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The sociological description of non-social conditions of research

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Abstract

The aim of our paper is to outline an approach to comparative investigations of natural, i.e. essentially non-social influences on human actions. Any sociological approach that does not subscribe to radical constructivism implicitly or explicitly acknowledges that nature interferes with human action. It must find a way of including non-social influences in sociological explanations. Sociology of science is especially affected because scientific research is aimed at investigating nature and therefore shaped by it in a rather unmediated way. The solutions offered by sociology of scientific knowledge – especially Actor-Network Theory and the 'Mangle of Practice' – are insufficient because they combine highly abstract concepts with idiosyncratic descriptions, both of which are unsuitable for comparative approaches. As a solution to the problem, we propose to identify sociologically relevant classes of non-social factors (epistemic conditions of action) and to look at the channels through which these conditions affect social action. These channels of influence can be described by linkage variables which depend on the non-social factors but are at the same time compatible with sociological descriptions of actions. This approach is demonstrated by two examples.

Contents

1 How to observe the observation of nature?[1]

Any sociological approach that does not subscribe to radical constructivism implicitly or explicitly acknowledges that nature interferes with human action. Consequently, any such approach must either prove that nature's interference is sociologically irrelevant, or it must find a way of including natural - i.e. essentially non-social - influences in sociological investigations and explanations. Sociology of science is especially vulnerable in this respect because scientific research is a human action that is aimed at investigating nature. Both the part of nature investigated by scientists and scientists' current understanding of that part must be assumed to have a decisive impact on the content and the results of research action.

The question whether – and if so, how – nature and knowledge about nature must be dealt with by the sociology of science has been subject to a long and sometimes heated debate. Unfortunately, this debate has focused on the philosophical foundations and consequences of the above-mentioned questions. What has not been addressed is the methodological question of *how* empirical sociological investigations of science should include nature and knowledge about it. This is unfortunate because empirical studies of science must solve two rather difficult problems in order to achieve progress in explaining science. Firstly, empirical findings obtained by studying different scientific fields must be related to each other. Causal explanation ultimately rests on the opportunity to compare different settings, i.e. settings that are characterized by varying conditions and outcomes of processes. For science studies that start with the premise that nature matters, the comparative strategy inevitably implies a need to find an approach that enables a comparison of different natures. Secondly, if nature matters it is still only one of several factors that explain scientists' actions. Consequently, its impact on actions must be integrated with social factors that affect actions into one explanatory framework.

Thus, a methodologically sound approach to the integration of nature into sociological explanations of science must make the impact of nature on social actions comparable in two different dimensions: Different natures that influence actions in different settings must be comparable to each other, and the impact of nature on human action must be comparable to social influences on the same action. These tasks have not yet entered the methodological discussion of science studies, let alone be solved. Currently, comparative empirical studies of science that include nature seem to be

[1] We would like to thank Renate Mayntz for her critical and helpful comments on an earlier version of this paper.

impossible by design. The literature is dominated by single case studies, and 'comparisons' are reduced to observations that it is this way in one case and a different way in the other case. Knorr-Cetina's book on Epistemic Cultures illustrates this point nicely because it is explicitly aimed at a comparison of "epistemic cultures" in high energy physics and molecular biology (Knorr-Cetina 1999). It provides detailed descriptions of high energy physics' and molecular biology's epistemic cultures. However, the methodology of comparison is questionable:

> Using a comparative optics as a framework for seeing, one may look at one science through the lens of the other. This 'visibilizes' the invisible; each pattern detailed in one science serves as a sensor for identifying and mapping (equivalent, analog, conflicting) patterns in the other. A comparative optics brings out not the essential features of each field but differences between the fields. (Knorr-Cetina 1999: 4)

The comparison is being conducted by constructing a separate account of each epistemic culture that uses the "optics" provided by the other field. Rather than developing a framework that enables comparisons of both epistemic cultures in the same dimensions, Knorr-Cetina develops one framework for describing the epistemic culture of high energy physics and a different one for describing molecular biology. Thus, we have two mostly idiosyncratic frameworks and descriptions. Consequently, Knorr-Cetina is able to convincingly show that the epistemic cultures are different, but she can neither explain why they are different, nor can she answer the question whether (and if so, how) certain features of one epistemic culture correspond to (different) features of the other epistemic culture.

To compare different natures or to compare natural to social influences on action might be of minor importance for studies that aim to show how scientific knowledge is constructed in different settings. Since every knowledge claim is unique, idiosyncratic descriptions of knowledge production seem to be a natural outcome of such studies. However, for studies of this type to be integrated or generalized, the idiosyncratic descriptions of nature must be overcome, and comparable descriptions achieved. This is of even greater importance for institutionalist studies, i.e. for studies that aim to investigate the impact of institutions on knowledge production. Institutions are social macrostructures that span different social settings. The impact of institutions on scientists' actions must be assumed to be influenced by the specific local conditions, among them the subject matter of scientists' work, i.e. nature. Therefore, studies of institutional influences of scientists' action have two options: They can either try to ignore the content of scientific work and the specifity of local settings (as the old sociology of science did) or they can address the problems of comparing natures.

We have had to address these methodological problems in our institutionalist research projects because it is impossible to provide valid accounts of institutional impacts on science without explicit reference to all other influences on scientists' actions. The aim of our paper is to propose an approach that enables comparative empirical influences. Developing an own approach is a quite ambitious undertaking, but we will argue that we had no choice because of the traditionally awkward handling of this problem by the sociology of science (2). Currently, we are left with a rather clear description of the underlying methodological problem and two prominent solutions that don't support comparative research (3). Our own proposal rests on the idea of introducing epistemic (among them natural) conditions of action (4). We will use examples from our own empirical studies to illustrate our approach (5). As a conclusion, we will discuss the limitations of our approach and whether they can be overcome (6).

2 The Sociology of Science's struggle with nature

Sociology of Science didn't have any problems with nature unless it turned to the study of scientific practice in the seventies. Before that turn, the sociology of science had focused on the macro-level, i.e. on the social structure of scientific communities. The production of knowledge in laboratories and scientific discourses was black-boxed (Whitley 1972; Woolgar 1988: 39-41; Knorr-Cetina 1995: 140-141). Assumptions about the inside of the black box, i.e. about what scientists do when they conduct research and argue about results, were taken from rationalist philosophy of science. As a result, scientific research was regarded as unveiling laws of nature by following a specific rational methodology.

While nature was irrelevant for this approach, knowledge about nature was not. Kuhn introduced the idea that scientific communities were held together and organized by a paradigm, i.e. by knowledge, rather than by shared norms. This idea led to the question how social order varies with the specific knowledge of different scientific communities (Whitley 1972). Research on cognitive structures as a condition of scientific activities investigated attributes of fields such as restrictedness (Whitley 1977; Rip 1982) or paradigmatic maturity (Böhme et al. 1983). Weingart provided an extensive list of cognitive structures (Weingart 1976: 33-92). The most far-reaching attempt in this context was the project of Whitley (1984). Applying ideas from the organizational sociology's contingency approach, Whitley tried to link cognitive features of scientific disciplines to the disciplines' social organization. To describe cognitive features of disciplines, he used the variables applied to the description of an organization's technology – task uncertainty and task interdependence. These variables were used in organizational sociology to link an organization's technology to its social structure. In Whitley's account, they link cognitive characteristics of scientific work to social relations between scientists. Since he used abstract variables, Whitley could develop a comparative analysis of scientific disciplines' cognitive characteristics. However, he never obtained empirical data on these characteristics of scientific fields.

All the attempts to describe cognitive structures were clearly oriented towards comparative analyses, however, none of them was empirical. No methodology was provided that could guide empirical analyses and subsequent comparisons of scientific fields' cognitive structures. While this strand of the sociology of science hinted to an important problem, all solutions offered remained ultimately speculative.

In the mid-seventies, a new sociology of scientific knowledge radically departed from the old rationalist assumptions about science and turned scientific practice into an object of empirical investigation. A first important step was the 'strong programme' proposed by Bloor (1976). It introduced a symmetry principle into the methodology of science studies: Scientific statements that are believed to be true and those that are believed to be false must be explained by the same kinds of causes (ibid.). Beginning with the second half of the seventies, empirical studies of scientific practice challenged both the restriction of traditional sociology of science to the macro-level and the black-boxing of scientific practice. Reports on practices of experimental research and scientific discourse easily destroyed the rationalist picture that has been used as a surrogate for empirical findings by the old sociology of science (e.g. Latour and Woolgar [1979] 1986, Knorr-Cetina 1981; Lynch 1985; Pinch 1986; Pickering 1984, Gilbert and Mulkay 1984).

With its empirical studies of scientific practice, the sociology of science confronted nature's impact on actions in science – and initially ignored it. Driven by its empirically justified negation of the earlier position, the new mainstream of science studies proceeded to a radical constructivist position that led directly into the philosophical debate about realism and relativism. The sociology of scientific knowledge (SSK) emphasized that scientists construct scientific knowledge by actions that are not epistemologically different from everyday practice. Scientists 'tinker' by adopting to local opportunities and restrictions, provide 'cleaned up' accounts of their research activities in their publications, and act strategically and politically in order to let their results dominate scientific practice of their colleagues. Driven by the will to ridicule scientists' and philosophers' 'naïve realism' (the belief that scientists in their research depict the laws of nature), early SSK tried to reduce explanations of scientific practice to social factors, thus assuming a radical constructivist and relativist standpoint:

> The whole field of social studies of science pioneered by Collins and several other social realists hinges on this: nonhumans should not enter an account of why humans come to agree what they are. (Callon and Latour 1992: 352)

While the constructivist turn lead to many philosophical discussions, there was much less methodological discussion about the new type of empirical science studies.

However, comments were made on one major methodological decision that later proved to be consequential for accounts of nature. The decision that must be made comes with the ethnographic method: To what extent must an observer understand the specific culture in order to provide adequate descriptions and explanations? When the ethnographic method diffused from anthropology to science studies, the question was decided in the 'source field': Mainstream anthropology had agreed upon the necessity of understanding the content of actions under investigation (Latour 1990: 146). This position was explicitly formulated by Knorr-Cetina in an article on anthropology and ethnomethodology (Knorr-Cetina [1980] 1993: 170) and applied in her ethnographic studies (Knorr-Cetina 1981: 31, note 64). It was also used in an ethnographic analysis of theoretical physics (Merz and Knorr-Cetina 1997: 74; Knorr-Cetina and Merz 1997). In their investigation of 'spoonbending' Collins and Pinch took a similar position by conducting a participant observation (Collins and Pinch 1982, for a methodological discussion see Collins 1984). The same position can be assigned to Lynch (1982, 1985) and to Traweek (1988: 9-11).

Latour and Woolgar took a diametrically opposing standpoint by stating that their ethnographic observations of science are conducted with the perspective of an "very naïve naïve observer" (Latour 1990: 146; see also Salk [1979] 1986: 12; Latour and Woolgar [1979] 1986: 29-30; Woolgar 1988: 83-96). They describe their methodological decisions as follows:

> 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)

This methodological approach was criticized by Lynch (1982: 508-509) and defended by Latour and Woolgar (1986: 278-279).[2] With this exchange, the discussion about SSK's methodology of empirical research was closed. The methodological question as to what extent the varying approaches affect the outcomes has not even been raised. Owing to this lack of discussion, it cannot be said how the different methodological standpoints affect the kinds of results produced.

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[2] In the context of ethnographic methodology, Latour's and Woolgar's position has been criticized as "outsider myth" according to which " only outsiders can conduct valid research on a given group; only outsiders, it is held, possess the needed objectivity and emotional distance". (Styles 1979: 148, for a discussion of different approaches to observation see Hammersley and Atkinson 1995: 80-123).

Beginning with the mid-eighties, some proponents of the new SSK recognized that their accounts of the social construction of scientific knowledge remain incomplete without a reference to the role nature plays. Two interpretive frameworks to overcome this weakness have become prominent: Actor-Network Theory (ANT, e.g. Callon 1986, Latour 1988, Law and Callon 1988) and Pickering's "Mangle of Practice" (Pickering 1995). Both approaches start with the premise that nature's influence has to be accounted for in explanations of scientific practice, and both offer a solution to this problem.

The central idea of ANT is symmetry – not to start with a difference between 'nature' and 'society' but to treat the observed entities as actors (or actants) depending on their activities in the construction of knowledge.[3] Consequently, not only humans but scallops, kerosene, microbes, scientific devices, or texts – whatever is observable – can obtain the status of an actor in this network. Thus nature is included by selecting its bits and pieces that seem relevant to the observer and ascribing the ability of intentional action to them. In this framework, nature gains a status equal to social actors and social relations and is described in a sociological language. What aspect of nature is relevant to the observer (and is therefore included into the actor-network) depends on how the observer interprets the scientists' actions and the processes they deal with.

Central to the approach of Pickering (1995) is the concept of resistance and accommodation. He introduces nature (in his account "material agency") as a source of emergent resistances to researcher's goal-attainment. In order to achieve their goals, researchers are forced to accommodate their practices to the resistances that emerge in their practice until they reach their aims (which are subject to re-definition in the course of accommodation). The resistance is locally and temporally emergent.

> As active, intentional beings, scientists tentatively construct some new machine. They then adopt a passive role, monitoring the performance of the machine to see whatever capture of material agency it might effect. Symmetrically, this period of human passivity is the period in which material agency actively manifests itself. Does the machine perform as intended? Has an intended capture of agency been effected? Typically the answer is no, in which case the response is another reversal of roles: human agency is once more active in a revision of modelling vectors, followed by another bout of human passivity and material performance, and so on. The dance of agency, seen asymmetrically from the human end, thus takes the form of a *dialectic of*

[3] It is impossible to give a complete and just account of ANT in this paper. A large and diverse amount of literature on ANT has been produced, and the framework has been developed in many different dimensions. Our account of ANT is focused (and thereby reduced) to the way nature and its impact are accounted for.

> *resistance and accommodation*, where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it. (ibid.: 21-22)

Consequently, Pickering insists that his concept of resistance is significantly different from the notion of constraints because the latter are restrictions to human actions that transcend time and space (ibid.: 65). It must be noted here that while Pickering's theory hasn't found many followers, many observational studies' way of describing scientific practice is similar to his. All these studies provide detailed descriptions of scientists' struggle with all kinds of obstacles, among them material resistances and the resistance of theoretical objects. Examples for such descriptions are parts of Knorr-Cetina 1981, Lynch 1985, and Merz and Knorr-Cetina 1997. The notion of 'thin description' introduced by Knorr-Cetina and Merz (1997) seems appropriate for describing ethnographic observations that are reduced to this aspect of scientific practice. The important difference between these 'technically informed observations' and Pickering's approach is that only the latter is developed into a theoretical account of how nature affects scientists' actions. In the other studies mentioned this aspect remains implicit.

Both ANT and 'Mangle' respond to the problem of including nature in sociological accounts of scientific practice. In doing so, they offer solutions to one of the crucial problems of science studies. In the following section, we will define the problem and evaluate the solutions offered from the perspective of comparative research.

3 How to account for nature?

3.1 The problem: Whose accounts?

The problem both concepts try to solve has been discussed most explicitly so far in the so-called 'chicken debate'. The debate is named after the title Collins and Yearly gave their critique of ANT ("Epistemological Chicken", Collins and Yearley 1992a). It became clear in this debate (Collins and Yearley 1992a, 1992b; Callon and Latour 1992; Pickering 1995: 10-13) that sociological studies of scientific practice must make a principal methodological decision: It must be decided how to treat scientists' accounts of the role nature plays in scientific practice. According to Collins and Yearly, there are only two options:

1) Sociologists of science can treat all accounts of nature's influence on scientific practice as socially constructed and not related to nature 'out there'. In doing so, the sociological observer remains completely in the realm of the social.

2) Sociologists of science can include nature's influence on scientific practice. In this case, part of the explanation is handed over to the scientists themselves because it is the scientists who possess the necessary knowledge about nature. Since sociologists are not able to assess the status of this part, they must transfer the authority of explanation to the observed scientists.

Collins and Yearly accuse ANT of selecting the second option and, in doing so, going back to the pre-SSK stage of naïve realism. When scientists decide what natural influence matters, their opinion on what is a true account of nature re-enters sociological explanations.

The critique of Collins and Yearly goes right to the heart of the matter: If an independent impact of nature on scientific practice has to be included in sociological explanations, how will a description of this impact be obtained? The only source that seems to exist is the scientific knowledge that is produced and held by the very scientists whose actions are to be explained. That is why Collins and Yearly propose to stick to a radical constructivist, strongly relativist account. However, the first option has not yet solved two fundamental problems. Firstly, it "fails to give a satisfactory account of why the structure of the world should depend upon scientific consensus". (Sismondo 1996: 116) Such an account is, however, necessary in order to explain technical success, i.e. the fact that at least some applications of scientific knowledge produce the intended results (ibid.). Secondly, radical constructivism should be able to causally reduce a scientific consensus or decision to purely sociological factors. "But in fact no plausible such reduction has been presented, nor has even an indication been given of how one could make such a reduction." (ibid.)

3.2 Solutions: Sociologists' accounts!

Callon and Latour (and later, Pickering) accept the problem posed by Collins and Yearly but not the solutions they provide. Callon and Latour state that ANT provides a way out of the dilemma by rejecting the ex-ante distinction between social phenomena (the responsibility of sociologists) and natural phenomena (the responsibility of scientists). By giving up this ex ante – distinction, they homogenize the field of observation and thus are able to make the whole field a subject matter for sociologists. Decisions about how social or how natural a phenomenon is are left to later scrutiny. In other words: Callon and Latour propose sociological accounts of nature's influences that are independent of scientists' accounts. The underlying assumption is that these accounts are sufficient to explain scientific practice even when they have nothing to do with scientists' own accounts. Latour made this ambitious aim explicit:

> The study of science and technology has been deeply modified in the last 20 years through the use of what has been called a principle of symmetry (Bloor, 1991). Truth

> and falsity, efficiency and irrationality, profitability and waste have been treated in the same terms instead of being partitioned in two incompatible realms. … Very quickly, however, it appeared that the social theory that had been used to study rationality as well as irrationality in a symmetrical fashion was deeply flawed because it had been devised in contraposition to the world of objects. This birth defect made very difficult the use of the resources of the social sciences to study the natural world. (Latour 1994: 791)

Characteristic examples of this approach are Callon's description of attributes of scallops (Callon 1986), Latour's account of attributes of microbes (Latour 1988, referred to "Microbe as new social actor" in the index, ibid.: 272) and the following account of attributes of fuel:

> At the start, Diesel ties the fate of his engine to that of any fuel, thinking that they would all ignite at a very high pressure. … But then, nothing happened. Not every fuel ignited. This ally which he had expected to be unproblematic and faithful betrayed him. Only kerosene ignited, and then only erratically. … So what is happening? Diesel has to shift his system of alliances. (Latour 1987: 123)

"Ally", "faithful", "betrayed" are clearly sociological terms that ascribe consciousness and intentional action to fuel.

Pickering's argument is basically the same in that he provides his own accounts of nature's influences. However, his accounts are not sociological but are those of scientists who are surprised by material resistance. By limiting the description of nature to real-time observations of scientific practice, Pickering avoids the necessity to assess whether the scientists' assumptions about nature (i.e. about the resistances) are true or false. They are causes for temporally emergent accommodations to temporally emergent resistances.

> We can take material agency in science just as seriously as SSK takes human agency, and still avoid Collins and Yearley's dilemma, if we note that the former is *temporally emergent* in practice. … Thus, if we agree that, as already stipulated, we are interested in achieving a *real-time* understanding of scientific practice, then it is clear that the scientist is in no better a position than the sociologist when it comes to material agency. (Pickering 1995: 14)

According to Pickering, the sociological observer knows exactly as much about the emerging resistances as do the scientists observed by him. Since the only knowledge that matters (influences scientific practice) is the knowledge a scientist has when he or she faces resistance, an account of temporally emergent resistance and accommodation is sufficient to explain scientific practice.

This real-time description is the common way of 'technically informed observation' that is applied by most constructivist studies of science – even in those of Collins

(1985), as Callon and Latour (1992: 354-355) have argued. As Sismondo has shown, this kind of observation can be constructivist and simultaneously assume an independent influence of nature at the same time (Sismondo 1993). Pickering adds to this real-time description an abstract framework in which he describes scientists' coping with natural influences ("material agency") in terms of 'modelling', 'resistance', and 'accommodation'. This framework is his own account of natural influences that is independent of scientists' accounts.

Both Callon's and Latour's rejoinder to Collins and Yearley and the later proposal of Pickering are correct in that they introduce a third option not seen by Collins and Yearley: Beside the alternative of either using scientists' accounts of nature (and thus believing them) or treating them as a collective belief (and thus ignoring nature), there is the option for sociological observers of science to construct their own accounts of nature.

3.3 Incomparable natures

Both the third option (developing an own account of nature) and the concrete solutions offered by the 'Mangle' and ANT are yet to be assessed in terms of how they support comparative research. We will leave aside here all criticisms of ANT and 'Mangle' that address philosophical problems and discuss both approaches only with regard to our methodological question: Do these accounts make nature's influence comparable with both other natures and social influences?

The answer is somehow disturbing. While both approaches do include natural influences, neither of them is able to overcome the idiosyncracies of the scientific practices under observation. Pickering's approach leaves us with the choice between the real-time description of emergent resistances and researchers' accommodation, on the one hand, and the highly abstract but very poor general language of describing nature's influences, a language that consists mainly of the words resistance and accommodation. The focus on emergent resistances reduces the account of nature to the unanticipated and not yet discovered impact.[4] It is consistent with this approach that Pickering rejects the possibility of finding general patterns that provide explanations:

> We just have to find out, in practice, by passing through the mangle, how the next capture of material agency is to be made and what it will look like. Captures and their

[4] Another problematic point is Pickering's assumption that in real-time observations sociologists are in the same position as the observed scientists when it comes to material agency. When scientists make sense of material resistance, they draw on their whole knowledge (including tacit knowledge) – a resource not available to the sociological observer. The observer is limited by what scientists are able and willing to communicate.

> properties in this sense *just happen*. This is my basic sense of emergence, a sense of brute chance, happening in time – and it is offensive to some deeply ingrained patterns of thought. The latter look for explanations – and the closer to the causal, mechanical explanations of classical physics the better – while it seems to me that in the analysis of real-time practice, in certain respects at least, none can be given. … The world of the mangle lacks the comforting causality of traditional physics or engineering, or of sociology for that matter, with its traditional repertoire of enduring causes (interests) and constraints. I must add though, that in my analysis brute contingency is constitutively interwoven into a pattern that we can grasp and understand, and which, as far as I am concerned does explain what is going on. That explanation is what my analysis of goal formation as modelling, the dance of agency, and the dialectic of resistance and accommodation is intended to accomplish. The pattern repeats itself endlessly, but the substance of resistance and accommodation continually emerges unpredictably within it. (Pickering 1995: 24)

Pickering distinguishes here between the level of the concrete, single research process (that cannot be explained but "just happens", see also ibid.: 206-207) and the general pattern of resistance and accommodation – a pattern, however, that doesn't explain anything but gives only a highly abstract description. This approach is consistent with Pickering's rejection of constraints and his insistence on the incommensurability of different research situations (ibid.: 186-192). Thus, the 'Mangle of Practice' is not designed for comparing natures (see also Gingras 1997: 330-331).

Similarly, it is impossible to integrate the natural influences (resistances) in a sociological account of scientists' actions within the framework of the 'mangle'. Of course, the descriptions of researchers' actions include manifold actor constellations, interactions, resource provision, organisational factors etc. Thus, the reconstruction of research processes convincingly shows how the intertwining of natural and social influences lead to the specific outcome of knowledge production. It is not possible, however, to go beyond this idiosyncratic description and to look for general patterns of such an intertwining. This would require a level of abstraction *between* the idiosyncratic descriptions of research processes and the highly abstract but therefore almost empty mangle. Knorr-Cetina and Merz have made this point:

> We do not agree with Pickering's attempt to subsume the different ontologies and dynamics of practice under such general headings as 'resistance and accomodation'. While such a general vocabulary may capture the idiosyncratic trail of a single scientist's negotiation of an innovation (upon which no other analytic can perhaps be worked), it does not address established schemes of resistance configuration and elicitation, of object and subject formation, and so on. In other words, it does not address the different ontological and performative orderings routinely producing and embodying practice. (Knorr-Cetina and Merz 1997:129)

ANT differs from the 'mangle' significantly in that it makes it possible to apply the rich sociological language to nature – a language that is even enriched significantly by the authors of ANT.

> All the shifts in vocabulary like 'actant' instead of 'actor', 'actor network' instead of 'social relations', 'translation' instead of 'interaction', 'negotiation' instead of 'discovery', 'immutable mobiles' and 'inscriptions' instead of 'proof' and 'data', 'delegation' instead of 'social roles') are derided because they are hybrid terms that blur the distinction beween the really social and human-centered terms and the really natural and object-centered repertoires. (Callon and Latour 1992: 347)

However, this enrichment does not change the language's basic property of being sociological. As the examples given in section 3.2 clearly show, Callon and Latour cannot avoid the language of intentional actions and social relations. Even the "new vocabulary" is intrinsically sociological in that it still refers to acting entities, actions, relations between acting entities and non-acting entities.

The specifically sociological way ANT accounts for nature's influences on scientific research seems to be related to the methodological decision for naïve observation, a decision that makes it necessary to describe science "without resorting to any of the terms of the tribe" (Latour 1988: 8-9). An observer who refuses to learn the natives' culture is left with only one description language and explanatory framework for all he observes, namely the language of a sociological observer. This leads into problems whenever parts of the object under study cannot be explained in the observer's language (Lynch 1982). ANT can thus be seen as the offshoot of the methodological decision described in section 2: By taking (and conserving) the position of a naïve observer, a sociological observer who must account for nature's influences has no choice but to describe them in a sociological language and framework.

The sociological description of nature's influences pre-defines all observable phenomena as something sufficiently explainable by sociological observers in a sociological language. However, there is a price to pay for the achieved sovereignty about nature. This price is, again, idiosyncracy. Neither can different Actor-Networks be compared to each other beyond a comparison of their successes, nor can the relative strength of human and non-human actors be weighted and their impact synthesized. Both deficiencies become most visible in a comparison Latour has tried himself: the analysis of Pasteur's success over Pouchet (Latour 1987: 84, 1989). Pouchet replicated some of Pasteur's experiments in order to show that, contrary to Pasteur's account, there is something like 'spontaneous generation'. Pouchet actually observed microbes growing in media that had been sterilized according to Pasteur's instructions, i.e. microbes that behave against Pasteur's predictions. Latour concludes that non-human "allies" (again a sociological term) have to be included into the list of both Pouchet's and Pasteur's allies. The final list of allies (at the time when the dispute is being settled) looks as follows (Figure 1).

The lists of human and non-human allies make it obvious why Pasteur won. However, it seems impossible to compare the non-human and human allies' contribution to the victory. We are again left with the two options of idiosyncrasy and abstractness. On the level of the process under investigation, the contributions made by Pasteur's non-human allies are described in great detail, and a convincing reconstruction of the process is given in the language of ANT. However, the description is given in a way that makes it impossible to compare content and strength of these contributions. We can neither compare them to the contributions of human allies in the same network, nor can we compare them to the contributions of non-humans in a different Actor-Network. On a more general level, we are left with the information that one Actor-Network succeeded and another failed, but without a tool of comparing successful or failing Actor-Networks. Consequently, the question where the (similar or dissimilar) non-human actors in all the laboratories come from cannot be answered.

Pouchet's allies	Pasteur's allies	
no supporter	supporters	
accused of atheism	academy	human
provincial	in Paris	
abstracts only	full articles	
protocols	good protocols	
- - - - - - - - - -	- - - - - - - - - -	*No dichotomy*
ill equipped	well equipped	
ferments after	no ferments after	
sterilization	more heat	non-human
etc.	etc.	
Symmetric treatment: all the allies are listed, no matter how long and heterogeneous the list		

Figure 1 Latour's list of heterogeneous allies (Latour 1989: 109)

Thus, both approaches (ANT and 'mangle') provide the methodological imperative that nature must be included in analyses of scientific practice. Moreover, both approaches provide us with means for describing these influences in a language available to sociological observers. However, neither of them supports a comparative approach to the analyses of research processes that include different natural influences and to the integration of natural and social causes into comparative studies.

4 Linkage variables between epistemic and social conditions of action

4.1 Epistemic conditions of action

Our own approach emerged as we felt it necessary to include nature and knowledge about it in institutionalist studies of science. We call our studies 'institutionalist' because of their focus: With these studies, we try to investigate the impact of institutions on knowledge production. This is a principal difference to the constructivist studies' attempt of explaining how scientific knowledge is produced in scientific practice. Institutionalist studies focus on one type of factors – institutions – and treat other influences on knowledge production as intervening variables. As we already mentioned in the introduction, a comparative design is frequently applied in institutionalist studies because institutions are macrostructures that span many situations.

When we tried to compare institutional influences on scientists' actions in different fields we soon recognized that it is impossible to account for field-specific effects of institutions without reference to the different contents of scientific work. Since neither institutionalist methodology nor any methodology of science studies provided support for a comparative empirical approach, we had to find our own solution.

Our methodology differs from revolutionary approaches like 'Mangle' and ANT in that it is rather traditional and mundane. We turned to theory of action because it is the background of institutionalist approaches. Theory of action suggests treating nature's influences as conditions of action that overlap with other conditions of action, among them institutions. The opportunity to do so is provided by the new institutionalism that has been developed in several social science disciplines, but is progressing only slowly in sociology of science. One of the central ideas of the new institutionalism that is important for its application to science is that it regards institutions as only one of several heterogeneous factors that affect human action. Other factors that overlap and may counteract institutional influences are actors' goals, interests and perceptions, conflicting institutions, other social conditions of action and non-social conditions of action. However, institutionalist studies are liable to at least one weakness of the old institutionalism – the neglect of scientific practice and the non-social factors that affect this practice - because of their inherent macroscopic orientation (Gläser and Laudel 1996).

Backed by the idea of overlapping, possibly non-social conditions of action we introduced a new type of conditions of action in our analytical framework, namely *epistemic conditions of action* (Gläser and Laudel 1996, 1999).[5] We started with the pre-

[5] We have been struggling with the question of how to term these specific conditions of action for a long time. Our current solution 'epistemic conditions of action' is inspired by Rheinberger's concept of 'epistemic things (Rheinberger 1997). It is intended to emphasize conditions produced by the 'technology' (materials, means, and practices) of creating new knowledge.

SSK idea of cognitive structures because it provided a comparative approach. Two major shortcomings of this idea were that it (a) was limited to contents and properties of knowledge and (b) was not linked to a methodology of those structures' empirical identification. Therefore, we started with open frameworks that allowed new cognitive structures to be included (among them influences of nature and of instruments) whenever they surfaced in our empirical data (mainly transcripts of qualitative interviews). As a result of this approach (first applied in Laudel 1999), we were confronted with long and rapidly growing lists of cognitive structures (ibid.: 221-222). Moreover, we faced exactly the type of idiosyncracies that prevent comparisons of scientists' practices within the ANT and 'mangle' approaches. While we were able to identify epistemic conditions of actions, we couldn't compare them.

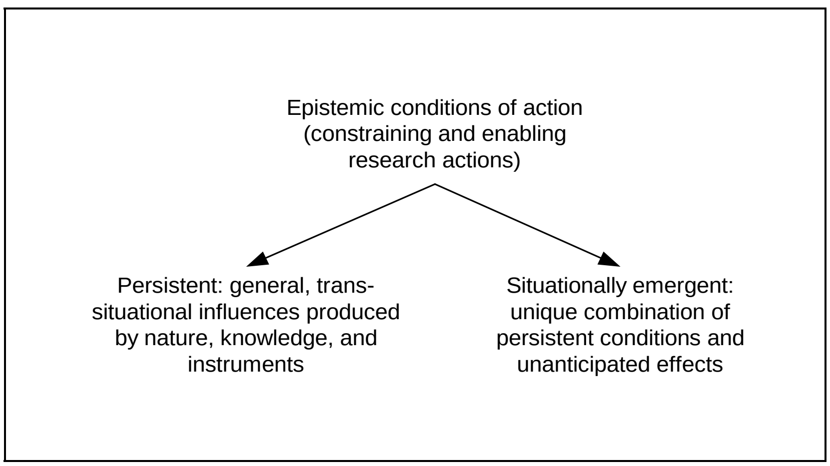

Figure 2: Emergent and persistent epistemic conditions of action

This 'emergent resistance' triggered our search for general properties of knowledge, instruments and nature that affect human actions. These general properties could be defined as constraints if they were not as enabling as they are restraining in their influence. We assume that they exist even if the terminology might need some refinement.[6] Thus, contrary to Pickering we regard temporally emergent resistances as resulting from an overlap of two different types of epistemic conditions of action (Figure 2): Firstly, in every research process general (i.e. trans-situational) conditions of action are combined in a very unique way. This is so because the part of nature that is addressed, the instruments and the knowledge of the scientists involved provide a setting that

[6] As Gingras (1997: 324) has argued, arguments for the existence of trans-situational (in this case "time-invariant") structures that affect scientific practice can be found even in Pickering's descriptions of scientific practice (Pickering 1995: 109).

cannot be replicated.[7] Secondly, this unique combination overlaps with effects from unanticipated reactions of nature and instruments to scientists' actions.

We regard epistemic conditions of action as non-social in the sense that they provide an 'objective reality' that cannot be wished away (Sismondo 1993). Only in this sense both knowledge and instruments, albeit products of human and therefore social construction processes, can be regarded as non-social. In the words of Berger and Luckmann (1967: 57): "The paradox is that man is capable of producing a world that he then experiences as something other than a human product." There are two more arguments for treating all three types of factors (nature, knowledge, and instruments) analytically as non-social: They can affect the success of the production of scientific knowledge directly, i.e. without mediation by other actors, and the morphology of these factors cannot be explained exclusively by social factors.

With these considerations, we began to compile a list of general epistemic conditions of action that are sociological relevant because they are likely to affect scientists' actions (figure 3). Nature is one source of such epistemic conditions of action. The most important conditions it provides are the research objects' unknown attributes. These unknown attributes partly constitute the research's subject matter, that is they must be 'produced' in empirical research and theoretically reconstructed. Because they are unknown, these attributes cause emergent resistances (Pickering 1995), respectively, anomalies in research processes (Star and Gerson 1987). In addition to the specific attributes that are investigated by the researcher, research objects have general attributes that influence conditions of action. In our investigations, a research object's complexity and its internal dynamics have played a role. A research object's dynamics influence the time needed for research processes. For example, some elementary particles exist for only fractions of a second, some micro-organisms reproduce themselves in about 20 minutes, and a cloned sheep needs several months to grow.

As we have stated above, knowledge may appear as hard in human action as does a material object. For example, the structures of mathematical theory restrict researchers' choices between mathematical techniques not only in mathematics (Pickering and Stephanides 1992) but also in theoretical physics (Merz and Knorr-Cetina 1997). Of equal importance are epistemic conditions of action that are produced by general attributes of knowledge. Important examples that have been discussed in the literature so far are the structure of theories, the degree of codification of knowledge, and

[7] Since this is rather obvious we provide only two short arguments: The replication of experiments occurs only seldom (Collins 1982), and even when it occurs the original situation is not replicated because knowledge about its outcome is part of the replication. Furthermore, every scientist possesses an individual combination of scientific knowledge (including tacit knowledge) that cannot be replicated.

interdisciplinarity or, more generally, the variety of different knowledge systems that must be integrated in the course of a research process. These epistemic conditions of action were already used to describe scientific fields twenty years ago. They are the legacy of pre-SSK approaches to 'cognitive structures'.

The third source of epistemic conditions of action is the instruments used by scientists. Instruments can be understood as a synthesis of the two original sources in that they are purposefully constructed by processing parts of nature and simultaneously using the current (incomplete) knowledge about nature. One important characteristic of instruments is their range of applicability. Depending on the effects built into an instrument, the method that is based upon that instrument can be applied to a narrower or wider range of different objects. For example, electron microscopy is successfully used in biology, physics and chemistry because the interaction of electrons with matter that is built into the instrument applies to many research objects. Other methods, such as immunoassays, are very specific because they can only be used to identify one substance.

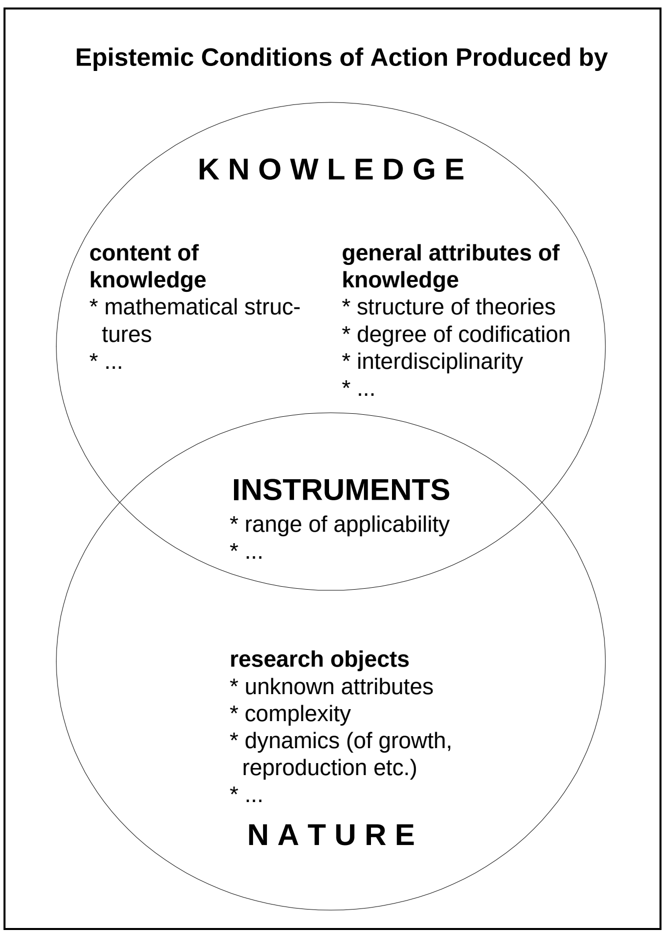

Figure 3 Sources of epistemic conditions of action

The concept of epistemic (and among them, natural) conditions of action is similar to ANT and 'mangle' in that it uses the same option of accounting for nature: It escapes the apparent alternative between ignoring or believing scientists' accounts of nature by developing a sociological description language. However, the chosen framework – new institutionalism and action theory – leads to a different kind of language. The emphasis laid on comparison led to a language that is too abstract to give a detailed account of how knowledge is produced in a specific situation. However, applying this level of abstraction makes it possible to compare research processes.

Apart from the level of abstraction, the concept of epistemic conditions of action differs from both approaches discussed above in some crucial points. The main difference to Pickerings ' Mangel of practice' is that epistemic conditions of action do not reduce nature's influence to temporally emergent phenomena. As Gingras (1997) has shown, one is not forced to chose between eternal, immovable boundaries and emergent phenomena. Epistemic conditions of action include both situationally emergent conditions and general, trans-situational influences produced by materiality, knowledge and instruments. Thus, in comparing researchers' situations and actions, certain epistemic conditions of action can be assumed as being equal in the situations that are to be compared, while others vary.

The differences between a concept of conditions of action and ANT are obvious: With 'classical' action theory, symmetry is given up and intentional action is preserved for human actors. What is conceptualized as non-human actors in the framework of ANT is 'downgraded' by us to conditions of human actors' actions and is described in a language that is specific to that type of conditions. Symmetry enters on a lower level: Epistemic conditions of action are neither superior nor inferior to other conditions. How strongly epistemic and other conditions of action shape the course of action is an empirical question about which no ex ante – decisions can be made.

4.2 Linkage variables

The proposed language of epistemic conditions of actions seems to provide a solution to the problem of comparing different natures. While it is rather difficult to compare resistances provided by a bioassay and an electron microscope ('Mangle'), or the intention of these two instruments (ANT), they certainly can be compared with regard to the spectrum of objects (and therefore research problems) they can be applied to. Admittedly, comparisons are still difficult, especially when small variations are to be assessed. But the analysis of epistemic conditions of actions provides at least dimensions of comparison that can be applied to all research processes and enables significant differences to be identified.

While some progress has been made concerning the comparison of natures, it seems doubtful that introducing specifically epistemic conditions of action can solve the second problem of comparison – the comparison of natural and social influences. It seems as difficult to compare the influence of a high degree of knowledge codification with that of a powerful actor, as it would be to compare the intentions and strength of scallops with that of marine biologists.

The reason for this enduring difficulty is that while the problem of comparing natures can at least partly be solved by abstraction, the comparison of natural and social influences is possible only if both types of influences are described in the same language. That is why the solution provided by ANT is so seductive: With its symmetry principle, all causal influences are described in the same (the sociological) language. However, as we have demonstrated, the unified language is a necessary but not a sufficient condition for comparison. Additionally, a theoretical framework is needed that provides grounds for assessing and integrating the different causes. Unless the proponents of ANT provide a true theory, i.e. a general theory that allows such integration, ANT remains a powerless language.

An approach based on the concept of epistemic conditions of action is in a better situation concerning the theoretical background because all of the good old sociological theories can be used as long as they are based on a concept of social action. However, epistemic conditions of actions are described in a non-sociological language that refers to phenomena outside the social realm.

There is one comparative sociological approach that has encountered this problem before: In the context of organisational sociology's contingency approach, relations between organisational technology and organisational structure have been investigated. In order to study this relation, an independent description of organisational technology had to be achieved. Technologies in organisations were described by variables such as complexity, task uncertainty (variability), task interdependence, etc. (Burns and Stalker 1961; Woodward 1965; Thompson 1967; Perrow 1967) By applying these variables, organisational sociology was able to compare technologies across organisations. Because the contingency approach was a quantitative one, the synthesis of technological and social variables was also unproblematic – the unifying language of mathematics could be used.

Empirical studies of organizational technology operationalised the concepts mentioned above by formulating questions about work processes, i.e. human actions. In doing so, they addressed the level on which social and non-social conditions mix. This strategy was enforced by the quantitative approach, which is restrained to fixed-choice questions. Its main shortcoming is that no independent analysis of technology is possible. Analyses of technology must be reduced to respondents' characterization about their work practice. This restriction is severe because technology cannot be analyzed independently of respondents' perceptions. An independent analysis would require interviews about and observations of the tools and machines applied, tool-specific or material-specific practices; and it would require collecting information about technologies from technical descriptions. Nothing of this can be done in a quantitative study (for a discussion of these problems see Perrow 1979: 164-166).

Contrary to the contingency approach we think it necessary to distinguish between the two levels of analysis constituted by the epistemic conditions of action and by the practices of scientific work. To establish relations between both levels requires a translation of epistemic conditions of action into factors affecting human action.[8] Our search for such a translation began with the question what aspects of research actions can be affected by epistemic conditions. This question led to the following list of general attributes of actions:

- they take time;
- they use resources;
- they are conducted to achieve a goal (i.e. an anticipated situation);
- they are more or less successful with regard to goal attainment;
- they are linked to other actions of the same and of other actors.

These basic attributes of an action are part of the sociological analysis of actions. At the same time, they provide the 'channels of influence' for epistemic conditions of actions and thus for nature. Since these attributes provide the 'channels of influence' for social conditions of actions, too, the different kinds of influences can be treated symmetrically and can be integrated on that level. For the purpose of a sociological explanation of action, the 'channels of influence' can be described by 'linkage variables'. The following linkage variables have been proven useful in our comparative institutional projects:

- 'Eigentime' of research processes: influences on time characteristics of research processes caused by the dynamics of research objects or methods (e.g. speed of growth, frequency of occurrence of phenomena);
- 'resource demands': quantity and quality of resources that are required to achieve a certain goal;
- 'epistemic room of maneuver': Research actions that are possible with the objects and methods available;
- 'risk of failure': the probability of a research project leading not to the intended results; and

[8] We apply the traditional meaning of the concept 'translation', i.e. "a rendering of something into another language or into one's own language from another" (Webster's dictionary).

- ‘relations to other actors’: relations to other researchers as well as to individual and collective actors outside science that are mediated to the segment of nature addressed by the research.

Some of these variables are similar to variables used by the contingency approach. An important difference is that our linkage variables are not intended to be quantifiable. They have only nominal (in some cases ordinal) scales. ‘Risk of failure’ is a variable that describes the research process as a whole rather than one of its attributes. However, general concepts such as ‘task uncertainty’ are not suitable because research is non-routine work. It is important to single out the possibility that a research process might fail completely.

5 Applications

5.1 Impact of funding programs on scientific work[9]

In 1994 the Deutsche Forschungsgemeinschaft (DFG), Germany’s most important funding agency for university research, set up a specific funding program for East German universities’ research. The aim of the program was to overcome specific problems that emerged in the restructuring of East German universities following German unification. Scarcity of resources in East Germany and the simultaneous filling of thousands of positions at universities hindered the development of universities’ research profiles. Additionally it was feared that with the transfer of West German university structures their deficiencies were being transferred, too. The main deficiency of these structures that should be fought from the beginning is their suppression of interdisciplinary collaboration.

The program offered funding to networks of five to ten university research groups that provided a joint research theme and proposals for collaborative research. The external funding was supposed to provide an initial boost for the network that leads to an autocatalytic development of continued interdisciplinary research and attraction of external funding. The initial funding period was 3 years, with an option of two more years of funding after the network passed a second peer review. A total of 21 networks was funded between 1994 and 2001, the program’s budget was ca. 60 million Euro.

The funding decisions were based on the elaborate system of peer review that is applied by the DFG in funding collaborative research centers (Laudel 1999). A group of ca 10 scientists reviewed a network’s proposal (including proposals for all projects of the network), discussed it with the applying scientists and made a decision

[9] This example is based on Laudel and Valerius (2001).

afterwards in a closed session. The main scientific criteria for the evaluation were scientific quality of the proposal and the individual projects; coherence of the proposal (degree of integration of the contributing fields); applicants' competence; and quality and significance of the planned collaborations. Owing to the political aim of promoting the development of university research profiles, the importance of the network for the university's research profile and the university's contribution to the network's resources were included in the assessment.

While the list of evaluation criteria was not surprising, the DFG and its reviewers modified the application of these criteria in the light of the special situation of East German university research. Many members of applying networks had taken their positions only recently. They didn't know each other well and had no record of collaborative work. Thus the situation at East German universities was perceived as 'pioneering'. The reviewers had to compromise because the funding program should help to build what is usually a prerequisite for funding.

The solution to this problem was that while there was little compromise with regard to applicants' general track record and to projects' quality, other criteria were applied less rigidly. Thus, several proposals were accepted that would have been regarded as too risky under normal conditions because of the unusual interdisciplinary collaborations planned or because applicants had no prior publications in the field for which they applied.[10]

One of the networks tried to establish a new area of research that hasn't as yet been well developed in Germany. This proposal met the expectations of the DFG and its reviewers, with regard to both the university's profile and the German research landscape. However, the start from scratch was both risky and time-consuming. It was risky insofar as the initial thematic coherence of the collaborators was low, as was recognized and criticized by the reviewers. The network's dynamics were affected by the fact that development of the new research area required the establishment of a new method that used genetically modified animals. To 'construct' and breed these animals is a process with a longer Eigentime. The process is also risky because first attempts may fail and because you cannot tell in advance what the phenotype of transgenic animals will be like.

> Well, I didn't think all criticisms were fully justified because we could neither predict nor guarantee what animals will occur and how we would further treat these animals. I mean, if they had shown a certain phenotype we would have jumped at it and characterized in more detail. … But we couldn't make a concrete plan, couldn't work

[10] Peer review is known for promoting low-risk mainstream research (Chubin and Hackett 1990; Neidhardt 1988, Travis and Collins 1991).

> on a hypothesis in this respect. … or it was basically … and it was perhaps our fault that we couldn't get this message across to the reviewers, that there is a part we simply can say nothing about.

The Eigentime (and the time needed for publication, which must be added in this case) collided with the institutionally defined funding periods, a contradiction that surfaced in the intermediate peer review after three years.

> It simply took more time. [The methods] take their time … [The reviewers] had liked hard results or publications … Because it hadn't been published, i.e. it had not been accepted by the scientific community, they didn't believe us. We could project nice data at the wall with slides. They didn't want to accept it. Thus they would accept only a paper and this will come - it isn't out even now. … Thus, even now we don't have the paper out because simply too many things must be checked. It is necessary to check things, to secure them twice. All that hadn't happen.

In the peer review after three years, the reviewers registered progress with regard to the network's coherence but still criticized it. Moreover, the network hadn't invited guest scientists in the first three years in spite of the planning (which was part of the proposal). The reason for this was the same: The guests whose visit was intended were supposed to work with the new method, and since this method hadn't been established within the initial three years, it made no sense to invite anybody. However, the network managed to conduct a workshop that was attended by the international elite of the field.

The peer review was affected by the fact that establishing a new field implied that the researchers had no peers in Germany. Thus, their subject matter affected another of our linkage variables by creating special relations to colleagues in the German research system. The reviewers had limited expertise in the field under discussion because the field was new to Germany.

> We [meaning Germany] really lack competent research on transgenic animals […] And this is generally seen as a deficiency, but unfortunately the scene is at odds with itself very much, as far as it exists anyway.

Since the reviewers felt unable to judge the preliminary results presented to them, they had to rely on the quality control by the international scientific community, an assessment that had not taken place when the decision was to be made. This was particularly unfortunate because some important and surprising data had been produced:

> We … experienced a big surprise. And this turned out only in the end of the first funding period. Unfortunately, this big surprise wasn't enough to convince the jury to continue because they didn't believe us. They said: "This cannot be!" One reviewer explicitly said he doesn't believe that this has been overlooked for such a long time.

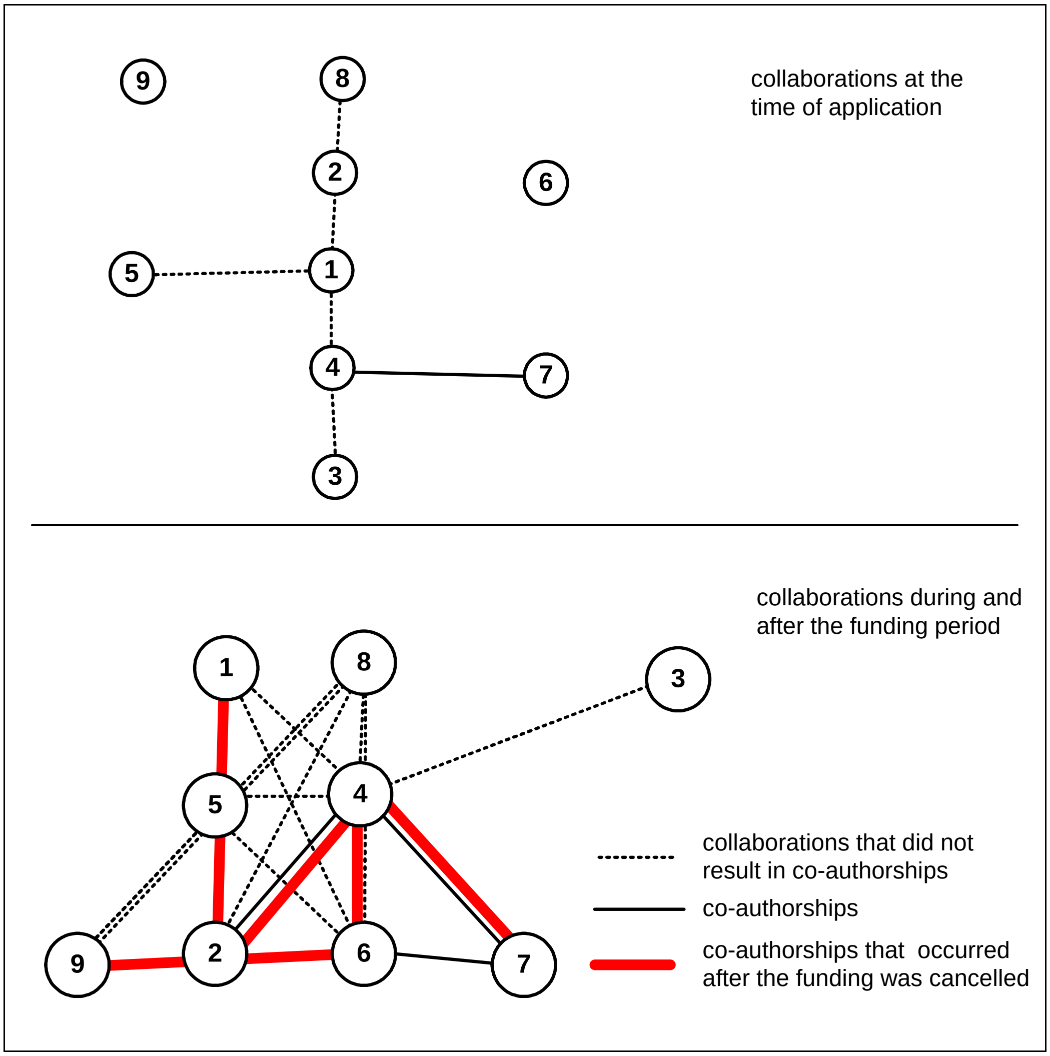

Figure 4 Dynamics of collaborations in the network that failed

The reviewers were critical to an extent that led to the canceling of funding. The network decided to continue its work, albeit with significantly reduced resources because it was limited to the recurrent funding that is rather small in Germany and especially in East Germany. The scientists didn't apply for other external funding because they felt they couldn't overcome the problems of peer review.

> We were unlucky in that we had nothing because we hadn't the animals yet. Without prior results … you don't get a proposal through.

Both the analysis of collaborations and a retrospective publication and citation analysis confirmed the conflict between Eigentime and funding time (figure 4). Starting with an initially low degree of connectedness, the network had developed a dense network of collaborations, albeit with few collaborations that led to co-authorships. As was to be expected from our discussion, most co-authorships occurred after the funding was cancelled. These co-authorships belong to publications in high-impact journals. Given the time characteristics of the work and of the publication process, the picture confirms that the funding period (with an intermediate review after three years) did not match the research process's Eigentime.

5.2 Institutional pressure on basic research[11]

The second example stems from an investigation of how East German non-university basic research reacted to changing institutional conditions after German unification. The institutional system of the German Democratic Republic (GDR) featured complete hierarchical governance, with both institutes and scientists having little formal autonomy. Through this hierarchy strong pressure was exerted to link research to industry, resulting in a functional integration of basic and applied research (Gläser and Meske 1996). Following unification, public-sector basic research became part of the institutional system of the Federal Republic of Germany, which provides many institutes and scientists with significant formal autonomy. It was therefore expected that East German basic research would abandon the previously enforced application orientation and take advantage of the wider possibilities now available to follow internal stimuli independently of possible links to applications. This hypothesis was tested and must be rejected for several reasons (Gläser 1998). The institutional pressures were complex and counteracted by both epistemic conditions of actions and 'microclimates of autonomy', as the following example demonstrates.

One of the research institutes under investigation was a successor of an institute of GDR's Academy of Sciences (AoS) devoted to nuclear research. This institute hosted both basic and applied research. Basic research was shielded from institutional pressure towards 'useful' research because the hierarchy had defined part of the institute's mission as basic research. The institute split its resources and devoted a share of them to applied research. Because of this practice, the institute as a whole could always meet research policy's expectations, and basic research could be conducted with the remaining resources. One basic research program conducted in the institute was nuclear physics:

> You know, the nuclear physics did basic research. In nuclear physics one does nothing than basic research, therefore, we had basic research.
>
> * * *

[11] This example is based on Gläser (1998).

> I mean, the question really is: what happens, if nuclear material is heated and compressed to different temperatures and different densities? How does the nuclear material behave? What new particles are produced within the nuclear material? How does the mechanism of outbreak work? Because all the stuff flies apart thereafter, when it has reached the highest density and temperature. Then the whole system expands again. And the question is: what happens when these parts of the nucleus – say neutron, proton, ion, kaon and so forth – what is happening when this stuff flies apart again? When … the nuclear material expands again.

This strand of experimental nuclear physics depends on big research devices (accelerators and detectors attached to them) because nuclear material must be heated and compressed, and the traces of the particles must be recorded. Thus, the attributes of the subject matter studied cause an especially high resource demand. These resources are provided in only few places. Researchers in the field of nuclear physics either have direct access (by working in an organization that has this equipment), or they are integrated in collaborations by building detectors and contributing to the analyses of the data obtained by these detectors. This kind of research has a long Eigentime because of the extensive design and construction work involved.

After the institute was founded, an accelerator was built that was state of the art at the time of the investment (i.e. in the fifties and sixties). Thus, researchers were able to conduct experiments at that time. However, their international collaboration was severely limited. While scientists could collaborate with the big science center of the USSR in Dubna, GDR's communist party and government for financial and political reasons suppressed participation in collaborations with western scientists. Therefore, the epistemic room of maneuver for most experimental researchers in the institute (and for theorists who worked with experimental data) was determined by the institute's equipment.

Owing to the priority of applied research and to the generally scarce resources for research in the GDR, the big devices were never renewed or replaced. The experimental conditions that could be produced at the institute remained basically the same. While their colleagues all over the world received new equipment and moved on after the most important problems of these experimental settings had been solved, the researchers in the AoS's institute were tied to the experimental conditions produced by the old equipment. Therefore the researchers' epistemic room of maneuver was continuously shrinking.

> Yes, well, we were really muddling through in the last phase, so to speak. We were looking for niches and experiments that were left out by other people, where we said: it would pay to conduct a measurement. But it was increasingly the case that, when we wrote up the results and compared them with the international literature, we said "Well, these people could do it better, our contribution is really very modest". And in

> the end we barely managed that the core journal in our field took our contributions. But this was getting more and more difficult. That is why about one year before the change, when nobody expected things to change, we conducted a big study and reviewed all the literature in order to find things we could do. But to be honest, it was also the case that nobody wanted to cut of his own branch. That means, if one had said " I can do absolutely nothing – I clearly admit it" this would have led into problems.

Thus, in the second half of the eighties researchers had to look for problems that hadn't been solved yet and could be investigated with the old equipment they were left with. Some of the researchers could work at the facilities in Dubna, and for very few of them the second half of the eighties brought the opportunity to work in Scandinavian countries. Most of the researchers, however, had to find research themes in a rapidly shrinking epistemic room of maneuver.

With the breakdown of the wall in 1989, political barriers to international collaborations disappeared. While the limitation to the scientists' epistemic room of maneuver produced by the institute's equipment remained, their epistemic room of maneuver was significantly widened by the opportunities to participate in international collaborations. The scientists began to integrate themselves systematically into various collaborations in West Germany, a process that was supported by the West German government.

> That means that we very swiftly tried to exploit what is available in Germany. And we had been integrated very, very well by our colleagues who of course knew us all from our publications. We knew about each other, but we by and large didn't meet each other because not all of us might travel, or only very, very few might travel, and it was impossible to conduct substantive work on that basis. After the 'Wende' we have abandoned the work at the cyclotron here and from than on we have conducted our experiments exclusively at external big devices.

In this phase, some scientists began a new research program by getting involved in (and becoming committed to) long-term collaborations whose Eigentime and the funding tied to them in turn narrowed the epistemic room of maneuver.

> At the time when we re-oriented ourselves we have been very free. […] Well, but it is of course as follows: When one has made a decision about a new direction or about a new project, then one must stick to it for many years. Especially in our case it is as follows: We are building a big spectrometer and it takes many years till it is completed. And when one has begun the collaboration and has accepted certain tasks – in this collaboration it is approximately 50 to 60 people, it is five universities and institutes that collaborate – one cannot leave. In this respect there is no freedom anymore, but you can only work focusedly and hard. And another aspect is, of course, that a lot of money goes into it as times go by. We have already received money from the BMBF [the Federal Ministry for Education, Science, Research and Technology] during two

> funding periods, and a lot of money, and we have invested it in building this device. And thereafter, it will be used for measurements many years. However, this phase hasn't come yet. We are already conducting measurements, but they are not the final ones and the device is being continuously completed, and naturally we want to harvest the fruits of this labor. In this respect, we don't have the freedom to leave now.

The collaborative work with West German colleagues and with scientists from other western countries made it possible for the institute's researchers to conduct high quality work. When the institute was evaluated two years later, the successful search for niches and the few international collaborations in the second half of the 80ies, the recent collaborative work and the involvement in long-term collaborations added up to a good record. It was recommended to continue the work in a new institute.[12]

> What happened at our place … after 89 all our people swarmed. … And they participated in West Germany in the most important projects, something they couldn't do before, and developed their own positions, tried to propose their experiments of their own, to build devices of their own which were built here … So this was a complete new shaping of the institute. Before this we worked on our own, till '89, and thereafter we have been directed by the big German and European projects in nuclear physics.

* * *

> In autumn was the 'Wende' and in May 90 we already had a new project. … And we received special financial support for this project [from the BMBF]. And that is why we could promptly begin to work. And we presented already this new direction to the evaluation committee, that we intend to do this and partly already are doing this. And this probably decided it. They said "all this is already under way".

However, the newly founded institute faced a problem: It had no big research device. This was perceived as a problem because within the science system of the Federal Republic of Germany, the institutionalisation of research in non-university research institutes instead of universities had to be legitimized. In the case of nuclear research, the common justification was that it is big science and must be organized around a big research device. Because it also focused on useful research, FRG's science policy of the nineties was reluctant to invest in basic research. The general political expectation that all research should be somehow useful had gained ground, and it had become almost impossible to obtain a large investment without reference to theses aims. Thus, the

[12] In the course of German unification, GDR's AoS was dissolved, and all its institutes were evaluated by working groups from the German Science Council (Wissenschaftsrat, as national board advising the government on science policy) in 1990 and 1991. As a result of the evaluation, the founding of new institutes and the work that should be continued was recommended. All East German scientists who formerly worked in the AoS's institutes had to (re-) apply for posts in the new institutes.

institute saw its future endangered by the fact that it had no such equipment but had to conduct all its experimental work externally.

> Well, this is our big hope at the moment, that we will make an enormous step and will also strengthen our position here at this place with the building of the new [device]. It will be certainly necessary to conduct external experiments in the future, too. But it is unthinkable that such a big institute like ours lives exclusively on this kind of 'travel physics', to use a pejorative term.

As it has done before, the institute searched for niches to secure its future existence. Now, the search for niches had the goal of finding a big research device that didn't so far exist in Germany. In this search, the range of applicability of all other big research devices currently used limited the scientists' epistemic room for maneuver. A second consideration was that the investment couldn't be justified by exclusively referring to basic research. Thus, a device had to be found whose range of applicability enabled its use in basic and applied research simultaneously.

> In Germany these days you can get money for a new engine at a certain scale only if it is evident that the research you are conducting has a strongly applied opportunity. They don't demand a guarantee. But the application must be of a kind that it really leads to something. Otherwise you get money for basic research neither from [the federal state] nor from the BMBF nor from any other source.

Finally, a research device was found. Plans for the future structure included a new resource splitting: Some scientists in the institute were expected to change their fields and conduct more applied research with the new device, thus contributing to the institute's legitimacy in the German science landscape.

> And it is the case that our dealing with applied aspects is well recognized. But there is no pressure or enforcement to make this our main task. No institution has said this, and our institute's current profile is accepted. However, owing to [the new device] we will see the re-orientation of some of the physicists in the institute...

5.3 Discussion

In both cases the frictions between epistemic and institutional conditions of actions is obvious. In the first case time characteristics of the research process's subject matter exacted the strongest influence. By using the linkage variable Eigentime, these time characteristics could be linked to the funding program's institutionalized time. The contradiction manifested itself in the decision situation of the reviewers. This wouldn't have been a problem for the network under review if the reviewers had belonged to the same specialty and thus had been able to evaluate the preliminary raw data presented to them at the peer review. However, since the combination of fields was new in Germany, an unusual relation to the reviewers was inevitable. Feeling not completely certain about the data presented to them, the reviewers refused judgement and applied the rather formal publication criterion, this led to the negative decision due to the Eigentime problem.

If epistemic conditions of action and linkage variables were omitted, the explanation of this example had to refer either to 'bad research' (of researchers who were unable to achieve their goals in time) or to 'hostile reviewers' (not believing the data presented to them). Both types of explanations are quite common. However, both explanations would have not only been wrong in this case, but also insufficient for two reasons. Firstly, none of the standard explanations could have explained the delayed publication of a significant number of high quality papers. Secondly, both explanations would have muddied the waters with regard to findings of the overall institutionalist analysis, namely

- the fact that the high risk – strategy that was applied due to the special situation of East German university research enabled successful research that would not have been funded under 'normal' conditions; and
- the fact that there are self-inflicted failures of funding programs due to the latters' insufficient adaptability to field-specific conditions.

In the second case, the subject matter's attributes like energy level and scale of the intended effects lead to the specific resource demand that characterizes big science. This resource demand is coped with institutionally by either spending the resources and investing in a big device or by participating in big collaborations that use such a device elsewhere. After an initial investment in a big device, GDR's science policy institutionally suppressed both options and thus prevented that its nuclear physicists could access state-of-the-art equipment. This led to a shrinking epistemic room of maneuver that in turn affected the quality of research and threatened to prevent any experimental research in the near future. The breakdown of the wall removed the institutional barriers to the collaborative research option, thus significantly widening the epistemic room of maneuver. The solution found under the new conditions of unified Germany was to define a big device that enables basic as well as applied research. Thus, the new device's range of applicability widened the researchers' epistemic room of maneuver in a way that they can meet science policy's expectations and continue basic research simultaneously.

The application of linkage variables such as resource demands and epistemic room of maneuver enabled a detailed account of the conditions under which an ubiquitous institutional pressure towards 'usefulness' of basic research

- does not affect basic research because the latter is shielded by institutional micro-climates;

- does not affect basic research because the latter's epistemic room of maneuver is wide enough to meet political expectations without being distorted; and
- distorts basic research in the long run (Gläser and Meske 1996; Gläser 1998, Gläser 2000).

6 Conclusion: solved and unsolved problems

A comparative approach applies a general framework to different settings in order to assess the individual condition-outcome relationships. Sociological theories provide many approaches that support comparative analyses of *social* factors. To expect Social sciences of being able to include factors outside their claimed range of validity seems to be extremely unfair. However, there is a strong feeling that these factors affect social action. If this is the case, there is no choice: we must include non-social factors in sociological explanations.

This refers first of all to nature. But if we apply the idea of an 'objective reality' that cannot be wished away, there are grounds to treat scientific knowledge and instruments, though they are socially produced, as non-social factors too. A similar case can be made for raw materials, instruments, and knowledge that are applied in human actions other than research. 'Epistemic conditions of action' are a specific case of 'cognitive/technological conditions' of human action, i.e. of conditions produced by material objects, material means of actions and knowledge applied in human actions.

During the last two decades, the sociology of science has observed and recognised the effects of non-social factors on the production of scientific knowledge. Owing to the specific aim of the sociology of science's current mainstream, the sociology of scientific knowledge, attempts to include non-social factors in social explanations have focused on the inclusion of these factors as causes in the explanation of how knowledge is produced.

On the lowest level of abstraction, processes of knowledge production have been described in great detail. On this level, all constructivist studies seem to have achieved the inclusion of non-social factors. In these descriptions, basic terms coined by the observed 'tribe' are used. Since every knowledge claim is unique, it seems quite natural that all descriptions of how these claims are produced are idiosyncratic.

The trouble begins when these accounts are to be compared. As we have indicated above, a comparison requires a framework that can be applied to all cases. Such a framework requires abstraction. We have found two offers of abstract frameworks: ANT and the 'Mangle of Practice'. However, both frameworks are constructed in a way that supports neither the comparison of different natural influences nor the integration of natural and social influences into one explanation. Abstract description languages are obviously a necessary but not a sufficient condition for comparisons. The frameworks must also be rich enough and specific enough to enable a comparative assessment of the factors' variations. We cannot see how this is possible within a language that is reduced to modelling, agency, resistance and accommodation. Nor can we see how the description of all influences in sociological terms does solve this problem. While it is possible to ascribe social characteristics like intentions or faith to non-humans by definition, it is impossible to compare these characteristics in terms of contents and strength. To us, both frameworks seem to be description languages for ex post-descriptions rather than analytical tools.

This is the situation we faced when we looked for analytical tools that support the integration of non-social factors into institutionalist research designs: a rich practice of idiosyncratic descriptions and two abstract description languages that don't support comparisons. We tried to introduce the pre-SSK cognitive structures (enriched by structures stemming from nature and instruments) and faced idiosyncracy. We tried a generalized description of epistemic conditions of action and were unable to integrate these conditions into social explanations.

Our current solution is to maintain epistemic conditions of action and to additionally apply linkage variables that describe the channels through which actions can be affected. This seems to do the trick, at least for the type of investigation we conduct, i.e. for comparative institutionalist investigation.

What problems must be solved in order to include nature in social explanations? Thanks to the chicken debate, the central problem has been described with some clarity. The point of departure is the observation that there are non-social factors (in the sense that their morphology, dynamics and effects cannot be explained sociologically) that affect scientists' actions and therefore must be included in social explanations of science. Sociologists who do not accept this statement are on the side of Collins and Yearly. Moreover, they are lucky because they avoid a very uncomfortable situation. However, they cannot provide satisfying explanations of scientists' actions.

If we accept the existence of non-social factors and the necessity to include them into our descriptions and explanations, the question is how this can be done. Science claims authority over these factors and provides a systematic account for them. However, its account cannot be used to explain scientists' actions.

(a) If it would be applied exclusively (the only alternative to ignoring these factors that is provided by Collins and Yearly), it would not provide social explanations because scientific accounts omit social factors.

(b) If it would be combined with accounts of social factors, we face the problem of idiosyncratic descriptions and the incommensurability of two types of explanatory factors that prevents us from synthesizing them in one explanation.

A solution to this dilemma seems to be the development of distinctively sociological accounts of the influence of non-social factors on scientists' actions. This is essentially the way ANT and 'Mangle' try to solve the problem. Pickering's solution is a hyper-abstraction that allows us to account for non-social influences either in an idiosyncratic language of real-time observation or in a language that is reduced to very few concepts. ANT advocates a generalized symmetry principle that leads into a sociologisation of nature. This doesn't work because the concepts lose their meaning when applied to non-humans. Thus, both approaches don't enable comparisons.

Our own approach falls in the third group, too. It is an attempt to identify *sociologically relevant classes of non-social factors* (epistemic conditions of action) and to describe the *channels through which they affect action* (linkage variables). This solution appears to work in our empirical investigations.

Because our approach has a specific task in an institutionalist analytic framework, its range of applicability is limited. Generally, there is a price to pay for the abstraction that is necessary in comparative approaches. Our approach cannot be applied in in-depth studies of processes and outcomes of knowledge production, i.e. in studies that are aimed at a 'real-time understanding' or 'thin description' of scientific practice.

A general problem that affects all attempts to account for nature is that it is not possible to completely avoid idiosyncracies. Scientists apply the current state of scientific knowledge which is unique to the research situation. Moreover, nature may (and usually does) interfere in unanticipated ways. We are aware of this problem but are currently unable to say to what extent it will affect our methodology. In the studies we have conducted so far, the epistemic conditions of action and linkage variables were sufficient to 'catch' the idiosyncracies.

The most important practical problem we are currently facing is that our approach implies translation processes (in the common sense of the word) that are very demanding. In observations and interviews with scientists, the world of natural science must be translated into epistemic conditions of actions. This means that an understanding of the content of scientific work (as scientists see it) must be achieved. However, the language of epistemic conditions of actions seems to be applicable because it uses concepts not alien to scientists (Laudel and Gläser 2004)

Thus, while solving the most urgent problem of comparing scientific practices, our solution is preliminary in many respects. We have been able to develop it as required by our empirical projects. It awaits further discussion.

References

Berger, Peter L./ Thomas Luckmann, 1967: The Social Construction of Reality, Harmondsworth: Penguin.

Bloor, David, 1976. *Knowledge and Social Imagery*. London: Routledge & Kegan Paul.

Burns, Tom R., and G.M. Stalker, 1961. *The Management of Innovation*. London: Tavistock Institute.

Callon, Michel, 1986. Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Brieuc Bay. John Law (ed.), *Power, Action and Belief*. London: Routledge, 196-233.

Callon, Michel, and Bruno Latour, 1992. Don't Throw the Baby Out with the Bath School! A Reply to Collins and Yearley. Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: The University of Chicago Press, 343-368.

Chubin, Daryl E., and Edward J. Hackett, 1990. *Peerless Science: Peer Review and U.S. Science Policy*. Albany, N.Y.: State University of New York Press.

Collins, H. M., 1984. Researching spoonbending: concepts and practise of participatory fieldwork. Colin Bell and Helen Roberts (eds.), *Social Researching. Politics, Problems, Practise*. London: Routledge & Kegan Paul, 54-69.

Collins, H. M., and Steven Yearley, 1992a. Epistemological Chicken. Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: The University of Chicago Press, 301-326.

Collins, H. M., and Steven Yearley, 1992b. Journey Into Space. Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: The University of Chicago Press, 369-389.

Collins, Harry M., 1985. *Changing Order: Replication and Induction in Scientific Practice*. London: Sage.

Collins, Harry M., and Trevor Pinch, 1982. *Frames of Meaning: The Social Construction of Extraordinary Science*. London: Routledge & Kegan Paul.

Gilbert, G. Nigel, and Michael Mulkay, 1984. *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*. Cambridge: Cambridge University Press.

Gingras, Yves, 1997. The New Dialectics of Nature. *Social Studies of Science* 27: 317-334.

Gläser, Jochen, 1998. *Kognitive Neuorientierung der ostdeutschen außeruniversitären Grundlagenforschung als Folge des Institutionentransfers*. Discussion Paper P98-402. Berlin: Wissenschaftszentrum Berlin für Sozialforschung.

Gläser, Jochen, and Grit Laudel, 1999. *Where do the Actants/Mangles Come From?* Paper presented at the Sociality/Materiality: The Status of the Object in Social Science, Brunel University, UK, 9-11 September.

Hammersley, Martyn, and Paul Atkinson, 1995. *Ethnography: prinicples in practice*. London: Routledge.

Knorr-Cetina, Karin, 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. Oxford: Pergamon Press.

Knorr-Cetina, Karin, 1995. Laboratory Studies. The Cultural Approach to the Study of Science. Sheila Jasanoff, Gerald E. Markle, James C. Petersen and Trevor Pinch (eds.), *Handbook of Science and Technology Studies*. London: SAGE, 140-166.

Knorr-Cetina, Karin, 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge: Harvard University Press.

Knorr-Cetina, Karin, and Martina Merz, 1997. Floundering or frolicking: How does ethnography fare in theoretical physics? (And what sort of ethnography?): A reply to Gale and Pinnick. *Social Studies of Science* 27: 123-131.

Latour, Bruno, 1987. *Science in Action*. Cambridge, Mass.: Harvard University Press.

Latour, Bruno, 1988. *The Pasteurization of France*. Cambridge: Harvard University Press.

Latour, Bruno, 1989. Clothing the Naked Truth. Hilary Lawson and Lisa Appignanesi (eds.), *Dismantling Truth: Reality in the Post-Modern World*. New York: St. Martin's Press, 101-126.

Latour, Bruno, 1990. Postmodern? No, Simply *A*Modern! Steps Towards an Anthropology of Science. *Studies in History and Philosophy of Science* 21: 145-171.

Latour, Bruno, 1994. Pragmatogonies: A Mythical Account of How Humans and Nonhumans Swap Properties. *American Behavioral Scientist* 37: 791-808.

Laudel, Grit, 1999. *Interdisziplinäre Forschungskooperation: Erfolgsbedingungen der Institution 'Sonderforschungsbereich'*. Berlin: Edition Sigma.

Laudel, Grit, and Jochen Gläser, 2004. *Interviewing scientists*. REPP Discussion Paper 04/1. Canberra: The Australian National University.

Laudel, Grit, and Gabriele Valerius, 2001. *Innovationskollegs als "Korrekturinstitutionen" im Institutionentransfer?* Abschlussbericht zum DFG-Projekt ,Innovationskollegs als Instrument der Umgestaltung der unviversitären Forschung im ostdeutschen Transformationsprozess - Akteure, Strukturen und Effekte'. FIT Arbeitsberichte. Frankfurt (Oder): Europa-Universität Frankfurt, Frankfurter Institut für Transformationsforschung.

Law, John, and Michel Callon, 1988. Engineering and Sociology in a Military Aircraft Project: A Network Analysis of Technological Change. *Social Problems* 35: 284-297.

Lynch, Michael, 1985. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*. London: Routledge & Kegan Paul.

Lynch, Michael E., 1982. Technical Work and Critical Inquiry: Investigations in a Scientific Laboratory. *Social Studies of Science* 12: 499-533.

Merz, Martina, and Karin Knorr-Cetina, 1997. Deconstruction in a 'thinking' science: Theoretical physicists at work. *Social Studies of Science* 27: 73-111.

Neidhardt, Friedhelm, 1988. *Selbsteuerung in der Forschungsförderung*. Opladen: Westdeutscher Verlag.

Perrow, Charles, 1967. A Framework for the Comparative Analysis of Organizations. *American Sociological Review* 32: 194-208.

Perrow, Charles, 1979. *Complex Organizations: A Critical Essay*. Glenview, Ill.: Scott, Foresman and Company.

Pickering, Andrew, 1984. *Constructing Quarks: A Sociological History of Particle Physics*. Chicago: University of Chicago Press.

Pickering, Andrew, 1995. *The Mangle of Practice. Time, Agency and Science*. Chicago: The University of Chicago Press.

Pickering, Andrew, and Adam Stephanides, 1992. Constructing Quaternions: On the Analysis of Conceptual Practice. Andrew Pickering (ed.), *Science as Practice and Culture*. Chicago: The University of Chicago Press, 139-167.

Pinch, Trevor, 1986. *Confronting Nature: The Sociology of Solar Neutrino Detection*. Dordrecht: Reidel.

Rheinberger, Hans-Jörg, 1997. *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Stanford: Stanford University Press.

Rip, Arie, 1982. The Development of Restrictedness in the Sciences. Norbert Elias, Herminio Martins and Richard Whitley (eds.), *Scientific Establishments and Hierarchies*. Dordrecht: Reidel, 219-238.

Sismondo, Sergio, 1993. Some Social Constructions. *Social Studies of Science* 23: 515-553.

Sismondo, Sergio, 1996. *Science without Myth: On Constructions, Reality, and Social Knowledge*. Albany: State University of New York Press.

Star, Susan Leigh, and Elihu M. Gerson, 1987. The Management and Dynamics of Anomalies in Scientific Work. *Sociological Quarterly* 28: 147-169.

Styles, Joseph, 1979. Outsider/Insider: Researching Gay Baths. *Urban Life* 8: 135-152.

Thompson, James D., 1967. *Organizations in Action*. New York: McGraw Hill.

Travis, G. D. L., and H. M. Collins, 1991. New Light on Old Boys: Cognitive and Institutional Particularism in the Peer Review System. *Science, Technology, and Human Values* 16: 322-341.

Traweek, Sharon, 1988. *Beamtimes and Lifetimes: The World of High Energy Physicists*. Cambridge: Harvard University Press.

Weingart, Peter, 1976. *Wissensproduktion und soziale Struktur*. Frankfurt: Campus.

Whitley, Richard, 1984. *The Intellectual and Social Organization of the Sciences*. Oxford: Clarendon Press.

Whitley, Richard D., 1972. Black Boxism and the Sociology of Science: A Discussion of the Major Developments in the Field. Paul Halmos (ed.), *The Sociology of Science (Sociological Review Monograph 18)*. Keele: University of Keele, 61-92.

Whitley, Richard D., 1977. Changes in the Social and Intellectual Organisation of the Sciences: Professionalisation and the Arithmetic Ideal. E. Mendelsohn, P. Weingart and R. Whitley (eds.), *The Social Production of Scientific Knowledge*. Dordrecht: Reidel, 143-169.

Woodward, Joan, 1965. *Industrial Organization: Theory and Practise*. London: Oxford University Press.

Woolgar, Steve, 1988. *Science: The Very Idea*. Chichester: Ellis Horwood.