] Ten Have (1995: 254-256) makes a similar distinction between "the lay world" and "the professional world".

In this article, we will address the general problem of understanding scientists and the ramifications for qualitative interviewing. We begin by discussing the methodological position of 'naïve observation', which denies the necessity of understanding the science of one's subjects, and the two main strategies of gaining a scientific understanding, namely native observation and informed observation (2). By characterising our own (comparative, neoinstitutionalist) investigations, we will discuss three reasons why it is necessary for these studies to acquire knowledge about our subjects' science (3). Using our own experiences and mistakes, we will then discuss the three main practical tasks that must be solved in interviewing scientists (4). As a conclusion, we will discuss risks and limitations inherent to the strategy we propose (5).

2 Methodologies of observation

2.1 Naïve observation?

The first extensive ethnographic study of scientific practice was published by Latour and Woolgar ([1979] 1986). Latour and Woolgar took a surprising methodological position by stating explicitly that their ethnographic observation was conducted by applying the perspective of a "very naïve observer" (Woolgar 1988: 83-96; Latour 1990: 146; Latour and Woolgar [1979] 1986: 12, 29-30). Latour characterised this methodological approach as deviating from mainstream anthropology (the 'source field' of the ethnography of science), which has agreed upon the necessity to understand the content of actions under investigation (Latour 1990: 146). He describes the "naïve" investigators' perspective as that of an

> ... outside observer who does not know the language and the customs of the natives who are not supposed to read what he writes. As Woolgar has pointed out many times, […] this is a very naïve version of the naïve observer - a version that is now abandoned in mainstream ethnography and which seems to survive in so called 'lab studies'. (ibid.)

Latour and Woolgar have good reasons for their methodological decision:

> We take the apparent superiority of the members of our laboratory in technical matters to be insignificant, in the sense that we do not regard prior cognition (or in the case of an ex-participant, prior socialisation) as a necessary prerequisite for understanding scientists' work. This is similar to an anthropologist's refusal to bow before the knowledge of a primitive sorcerer. For us, the dangers of "going native" outweigh the possible advantages of ease of access and rapid establishment of rapport with participants. (Latour and Woolgar [1979] 1986: 29)

Woolgar later reinforced this point by stating that there is a higher risk of 'going native'[3] when observations of science are concerned:

> The standard tension of any ethnographic study is present here. We want to see things from the natives' point of view but we don't want uncritically to adopt their belief system. [..] Note, however, that in one important sense it is more difficult to remain 'strange' in the exotic culture we call science than it is when conducting an ethnography of, say, the Navaho Indians. When the latter informants tell us that they are dancing in order to make it rain, we can readily draw upon scepticism which is 'in-built' in virtue of our membership of 'advanced Western culture'. But when informants amongst the tribe of scientists explain that the right-hand side of an equation 'follows' from the application of the rule of commutativity, we find it much more difficult to resist the apparent authority of this explanation. Why? Simply because respect for scientific rationality is deeply embedded in our own (ethnographers') culture. (Woolgar 1988: 86)

Latour and Woolgar certainly do have a point here: Everybody who is going to observe science has received a science education and a partial socialisation as a scientist prior to the observation. It is therefore more difficult for an observer to stay the 'stranger' in a scientific environment than in others. Scientific practice is laden with reasoning and justifications, and "in the case of a scientific culture in particular, there is a strong tendency for the objects of that culture (facts) to provide their own explanation" (Latour and Woolgar [1979] 1986: 278). To reveal and to investigate taken-for-granted practices of scientific work can be assumed to be more difficult, and the danger of 'going native' is higher.

This approach has been critized by Lynch (1982: 506-509; 1993: 93-102). Lynch pointed out that Latour and Woolgar didn't stick to their approach, which becomes visible in their descriptions. Lynch stated that

> … the account which resulted from their inquiry is far more comprehensive and detailed in its access to technical practices than could possibly have resulted from the 'observers' initial man-from-Mars posture towards the work of lab members. (ibid.: 507)

According to Lynch, a stranger's "accounts of what scientists do are continually and necessarily reflexive to the stranger's understanding of those practices" (ibid.: 509). Interestingly enough, in Latour's and Woolgar's book the observer's understanding of scientific practices appears to vary significantly throughout the book. Chapter 2 takes the "vary naïve" perspective:

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[3] 'Going native' is one of the central methodological problems in anthropology. It describes the observer's gradual adoption of the observed culture's belief systems and perspectives, which leads to a loss of analytical distance and to the inability of questioning taken-for-granted positions and practices (Hammersley and Atkinson 1995: 109-112).

> Our anthropological observer is thus confronted with a strange tribe who spend the greatest part of their day coding, marking, altering, correcting, reading and writing. (Latour and Woolgar [1979] 1986: 49)

It is this purposeful ignorance of the content of the observed actions and the sole occupation with their outward appearance as intelligible to the scientifically ignorant sociologist, as Lynch describes:

> … Latour and Woolgar present their ethnography from the point of view of a fictional "observer" who sees what is going on in the lab without being taken in by the scientists' beliefs in an unseen biochemical order of things. The observer describes just what he finds intelligible in the lab: the traces, texts, conversational exchanges, ritualistic activities, and strange equipment. (Lynch 1993: 96)

This means that the observers were forced to impose concepts they find intelligible on the life-world they study rather than being able to re-interpret this life-world in their own framework *after* they have understood it. Influential concepts such as "inscription devices" are the result of that strategy (ibid.: 51-53).

Later in the same chapter, when the authors are describing the laboratory practice (ibid.: 53-69) and are classifying scientific statements (ibid.: 69-88), more background knowledge about the practices creeps in. Otherwise, Latour and Woolgar could not have decided on what principles assays are based and what it means to repeat an assay (ibid.: 59-60); or what parts of a scientific statement are modalities, i.e. can be deleted without rendering the statement completely senseless (ibid.: 77-85). The story of the construction of a fact - TRF(H) - in chapter 3 could not have been told without reference to the scientific content of the respective activities. For example, statements such as "In total, four groups have worked on the isolation of TRF …" (ibid.: 114) are based on what "working on the isolation of TRF" means to the observed field. In chapter 4, the observers draw a picture of scientists socially negotiating when constructing facts. In these discussions, the scientific content of scientists' actions and accounts is systematically re-interpreted as being a resource in social negotiations. However, this is possible only because the analysts understand the significance of the scientific content of conversations and practices.

Lynch was indeed right in pointing out that the perspective of a "naïve observer" is not only methodologically problematic but also difficult to maintain. While some general concepts such as 'inscription device' (or possibly even the whole Actor-Network Theory) may be a consequence of a naïve observation, the explanation of the "microprocessing of facts" is obviously not. Another indicator of the limitations of "naïve observation" is that this approach has not been applied by other ethnographers of science.[4]

2.2 Informed observation

With "informed observation" we refer to social studies of science undertaken by sociologists who acquire a scientific understanding of the field they study by self-education prior to or at the beginning of their empirical study. The necessity of understanding scientists' work scientifically has been first discussed by Zuckerman in her methodological reflections on interviewing Nobel laureates (Zuckerman 1972). After the sociology of scientific knowledge has become the mainstream of the Sociology of science, the problem has been repeatedly addressed in the context of ethnographic studies of scientific practice. All other ethnographers of science besides Latour and Woolgar have taken the position that an informed observation of this kind is necessary.[5] Collins and Pinch conducted their participant observation of research on 'spoonbending' as an informed observation (Collins and Pinch 1982), for a methodological discussion see Collins (1984). They took part in an investigation of paranormal phenomena by taking the role of researchers. Therefore, they had to acquire "native competence" (ibid.: 54). In one of his articles on his studies of the search for gravitational waves, Collins explicates his methodological position:

> The more narrow methodological stance adopted in this article is "participant comprehension" …. Participant comprehension is an interpretation of participant observation under which the field-worker tries to acquire as high a degree of native competence as possible and interaction is maximized without worrying about disturbing the field site; this ideal should always direct the research effort, even though the degree of native competence attained will vary from study to study. (Collins 1998: 297)

Collins further states that while he has not "achieved anything like full native competence in gravitational radiation research", he believes that he has gained "enough understanding to be able to carry out the kind of sociological analysis presented here" (ibid.: 298). He bases this judgement on comparisons to parapsychology (where he became a "full-blown expert") and to the theory of amorphous semiconductors, which he had to abandon because he could not understand any of the science. (ibid., note 6).

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[4] Latour himself obscures his position by stating in a review of Lynch (1985) that "that one should become familiar with the practices of the people one wishes to study … is the basic tenet of all ethnographic work." (Latour 1986: 544).

[5] This was also Woolgar's position before he turned to laboratory studies. In an article on the discovery of pulsar phenomena, he wrote: "In research of this kind, I obviously needed to be aware of the scientific issues in order to correspond with or interview participants." (Woolgar 1976: 396).

The same approach can be assigned to Lynch (1982; 1985; 1993; 1994), who traces the methodology back to Winch (1958; 1974) and to ethnomethodology's requirement of "unique adequacy" (Lynch 1993: 271-275). [6] "Unique adequacy" requires that ethnomethodologists gain the capability to perform the characteristic practices in the field under study (Garfinkel and Wieder 1992: 182-184). When these capabilities could not be developed by simply staying in the field long enough, some of the ethnomethodologists took the relevant formal training (Lynch 1993: 274). Lynch himself did not undergo the formal training. Instead, he was given "a rather informal course of training in the substantive and methodological features of the lab's research" (Lynch 1985: 1-2). After his training, he was still

> … unable to participate in the lab's researches, though I achieved a competence in some of the analytic skills used in assembling and interpreting electron microscopic displays of brain tissues. These limited competences gave me considerably more access to the talk and conduct which I witnessed in the lab than would have been possible had I relied solely on the analytic skills of a social scientist while observing members activities. (Lynch 1985: 2)

The practical difficulties of informed observation are seldom discussed. Collins reports that he had to abandon one case study because he was not able to acquire enough competence (see above). Lynch discusses his "limited competence" but comments that it is impossible to tell what is missing because of these limitations (Lynch 1982: 529). Thus, both authors confirm the principal limitation of informed observation – the sociological observer can achieve an understanding of the science that is being observed, but is not able to actually perform the type of research that is observed. The consequences of these limitations for science studies are not discussed.

2.3 Native observation

One special way of conducting informed observations is 'native observation', i.e. an observation conducted by scientists from the field who have turned into sociologists. Examples of this biographical turn are the radio astronomer Edge (Mulkay and Edge 1976), the physicists Pickering (Pickering 1984, 1995), Pinch (1986)[7] and Merz (Merz and Knorr-Cetina 1997; Merz 1999), the immunologist Löwy (1997), the biologist

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[6] Hirschauer (1994: 338-345) traces the principle of informed observation back to Malinowski ([1922] 1972) and Schütz (1962). The necessity of informed observation was also stated explicitly by Knorr-Cetina in an article on anthropology and ethnomethodology (Knorr-Cetina [1980] 1993: 170) and applied by her in her ethnographic studies (Knorr-Cetina 1981: 31, note 64). Another ethnographer who chose informed observation is Traweek (1988: 9-11).

[7] Pinch notes the requirement that the sociologist has "to familiarize himself or herself with the technical issues which are at the core of the scientific 'life world'" and states that his "own background in physics has proved invaluable in this task" (Pinch 1986: 197). He even included a section on "Some Technical Details of Solar-Neutrino-Detection" in his book (ibid.: 41-48).

Cambrosio (Cambrosio and Keating 1988, 1995), and the biochemist Fitzly (Fitzly 2000). All these observers studied scientists of their own research field or at least of their broader research discipline. Cambrosio even attended a special scientific training session on the subject he and his colleague were studying (Cambrosio and Keating 1988: 249).

We think that the strategy of 'native observation' deserves a special discussion. Being 'a native of the tribe' is an important asset for an informed observation. Only native observers are able to close the gap between the observer's and the subjects' knowledge. As Knorr-Cetina and Merz argued in a comment, native observation enables a deeper understanding of scientific practice:

> Methodologically speaking, the paper argues for a version of thin description and a reading of deconstruction as a useful craft in ravelling and unravelling the embodied entanglements of practice. In leaving out much of the sociology of collaborations, the cultural meanings of mathematical work, the social history of string theory and the politics of problem solving in the lab, the question asked is whether and in what way such a version of ethnography is of interest to STS. Are thin descriptions of the material dynamics and performative orderings of behavioural domains equally worth our effort as studies of their manifold meanings and contextual relationships? Or can we, while conceiving of ourselves as social students of science, finally draw a line and freely admit to be disaffected by such orderings (that is, by how string theorists do a computation)? (Knorr-Cetina and Merz 1997: 129-130)

Mulkay even went as far as stating that "if we are to study in detail the operation of scientific communities, we must have the active cooperation of participants or ex-participants" (Mulkay 1976: 210-211). However, there are possible disadvantages of native observation as well. An observer or interviewer who identifies herself as native inevitably changes the relationship between observer (interviewer) and participant (interviewee). This was first observed by Mulkay and Edge:

> A second possible source of bias arises from the fact that one of us was originally trained as a radio astronomer. In many ways this was, of course, an immense advantage. It enabled us, for instance, to explore in detail the scientific and technical literature, and it made possible an exceptional degree of cooperation between researchers and respondents. On the other hand, it meant that one of the interviewers was regarded by respondents, on some issues at least, as another participant. It was, therefore, impossible for the interviewer to avoid being drawn sometimes into a dialogue with his subjects, during which he was expected to act, not as an impartial outsider, but as an involved colleague. As far as we can judge, however, respondents did not hesitate to disagree with the interviewer in these exchanges of judgements and opinions. (Mulkay and Edge 1976: 3-4)

An emerging role ambiguity of the observer/interviewer was also observed by Löwy who commented that some of the scientists she observed regarded her as ex-colleague with an unclear professional identity. Her observation was sometimes assessed as "secret longing to return to the laboratory". Some scientists "were not sure how to classify a fellow researcher who shared with them expert knowledge and familiarity with the laboratory culture, but professed radically different goals". She herself felt as a "'native of nowhere' – an inadequate immunologist and an awkward historian". (Löwy 1997: 93) She concludes:

> 'Going' native is perhaps helpful in studying modern science, but investigators who observe scientists' activities still need to decide how 'native' should one go, and for how long. (ibid.)

From these methodological observations we cannot conclude how these problems influenced the social accounts of the scientific practices she studied.

In the study on radio astronomy, Mulkay observed that there is a danger that native observation "may lead to the investigators' taking over false, or incomplete, assumptions from the group under study" (Mulkay 1976: 211). In the case of another native observation, the observers were directly accused that their "going native" had compromised the study. To conduct a native observation was also a deliberate decision in an ethnographic analysis of theoretical physics (Knorr-Cetina and Merz 1997: 125; Merz and Knorr-Cetina 1997: 74). It was criticised by Gale and Pinnick (1997). Gale and Pinnick accused Merz and Knorr-Cetina of introducing a third, "explanatory" language (additionally to the participants' and the observer's language) that was so close to the participants' language that it imports the participants' metaphysical realism in their explanation. By using this third language, Merz and Knorr-Cetina adopt the perspectives (especially the philosophical perspectives) of their participants – a specific case of 'going native' (Gale and Pinnick 1997: 117-121).

Knorr-Cetina and Merz rejected the critique by pointing out that Gale and Pinnick criticise their methodology without criticising the results obtained by applying this methodology (Knorr-Cetina and Merz 1997: 126). Indeed, Gale and Pinnick mentioned only one negative consequence of the approach chosen by Merz and Knorr-Cetina – the adaptation of physicists' metaphysical realism. But not even this critique is justifiable. Rather than invoking metaphysical realism, the reference to mathematical structures' "hardness" by Knorr-Cetina and Merz is nothing but the application of a well-known sociological insight that applies to mathematical objects as well: "The paradox is that man is capable of producing a world that he then experiences as something other than a human product." (Berger and Luckmann 1967: 57). Thus, while native observation certainly bears the risk of 'going native', this point has not yet been proven.

3 A case for 'informed interviewing'

3.1 The problem

There seems to be an overwhelming support for the methodological principle of 'informed observation', which can be accommodated by either 'native observation" or by sociological investigators acquiring a scientific understanding of the field to be studied, and the practices within the field. These approaches have been developed within ethnographic studies of scientific work, where only one field (or laboratory, or discourse) is being studied at the time, and where longer time frames and longer stays in the field make it possible to acquire the necessary knowledge. But what if these conditions do not exist?

Our own research interest is best described as neoinstitutionalist. We are interested in how institutional conditions of action (as provided by funding programs, science policy, law, formal organisations, informal rules within scientific communities etc.) affect the production of scientific knowledge. For example, we ask how institutional conditions of actions affect interdisciplinary collaboration (Laudel 1999, 2001), or how the institutional change that accompanied German unification affected links between basic research and applications (Gläser 1998, 2000). Following ideas of the new institutionalism that have emerged in political sociology, organisational sociology and economics, we regard institutions as inherently inconsistent, variable conditions of actions that are one of a whole set of factors affecting actions. We are interested in how these factors affect the content of scientific knowledge that is produced. This research interest differs from the Mertonian sociology of science in that it regards the content and forms of practices and knowledge as explanans.[8] It differs from the sociology of scientific knowledge in the explicit regard of social macrostructures, namely institutions.[9]

In order to understand the impact of macrostructures such as institutions, it is not sufficient to study them in only one situation. If institutions are only one of a variety of factors shaping human behaviour (as the new institutionalism concedes), than they must be studied in different settings, i.e. functioning under different conditions, being overlapped by varying other conditions etc. Investigating institutions therefore requires comparative studies across several organisations and fields.

Comparative studies of that type are subject to some severe restrictions. The most important one is economic, there is not enough time to conduct a series of ethnographic observations. Therefore, our main empirical method is qualitative

[8] For comments on this 'blind spot' of Mertonian sociology of science, see e.g. Whitley (1972).

[9] For comments on this 'blind spot' of laboratory studies, see e.g. Knorr-Cetina (1995: 162) and Kleinman (1998: 288-289).

interviews. The methodological principle of 'informed observation' of course applies to this type of comparative study and to qualitative interviews as well. However, its application now means that the observer has to become familiar with a variety of different scientific fields at once. It would appear much easier to avoid this demand and not to insist on 'informed interviewing'. In the following sections we discuss the three main reasons why informed interviewing is nevertheless necessary.

3.2 Informed interviewing is necessary because of the role of epistemic conditions of action

Scholars in Science and Technology Studies today widely agree that the objects and methods scientists are dealing with in their scientific practices affect these practices and that these influences must be accounted for in sociological explanations. Concepts like the 'non-human actants' of Actor-Network Theory (e.g. Callon 1986; Latour 1988; Law and Callon 1988); the "Mangle of Practice" (Pickering 1995) or "thin description" (Knorr-Cetina and Merz 1997, see 2.3) indicate that the epistemic content of scientific work is regarded as important for sociologists. As can be expected from the previous discussion, the approaches differ in the ways in which they include epistemic conditions of action. Actor-Network Theory uses sociological descriptions of epistemic conditions of actions, i.e. of nature, instruments and knowledge that are involved in scientific practice, and ascribes social characteristics to them. This approach is consistent with the concept of naïve observation. The alternative approach is to develop scientific understanding of the problems and strategies that play a role in the field under investigation, and to use this understanding in the explanation of scientific practice.

Let us turn to an empirical example from an investigation of institutional conditions for interdisciplinary collaboration (Laudel 1999, 2001). In order to find causal relationships between the institutional conditions of action and results of collaborative work, all factors that promoted, hindered, enabled or prevented a collaborative project's success must be analysed. When a scientist answered: "the collaboration didn't work", it had to be clarified what "it didn't work" actually meant i.e. to what kinds of causes the scientist referred. In one case, the further probing solicited the following explanation:

> The .. protein .. he [the biochemist] gave us, .. was always too contaminated .. it has never worked. .. If you want to crystallize it, it must be perfectly pure, otherwise it doesn't work. Some proteins are very difficult to purify...

The scientist referred to a 'material resistance' (the protein's insufficient purity) as the main cause for the collaboration's failure. This was confirmed by other interviews and documents. It became clear that neither lack of resources, nor difficult personal relations (the partners collaborated successfully in other projects and got along well) nor other social reasons could explain the collaboration's failure. Epistemic conditions of action (the difficulties of protein purification and the high purity that is required by crystallization methods) had to be included in this explanation. More generally, epistemic conditions of action had to be included in the investigation in order to provide accounts for the success or failure of collaborations. In order to do that, we had to include the research content and research actions into the interview strategy.

On a more general level, differences between collaborative behaviour in different fields need to be explained. For example, one could ask "why do sociologists collaborate relatively seldom whereas experimental physicists do it permanently?" An answer to that question would have to include not only social properties and practices, but epistemic conditions of actions as well. Knorr-Cetina applied this approach by describing epistemic cultures that clearly differ in their collaborative practices (Knorr-Cetina 1999).

3.3 Informed interviewing is a consequence of the operationalisation of research questions

The concept 'operationalisation' is primarily used in quantitative methodology for describing the design of indicators that measure the variables referred to by the hypotheses. We use it for qualitative methodology in an analogous way, namely for describing the process of identifying phenomena we must investigate and information we must gather in order to answer our research question. Thus, operationalisation means (in both cases) translating a research question into an empirical strategy.

Part of the empirical strategy is to ask scientists about things they know in a language they can understand. In the case of the project on interdisciplinary collaboration, one of the initial assumptions was that different types of collaborations are affected by institutions in different ways. Therefore, the operationalisation of the concept 'collaboration' was crucial. To presuppose a shared meaning of concepts like 'collaboration' is rather dangerous, as becomes clear when we look at the following statements given by different scientists in the pre-study:

> S1: Well, I would call it collaboration if something written comes out of it that is published together.
>
> S2: And there are many colleagues with whom I collaborate without direct results in the form of joint publications, but we help each other and maintain relations.
>
> S3: H. had tried for a long time to isolate a protein and didn't succeed. Then S. asked E. to give us the recipe.
>
> S4: ... additionally the chemists provide advice, they look at the physicists' substances.

S5: We agreed that I take a group from his field into the NMR department, that is that I give them the opportunity to use the equipment.

These descriptions of actions refer to a wide range of collaborative behaviour. Several types of collaborations can be distinguished such as giving access to research equipment, collaboration involving a division of labour, services, and transmission of know-how. A scientist's understanding of 'collaboration' depends on his or her everyday research practices and the types of collaborations he or she is engaged in.

Concepts like collaboration belong to both sociology's theoretical language and scientists' everyday descriptions of their actions. This tempts sociologists to pass the sociological question on to our 'research objects', i.e. to the scientists. But if we do so we can't interpret the answers without knowing the exact meaning given to the term by each respondent. As the above example demonstrates, those meanings may widely vary.

For a sufficient operationalisation of the research question about collaborations we had to refer to the scientific content. Scientists were asked about the content of working relationships to their colleagues, as in the following example:

Q: You wrote [in your proposal] that relations exist to the scientists A., B., and C.

A: I must say that these relations are purely thematic, there are no practical relations.

Q: What does that mean?

A: That means we have potential relations or similarities regarding the theme and the methodology. We both investigate membrane proteins that are integrated. ... These are, so to speak, thematic parallels or relations, similar problems of different systems. But we have no practical collaboration, because the biological systems are different. Thus, there would be relations by questions and methodology, but there are no collaborations in the word sense.

Q: But there is an exchange regarding methods?

A: Not much.

Q: In what form?

A: There is some exchange of know how about reconstitution methods of membrane proteins.

The interviewed biologist finally described an exchange of know how. This was identified in the investigation as a specific type of collaboration. The interviewee, however, did not regard the exchange of know how as collaboration ("there are no collaborations in the word sense"). If the interviewer would have simply asked "with whom do you collaborate?" she would never have been told about these specific collaborations because the interviewee understood the term 'collaboration' differently. In our example, the scientist regarded transmission of know how a very weak interaction that does not merit the notion 'collaboration'.

Operationalisation in this sense means attempting to link what the investigator is interested in to the interviewees' everyday experiences, which are formulated using concepts of their science. In the case of interviewing informed observation appears to be indispensable because qualitative interviewing means freely talking about the scientists' experiences, namely scientific practice.

3.4 Informed interviewing is necessary to probe deeply and to demonstrate competence

Our third reason for informed observation is provided by the general methodology of qualitative interviewing. The literature on qualitative interviewing provides two arguments for a well informed questioning. A first argument refers to the qualitative interview's specificity and depth. In order to reconstruct social situations by means of qualitative interviewing, we need specific and detailed information about these situations. The interviewer must translate her interest into the context of the interviewee. Otherwise, an understanding of the interviewee's attitudes and actions is impossible (Merton, Fiske, and Kendall 1956; Hopf 1978: 99-101). Moreover, the interviewer must often support the interviewee in explaining the meaning of situations and the interviewee's reactions to them (ibid.). The interview sequence about collaboration provided in the previous section illustrates that point: without a specification of the interviewee's interactions with his colleagues, collaborations could not be identified.

The second argument states that in order to be taken seriously it is important for the interviewer to demonstrate competence. As Rubin and Rubin put it:

> Your informed questions signal the interviewees that you have done your homework, made an effort, and have not just come to pick their brain. You have gone as far as you can go with the available material and now you need some help. (Rubin and Rubin 1995: 198)

The aspect of competence was emphasised by Zuckerman in her article on Interviewing Nobel Laureates. She describes the functions of her preparations as "giving evidence of the seriousness of the interviewer" and "legitimise expenditure of time on the interview":

> Almost all the Nobelists are acutely concerned with maximising the use of that inevitably scarce resource, time ... In part, their commitment to the intellectually profitable use of their time led them to subject the interviewer to an almost continuous series of tests to ascertain the degree of her competence and commitment. (Zuckerman 1972: 165)

Sometimes she was perceived by the Laureates as a "combination layman-expert" in their research fields (ibid.: 173). Zuckerman quoted one interviewee who told her

> I said to myself before you came, "If she wants to ask me about social things, I will get her out of here fast." But you asked me about important things. What is written

> about science is never quite right. You have to hear it from the people who were there. (ibid.: 165)

Finally, her questions based on the prepared materials often called forth responses that otherwise would not have been elicited (ibid).

The understanding of an interviewee's science that an investigator can acquire prior to an interview is, of course, very limited. However, our experience supports that this limited understanding is sufficient to conduct in-depth interviews. Even if we got it wrong, it was at least obvious – and always very beneficial to the interview atmosphere! - that we tried.

Again, we would like to stress the point that these general methodological arguments hold true not only for qualitative interviewing as a 'stand alone' method, but for the interviews embedded in ethnographic observations as well.

4 Informed interviewing: Three tasks

4.1 Creating an 'ad hoc- pidgin'

One important aspect of any qualitative interview is that it must be conducted in a language that enables the investigator to obtain relevant information. Consequently, the language must be understandable to both the interviewer and the interviewee, and must facilitate the description of the interviewee's world. If the world is sufficiently remote from the everyday world that can be assumed to be shared by interviewer and interviewee, the emerging language can be regarded as an 'ad-hoc – pidgin'. We borrow the term pidgin from Galison who used the metaphor of pidgins and creoles to explain the stabilisation of interdisciplinary collaborations (Galison 1996). It seems useful because in a sociological interview, a similar situation occurs. Interviewee and interviewer attempt to accomplish an ad hoc-collaboration for producing information the interviewer needs. In this collaboration, two worlds – the world of sociological investigation and the scientist's work world – intersect, and in order to communicate about it, a common language must be constructed. The interviewer is suggesting such a language by using concepts from the scientist's world (which she obtained during her preparation, see 4.2) and simplifying the relationships between them. The main difference between the original meaning of the concept and Galison's use of it, one the one hand, and the situation in an interview is that despite all of the interviewer's preparations, the language must be created almost instantaneously, namely in the course of one interview.

The strategies for creating such a pidgin depend on the subject matter the sociologist is interested in as well as on the way this subject matter is experienced by the scientist in his or her everyday practice. In our interviews, we repeatedly observed that scientists switch between more technical and more social descriptions. When asked about their research processes, scientists described them in a predominantly technical way by referring to the epistemic content of their work – research problems, objects and methods of experimentation, instruments etc. For example, scientists present the system of experimental operations (synthesizing substances, measuring etc.) when describing collaborations. They told us that they used certain research methods, special substances etc. and therefore collaborated with scientists from other research groups who could provide them. Social relations and interactions that enabled, performed and accompanied this system of operations appear to be more in the background of the interviewees' reconstructions. Conversely, scientists describe their research fields as a constellation of actors (mainly research groups) and don't seem to think about it as an evolving body of knowledge.

On the basis of this observation,[10] we developed different strategies for obtaining information about the interviewee's 'local' work and about her community, respectively. When interested in single research processes, we suggest a more technical pidgin, i.e. we try to use a more technical language in order to investigate both epistemic and social aspects of the situation. In explorations of characteristics of scientific fields, we apply a more social pidgin and use it to obtain information about both types of characteristics.

a) Communication about the interviewee's 'local' work

For the exploration of a scientist's research projects we use a pidgin that is predominantly technical. As a skeleton of such a language, some concepts describing general elements of research processes can be used. In any empirical research process researchers start with a question that is somehow rooted in a theoretical background, investigate a research object by applying methods that must be developed or adapted, and interpret the empirical results. Although there will be only a few research projects that follow exactly that sequence of steps, the steps themselves will occur in one form or another in all research processes, and scientists' perceptions of research processes correspond to this model.

We can use this very abstract level of common experiences to formulate questions about the interviewee's research. In the investigation of scientists' collaborations interviewees were asked about the elements of their research processes, e.g. by using the following questions:

> What research problem do you deal with?

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[10] This observation is only a tentative conclusion from our interviews, which we use as a working hypothesis. Systematic secondary analyses of more interviews would be necessary to gather sufficient empirical support for this hypothesis.

Could you explain to an outsider what it is you try to find out?

What methods do you apply? What equipment do you use?

What substances do you use? Where do these substances come from?

Wherever possible, these questions were specified by detailed knowledge that had been acquired in the preparation of the interview by reading research proposals, research reports etc. (see 4.2). The questions about elements of research processes led to hints about other researchers who contributed to the interviewees' research in different phases. Thus, the cognitive links that were created via the exchange of substances, joint use of equipment etc. hinted to other researchers who were identified as collaborators.

> Q: And have you applied additional methods in your project?
>
> A: Well, I would say yes, I did try something different… We tried to characterise these layers by ellipsometric methods, for example, because we were never sure how they look like …
>
> Q: Where did you do this?
>
> A: I gave the layers to these people, that means in the clinical research centre. Y and the current undergraduate student are working at it. And they tried it, because I do not know the equipment very well. I looked at the equipment and watched them when they were working.

In this part of the interview, the interviewer and the interviewee jointly deconstructed the interviewee's research project, which led to the identification of collaborations. This strategy worked well in all interviews on collaboration. By 'disassembling' the research process into its elements it was possible to find opportunities for collaboration, as well as real (successful and unsuccessful) collaborations. The variation of links between the research processes that were reported in the interviews supported the construction of types of collaborations.

A similar strategy was applied in an investigation of East German basic research. The aim of the project was to find out whether the radical institutional changes that accompanied German unification led to changes of the basic/applied character of East German research (Gläser 1998, 2000). In this project, an in-depth description of the basic/applied character and its dynamics was needed. As was the case with collaborations, the elements research problem, research methods and research objects were used to ask detailed questions about actual and possible links of interviewees' projects to contexts of application.

> Q: If I understand it correctly, your work is purely theoretical.
>
> A: This is a theoretical research group, basic research, but we have always worked close to the experiments and do it now more intensively because we benefit from new opportunities to collaborate with the right groups.
>
> Q: That would have been my next point: It is possible in the field of theory to work far remote from experimental systems, which means to work with models that

are so abstract that they do not correspond to experimental systems, but does this happen in your group?

A: We don't do this. Actually, the work with the polymers might be slightly more Hamiltonian-oriented, but not in our group. We have very, very close connections to experimental groups.

Q: Work on semiconductors and connections with experimental groups suggest that there is a link to applications?

A: Yes, this link surely exists in the end. Depending on how the funding agency regards its importance, one can emphasise it more or less. I wouldn't regard it primary for me and my work. It is actually the explorative side of basic research. It is not excluded that there is an application in the end, but that is not our primary concern.

Q: Would these applications emerge from your research, or would they be a result of experimental research?

A: This would be a result of the experimenters' work.

The interviewers' success depended primarily on the common language that was constructed at an appropriate level of simplification. This was especially problematic in the investigation of theoretical research processes that cannot be disassembled into comprehensible manual operations. Extensive mathematical knowledge might be necessary even to understand what are the elements of the research process. Therefore, in some interviews with theoretical physicists we had to rely on global descriptions because we didn't comprehend the mathematics well enough.

b) Communication about the interviewee's research field

To achieve an 'ad-hoc pidgin' for the communication about research fields is more difficult. Attributes of research fields are aggregations of research processes or emergent characteristics of the field level. A big problem that hinders all communications about research fields is the latter's fuzzy and fractal structure. The simple question "To which research field does your work belong?" already leads to difficulties because the interviewee can subsume his or her research under a broader or narrower research field at will. The term 'field' is subject to widely varying interpretations, as was described for 'collaboration' and 'basic/applied character' (see 3.3). The interviewees name their field close to the level of a discipline (for instance, "Organic Chemistry"), or as a subfield, or even by describing the subject matter of their current research. Therefore it is very difficult to agree on the conversation's subject matter in these parts of an interview.

A second problem is that characteristics of a field that are needed in science studies - size, age, growth dynamics, internal structure (how many subfields and their degree of connectedness) etc. – are not part of scientists' everyday experience.[11] Scientists, of course, do understand the terms "size of a field" or "dynamics of the field". However, questions about these characteristics force them to look from above on their own field and even to compare it with other fields to which they do not belong. Thus, a badly operationalised question could lead to answers that were hardly interpretable, or even to a blunt rejection.

> Q: How would you – to provide me with a picture – describe the field 'integrated optics'? How big is it approximately?
>
> A: How big it is I can't answer because I don't know what the scale is.

The characteristics of fields that sociology of science is interested in are not established with an absolute scale but only in a comparative perspective. Younger scientists especially often have difficulties comparing their own field with others. In our interviews, only senior scientists who are core members of one scientific community and are familiar with others were able to give comparative descriptions of their fields. And even these scientists had difficulties giving a description of their field. Moreover, their descriptions depend to a certain extent on the fields that are used for comparisons.

For these reasons, we translate the cognitive characteristics of research fields into social indicators, thus creating a pidgin that is primarily social and thus closer to the scientists' experiences of social interactions with other members of their field. For example, the following questions were used to obtain information about a field's size:

> How many scientists do you know who work in your field?
>
> Is there sharp competition in your field?
>
> Has the field its own conferences? How many people usually attend these conferences?
>
> Has the field its own journals?
>
> Which groups work in [your country] in your field?
>
> Which groups work internationally in your field?

In the following example, the interviewee was able to answer the question and additionally introduced a comparison with another field whose conferences he has attended.

> Q: When you attend conferences: How many polymer physicists are there? I am trying to learn something about the size.

[11] This does not mean that research fields are not an important environment for scientists, as was claimed by Knorr-Cetina (1982) and Luukkonen (1995: 364). In our interviews, fields were always an important frame of reference for scientists, but a frame of reference that was constructed by them in varying and often idiosyncratic ways that did not explicitly include the characteristics we were interested in.

A: This can be answered relatively precisely: In the German Physics Society a Committee 'Polymer Physics' exists which usually brings together 250 to 300 people. That is a small group. If you compare it with others, solid state physics are 1000 or even more. We can't match this.

The interviewee could easily answer the question because it was related to his personal experience. He was not forced to interpret an abstract concept he never or rarely applies to his field (size), but was asked for empirical information he had no problems providing.

4.2 Preparing the interview

Choosing an informed observation approach requires learning about the fields under investigation and about the interviewees' research prior to the interview. We usually apply two strategies of information collection. Firstly, we try to obtain general information about the research field(s) under investigation by studying reference books of the discipline the field belongs to or of the field itself. We used these books especially to get information about the field's most important methods and to understand basic, often used terms. Of course, it is impossible to catch up with years of scientific training by studying reference books. But it was possible in almost all cases of experimental research to develop a general understanding of what the work in the field is about and how problems are tackled. The following quotation exemplifies why it is useful to get this kind of knowledge:

Q: What is the common background of the projects you are conducting?

A: Organic Chemistry.

Q: This is very general. Organic chemistry is a very large field.

A: Synthesis and Preparation of natural substances and synthesis of derivatives. That could be said, generally.

Since the interviewer knew that organic chemistry is too large to form the common background of a single scientist's projects she was able to extract a specification. The strategy of studying reference books becomes rather difficult or even impossible if a whole range of research fields has to be investigated. However, we would still propose to try.

The second strategy is essential for informed interviewing and should always be applied. It is crucial to collect information about the interviewee's research prior to the interview. Zuckerman (1972: 163-166) reported how extensively she prepared her interviews with Nobel laureates. She studied the laureates' addresses given on the occasion of their Prizes, prepared publication lists, and read publications like those written by the laureates for lay audiences. She prepared a summary of each laureate's career and his work as a preparation for the interview.

We usually prepare our interviews in a similar way. As a rule we use the following sources to get information about the scientists work:

- Research proposals and research reports;
- Publication lists from publication databases like the Science Citation Index; and increasingly in the last years
- Information obtained from the internet about research projects, methods and equipment of the group and the like.

In the following quotation the interviewer used information about collaboration from the interviewee's research proposal:

> Q: I read in the proposal about the project B1 that there has been already a collaboration, which means already in 1985 …
>
> A: Oh, yes. Look, I forgot that.
>
> Q: I assume the methods belonged to the group X, namely UV-laser.
>
> A: Right, yes, yes. I completely forgot this; we had a lot of publications there, too. There was a postdoc … We worked well together, liked each other, he was a very good scientist and was interested in our approach. I did a lot of work together with him. He was postdoc in the group X, and that is why I had access to the physics department to some very exotic laser that does not exist even in group X. It was necessary to convince a very strange laser physicist that our biological dirt is worth an investigation. In this case X and his postdoc helped me very much … This was the ignition for one of our most successful projects that led us very far, also internationally.

The interviewed biologist provided detailed information about a research collaboration he had already forgotten. It was all in his head, but the description had to be triggered by a question, and the question required prior information.

It is also useful to study posters often located in front of the working rooms and labs in the time directly prior to the interview. This sometimes makes it easier to start the informal talk with scientists, which leads into the interview. If the opportunity occurs we visit the interviewee's laboratory (we often get invited to a lab tour after the interview). This is an excellent opportunity to enhance one's understanding of the science, e.g. by getting a graphical image of the equipment and how it is used, and by obtaining additional explanations of the laboratory practice.

4.3 Negotiating the level of communication

Each interview begins with a phase of implicit negotiations. Part of these negotiations is that the interviewer suggests a vocabulary for the pidgin, which is changed by the interviewee's responses. In this introductory phase, while it is being negotiated what technical terms can be used by the scientists so they are properly understood by the interviewer, it is simultaneously negotiated how 'scientific' explanations may be in order to be understood. This negotiation phase has been experienced by Zuckerman:

> Intensive preparation brings growing familiarity with the technical language developed by the laureates. In the early phase of most interviews, the laureates tried to avoid the use of language I might not understand. When given cues that they would be understood – particularly by my using such terms –they relaxed and their vocabulary more closely approximated their usual one. …
>
> The scientific language as well as the trade vernacular was used to convey the sense that the laureate was not talking to a total alien. It was not intended to convey expertedness on the part of the interviewer and did not seem to be perceived as such an attempt. (Zuckerman 1972: 170)

The introductory phase of the following interview (from the project on East German basic research) is an instructive example of carelessness in the negotiation phase. The interviewer had done his homework but blunders in the introduction by asking shallow questions and downplaying his preparation:

> Q: The first question is: What are you currently working at, that means, your department? I have read a bit in your yearly report, but I am a layperson in physics. What I have found out is that you are dealing with laser physics.
>
> A: Yes.
>
> Q: And, if I understood it correctly, primarily with the development of methods?
>
> A: Yes. And application of these methods.
>
> Q: And application of these methods, too?
>
> A: Yes, yes. It is of course the question how precise an answer you want. When you say you are a layperson, then it is of course not really important for your investigation what we do in detail, but probably only a rough description.
>
> Q: Yes.
>
> A: It is of a basic character and indeed aimed at the further development of certain methods of laser spectroscopy, which can reveal very fast processes in molecules. … But it is basic investigations, first steps, which are investigated. When we had nanoseconds it turned out that the fastest reactions appeared to take nanoseconds. You know what a nanosecond is?

The interviewer presented himself twice as a rather uniformed layperson and was consequently treated by the interviewee as such. The interviewee considered details of his work as unimportant to the interviewer and explained his works as simple as possible. This created a problem for the interviewer who needed detailed descriptions of the scientists work to answer his sociological research question. Thus, he had to repair the damage in the subsequent phase of the interview in order to get the appropriate level of communication.

In the following example, the interviewer starts with the very general question without indicating her knowledge about the communication's subject. She is being treated as a layperson, and the communication begins at this level. However, the interviewer begins a negotiation which raises the level at which communication takes place.

> Q: What is your research field and since when have you been working on it?
>
> A: Well, you are not a natural scientist. How precise would you like to know it? My research field is biochemistry of the neural system, neural chemistry as it is called. I have been working on it for a long time … Biochemistry of the neural system concerns the signal processing and signal transmission by certain protein molecules, which are called receptors.
>
> Q: Your special object of investigations is the acetylcholine receptor.
>
> A: This is a receptor that acquires neural impulses and transforms them into an effect.

By informing her interviewee that she knew about the acetylcholine receptor, the interviewer signalled that technical terms could be used in the interview, and began to move the interview to a level of more detailed descriptions of research processes. Later in the interview, the interviewee without hesitation used his technical language to describe the emergence of collaborations.

> ... If I remember correctly, we had several plans at this time. We primarily investigate structures. One assumes that these biologically important molecules - such a receptor – can be understood if its spatial structure is understood. And we talked to the X's Group, which consists of very good crystallographers, about how we crystallize this thing. … This was a starting point for trying this together with them.

Our final example demonstrates a better introductory phase, namely a truly informed beginning of the interview.

> Q: ... I have looked up in the internet what you are working at, what your research field is. And I understood it as follows: You conduct surface investigations of semiconductors and metals and aim at a microscopic understanding of the interaction of molecules and atoms on surfaces.
>
> A: That is a big part. Another important part are organic thin layers, organic materials that are deposited on anorganic solid states and reverse, in order to make devices. But we are primarily concerned with the foundations. This belongs to the area of soft matter … And there we use our technologies for advancing the microscopic understanding.

In this interview, the interviewer begins with a description of the interviewee's research field as she understood it from information collected prior to the interview. In doing so, she is trying to communicate the level of her understanding of the interviewee's research and the technical terms that can be used in the interview. With his answer, the interviewee is adapting to this level of communication.

There is also a danger in the interviewer's self-presentation as scientifically well informed. Scientists can forget that it is not a colleague they are talking to, and can therefore move up to a scientific level the interviewer cannot understand. Whenever this happens, the interviewer must negotiate the level downwards by stating that the scientific argumentation was incomprehensible. Thus, the aim of these negotiations cannot be to pretend an understanding that is not there (e.g. to impress the interviewee), because the interview can produce useless (because incomprehensible) scientific talk. It is important to achieve a level at which the interviewer can still understand all the dialogue of the interviewee.

5 Summary and conclusions

We have encountered several limitations to our approach of informed qualitative interviewing. One limitation is produced by specific fields like mathematics or theoretical physics. While it was usually possible for us to understand the problems and strategies of experimental research, we couldn't achieve a similar simplified understanding of the practices of mathematics and theoretical physics. Apparently, some fields can be studied only by native observation.

A second limitation occurs if comparative research across several fields is conducted. In this case, the sociologist, who of course has the prime task of preparing the investigation sociologically, is endangered by information overload. There are limits to a scientific preparation when one has to interview a molecular biologist on Monday, a solid state physicist on Tuesday, an electrical engineer on Wednesday and a physical chemist on Thursday. However, this problem can be coped with if it is acknowledged that qualitative interviewing is no 'quick' ('and dirty') method but takes at least as much time as a solid ethnographic observation.

A third limitation is that informed interviewing cannot be extended to the background of scientific work that is acquired by systematic scientific education, and which often remains hidden in scientific work. This principal limitation marks a difference to studies of many other social groups. There is a kind of knowledge that is implicitly present and partially communicated but had been acquired by ways that are qualitatively different from the practices that are currently observed, and are located outside the field under study. It is not always tacit knowledge but sometimes knowledge that is so elementary and evident for scientists that it is not communicated. While ethnographic observations (and, as we would like to add, qualitative interviews) are well equipped to identify the cultural taken-for-granted assumptions in many social settings, it is not clear to what extent we can identify the scientific taken-for granted assumptions of scientists unless they are challenged by the scientists themselves.

With regard to identifying taken-for-granted assumptions, we would like to come back to a point that has been argued in the context of ethnographic methodology. Yes, there is the serious danger of *not* getting certain information in an informed interview. The interviewee will form assumptions about the interviewer, and about what the interviewer already knows. Therefore, informed interviewing increases the danger of not being told something that should be told because your interviewee thinks you already know this. This can be partly helped by careful in-depth probing during the interview. However, there seems to be an unavoidable trade-off between not being told something because you are assumed to already knowing, on the one hand, and not being told because you are assumed to being unable to comprehend.

Our methodological discussion is, of course, limited by our own experience. While some clear patterns exist in our interviews, any generalisation requires the inclusion of the experiences of as many investigators as possible. Given the current level of methodological discussion on informed interviewing, any discussion must start from scratch, and discussing one's own experiences is not the worst way to begin.

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