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Interviewing Scientists

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Abstract

With this article, we would like to initiate a discussion about a methodological problem that is central to many empirical science studies but has received far too little attention, namely scientifically informed interviewing. To what extent do we have to understand scientists' work *scientifically* in order to explain their behaviour *sociologically*? As far as it is existent at all, the methodological debate in science studies has focused on ethnographic observations. In this debate, the two approaches of naïve observation and informed observation (which sometimes takes the form of native observation) can be distinguished. The general methodology of ethnographic observation clearly favours the informed approach, as does the general methodology of qualitative interviewing. 'Scientifically informed interviewing' specifies this general methodological insight for science studies but is also necessary because in some investigations we must systematically collect data on the content of our respondents' research. This kind of interviewing requires extensive preparation of interviews, the construction of an 'ad hoc – pidgin' for the communication during the interview and the negotiation of an appropriate level of scientific depth between the interviewer and the interviewee. We make suggestions how to solve these tasks (and how not to) and discuss limitations of the approach of informed interviewing.

1 Do we need to understand science?

With this article, we propose a discussion about a methodological problem and its practical consequences for interviewing scientists.[1] To what extent do we have to understand scientists' work *scientifically* in order to explain their behaviour *sociologically*? This question specifies a fundamental methodological 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 all sociological studies must accomplish an interpretive understanding of social action, the extent to which this is necessary and the difficulties resulting from this task vary between fields of inquiry. We will argue that some studies of science depend on an understanding of science not only because it is important to understand frames of reference of respondents but also because we need to include the materiality of research actions in our explanatory models, and the research of our respondents provides the only access to these explanatory factors.

Another property of our subject that makes understanding a difficult task is the way in which its practitioners have been prepared for their tasks. Being a competent member of the scientific 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 completely closed by staying in the culture.[2] In this respect, the sciences are different from the many social settings that are 'self-explanatory', i.e., which contain all knowledge that is needed to be a competent member (e.g. the communities of sports fans). 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 in principle they can acquire as much knowledge as any other prospective member of the culture.

In this article, we address the general problem of understanding scientists and the ramifications for qualitative interviewing by answering three questions. Why should sociologists attempt to understand the science of their interviewees? What happens prior to and during a scientifically informed interview? What are the limitations of scientifically informed interviewing?

Except from some early reflections by Zuckerman, the problem of scientifically informed interviewing has not yet been discussed. When methodological problems of science studies are considered at all, the debate is almost exclusively focused on problems of ethnographic studies. In this debate, the problem of scientifically informed observation has been an important point. We therefore begin by identifying three approaches to the problem of 'informed observation' in science studies

[1] Along its way since its first presentation at the joint 4S/EASST conference in Vienna 2000, 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. We are also grateful to the reviewers of STI studies, whose critical comments led to another significant revision.

[2] The sciences share this property at least with the professions; see Ten Have's (1995: 254-256) distinction between "the lay world" and "the professional world".

(2). Thereafter, we outline the position of general qualitative methodology, which is all in favour of informed interviewing, and demonstrate why some research questions demand an engagement with the content of our subjects' work that goes far beyond what is demanded by general methodology (3). Using our own experiences and mistakes, we then discuss the three main practical tasks that must be solved in scientifically informed interviewing of 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 give 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. In our perspective, 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 'inbuilt' 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)

This is of course an important methodological point: 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

[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).

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.

While the danger of 'going native' is real and the consequences would be severe, the methodological conclusion drawn by Latour and Woolgar has problematic consequences of its own (Lynch 1982: 506-509; Lynch 1993: 93-102). Lynch raised two objections by pointing out (a) that Latour's and Woolgar's descriptions prove that they hadn't been able to maintain their naïve approach, and (b) that the naïve approach would severely limit the understanding of the object of observation.

(a) Lynch observed 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:

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)

Later in the same chapter, when the authors are describing the laboratory practice (ibid.: 53-69) and are categorising 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 scientists in 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.

(b) This purposeful ignorance of the content of the observed actions and the sole occupation with their outward appearance reduces the understanding of the observed practices to what is 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)

Not only is the observation reduced to what the naïve observer finds intelligible – the observers also can record their observation only in their own language. Thus, the naïve observers were forced from the beginning to select events and actions that seemed intelligible to them and to record them in a sociological language and attached conceptual frameworks. Influential concepts such as "inscription devices" appear to be the result of that naïve approach (Latour and Woolgar 1986 [1979]: 51-53).

We agree with both points made by Lynch. The perspective of a "naïve observer" is not only difficult to maintain but also methodologically problematic. 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, see 3.1). 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. With the exception of Latour and Woolgar, all ethnographers of science 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).

The same approach can be assigned to Lynch (1982; 1985; 1993; 1994), who bases it on ethnomethodology's principle of "unique adequacy" which requires ethnomethodologists gain the capability to perform the characteristic practices in the field under study (Garfinkel and Wieder 1992: 182-184; Lynch 1993: 271-275).[6] When these

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[4] Interestingly, Latour used a review of Lynch's (1985) book, which is based on informed observation, to state: "that one should become familiar with the practices of the people one wishes to study (...) is the basic tenet of all ethnographic work, and it is hard to dispute." (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).

[6] Lynch links his methodology to the work of Winch (1958; 1974). 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

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 rarely addressed. Collins reports that he had to abandon one case study because he was not able to acquire enough competence (see above). Lynch mentions 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 some understanding of the science that is being observed, but cannot become competent enough to perform the research they observe. 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), and the biologist Cambrosio (Cambrosio and Keating 1988, 1995). 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. They argued that "thin descriptions of the material dynamics and performative orderings of behavioural domains" are of interest to science studies (Knorr-Cetina and Merz 1997: 129-130). Given the limitations of informed observation, native observation appears to be the only way to arrive at this kind of account of scientific practice. Mulkay even went as far as stating "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).

While native observation solves the problem of understanding the field under study, it is not without problems. The observed or interviewed scientists are likely to relate differently to a former colleague who has turned into

(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).

a sociological observer. 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 an 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.)

Unfortunately, the authors who noticed particular relationships between scientist-observers and respondents did not discuss the possible impact of these relationships on their study. We are therefore unable to tell how the described problems influenced the social accounts of the scientific practices they studied.

Another problem is the greater danger of 'going native'. 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). As is the case with the particular relationships discussed above, the risk of 'going native' has been acknowledged but not discussed with regard to possible changes in the results of the studies. While native observation certainly bears the risk of 'going native', it has not yet been proven that this actually occurred, and the consequences for the empirical studies are unknown.

3 Informed interviewing in science studies

3.1 Informed interviewing as a principle of general qualitative methodology

While the argument for 'naive observation' of scientific practice has admittedly been made against the methodological mainstream of ethnography, no such stance has been taken with regard to qualitative interviews.[8] The general methodological tenet – that preparation of interviews and informed interviewing are prerequisites for success – has remained unchallenged in science studies.

The central argument for informed interviewing is based on the understanding of the interview situation as a communication process in which the two partners jointly construct the meanings of both, questions and answers (Cicourel 1964: 96-100; Briggs 1986; Holstein and Gubrium 1995: 45-46). In order to solicit the specific and detailed information they need, researchers must translate their interests into the contexts of their interviewee. Otherwise, neither formulating appropriate questions nor understanding the interviewee is possible (Merton, Fiske, and Kendall 1956; Hopf 1978: 99-101).[9] As Briggs' discussion of "communicative blunders" in interviews proves, the failure to understand the respondents' social world may result in asking the wrong questions, receiving answers to questions not asked, or simply not comprehending the right answers to questions (Briggs 1986: 39-60).

Thus, general methodology of qualitative interviewing unanimously considers informed interviewing as essential for the crucial task of understanding. Briggs demonstrates that even in the interviews he conducted as part of his ethnographic observation, his lack of understanding of the social context and worldviews of his respondents was a source of errors, of the inability to ask properly and to understand the answers. He was able to overcome these problems because his stay in the field made it possible to learn enough about the frames of reference of his informants. As we have noted in our discussion of naïve observation, even the deliberately naïve approach of Latour and Woolgar yielded to learning, which led to a better understanding of frames of reference and meanings of the field.

A second argument for informed interviewing refers to the social relationship between interviewer and respondent. Being informed helps to demonstrate competence, and thus to be taken seriously. 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

[8] The extent to which 'informed interviewing' is necessary at all depends on the research question and on the kind of qualitative interview that is used in research. In this article, we focus on semi-structured interviews, i.e. on interviews based on an interview guide, which are used to obtain information about the impact of specific conditions on respondents' work processes (see 3.2). Other kinds of interviews (e.g. narrative interviews conducted with the aim to explore how respondents construct their life-stories) may not require or enable informed interviewing.

[9] Zuckerman (1972: 165) confirmed that her preparation of interviews with Nobel laureates often called forth responses that otherwise would not have been elicited.

available material and now you need some help. (Rubin and Rubin 1995: 198)

This advice is in accordance with the experience of Zuckerman, who interviewed 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 maximizing 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 the Laureates perceived her 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)

3.2 Scientifically informed interviewing for collecting scientific data

When we apply the general methodological principles to qualitative interviews with scientists, we inevitably arrive at the conclusion that we need to learn their science in order to understand them. This holds true for all research in science studies that uses scientists as informants. In particular, it applies to qualitative interviews both as a 'stand alone'-method and as interviews with key informants as part of an ethnographic study.

In some areas of science studies, the necessity of scientifically informed interviewing follows not only from general methodology but also from the theoretical intentions of the research. Whenever the content of the science under investigation forms part of the aimed-for sociological explanation, it is not sufficient to understand our respondents' research as a relevant social context and frame of reference. We must systematically investigate the content of our respondents' research in order to obtain information about knowledge, technology and nature, which ultimately informs our sociological explanations. Empirical research of this kind requires to understand the problems, strategies, and logic of scientific research, and to include non-social factors in our explanations. We can leave aside here the differences between the concepts of 'non-human actants' (e.g. Callon 1986; Latour 1988; Law and Callon 1988), the 'mangle of practice' (Pickering 1995), and 'thin description' (Knorr-Cetina and Merz 1997, see 2.3) and focus on the point they have in common: Understanding and explaining scientific practice requires the inclusion of the non-social phenomena scientists deal with (Gläser and Laudel 2004).[10]

We can illustrate this point by using our own research as an example. 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), how the institutional change that accompanied German unification affected links between basic research and applications (Gläser 1998, 2000), or how evaluation-based funding of university research affects

[10] This problem is not unique to science studies but has been acknowledged and explicitly discussed here. One of the solutions to the problem of integrating social and non-social factors in explanations – Actor-Network-Theory (e.g. Callon 1986; Latour 1988; Law and Callon 1988)– has become influential beyond science studies.

the content of this research (Gläser and Laudel 2007). 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 *explanandum*.[11] It differs from the sociology of scientific knowledge in the explicit regard of social macrostructures, namely institutions, as part of the *explanans*.[12]

Following ideas of the new institutionalism that have emerged in political sociology, organisational sociology and economics, we regard institutions as only one of several factors that shape social action. In science studies institutional effects are likely to be field-specific, because the influence of institutions is modified by the epistemic practices that are characteristic of specific areas of inquiry. Thus, epistemic conditions of action and epistemic practices must be included in institutionalist analyses as intervening factors. We must conduct comparative studies across scientific fields and assess the mediation and modification of institutional influences by field-specific epistemic conditions of action and epistemic practices (Gläser and Laudel 2004).

An empirical example for this kind of research is 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 institutional conditions or actions, nor 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 that took place under similar institutional conditions. In order to do that, we had to address the content of research in our interviews, and had to ask about it in the interviewee's language and frame of reference. We had to understand that it is necessary to crystallize a protein in order to analyse its structure, that the protein had to be “perfectly pure” for crystallization to work (and what “perfectly pure” meant in this context), and that purifying proteins is not an equally straightforward procedure for all proteins.

Our point is that studies of institutional influences on the content of research, and probably many other areas of science studies, need scientifically informed interviewing not only to properly construct meaning and understanding of social factors a study is interested in but also of factors that are

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

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

alien to social studies of science, and are commonly described in the scientific languages of the respondents' fields. In these studies, we need to scientifically prepare interviews not only to achieve the kind of communication between interviewer and respondent that is deemed necessary by general qualitative methodology. We also need to gather information on non-social factors that need to be included in our explanations. The source of this information is our respondent, and the only frame of reference in which they can provide this information is their science.

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 a sociological interview of scientists is very similar to an interdisciplinary collaboration. The interactionist perspective on interviewing maintains that interviewer and respondent collaborate with the aim of producing information needed by the interviewer. In this ad hoc-collaboration, two worlds – the world of sociological investigation and the world of the scientist's work – intersect, and in order to communicate about it, a common language must be constructed. In this process, the task of the interviewer is not merely to adjust their language to the different cultural background of the respondent, but to create a language in which the relevant work experiences can be described in a way that is intelligible to both sides. In this process, the interviewer must adopt elements of the respondent's language and vice versa.

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 'pidgin' in Galison's account 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 perceive it as an evolving body of knowledge.

On the basis of this tentative conclusion from our interviews, 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?
- 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: The first thing is already this tricky question about understanding what you are trying to find out; what your current research is about. I had a look at your website and there was mentioning that you basically use this low energy electron microscope (...) to study the dynamics of processes on surfaces of semiconductors.

A: Yes, particularly the III-V systems.

Q: Yes. So, that would establish your object and the method, but what's the problem you are trying to solve?

A: Yes, well there are really two aspects to what we're trying to do. One is to look at III-V semi-conductors - so the idea is to really understand how atoms move around on the surfaces, the very basic statistical mechanics, thermodynamics of self organizations, how objects such as quantum dots form and that's a very big controversial area at the moment (...)

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. The basis of this deconstruction was that both partners knew that a variety of method could be used to achieve the aim of the project and that the equipment of the interviewee's lab limited the range of methods that could be used locally. The deconstruction 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. 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.

Constructing this conversation required knowledge about the two kinds of theoretical practice in physics – investigating theories that have no links to experimental research at all and theoretical interpretation (modelling) of experimental data - and about possible links between theoretical physics and applications – the object that is modelled (semiconductors) and the experimental research groups whose data form an input for theoretical research. The interviewers' success depended primarily on the common language that was constructed at an appropriate level of simplification. This was particularly problematic in the investigation of theoretical research processes that cannot be talked about in terms of comprehensible manual operations. Extensive mathematical knowledge might be necessary even to understand 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 at 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.[13] 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 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 they had sometimes difficulties. Moreover, their descriptions are shaped by the fields they select as references.

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?
- Does your field have its own conferences? How many people usually attend these conferences?
- Does your field have its own journals?
- Which groups work in your country/world-wide 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.

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

Conducting a scientifically informed interview requires extensive prepara-

[13] 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 even though our respondents constructed them in varying and often idiosyncratic ways, which complicated the task of soliciting comparable descriptions.

tion. We must not only develop our own conceptual schemes and translate them into interview questions but also (at least to some degree) the conceptual schemes and research strategies of our respondents. Unfortunately, interviewing as a 'stand alone' strategy of data collection does not enable learning processes in the field. The interviewer has only one opportunity – the one to two hours of interaction in the actual interview - 'to get it right'. Therefore, in qualitative interviewing the learning must take place prior to data collection. This is particularly demanding when a study features a comparative approach that includes several fields, thus requiring the understanding of not one scientific culture (as usually is the case in ethnographic studies), but several of them simultaneously. We have faced this problem in our own comparative studies of institutional influences on the production of scientific knowledge. The comparative approach severely limits the time one can spend for understanding the fields.

We usually apply three 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 PhD thesis:

Q: You wrote in your PhD that the general thing, doing time lapse studies, has been done before, but not with the object that you were studying. Is that right?

A: Yes, that's right. There was a lot of early work in grasshoppers, which have huge neurons, just massive, and they are very easy to do, bring a needle down and inject them. And it's all very clear what the cell is doing in the grasshopper because everything is so big (...).

In his much longer answer, the interviewed biologist provided detailed information about the field and the position of his research in it, the reasons why he did follow this line of research, and about the specific methodological problems he has to deal with. The informed question and the reference to his PhD triggered the hoped-for response.

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.

A third strategy we developed recently is analysing the interviewee's publications. This method circumvents the difficulties of understanding the science by bibliometrically analysing structural properties of the interviewee's oeuvre. The results can be visualised as 'bibliometric research trails' – evolving networks of publications -, which we used to discuss the content of the interviewee's research as it unfolded over time (Gläser and Laudel 2007). While bibliometric analyses are a useful additional tool for understanding a respondent's research, they cannot replace the understanding of content – without having some idea about the content of the science, we would not even understand what the structure means.

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 deployed 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: And your special object of investigation 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 for-get 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 does not exist (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 Conclusion

Scientifically informed interviewing is necessary in science studies because it is the only way of understanding what our respondents mean when they answer our questions, and because we often must use interviews to collect data about the content of our respondent's research. This kind of interviewing requires extensive preparation of interviews, the construction of an 'ad hoc – pidgin' for the communication during the interview, and the negotiation of an appropriate level of scientific depth between the interviewer and the interviewee.

While the necessity of informed interviewing has not been explicitly questioned, the extent to which interviews are prepared scientifically is likely to vary. Owing to a lack of material, it is impossible to compare naïve and informed approaches to interviewing the same way as we have done for observations in this article. Neither are there outspoken advocates of naïve interviewing, nor is it easy to recognise studies that applied such an approach. Though both, the danger of 'going native' and the danger of shallow accounts of scientific practice are real in interview-based studies, too, we cannot yet discuss them because we do not have sufficient information about methodologies of science studies, let alone links between methodologies and results. Judging from results of quantitative studies on the impact of evaluation-based funding on the content of research, which are by design forced to rely on naïve interviewing, the shortcomings of such an approach may be severe (Gläser et al. 2002).

Since the main purpose of this article is to invite readers to a methodological discussion and exchange of experiences, we conclude this article by pointing out limitations to our approach. A first 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 at some level of simplification, we couldn't achieve a similar simplified understanding of the practices of mathematics and theoretical physics. In these fields, simplified descriptions of problems, objects, and methods of research appear to be more difficult to achieve. We tried, and our respondents tried – but in many cases 'the collaboration didn't work'. Thus, it seems that some fields can be studied only by native observation respectively interviewing.

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. Acknowledging this problem implies to give up the idea that qualitative (semi-structured) interviews are an 'easy' or 'quick' method. One has to invest an enormous amount of time in order to interview scientists properly.

A third limitation is that informed interviewing cannot be extended to the background knowledge of scientific work that is acquired by systematic scientific education and experience. In our opinion, the scientific taken-for-granted assumptions and the tacit knowledge cannot be investigated by qualitative interviews. The method of choice for studying the role of this knowledge is ethnographic observation. The fact that even ethnographic observations have their problems here (as the comments of ethnographic observers on the limits of their understanding indicate, see 3.2) hints to a difference between science studies and studies of many other social groups. Scientists carry knowledge that is implicitly present and partially communicated but had been acquired by ways that are qualitatively different from the practices that can be currently observed, and are located outside the field under study. Since we cannot investigate this background, 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.

Applying a scientifically informed interviewing strategy even increases the danger of not being able to identify taken-for-granted assumptions. Our respondents might not tell us because they think we know. This point 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 doing this with one's own experiences is not the worst way to begin.

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