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What Comes after Constructivism in Science and Technology Studies?

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Content

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Introduction: What Comes after Constructivism in Science and Technology Studies?

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Abstract

Constructivism has become the overarching scientific paradigm in the social study of science and technology (STS). The notion that scientific facts and technological artefacts result from processes of social construction is the major scientific innovation of the preceding decades in the sociology of science and technology. With constructivism being the established paradigm in this field of research: what comes next? What comes after constructivism in science and technology studies? The contributions of this special issue of *Science, Technology & Innovation Studies* suggest different answers to these questions which can roughly be subsumed under the three headings "Spelling out Constructivism", "Adding Disregarded Aspects", and "Going beyond Constructivism".

1 Introduction

Constructivism has become the overarching scientific paradigm in the social study of science and technology (STS). The notion that scientific facts and technological artefacts result from processes of social construction is the major scientific innovation of the preceding decades in the sociology of science and technology.

In the field of science studies this notion has had the characteristics of a revolutionary change. The emerging sociology of scientific knowledge was no longer content with merely analysing the institutional dimension of science as Robert K. Merton (1973) did. Its proponents no longer accepted the distinction according to which scientific truth is to be explained by the inner logic and rationality of science itself, whereas social influences are treated to be "extra-theoretical factors" accountable for scientific endeavours to go astray. In contrast to such a "sociology of error" (Bloor 1976: 8)[1] and in contrast to the Mertonian sociology of scientific institutions the then new sociology of scientific knowledge claimed that the content of science and not only its context should become the subject of sociological explanation.

No doubt, this approach in its different variants as "Strong Programme" (Bloor 1976), "Empirical Programme of Relativism (EPOR)" (Collins 1981; Collins 1983) or "Laboratory Studies" (Latour/Woolgar 1979; Knorr Cetina 1984) has turned out to be extraordinarily successful. A large number of empirical studies, were undertaken to show that and how scientific facts are constructed socially. These studies have demonstrated that many of the sociologists' conceptual and methodological tools for analysing and explaining social processes are also suitable for reconstructing and understanding the processes of generating scientific knowledge. It has been shown that scientific controversies are processes of social negotiation whose outcomes are a function of the interests, strategies and coalitions of the parties involved. Gaining common acceptance for scientific claims depends on the rhetoric skills, allies, and institutional resources (e.g. the already established scientific knowledge) the actors are able to mobilise. It has been demonstrated that many of the epistemic practices of scientists in their laboratories are similar to our normal everyday cultural practices of interpreting and understanding the world. Therefore, the same ethnographic methods which are used to study cultural practices turned out to be useful to study the epistemic practices of scientists and thus their cultural construction of scientific knowledge. Many studies have applied these basic methodological insights and have provided considerable evidence suggesting that social construction is a non-negligible aspect of scientific knowledge production.

With a delay of several years, social constructivism became adopted by technology studies, with Trevor J. Pinch and Wiebe E. Bijker (1984) being the pioneers of this development. The core assumption of the then new social constructivist sociology of technology is that technological artefacts are regarded as functional because they are successful – an assumption that contradicts the traditional view that technology is successful because of its functionality. From the social constructivist point of view, functionality is a relational feature, a feature a technological artefact gains by being seen as an appropriate solu-

[1] Bloor (1976: 8) blames Mannheim to have based his sociology of knowledge on the aforementioned distinction. It is true that Mannheim calls social factors "extra-theoretical factors" (cf. Mannheim 1985 <1929>: 230). But it is a somewhat biased interpretation to conclude from this that the Mannheimian "sociology of knowledge is confined to the sociology of error" (Bloor 1976: 8). More rightfully, Bloor might have pointed, for example, at Joseph Ben-David (cf. Ben-David 1971: 11-13).

tion to a relevant problem. Thus, becoming an appropriate solution to a relevant problem – becoming successful – defines the functionality and usefulness of technological artefacts. In other words: technological artefacts are constructed socially.

Social constructivism in the field of technology has never been perceived as being as revolutionary as in the field of science. It is true that social constructivism is opposed to common assumptions about technological functionality. And it is opposed to assumptions about technological imperatives governing paths of technological development. However, few people who share these assumptions – laymen, engineers or students of technological change – would disagree with the proposition that a technological artefact's success is dependent on its users' acceptance. Thus, social constructivism is less controversial in the realm of technology than in the realm of science. Technological artefacts are constructed as means to achieve human ends. Only those who challenge this basic assumption and believe that technological development has become an end in itself have reason to reject social constructivism in technology studies.

Nevertheless, applying social constructivism to technology brought about a major change. It triggered the development of the sociology of technology (and the social studies of technology respectively) as a distinct field of scientific research. Different strands of research on technology in historical, philosophical or political science, in sociology of industry or in innovation studies now became recognised and reevaluated as contributing to the social constructivist approach. Once explicitly articulated, the scientific paradigm of social construction of technology turned out to be a powerful focussing device which bundled and combined the hitherto fragmented research on social processes of technology development.

Twenty years after the initial formulation of the "Social construction of technology (SCOT)" as a research programme by Pinch and Bijker, we can look back to a considerable amount of empirical research. Many different technologies have been studied from the point of view of social constructivism. Maybe, some of the research has put too much weight on demonstrating the obvious, namely that technology is socially constructed (cf. Woolgar 1991: 36; Sismondo 1993: 543). But at the same time we have learned a lot about what is much more interesting: *how* technology is constructed socially (cf. Joerges 1995) It turned out that the interrelatedness between a technology's context of development and its context of use is of greatest significance for answering this question. Looking back it is thus safe to say that social constructivism is a successful research programme in technology studies, too.

The "science wars" debate (cf. Bammé 2004) has shown that social constructivism of science has not jet lost its provocative power. But it is provocative only for those who are doing science and not for those who are observing doing science.[2] For doing science, realism (naïve realism, critical rationalism, methodological positivism or other variants) is the standard operational epistemology. For researchers who do science the assumption that they deal with their research subject and not "merely" with social constructions is to a certain degree as inevitable as it is functional. Thus, it comes as no surprise that social constructivism, while being normal science for the scientific observers of science, still gives offence to the scientists observed.

Although less pronounced, a similar distinction between practitioners and observers can be found in the field of

[2] If one leaves aside the more specialised critique of postmodernist story-telling about science (e.g. the "Sokal hoax").

technology. As the scientist's, primary concern with her research object makes her lose sight of the social contingencies of the knowledge production process, the engineer's primary concerned with problems of technological feasibility makes him neglect the technology's context of use. The "acid test of the market", however, continually reminds him that it is ultimately the users whose interpretations and patterns of use turn his work into a successful (or failing) technological innovation.

Michael Guggenheimer and Helga Nowotny contended that the present state of science and technology studies is characterised by a "joy of repetition" (2003: 231). Even today, much research in science and technology studies is designed to demonstrate that this scientific truth or that technology is constructed socially. In the face of a scientific climate that favours the realism of the natural sciences rather than the constructivism of the social sciences, and in the face of an engineering culture with limited attention to the social features of technological design, the tendency to point out again and again what is already sufficiently proven is understandable. However, as Guggenheimer and Nowotny suggest, this repetition might also indicate a certain stagnancy of science and technology studies.

Fighting past battles – as it happened with the "science wars" debate – is not a promising future for science and technology studies. So what *is* the future of STS? With social constructivism being the established scientific paradigm of the social studies of science and technology, what comes next? What comes after constructivism in science and technology studies?

We suggest that are three different answers to this question: (1) spelling out constructivism, (2) adding disregarded aspects to constructivism, or (3) going beyond constructivism. According to Thomas Kuhn (1962), the establishment of a new scientific paradigm is followed by a phase of "normal science". Normal science means to implement in research practice what the paradigm at first merely promises, to concretise what is initially a general idea, and to deal with the paradigm's implications, many of which are unrecognised in the beginning. 'Normal science' thus means to spell out the new approach.

A second characteristic of a new paradigmatic approach – besides being little more than a rough idea initially – is its tendency to be excluding and unfair against antecedent or rivalling approaches. Since proponents are interested to highlight the originality and superiority of the new approach they tend to downplay all the achievements that different approaches have already contributed or may contribute in the future. A good example of this rhetoric strategy is Bloor's somewhat pejorative characterisation of Mannheim's approach as a "sociology of error". Once the new scientific paradigm is established, these over-accentuated demarcations become less important. This opens up the opportunity to look for aspects in which the new approach and its predecessors or rivals complement each other rather than holding competing views. For instance, after studying the very content of science has become an established approach, there is little reason why studying the institutional context of science should be seen as a competing rather than as a complementary area of research in the social studies of science (cf. Schimank 1995). Seeing constructivism as firmly established, the second answer to "What is the future of constructivism in STS?" is that now it is time to add disregarded aspects of this kind.

However, it may turn out that research governed by a paradigmatic scientific approach comes to face problems that can neither be solved by spelling out the approach nor by adding disregarded aspects. Such anomalies, if they are important enough and if a new paradigm is put forward with a plausible promise to solve these problems, may, according to Kuhn, lead to a new revolutionary situation and to the displacement of the established approach by a new one. In the case of constructivism, we see two problems that may have such a paradigm-changing quality: the reflexivity problem ("if scientific propositions are social constructions, then this holds for this proposition, too") and the problem of material agency. And there is at least one prominent scientific approach – actor-network theory – which claims to be a new scientific paradigm, to go beyond (and not behind) constructivism and to be able to solve both problems. The third answer, thus, is that the days of constructivism in STS are numbered and that postconstructivist or "posthumanist" (cf. Pickering 2005) approaches such as actor-network theory will take over.

It should be added that the distinctions between these three paths of constructivism's future are less sharp than the application of the Kuhnian terminology makes it sound. For instance, actor-network theory and other post-constructivist approaches are deeply rooted within constructivism so that one could argue that they are forms of spelling out implications of constructivism rather than new paradigmatic approaches. Nevertheless, it does not seem to be completely misleading to subsume the answers given by the authors of this special issue of the *Science, Technology & Innovation Studies* under the three headings "Spelling out Constructivism", "Adding Disregarded Aspects", and "Going beyond Constructivism".[3]

[3] Preliminary versions of this special issue's papers were presented at the Annual Conference 2004 of the German Association for Science and Technology Studies (Gesellschaft für Wissenschafts- und Technikforschung e.V.) in Berlin, Nov 26-27.

2 Spelling out Constructivism

In her article "The Topicality of the Difference Thesis: Revisiting Constructivism and the Laboratory", *Martina Merz* begins with the observation that constructivist STS never has been a monolithic endeavour, but from the very beginning existed in two variants. Although these two variants share basic conceptual assumptions and research issues, they differ strongly when it comes to the question of extending the foci and loci of STS research, especially when moving beyond the walls of the laboratory. According to the first variant of constructivist STS (termed the analogy approach), there are no epistemic particularities of knowledge production in the laboratory. Although an important cornerstone of STS, this variant is limited to examining and showing the locally constructed and negotiated character of facts and artefacts – issues that cannot be considered as challenges today. The second variant (termed the difference approach) states that there is something specific about the scientific laboratory: The power to reconfigure subject-object-relations, and this power is stronger than within any other social organisation, and can explain the success of the laboratory in modernity.

Focussing on the continuing topicality of the difference approach can, according to Martina Merz, lead to a whole research programme that can be summarised as "transcending" versus "extending the laboratory", both of which have not been spelled out within the constructivist approach. "Transcending" the laboratory asks how results that were locally produced in the lab can be successfully exported or transferred to other settings. Concrete questions on this line of research could be e.g. to investigate the conditions of the transferability of scientific results, thereby explaining its power in more depth, or to explore more systematically the epistemic practices that account for the disembedding and the re-embedding of objects and results. "Extending" the laboratory raises the question whether laboratory-like features of knowledge production can be identified in other societal realms, like e.g. in the practices of object reconfiguration in interdisciplinary research areas like computer simulation and environmental sciences. While still being in the line of constructivist STS, all of the questions raised in the article of Martina Merz can give way to a more concrete exploration of the issues related to the notion of the knowledge society.

With their article "Three Forms of Interpretative Flexibility", *Uli Meyer* and *Ingo Schulz-Schaeffer* subject one of the core concepts of constructivism – interpretative flexibility – to a systematic analysis that leads to a significant extension of the concept. The authors demonstrate that there are three rather than one form of interpretative flexibility, and that each of them is based on a specific regress of arguments in science. They adopt the work on the interpretative flexibility of scientific statements and the *regress of truth* (H. Collins). Meyer and Schulz-Schaeffer then analyse the approach to technological controversies of the "Social Construction of Technology (SCOT)" programme (Pinch and Bijker). They observe that this approach is far less convincing because it copied the concepts from science studies and overlooked that technological controversies deal with a different kind of interpretive flexibility, which is based on a *regress of usefulness*. In a third step, the authors use an empirical investigation of the controversy about neural networks to introduce a third and new type of interpretative flexibility that can be distinguished from the two others. This controversy addresses neither truth nor usefulness. The interpretative flexibility of statements about the potential of scientific or technological approaches is based on a regress of relevance: Which approach will best advance the scientific or technological development?

Having introduced three distinct forms of interpretive flexibility, the authors demonstrate the usefulness of their distinction by discussing switches of controversies. They identify a switch from the truth discourse to a relevance discourse in the controversy about gravitational waves, a switch from the usefulness discourse to a truth discourse in the controversy about bicycles, and a supplementation of the relevance discourse by a usefulness discourse in the controversy about neural networks. By demonstrating that all three forms of interpretative flexibility can and indeed do occur in scientific and technological controversies, they provide a powerful tool for the analysis of scientific controversies.

3 Adding Disregarded Aspects

In his article "Deliberative Constructivism" *Wolfgang Krohn* deals with the question: "How can we, as scientific observers of scientific enterprises, distinguish between good and bad constructions of knowledge?" (p. 42) Obviously, in knowledge societies this is a question of considerable relevance. But it seems to be a question that is impossible to answer from a constructivist point of view. If scientific knowledge is the result of a process of social construction, so are the criteria for the assessment of its quality. These criteria are thus shaped by interests, prejudices, status, values, and world views and cannot be used by a scientific observer to distinguish between good and bad constructions. At the same time, however, constructivism invites the observer to take a normative stance: "precisely because our methods and concepts in the production of knowledge and the justification of truth claims are culture bound, their relatedness can not only be observed but also controlled and adjusted – at least to some degree." (p. 43) This is, then, the dilemma of constructivism in the sociology of science: on the one hand, it shows that scientific knowledge is manmade, meaning that the criteria of good science can be established deliberatively; on the other hand, however, it deconstructs truth as the scientific criterion for good (or bad) knowledge.

Wolfgang Krohn proposes a constructivist solution to this dilemma which he calls "deliberative constructivism". His main argument is a dialectical one: "Any attempt to determine the limiting conditions of a culture provides already cognitive options for transgressing the limits. ... From the impossibility of a 'perfect' translation it does not follow that it is impossible to distinguish between better or worse translations. Instead, the better the limiting conditions of both languages are known, the fairer can the search for an improved translation be guided including options for slightly changing certain language features. A similar argument holds for the justification of truth claims." (p. 54) If one observes specific dependencies of scientific knowledge on certain social or cultural conditions one can use this knowledge as a guide for reducing these dependencies. This is the basic idea of deliberative constructivism. From this point of view, "reconstruction of the relativity of knowledge is a potential contribution to expand its irrelativity" (p. 56).

4 Going beyond Constructivism

Actor-network theory (ANT) and constructivism in STS are roughly of the same age, thus there is good reason to evaluate actor-network theory with as much scrutiny as the former. This is even more so since both ANT and constructivism departed from largely the same sharp critique of the understanding of science and technology as it was established in the social sciences until the late 1970s. Though both approaches share their point of departure most of us would hesitate to call ANT a constructivism proper. In pursuing the common goal of "opening up the black box", ANT created its own very special approach and vocabulary, often also addressed as "symmetric anthropology".

Instead of systematically evaluating the theoretical perspective established by Michel Callon, Bruno Latour, John Law, and others, *Thomas Berker* in his contribution "The Politics of 'Actor-Network Theory'. What Can 'Actor-Network Theory' Do to Make Buildings More Energy Efficient?" undertakes it to confront ANT with a proof of its usefulness by re-analysing its virtues in an empirical project in technology development, while at the same time following the line of critical comments and discussions developing the ANT approach further on. In doing so he employs two different images of ANT, or shall we say: perspectives, that is, "ANT in the making" and "ANT as a tool, which can be applied to understand the world". His core argument is that in order to get the best out of ANT for analytical purposes it is mandatory not to "privilege either the applications of 'ANT' or 'ANT in the making'". He, thus, pleas for refraining from re-establishing a false dualism of on the one hand the tool character of a black boxed ANT in an application perspective and on the other hand the open-ended process of theoretical advancement of ANT. Obviously this is in itself an ANT-based argument deeply rooted in the anti-dualistic concerns of the ANT's founding fathers.

Peter Wehling in his article "The Situated Materiality of Scientific Practices: Postconstructivism – a New Theoretical Perspective in Science Studies" focuses on a line of debate which is occasionally labelled as postcontructivist studies. He combines it with one of the most relevant perspectives in the current debate of a sociology of scientific knowledge: the debate about forms and consequences of scientific non-knowledge. His thesis is that the fruitfulness of postconstructivism and its attention for the situated material and discursive practices could be demonstrated with respect to the debate of non-knowledge: “no less than knowledge, non-knowledge is embedded and inscribed in practices conceived as material reconfigurations of the world.” (p. 94)

To demonstrate this, Peter Wehling firstly sketches the history of the postconstructivist debate. In the course of this argumentation he outlines the insight of SSK that beyond of “bringing back in” material factors a self-reflective notion as to basic assumptions of SSK emerged. With respect to this result he argues secondly that the self-reflective turn could be demonstrated with regard to three key concepts: knowledge, practice and performativity. To establish another concept of knowledge he refers to the works of Joseph Rouse and his idea of a “deflationary” and “non-reifying” concept of knowledge. Following Rouse, he regards practices not primarily as “doings of social actors”. Moreover, “an adequate conception of (scientific) practices has to encompass the material ‘configuration of the world’ (Rouse) which makes the activities of individual or collective agents become significant, coherent and intelligible.” (p. 89) Additionally, he maps the idea of the performativity of scientific practices – against a “traditional” representationalist approach of science. Thirdly, Wehling shows the embeddedness of scientific non-knowledge with respect to general concepts (such as the concept of “epistemic cultures” of Karin Knorr Cetina, which could be extended to a concept of “scientific cultures of non-knowledge”) and the debate on genetically modified organisms. He notices that the perspective offered could be fruitful “for initiating more self-reflective research practices, especially when such contrasting scientific cultures of non-knowledge [as in the field of genetically modified organisms, the molecular biologist and ecologist; the editors] are confronted with each other in public arenas” (p. 95).

5 Outlook

The contributions of this special issue on the question “What comes after constructivism in science and technology studies?” suggest that the constructivist approach is still a vivid source of inspiration in this field of research. Even the postconstructivist considerations are far from leaving the constructivist foundations behind. There is still a lot of work to do in order to spell out implications of the constructivist approach. At the same time, constructivism in STS now seems to be mature enough to ease initial cognitive restrictions, to broaden its scope, and to take considerations into account which complement the own point of view. In all these directions of considering the future of constructivism in science and technology studies much more is to be said than this special issue can cover. However, we hope it will serve as an impulse to re-examine the constructivist foundations on which much of our work is based.

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The Topicality of the Difference Thesis
Revisiting Constructivism and the Laboratory

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Abstract

Within science and technology studies, constructivism has never existed as a single variant but under alternative interpretations. In this article it is argued that the different variants have maintained their topicality in unequal measure. It focuses on two variants of constructivism: The first emphasizes the isomorphism of scientific and other practices and insists that there are no epistemic particularities in scientific knowledge production ("analogy approach"); the second accounts for the success of contemporary science by relating it to the specifics of scientific laboratories ("difference approach"). In this paper it is argued that the second variant can provide a set of challenging research problems that have not, to date, been sufficiently addressed in the literature. The problems center on the relation between laboratories and contexts of application, as well as on the concept of the laboratory and its possible extensions. In contrast, the issues associated with the analogy approach have been well explored in previous bodies of work. This article develops a research agenda for a constructivist account of knowledge production that may be employed within other discourses in the social sciences.

1 Introduction: Constructivism in Social Studies of Science

This article[1] addresses the claim that constructivism in science studies has lost its provocative gist and potential to surprise.[2] On the basis of the observation that, in the social studies of science, constructivism has never existed as a single variant but under alternative interpretations, the article proposes a rephrasing of this claim. What are these different variants and according to what criteria may they be distinguished? Surprisingly, only few attempts have been undertaken to sort through and systematically classify the different understandings of constructivism. One exception is an article by Sergio Sismondo (1993), who maintains that the construction metaphor has at least four different uses and interpretations. Sismondo's article received wide attention and was the subject of controversial discussion for two reasons: first, because of its attempt to bring some order into the muddle of constructivist interpretations; secondly, because of the way it evaluated the significance of these four interpretations for the practice of STS.

Sismondo differentiates constructivism with respect to the types of entities that have been constructed and identifies four types of entities: (a) *social objects* (e.g. knowledge, methodologies, habits) – the associated form of constructivism exhibiting affinity with "social constructivism" in the spirit of Berger and Luckmann (1966); (b) *conceptual entities* (e.g. theories, accounts, images) – the focus in this case being on how patterns or structures are generated from data and observations; (c) *artifacts* – herewith shifting interest to the level of material interventions in the laboratory; and (d) *objects of thought and representation*. The last variety, labeled also "idealist," "neo-Kantian" or "strong" constructivism, forms the most controversial interpretation as it asserts that material objects ("nature") are construed out of world-views ("science"). Strong constructivism has been a matter of particular contention between philosophers of science and the more radical constructivists in the field of the sociology of scientific knowledge; the controversy then spreading to other audiences in the wake of what was to become known as the "science wars."

Sismondo's contribution to the debate on constructivism consists – which relates to the second issue above – in his ranking of the different constructivist interpretations by importance. In particular, he downplays "strong constructivism" by considering it to be the least important interpretation for actual work done in social studies of science. This has led Karin Knorr Cetina (1993) to counter with a "strong constructivist thesis," according to which "the world is slowly molded into shape in ever new ways through successive generations of (scientific) practice" (Knorr Cetina 1993: 560). Other respondents have contested Sismondo on different grounds. Peter Taylor (1995), for example, has criticized the specific attention accorded to the type of entities produced, suggesting instead that the focus of attention be the different processes of production. He also argues for a stronger emphasis on "the process of science in the making as a co-construction" involving a diversity of agents and components (Taylor 1995: 353; cf. also Sismondo 1995).

What we learn from Sismondo's text and the critical responses it has triggered is that constructivism in social

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[1] I thank Richard Randell and two reviewers for their valuable comments on this article.

[2] See, for example, the call for papers of the 2004 Annual Meeting of the GWTF "Was kommt nach dem Konstruktivismus in der Wissenschafts- und Technikforschung" (Berlin, November 26-27, 2004).

studies of science is above all a multi-faceted thing.[3] It comes in different interpretations, each of which may serve specific theoretical or practical purposes and be part of a dedicated research program. The debate also hints at the possibility that different variants of constructivism follow different trajectories. This idea will be further explored in the present text, albeit with a focus on a different scheme of constructivist interpretations. Two interpretations that follow from the science-as-practice approach in the social studies of science with its interest in the constructive elements of scientific production are juxtaposed. The first interpretation stresses the analogy of scientific practice and other forms of practice and asserts that there is no epistemic particularity in scientific production (analogy thesis); the second interpretation seeks to account for the remarkable success of contemporary science and hence inquires into the specifics of scientific production (difference thesis).

The proposed distinction, which to date has not been discussed systematically in the science studies literature, allows one to separate off one variant of constructivism which, I argue, opens up interesting perspectives for future research, from a second variant whose general mechanisms are today rather well understood. The present article thus addresses the topicality of the two approaches in a double sense: On the one hand, it investigates the potential of both approaches to raise interesting new questions and perspectives for future work. On the other hand, it explores the topicality of the two approaches in the sense of their "aboutness" (Reinhardt 1981)[4] through drawing out the topical fields to which they relate.

In section 2 the two constructivist approaches will first be situated in the science-as-practice approach and then introduced in more detail. The following sections address two issues which, it is proposed, should generate questions for further research: the header "transcending the laboratory" hints at the relation between laboratories and contexts of application, which is explored for each of the approaches (section 3); the concept of the laboratory and its possible extensions is discussed with particular reference to the "difference approach" (section 4).

2 Constructivism and Concepts of the Laboratory

An interest in the process of knowledge production emerged in the late 1970s, just a few years after the new sociology of scientific knowledge (SSK) had taken off. Both the constructivist approach and SSK are convinced that science is not to be investigated merely as a social institution (in the tradition of Merton) but that science's epistemic core is a matter of investigation in its own right. In respect to their perspectives on science's epistemic core, however, the two approaches are complementary. Whereas SSK focuses primarily on the social causes of the scientists' convictions and knowledge-beliefs – on science as knowledge – the constructivist approach turns its atten-

[3] This article will be concerned with constructivism in social studies of science only. It will not address other "Spielarten des Konstruktivismus," as Knorr Cetina (1989) denotes different varieties that range from "social constructivism" as mapped out by Berger and Luckmann (1966) to "radical constructivism" in sociology (for example, the work of Luhmann) and the empirical program of constructivism in the sociology of science. For a discussion of different interpretations of constructivism in the social sciences and humanities see Hacking (1999).

[4] A related notion of topicality is addressed in linguistics. Michael Lynch (1991) pursues a different notion of how knowledge production is "sited" by providing two examples of "topical contextures" that define spatial orders associated with complexes of equipment and practice.

tion to the constructive elements of scientific production – on science as practice. Interest in the process of knowledge production has led to a greater appreciation that science is a practical accomplishment. One of its most significant observations has been that scientific practice is firmly embedded in local environments and should, consequently, be investigated in situ, thus bringing the privileged sites of knowledge production into view – the scientific laboratories. The social analysts' interest in the laboratory and its goings-on has given rise to the "laboratory studies approach" – the exploration of the minutiae of everyday scientific practice through participant observation methods, combined with ethnomethodology and discourse analysis.[5]

The science-as-practice approach has led to constructivist interpretations that are intimately linked to conceptions of the scientific laboratory. I distinguish two complementary interpretations, both of which have been elaborated by the same set of authors and which represent different focal points and targets of argumentation. While the first contends that scientific practice does not substantially differ epistemically from other realms of social practice (2.1), the second explores the reasons for the success of science and, thus, zooms in on science's unique features (2.2).

2.1 Analogy Thesis

The first perspective views laboratory research as inextricably tied to the locales in which knowledge is produced (for an overview see e.g. Lynch 1997: chap. 3). The laboratory is seen as a repository of competences, practices, tools and resources that the scientists draw upon. Scientists exploit the contingencies of local contexts with respect to the equipment and research facilities at hand, the interactional circumstances, the conventions embodied in laboratories, the combined expertise gathered in a research team and the organizational setting in which it is embedded. Scientists draw on a whole repertoire of improvisations and tentative solutions, different forms of tinkering and embodied skills, as well as different techniques of persuasion and negotiation. The research problems, consequently, are locally constituted, as are the research objects, the tools, and the ways in which scientists handle and assemble all these elements. Out of this seemingly messy set of things and actions, scientists "produce order" (Latour/Woolgar 1979) as they conceal the messy traces of their work. This implies that science does not merely represent reality as it is "out there;" scientific work is constructive. What later appears as a natural phenomenon or as unproblematic data is the outcome of a complex production and selection process. Thus, scientific practice is also an interpretive, representational and literary activity. Data and other outcomes and products of scientific practice are rarely – some would say never – unambiguous, complete, definite and univocal. They retain a high degree of interpretative flexibility.

The observations of the locally situated nature of scientific work with its high degree of contingency as well as the negotiated character of all the steps that intervene in the process of fact construction have led laboratory analysts to the conclusion that "nothing epistemically special is happening" (Knorr Cetina 1995: 151) in scientific knowledge production.[6]

[5] The first laboratory studies were published in the 1980s, for an overview see Knorr Cetina (1995).

[6] For a detailed account of the local situatedness of research see Knorr Cetina (1984: chapter 2); for a specification of the concept "locally organized activities" see Lynch (1997: 125-133).

2.2 Difference Thesis

A second perspective identifies the laboratory as the paramount site of knowledge production in modern science. Although scientific knowledge is of course also produced at other sites, the laboratory has come to symbolize the power and success of science – a development that originated in the 19th century. Bruno Latour and Karin Knorr Cetina, among others, have convincingly maintained that this power relies on specific forms of object work that are performed in – and are constitutive of – the laboratory.

In his discussion piece "Give Me a Laboratory and I will Raise the World," Bruno Latour (1983)[7] argues that scientists gain strength in the laboratory by inverting the hierarchy of forces according to their research interests. They do this by reversing the scale of phenomena at will in the laboratory, making some objects bigger, others smaller. For example, organisms are isolated and cultivated in a suitable milieu, which allows them to grow exponentially and become visible to the scientist's eye. As a consequence, scientists are enabled to do things in the laboratory that are not feasible outside the laboratory, where the existing scales are unmanageable and cannot be negotiated. The variation of scales has another favorable effect: it enables scientists to multiply experiments at reduced cost, allowing for an increased number of trials and errors. As a consequence, the laboratory turns into a learning environment, "a technological device to gain strength by multiplying mistakes" (ibid.).[8]

Karin Knorr Cetina (1992) similarly argues that the laboratory is "an enhanced environment" (ibid. 116) and that this accounts for the success of science. The mechanism that brings this about is the reconfigurating of subject-object-relations to the scientists' advantage, which can be viewed as a generalized notion of Latour's scale reversal. In the laboratory the phenomena of investigation are removed from their natural context. Scientists reshape them in order to control their temporal and spatial accessibility and render them fit for experimentation.[9] Lab objects can be duplicated, standardized and made amenable to a full sequence of experiments (cf. Amann 1994). In addition, social relations are reconfigured – "upgraded" in Knorr Cetina's terms – and aligned with the specific requirements of the objects in the lab. For example, collaborations are forged to confront the object world optimally, with form and size of collaborations differing widely across fields. Another example is provided by scientists who assume the function of human measuring devices or who become important repositories of unconscious experience.

To summarize this perspective: Knowledge production is closely associated with a specific mode of relations between the scientists and their laboratory objects. The power of the laboratory stems from the reconfigurations that shift the balance of subject-object relations to the benefit of the scientists. This mechanism accounts for the

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[7] For a thoughtful account that challenges Latour's claim that laboratories (in all cases) "raise the world" see Scott (1991).

[8] For the idea that the multiplication of errors allows for a reduction of uncertainty, see, in a different context, Donald MacKenzie's (2000) discussion of computer systems: "a computer system that errs frequently (and is therefore distrusted) is, under some circumstances, less dangerous than one that *almost* never errs" (ibid. 183).

[9] Objects are not only technically manufactured, they are also symbolically and politically construed (e.g. by way of literary techniques of persuasion) which resonates more closely with the characterization of laboratories according to the "analogy approach."

difference between the laboratory (and the subject-object-dynamics it defines) and other societal settings and turns it into an "enhanced environment."

Although they were developed by the same community of researchers the analogy approach and the difference approach have followed different trajectories and they have advanced at an uneven pace. The analogy approach has provoked a curious mix of praise, considerable attention and controversial reactions from colleagues, especially in its earlier years – and it was hotly debated once again by self-selected proponents of the sciences during what some have termed the "science wars" of the late 1990s. The difference approach, in contrast, has stayed largely out of the limelight.

The two variants have maintained their topicality in unequal measure also with respect to the associated research programs. The analogy approach has brought about a thorough understanding of the open, contingent and negotiated character of scientific work and of the processes and mechanisms it involves. Due to its earlier productivity and success one may hypothesize that the approach neither challenges nor surprises science studies scholars to the same extent any longer. By contrast the difference approach, which has never been as controversial and as publicized as its sibling, provides still today a challenging research agenda. To spell out what this challenge might look like, two sets of issues are discussed in the following two sections (3 and 4).

3 Transcending the Laboratory

Laboratory studies have convincingly demonstrated that knowledge production in the lab is a locally situated activity. This raises two important issues that concern the boundaries of the laboratory and which deserve further consideration. A first perspective on the relation of the laboratory and its boundaries focuses on how results that were locally produced in the lab can be successfully exported and transferred to other settings. What are the mechanisms through which scientific statements or facts transcend the laboratory and link up with very different problem contexts and societal settings? The two variants of constructivism provide different answers and raise further questions, which will be detailed in the next paragraphs. A second perspective focuses instead on the confines of the laboratory; that is, on the lab and its possible extensions. Such extensions, and their implications, are addressed through extending both the concept of the laboratory and the physical spaces available for empirical investigation (section 4). The two perspectives are separated here for analytical reasons, however, when addressed in the context of specific research problems in a dedicated project they will need to be considered jointly.

3.1 Analogy Approach

For the analogy approach, how statements or facts transcend the laboratory does not pose a specific challenge. If there are no epistemic differences between the practice of knowledge production in the laboratory and other kinds of (non-scientific) practice, as posited by this approach, then it should come as no surprise that the exporting of results beyond the boundaries of the laboratory should be, at least in principle, unproblematic. This still requires that the specific transfer mechanisms are spelt out in detail, which they have. The analogy approach argues for a continuity of practice. Through the identification of a variety of strategies that are employed by scientists, studies in this tradition have shown how local products are turned into universal scientific facts. One important strategy of scientists is to employ a full chain of representations, of which visualizations provide an interesting example. The visualizations with which scientists work do not simply portray nature; they are the result of a multilevel process of production, translation, and transformation. The intricate visualization and representation practices are conceived as a “transformation of rats and chemicals into paper” (Latour 1986) that not only fosters understanding of research problems and results but also assists scientists in communicating their results across local contexts and in convincing their colleagues of the work’s importance and validity.

Another strategy involves decontextualization – the production of objectivity effects through a step-by-step removal of reference to local contingence: Scientists do not disclose the open, contingent, and negotiated character of practical work in their accounts but instead produce condensed and purified versions of what goes on in the laboratory. Objectivity effects derive from rhetorical procedures, through which statements are transformed into solidified facts. Scientists, consequently, seem to be simply “reporting natural facts;” the constructed nature of knowledge disappears from view.

The general mechanisms by which statements are turned into facts and then travel within scientific communities and cross the boundaries of science have been well documented in this approach; this does not seem to be the case for the second.

3.2 Difference Approach

From the perspective of the difference approach the answer is less obvious. How can one explain that what holds within the confined settings of a laboratory is also valid outside of it? One might rather hypothesize the contrary – that the reconfigurations performed in the laboratory transform the configured entities in such a way that the results obtained by manipulating them are not transferable to the “world” in an unproblematic manner. This hypothesis follows from the assumption of an asymmetry between the laboratory and the world that underlies the difference approach. The laboratory order appears as clearly distinct from the natural order, the laboratory being characterized by a “homing in” (Knorr Cetina) of natural processes. This observation thus calls for an explicit discussion of the transfer modes of laboratory outcomes and their respective validity. It may come as a surprise that the processes of what one may call “re-reconfiguration” – how laboratory outcomes are successfully embedded into socio-material contexts beyond the laboratory – has not received sufficient attention.[10] There are, however, several important exceptions.

In his study on Pasteur, Bruno Latour (1988) provides an original account of how what holds in the laboratory is rendered valid also for application in other settings. Latour explains Pasteur’s success in the world outside the laboratory – measured, for example, by the effectiveness of the vaccine – by the fact that the external world had been made to comply with the laboratory conditions. The stables, for example, had to adopt strict hygiene conditions and the sheep were vaccinated. While Latour’s argument is convincing for the case at hand, one wonders how instructive it is for other cases. Do fields beyond the laboratory imperatively need to be molded according to laboratory conditions for laboratory-produced knowledge to be successfully applied in practice?

A general framework for explaining the success of scientists is provided by Actor-Network-Theory (ANT), for which Pasteur and other studies are illustrative (e.g. Callon 1986; Latour 1988, 2005). According to ANT, success does not result from the truth of the results that are put into practice

[10] For a similar assessment, see Heintz (1993: 545-546, note 34).

but is a function of how the laboratory is positioned in society. Scientists must successfully manage a heterogeneous network of actants (human actors, natural objects, material entities, etc.), they must capture the interest of previously uninterested outsiders, enroll actants into the network as allies, and translate and stabilize the actants' interests. Translation – the re-interpretation or appropriation of others' interests into one's own – is the key strategy employed to mobilize broader support. The network needs to be stabilized for a scientific fact or result to assume significance outside its production context and be turned into a black box.

Actor-Network-Theory has been very influential due to its radical reformulation of nature-society relations and of the dynamics that unfold from unstable states of nature/society. In respect to the question under consideration here, it provides a general answer at a high level of abstraction. This leaves the door open to alternative interpretations, especially if one is interested in the minutiae of social-epistemic practice and the specific solutions that different problem areas and scientific fields elaborate to provide for and guarantee the transferability of laboratory results. If this is the focus, one will need to move beyond the (too) general frame of ANT. A few suggestions for relevant questions and instructive cases to be considered in more detail are sketched in the following.

3.3 The Game of Disembedding and Re-embedding

Knowledge about the transferability of scientific results to settings beyond the laboratory is distributed unequally throughout the scientific spectrum. Whereas scientific fields closer to application contexts need to handle the problem of transferability explicitly, other subject areas disengage from the issue to pursue a purely "internalist" research agenda. In so far as laboratory studies have focused on typical laboratory sciences, the question of the transferability of results has remained in the background, simply because it was of minor interest to the observed practitioners. This raises the question of which scientific areas might render an investigation of the game of "dis-embedding" and "re-embedding" practices particularly insightful and productive – the "game" denoting, on the one hand, the dynamic interrelation between the subject-object reconfigurations that account for the power of the laboratory and, on the other hand, the strategies that connect the ensuing outcomes with broader contexts. It should be noted, however, that what constitutes these "broader contexts" of interest has to be identified separately for each and every case. For example, the contexts may range from adjacent fields of research to other scientific areas or even to extra-scientific domains.

The first recommendation of this article is that the difference approach be brought to bear on studies of research areas that vary in the degree to which "strongly contextualized knowledge" (Nowotny et al. 2001) is produced. Environmental sciences, medical sciences and engineering sciences are instructive cases. To date very little is known about the dynamic relation between laboratory cultures and the strategies employed to ensure the practical validity of results, which raises the additional question of the origins of evaluative practices and standards in the sciences. For example, do contemporary societal preferences for knowledge that is certified according to scientific standards have an influence on the reconfiguration practices in scientific laboratories? A comparative perspective would provide interesting insights into both the field-specific practices and the more general mechanisms by which laboratory knowledge is exported.

The second recommendation is that science scholars explore more systematically the epistemic practices that account for the disembedding and the re-embedding of objects and results. In this context a focus on modeling practices is of considerable interest. Models of all sorts (physical models, prototypes, model systems, formal models, computer models, etc.) play an essential role in knowledge production in general, and in the reconfiguration of objects in particular. They have recently been taken up as prominent topics of investigation in science studies, albeit not sufficiently in respect to the perspective presented here (cf. Knuuttila et al., in press). In the following I will present one important example: computer simulation as an epistemic practice that navigates continuously between the requirements of object reconfiguration and outcome re-embedding, which has become a key epistemic strategy across a wide range of scientific fields (cf. for a recent overview Lenhard et al. 2006).

Phenomena are numerically configured to render them amenable to experimentation in simulation studies. In many cases, to construct the numerical models that underlie the simulation involves a complex chain of modeling steps and approximations (cf. Winsberg 1999). From this perspective the computer appears as a functional equivalent of the workbenches of a traditional laboratory science, and simulation studies are perceived as being performed in a digital laboratory. Simulation allows scientists to mimic, shape and experiment on natural, technical or formal processes and phenomena such as natural systems or research apparatuses. Scientists exploit these options for various purposes: they explore new spaces of action, probe the consequences of theoretical assumptions or investigate the dynamics of a natural system.

What is important in the context of the present discussion is that studies of simulation practices reveal the need to carefully consider the disembedding *as well as* the re-embedding dynamics of object work (cf. Merz 2006). This is due to the fact that a simulation study in many cases is not an end in itself: it is typically explicitly targeted to the solution of practical problems, as its application in the environmental sciences (e.g. climate research) testifies. In many cases, simulation studies simultaneously address a scientific problem and produce predictions of use to other (often non-expert) communities within or outside science. The scientists must actively negotiate the balance between the reconfiguration and the re-embedding requirements of the study: reconfiguration – the transformation of objects as they occur "in nature" into the objects worked on in laboratories – requires a form of disembedding. Reconfigured objects are easier to deal with and it is possible to extract results from them in ways that advantage the scientist precisely because they have been partly disembedded from their natural environments. The work of re-embedding is required to link up the outcomes of simulation studies with the practical problem that motivated the study at its onset. The mechanisms and strategies that are employed to ensure that the results can be successfully transferred to sites beyond the laboratory are context-dependent: they may vary with the considered scientific area, the concerned scientific problem or the public significance of the issue at hand. The practice of how to transcend the digital laboratory may also involve very different systems of reference. Simulation studies in fields like particle physics, for example, are disciplined by the parallel performance of "real" (in contrast to computer) experiments: simulation results need to prove themselves in comparison to "real data,"[11] which perhaps explains why the disembedding tendencies of simulation in particle physics tend to be controlled

[11] In this case the transfer needs to prove itself in yet another laboratory, which makes the digital lab a lab in (and a part of) another lab (cf. Knorr Cetina 1999).

and kept in closely observed bounds. Whether this is the case also in problem areas that are inaccessible by way of material experimentation is a matter for empirical investigation, as is the question of how the scientific validity of the results is assured in these cases.[12] A twist to these variations is cases where simulation itself becomes part of a strategy to ensure the transferability of experimental results to the context of application.

These observations illustrate that the scientific practice of modeling provides, first, an interesting field for investigating the transfer and transferability of scientific results beyond the narrow confines of its production context – the (digital) laboratory. Secondly, the study of modeling practice generates important questions regarding the power of laboratories, which can be asked of other laboratory practices for purposes of comparison. Thirdly, it allows us to review and refine the difference approach in constructivist science studies.

4 Extending the Laboratory

The difference approach raises a second set of questions, concerning possible extensions of the laboratory concept, the laboratory's variable and shifting position in the sciences, and the different laboratory forms that have developed in science and, potentially, in other societal realms.

The first question relates to the concept of the laboratory and the processes of object reconfiguration through which it is defined. As noted above, the notion of object reconfiguration can be productively extended to also include the alternate object worlds that are produced by computer simulation. Computer simulation allows for the constitution of digital laboratories in which the phenomena under investigation are amenable to extremely flexible reconfiguration and manipulation. In this case, scientists are required to negotiate the different ontological orders and epistemic features between the simulated and the material object worlds. In current scientific practice, digital laboratories assume different positions and functions in the knowledge production process. While simulation may serve in certain cases as a substitute for "wet lab" experimentation, it is exploited in juxtaposition to wet lab experimentation in many other cases. In particle physics, for example, simulation parallels, precedes, frames and complements other experiment-related activities, with each experimental phase drawing on simulation in specific ways (cf. Knorr Cetina 1999, Merz 2006). These observations hint at the possibility that different laboratory orders (digital lab, wet-lab, etc.) may become intertwined in the course of a scientific project.

A second issue concerns the relation between laboratory practice and other modes of knowledge production in science – and what implications this has for the laboratory concept and the constitution of its boundaries. In accord with the logic of the difference approach, the early laboratory studies singled out the knowledge-production mechanisms of typical laboratory sciences as their topic of investigation. This raises the question of whether other epistemic forms deserve more consideration than they previously have been accorded. For example, recent work in the sociology and history of science has devoted increasing attention to the field sciences and their knowledge production regimes (cf. e.g. Kuklick/Kohler 1996). Modern field sciences combine field measurements with laboratory work, while "labscapes" (Kohler 2002) either draw nature into the lab or bring the lab to the field. A traditional field science like astronomy can be conceived in its

[12] For the case of environmental sciences, see e.g. Oreskes (1998), Oreskes et al. (1994), Shackley/Wynne (1996), Wynne (1996).

present form as an image-producing laboratory science that transforms its phenomena in a computer-based laboratory and then processes them in the form of representations (cf. Knorr Cetina 1995). The clinical setting in modern biomedicine also constitutes a kind of field. An extended body of literature has begun to address the processes of mutual constitution between the laboratory and clinical practice (cf. Casper/Berg 1995). The lab-field border is managed and negotiated differently in different sciences. These observations suggest that a more systematic investigation of the laboratory's position and boundary practices in the context of other epistemic strategies and knowledge production regimes should be pursued. In line with the recent interest in the diversity of scientific cultures and the particulars of fact construction, a challenge for future investigations lies in the direct comparison of laboratory cultures (cf. Galison 1996).

A third complex of issues revolves around the question of whether laboratories exist outside the institutions of science and research, specifically, at the science-society boundary. Under the header "society as laboratory" Krohn and Weyer (1989) have brought to our attention new ways that science is included in society, defined as a coincidence of research and implementation. In this case, the implementation of knowledge is the condition under which knowledge becomes validated and through which new research questions are generated (in fields such as genetic manipulation and human experiments in space). This gives rise to a new experimental situation, characterized by the impossibility to set or influence its boundary conditions, and by the multiplicity of actors who perform according to different cognitive and evaluative categories. While the implied laboratory notion is distinct from the one underlying the difference approach, one wonders whether "real-world experiments" (Gross et al. 2003) in all instances are free of any form of subject-object reconfiguration that privileges the knowledge-seeking parties, be they scientists or others or both at the same time. This question is associated with both an empirical research program and a conceptual agenda. First, it is motivated by a desire to explore how the laboratory is an arrangement that, in its dynamic of subject-object reconfiguration, belongs specifically (and perhaps even exclusively) to the realm of science in the present time. Secondly, it is motivated by a desire to investigate whether the laboratory concept of the difference approach can be fruitfully applied to knowledge production regimes at other societal sites and, should this be the case, to explore what one might learn about such regimes. The research agenda that underlies the present text is thus not to be misunderstood as a reification of the difference approach: to assert that the difference approach still provides a challenging research agenda is not synonymous with accepting the claim that science is fundamentally different from other forms of societal practice.

5 Conclusions

The science-as-practice approach in the social studies of science has given rise to alternative interpretations of constructivism, two of which are revisited in this text. Both interpretations focus on the position of the laboratory in science. The first (the analogy approach) maintains that there are no epistemic particularities in scientific knowledge production, drawing on observations of the locally situated nature of scientific work. The second (the difference approach) accounts for the success of science by linking it to the specific reconfiguration processes that symbolize the scientific laboratory. This paper has argued for the continuing topicality of the difference approach and its capacity to generate challenging research questions. However, the fact that the difference approach is privileged in this text is not to be interpreted as an assertion of its superiority over the analogy approach, which has provided us with a rich and detailed account of the manufactured and negotiated character of fact making. The power and fruitfulness of the difference approach lies in its attention to the specific subject-object relations and the reconfiguration processes that make up the laboratory qua enhanced environment. Although this article has focused on the differences, the analogy approach and the difference approach represent two sides of one coin. They are complementary and not in contradiction and, due to their common roots, they share defining tenets (the situated nature of knowledge production, analyzing science as practice, etc.).

Earlier laboratory studies privileged the investigation of typical laboratory sciences in order to identify the mechanisms that would account for the success of science. A promising next phase of research, it has been argued in this text, would be to extend both the topics and the fields of investigation within the difference approach. The section entitled "Transcending the Laboratory" addressed the issue of how laboratory-produced knowledge can be exported successfully to application contexts beyond the narrow confines of the laboratory. This raises questions regarding the awareness of scientists of the limitations and uncertainties of laboratorization processes and regarding their strategies and priorities for pondering "doability" (Fujimura 1987) either in the laboratory or in practice – or their neglect to do so. It also raises questions regarding the boundaries of the laboratory and the division of labor spanning these boundaries, between those responsible for knowledge production in the lab and those responsible for managing the "export" of knowledge and its application. In addition, new modes of object configuration have been developed, such as computer simulation, that are of increasing importance and which define new types of laboratories that perform according to new rules. The section entitled "Extending the Laboratory" addressed related questions by inquiring into the hybrid forms of knowledge production, in which one or different laboratory regimes complement, interfere with, or parallel other knowledge production regimes, both within science and across the institutional borders of science and research. The assumption of considerable variability in configuration forms, accompanying social forms, institutional arrangements, temporal structures, spatial organizations, and so forth calls for an empirical program from a comparative perspective.

Can laboratory-like features of knowledge production be identified at the boundary of science and other societal realms, or even in areas of society altogether removed from science? A constructivist perspective informed by the difference approach has the potential to further our understanding of the so-called "knowledge society." From a constructivist perspective, knowledge is not a mere resource; rather, the focus of interest is epistemic strategies of knowledge production and validation. With an eye to furthering our understanding of the knowledge society it is recommended that those epistemic forms and social arrangements that transcend the scientific laboratory be investigated more thoroughly than they have been to date, which would allow us to conceptualize the knowledge society as heterogeneously situated epistemic practices. A debate between constructivist science studies scholars and proponents of the knowledge society model has not (yet) taken place. This article is an attempt to identify issues and concepts that may serve as a point of intersection and contact between the two fields.

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Three Forms of Interpretative Flexibility

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Abstract

Interpretative flexibility is a central concept of social constructivism in science and technology studies. We think this concept, as it exists, can and should be elaborated. In this paper, we argue that interpretative flexibility can be traced back to three different forms of infinite regress: the regress of truth, the regress of usefulness, and the regress of relevance. Resulting from this analysis, we observe three different forms of interpretative flexibility. We will show that in controversies or debates concerning the meaning of certain scientific facts, technological artefacts or research approaches, concurrently or consecutively more than one of these different forms of interpretative flexibility may play a part. With this reconceptualisation of interpretative flexibility, we hope to contribute to a more elaborate understanding of the dynamics of the social construction of scientific facts and technological artefacts.

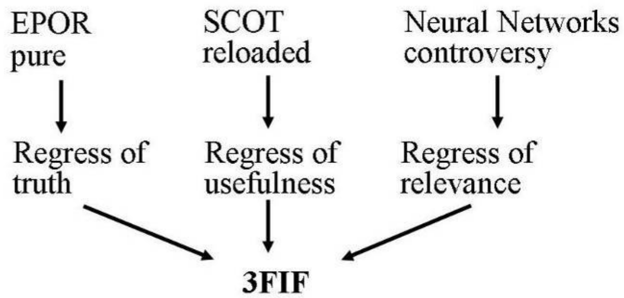
1 Introduction

Interpretative flexibility is a central concept of social constructivism in science and technology studies. We think this concept, as it exists, can and should be elaborated. The basic assumption of social constructivism is: The observed phenomenon "X need not have existed, or need not be at all as it is. X, or X as it is at present, is not determined by the nature of things; it is not inevitable" (Hacking 1999: 6). In science and technology studies, this basic assumption is applied to scientific facts and technological artefacts. However, scientists and engineers do refer in a certain way to the "nature of things." They do so by deducing scientific facts from empirical observations or by developing technological artefacts for given purposes. Hence, social constructivist approaches in the study of science and technology rely on an additional assumption: also the empirical observations and the purposes of technology scientists and engineers refer to, allow different interpretations to a certain degree. This is termed "interpretative flexibility." This is not to say that every empirical observation or assumed technological purpose will indeed be interpreted differently. More often than not, as a result of previous processes of social construction, one of the possible interpretations has become widely accepted and will not be questioned by anybody. But where no such consensus has evolved and interpretative flexibility still exists, arguments become circular and lead into an infinite regress. In these cases, the scientific facts are questioned because the underlying empirical observations are subject to interpretative flexibility and the empirical observations are questioned because the related scientific facts are subject to interpretative flexibility. The same holds for the relationship between technological artefacts and the purposes they shall serve.

Our reconceptualisation of interpretative flexibility is based on the observation that this infinite regress is not always of the same kind. To the contrary, we see sufficiently clear-cut differences between three kinds of infinite regress that can be derived from existing social constructivist research in science and technology. We call them the regress of truth, the regress of usefulness and the regress of relevance. Consequently, interpretative flexibility is not always of the same kind, too. In relation to the three different regresses, we will introduce a distinction between three forms of interpretative flexibility (3FiF). Regarding the regress of truth and the interpretative flexibility concerning the truth of scientific findings, we will draw upon the *Empirical Programme of Relativism* (EPOR) by Harry Collins. Trevor Pinch and Wiebe Bijker have applied the notion of interpretative flexibility to the development of technological artefacts. However, in the framework of their *Social Construction of Technology* (SCOT) the concept of interpretative flexibility remains underspecified. In conceiving the underlying regress as a regress of usefulness and interpretative flexibility as concerning the usefulness of technological artefacts, we hope to overcome some of the major problems of this approach. The notion of a regress of relevance and of interpretative flexibility concerning the relevance of evaluation criteria to assess the future potential of scientific or technological approaches has been developed in an analysis of the Neuronal Networks controversy, which one of us has worked on (cf. Meyer 2004).

We will show that the underlying regress affects how interpretative flexibility occurs, how different interpretations are negotiated, and how (if at all) a certain interpretation becomes widely accepted. In each of the three cases, interpretative flexibility constitutes a different situation: either a situation of contested truth or a situation of contested usefulness or a situation of contested relevance. Thus, with our reconceptualisation of interpretative flexibility we hope to contribute to a better understanding of the different meaning of interpretative flexibility within different situations of social construction of scientific facts and technological artefacts.

Figure 1: Three Forms of Interpretative Flexibility



In providing a differentiated view on interpretative flexibility, we do not only want to point out differences *between* the social construction of scientific facts and of technological artefacts. Additionally, we assume that this view is useful for analyzing different meanings of interpretative flexibility *within* processes of establishing scientific facts or technological artefacts. This is to say that interpretative flexibility of scientific findings is not only a question of contested truth and interpretative flexibility of technological artefacts is not only a question of contested usefulness. Both can articulate questions of contested relevance. Furthermore, interpretative flexibility of usefulness can influence the social construction of scientific facts and, inversely, controversies about truth can be part of the social construction of technological artefacts. This can already be shown in the "classical" case studies of the EPOR and of the SCOT. We will use the case studies of the gravitational waves controversy and of the development of the bicycle to illustrate our 3FIF concept.

2 The Regress of Truth

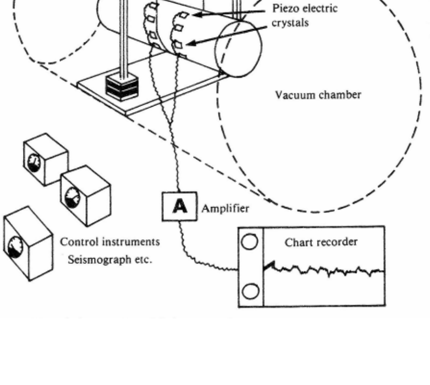
The basic assumption of the *Empirical Programme of Relativism* (EPOR) is that the natural world plays only a small or no role in the construction of scientific knowledge (cf. Collins 1981: 3). The facts upon which scientific statements are based do not possess an inherent meaning. They have to be interpreted to become meaningful. Thus, they can in principal (but not necessarily in the practice of research), be interpreted in different ways. Since Collins' main examples come from the realm of the natural sciences, especially physics, the subjects of possible interpretative flexibility are experiments and the resulting data. However, in most cases, the potential interpretative flexibility of experiments and their results does not occur in research practice, because the established scientific state of the art allows for only one of these interpretations. In such a case, their meaning is undisputable.

Experiments pupils carry out in school provide a simple example: the pupils' task is to produce the proper result but the interpretation is not in question. However, in some cases experimental results cannot be explained with recourse to undisputable knowledge. This is where interpretative flexibility becomes acute. When the results of an experiment and the existing scientific knowledge do not match, this can be explained in two different ways: either the experiment was implemented properly but the actual state of knowledge fails to explain its results; or the experimental design was faulty, thereby producing false results which do not question the actual scientific knowledge. In such a situation it is impossible to decide which one is the right explanation. An experiment is performed competently when it produces proper results. The aforementioned experiments in school illustrate this point: there, the proper results are known because they fit into uncontested scientific knowledge. Thus the teacher has no problem in deciding whether or not a pupil has performed the experiment competently. What is important is that pupils demonstrate their ability to conduct experiments properly by getting the right results. However, when the existing scientific knowledge does not help to decide whether an experimental result is reliable, the attempt to prove scientific claims experimentally leads into an infinite regress: Whether the experiment is implemented in a competent way or not can only be determined by the accuracy of the results. Yet, the decision about the results depends on the experiment and whether it is competently conducted. That is what Collins calls the "experimenter's regress" (Collins 1985: 79).

Scientific results are judged by the criterion of scientific truth. So Collins' experimenter's regress can be described as a *regress of truth*. From the scientist's point of view truth is often seen to mean that a scientific statement corresponds with the reality it describes or from with it draws generalizations. In contrast, from the point of view of the social scientist as observer of science true scientific observations and generalizations are observations and generalizations that are commonly accepted to be true within the respective scientific field – for whatever reason (cf. Bloor 1976). However, the idea of scientific truth implies that contradicting scientific statements cannot be true at the same time. Thus, the occurrence of contradicting scientific claims raises the need to decide between them. For this reason the interpretative flexibility of experiments and experimental results leads to scientific controversies. Solving a scientific controversy means to exclude, over time, all but one of the different interpretations of the initial situation of interpretative flexibility. Since it does not work to refer to experiments as the normal way of scientific decision making in situations of interpretative flexibility and since already established scientific knowledge does not help either, social negotiation is the only way to come to a solution. Collins calls this the process of closure of a scientific controversy. The central actors of this closure processes are the scientists directly involved in the particular research area. Collins calls them the "core set" of the controversy (cf. Collins 1983: 95).

Collins' most elaborate example of a scientific controversy and the underlying interpretative flexibility is the search for gravitational waves. Gravitational wave is the name for a physical phenomenon which could be described as a marginal, short-term shift in the structure of space. This shift is caused by the movement of big masses in the universe and is a theoretical result from Albert Einstein's general theory of relativity. An experimental proof of the existence of gravitational waves would therefore be seen as empirical evidence for Einstein's theory. In 1969 Joseph Weber, Professor at the University of Maryland, claimed that he had detected gravitational waves with a detector he had invented himself.

Figure 2: Diagram of Weber's Detector (cf. Collins 2004: 53)



However, there was a significant difference between his interpretation of his experimental results and what, until then, had been inferred theoretically. The amount of gravitational waves he claimed to have detected was too large to fit into the established knowledge about the structure of the universe. In terms of established knowledge this amount of gravitational waves implies a dynamic that would incinerate the universe in a relatively short period of time (cf. Collins/Pinch 1993).

In the following years, groups from different research institutes tried to replicate Weber's experiments. But nobody managed to detect gravitational waves. Weber's critics saw this as proof for errors in Weber's experiment. They concluded that his data was wrong. Weber, on the other side, saw his colleagues' failure to detect gravitational waves as a proof that they did not manage to build a working detector with the same sensitivity as his own.

Several research groups published their results, but their articles simply pointed out that they could not detect anything. They did not conclude that Weber must have been wrong; at least they did not assert this explicitly. As more and more groups failed to detect waves, the climate gradually changed and the scepticism regarding Weber' findings increased. Collins argues that the crucial change in the scientific community's opinion was caused by an article, which lacked new scientific findings. This article was special not because of what it said, but how it was said. The rhetoric was very different to all the articles previously published on this subject. The author directly attacked Weber and his research, claiming Weber to be absolutely wrong. Later, an assistant to Garwin, the author of this article, explained, what had happened: "At that point it was not doing physics any longer. [..] We just wanted to see if it was possible to stop it immediately without having it drag on for twenty years" (Collins/Pinch 1993: 134).

Collins regards this as the central element in the social closure of the interpretative flexibility in Weber's research. At last, in 1975, the scientific community, the core set, agreed that Weber was wrong and his experiments had been incorrect. The controversy had been closed.

3 The Regress of Usefulness

Assuming basic similarities between the social construction of scientific facts and the social construction of technological artefacts, Trevor Pinch and Wiebe Bijker have applied the main concepts of the EPOR to the social study of technology (cf. Pinch/Bijker 1984; Pinch/Bijker 1987). In their programme of *Social Construction of Technology* (SCOT), interpretative flexibility denotes that fundamentally different meanings can be attached to the same technological artefact (cf. Pinch 1996: 24). Persons, who share the same interpretation of a certain technological artefact and thereby influence the development of this artefact, are referred to by Pinch and Bijker as a *relevant social group*. In SCOT, these relevant social groups taken together are equivalent to the scientists within the core set of a scientific controversy in EPOR. They build the constellation of actors within which the social negotiation and reduction of interpretative flexibility takes place.

Additionally, SCOT adopts from EPOR the assumption that interpretative flexibility does not persist. "What one observes is that closure and stabilisation occur in such a way that some artefacts appear to have fewer problems and become increasingly the dominant form of the technology. This, it should be noted, may not lead to all rivals vanishing, and often two very different technologies may exist side by side (for example, jet planes and propeller planes)." (Pinch 1996: 25) In

Pinch and Bijker's opinion, the processes of closure have the same structure as in scientific controversies: The proponents of the different interpretations seek to establish their own to be the most convincing view. Some attempts to influence other relevant socials group's interpretations are more successful than others. In this process a certain interpretation becomes accepted by more and more relevant social groups and eventually leads to a certain technological artefact becoming seen as the appropriate solution to a certain problem by most of them. What gravitational waves are to Collins, bicycles are to Pinch and Bijker. They use the history of bicycle development to illustrate their concept: "The high-wheeler had the meaning of the 'macho machine' for young men of means and nerve, but for older people and women it had the radically different meaning of the 'unsafe machine'. Such interpretative flexibility may apply not only to a compound artefact but also to some components of it. For example, when the air-tyre was first introduced, it was for some groups an object of derision, aesthetically unappealing, and a source of endless trouble (punctures). On the other hand, for Dunlop it was the perfect solution to the problem posed by the vibrations of the bicycle." (Pinch 1996: 24-25) In this case, the closure of the debate results from redefining the problem: The high-wheeler literally lost the race, when the air tyres, which were originally developed to make bikes safer, proved to be a crucial factor to high speed in races. Even users of the macho machine preferred safe riding and winning over risky riding and losing.

We feel that the SCOT programme is less convincing than it could be. Its central concepts – interpretative flexibility, relevant social groups and closure – are defined less precisely than the corresponding concepts of the EPOR because they do not reflect phenomena that are specific to the process of technology development. The observation that certain objects or artefacts may have different meanings for different people and that this may lead to disputes about who is right and who is wrong holds for any object or artefact without an already established meaning and is in no way specific to technological artefacts. Defining interpretative flexibility by pointing at the different meanings a technological artefact from the point of view different social groups may have is nothing more than to define interpretative flexibility by referring to interpretative flexibility.

We need a narrower and more specific concept of interpretative flexibility of technological artefacts, one which takes into consideration the particular features of technology. In scientific controversies, the regress of truth is accountable for the specific form of interpretative flexibility of scientific claims. Thus, we have to look for a regress, which in a similar way, is accountable for a specific form of interpretative flexibility of technological artefacts. In our opinion, such a regress indeed exists. We call it the *regress of usefulness*. We reach to this conclusion by referring to the basic characteristic that distinguishes technological artefacts from scientific findings on the one side, and from other cultural artefacts on the other side: The specific technological quality of technological artefacts is that they are meant to produce desired effects sufficiently, reliably, and in a repeatable way, effects which would not be possible or would require more effort without the artefacts (cf. Schulz-Schaeffer 1999: 410). From this, it follows that the criterion for judging technological artefacts is their usefulness for a certain purpose, as truth is the criterion for scientific facts.

Consequently, interpretative flexibility of technological artefacts as far as their specific technological quality is concerned is interpretative flexibility with regard to usefulness. It occurs when there are different possible answers to the question whether a technological artefact with its particular functional features will be useful and how, for whom and, in which context this will or will not be the case. Thus, the reason for interpretative flexibility of technological artefacts to occur is that depending on the respective purposes of different groups of users and depending on the diverse requirements of different contexts of use these questions of usefulness can be answered differently. Interpretative flexibility of this kind also has its roots in an infinite regress: Whether a certain technological artefact possesses useful functional features will become clear only after it has found its users and has been implemented successfully in certain contexts of use. Yet, the decisions regarding the design of the technological artefact and its particular functional features have to be made before it can be used. This is what we the regress of usefulness.

Conceiving interpretative flexibility of technological artefacts as related to usefulness allows us see a similarity and a difference to the interpretive flexibility of scientific facts. As well as in scientific research there are cases of technology development where interpretative flexibility does not play a major part but is limited right from the start. In many cases it is already well known who the users of the artefact in development will be, how and for which purposes they will use it and what the contexts of use will be. Especially, this is the case when the new technological artefact is supposed to become the successor of an already exiting artefact or when the development process aims at enhancing an existing artefact. This is similar to the normal way of scientific research where the already accepted and (for the time being) undisputed scientific knowledge limits the range within which the data can be interpreted differently.

However, when interpretative flexibility becomes relevant, a major difference between scientific research and technology development has to be taken into account, a difference the SCOT lacks to notice: Interpretative flexibility of experiments and experimental results inevitably causes scientific controversies as long as the proponents of the different interpretations agree that contradicting scientific claims cannot be true at the same time. In contrast, for technological artefacts such a basic necessity to discuss divergent interpretations controversially does not exist. In principle, there is no reason why users should agree on what purposes a technological artefact shall serve and no reason why alternative technological solutions serving the same purpose should not be developed. Thus, while interpretative flexibility of truth necessarily evokes controversies, interpretative flexibility of usefulness does not. And while scientific controversies are aimed at closing the debate sooner or later, closure is not a necessary feature of debates concerning different meanings of technological artefacts. Sometimes, however, technological controversies occur that seem to be similar to their scientific counterparts. As we will see later (part 5.1, (3)), this is because the underlying interpretative flexibility, then, is related to truth and not to usefulness.

4 The Regress of Relevance

A third form of interpretative flexibility appears in debates about different future directions of scientific research or of technological development. Interpretative flexibility here means that because no undisputed point of view exists, it is possible to take up different positions regarding the question of which research approach or project of technology development is promising and which one will lead to a dead end. Under the condition of limited resources, i.e. under the condition that not each of the possible approaches of research or development can be adopted, questions of this kind lead into controversies which need to be closed. However, under this condition the attempt to answer these questions leads to an infinite regress as well. As a one best way solution, the most promising alternative research or development approaches should get the most resources. But which criterion allows one to judge, which of the research approaches or development projects competing for funding will deliver fruitful results and thus deeming them promising than others?

Since these future events are unknown the interested parties will try to predict them based on contemporary available research and testing results. Sometimes there is little doubt among the actors involved in which direction of future progress the existing state of the art points. But sometimes the contemporary scientific or technological knowledge turns out to be ambiguous in this respect. This is the case when the scientific or technological knowledge available relies on scientific methods or technological tests, which had been developed to specifically evaluate progress in one of the approaches under investigation. Then, it is most likely that the respective methods or tests will show better results for the approach it was originally designed for. –Lines of technological development are usually connected to corresponding modes of testing. And each mode of testing focuses on criteria, which are essential for exactly the line of development, it is supposed to evaluate. As a consequence, certain technologies and the corresponding tests are mutually reinforcing (Constant 1980: 22). The same mechanism can be shown for different scientific approaches and the corresponding experimental methods.

So, for deciding, which of the different approaches is more promising, proponents of a certain approach use tests and the corresponding evaluation criteria, which are consistent with their favoured approach. And of course, each side – by using their own evaluation criteria – will find prove, that the approach, they are advocating is the most promising one. At the same time, each side will question the relevance of the evaluation criteria of the competing approaches for predicting future success. The only possibility way to find out, which of the different criteria are the relevant criteria to predict future success, would be to compare the results of each endeavour. But the reason for identifying the more promising approach is due to resource scarcity, in which only one or a few of them can be funded. Consequently, the attempt to identify promising approaches of future work in science and technology also leads into an infinite regress, which we call the *regress of relevance*. Here, the relevance of available test or research results with respect to the question, whether or not a scientific or technological approach is promising, is subject to interpretative flexibility.

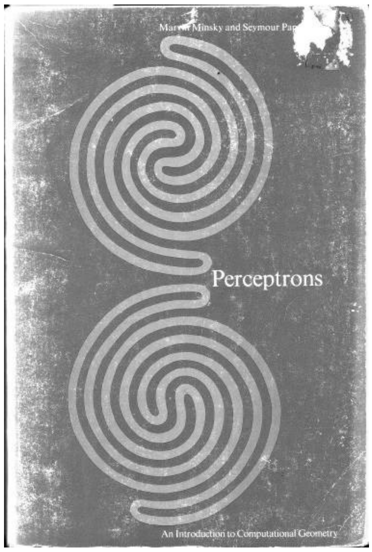
The research on Neural Networks in the 1960's provides an example for a controversy based on interpretative flexibility of relevance. Neural Networks were seen as a way to create intelligent machines by imitating the human brain activities. Researchers who followed this approach tried to build computational structures similar to the basic physiological structure of the brain. In contrast, Symbolic Artificial Intelligence (AI), being the main competing approach at the time, tried to identify the rules humans use when they are thinking. They expected to be able to create intelligent machines by programming knowledge, rules and reasoning procedures. Scientists of the Symbolic AI approach claimed that it would never be possible to create intelligent machines based on Neural Networks. Marvin Minsky and Seymour Papert, the most prominent advocates of Symbolic AI, presented mathematical proofs to support this claim. No scientific controversy took place. The proponents of the Neural Network approach did not contest the truth of Minsky and Papert's proofs. But they questioned the relevance of these results for the question, which of the two different approaches is more promising (cf. Meyer 2004: 75-79). The potential of a future direction of scientific research or technological development is contested by challenging that the scientific facts or technological achievements the proponents or opponents use to support their view are relevant concerning this matter. Contesting the potential of a future path of scientific research or technological development, thus, does not necessarily mean to challenge the truth or the usefulness of the scientific facts or technological achievements used as arguments.

In order to show the structure of a controversy concerning relevance, we shall look more closely at one set of arguments both sides used in the discussion about Neural Networks. In 1969, Minsky and Papert published a book, entitled "Perceptrons", where they laid down their arguments against the Neural Networks approach (cf. Minsky/Papert 1969). The cover of the book showed two figures, which look nearly identical. One of them consists of one single line and the other one consists of two lines. In their book, the authors presented a mathematical proof saying that Neural Networks would never be able to find out, which one is which.

In addition, they offered a very simple algorithm from the research on Symbolic AI to solve this problem.

Rosenblatt, one of the most prominent proponents of the Neural Networks approach agreed with their interpretation. But he also pointed out, that in his perspective, these results where completely irrelevant for analysing the potential of future research on Neural Networks. His argument was very simple: Neural Networks are supposed to imitate human thinking and recognition. Even with these very simple figures, humans are hardly able to distinguish, which of the two is connected and which is not. If humans are not able to do this, machines which are supposed to imitate humans do not have to be able to do it either (cf. Meyer 2004: 77-78). So the evaluation criteria for the two different approaches varied, depending on which of the two approaches was preferred. Proponents of Neural Networks used evaluation criteria which where consistent with their sub symbolic concept of Artificial Intelligence, proponents of Symbolic AI used criteria, which complied with there concept of rule-based Artificial Intelligence.

Figure 3: Cover of "Perceptrons" (cf. Minsky/Papert 1969)



But in spite of Rosenblatt's criticism on their evaluation criteria, Minsky and Papert successfully established their view concerning the relevance of these facts until the end of the 1960's. They managed to convince the main funding organisations that supporting the Symbolic AI approach would be much more promising than funding research on Neural Networks. They skilfully used their personal contacts within these funding organisations. They also focused their critique of the Neural Networks approach on problems which could be easily solved by means of Symbolic AI. The problem of connectedness was one of them. In the end of the 1950's a few hundred groups did research on Neural Networks. Ten years later, this number was reduced to just a few projects. These projects had to 'hide' in other research areas, because at this time it was not possible to get direct funding of research on Neural Networks. The controversy was closed.

5 The Empirical Relevance of Distinguishing between Three Forms of Interpretative Flexibility

The case of Neural Networks reveals that controversies in science can be based on interpretative flexibility of relevance instead of interpretative flexibility of truth. We will show in the following sections that interpretative flexibility of usefulness can also play an important part in interpreting science. In addition to that, we will also show that all three forms of interpretive flexibility can account for different meanings of technological artefacts. Interpretation processes that started as controversies about the truth of facts can be ended as decisions concerning questions of usefulness or relevance, and vice versa. The proposed distinction between three forms of interpretative flexibility allows for a more detailed analysis of these mixtures and transformations. As we hope to have shown in the previous sections, with each of the three forms, different ways to handle interpretative flexibility are connected. Therefore, it is important to distinguish between them for analysing the social construction of scientific facts and technological artefacts, especially in cases where more than one form of interpretative flexibility occurs.

In the following section, we will elaborate on the thesis that interpretation of scientific facts or technological artefacts may contain different forms of interpretative flexibility at the same time or one after another. First we will present three general observations. After that we will use the classical examples of SCOT and EPOR to show, how our concept allows a more detailed analysis of the processes, which led to the closure of these controversies.

5.1 Three General Observations

(1) The concept of paradigm shift, i.e. the replacement of an established paradigm by a new, but not yet very elaborate one, was presented by Thomas S. Kuhn (1962) for the scientific realm and adapted by Giovanni Dosi (1982) for technology. On a very high level of abstraction, these concepts describe a shift from reference to truth or usefulness to reference to relevance. If disputes in science between an established and a new and still evolving paradigm would be controversies related to truth and if the corresponding disputes in technology would refer to usefulness, the established paradigm would always win. If a new paradigm prevails against an old one, it is because of the future scientific or technological innovations it is expected to bring about. A new paradigm cannot prove the truth of its scientific assumptions or the usefulness of its envisioned technological solutions as good as an established paradigm can. This is something that will or will not be demonstrated by “normal science” and “normal technology development” within the frame of reference of this paradigm, work that in contrast to the competing established paradigm still lies ahead. A new paradigm is attractive because it seems to be more promising for solving scientific or technological problems in the future.

(2) Controversies concerning truth can be transformed into questions of usefulness. This can be observed when closure in a scientific controversy is not to be expected in the near future or when a controversy is regarded to be unsolvable. The question whether it is possible to perceive reality in itself or whether every perception of reality depends on the observer’s point of view is an example of a scientific problem many scientists assume to be unsolvable. Thus, in giving reasons for assuming a more epistemologically realistic or constructivist position, scientists tend to shift from truth-related arguments to arguments of usefulness.

This is how Hartmut Esser and Niklas Luhmann support their different point of view. Both agree that the basic epistemological problem is unsolvable (cf. Esser 1993: 53; Luhmann 1990: 531). However, both explicitly argue their position to be the more useful one. Esser's reason for a more realistic position is that epistemological realism is the simpler hypothesis (cf. Esser 1993: 54, 56). According to Luhmann scientific theories, on the contrary, should allow for a high resolution of the observed phenomena (cf. Luhmann 1990: 510). Accordingly, he sees the constructivist position as more useful since it provides a reflexive theory adequate for the complexity of the modern society (cf. Luhmann 1990: 531).

Thus, in transforming scientific controversies into different interpretations concerning the usefulness of scientific positions it becomes a question of purpose and context which position is more adequate.

(3) On the other side, differences in the interpretation of the usefulness of technological artefacts can be transformed into scientific controversies. This can be achieved by transforming the subject of interpretative flexibility – for example the question whether a particular functional feature of a technological artefact is useful within a certain context of use – into a subject of empirical scientific research. Donald MacKenzie (1989: 411) calls this "producing facts about artifacts". The process, which does the magic, is called testing. Testing technology means checking hypotheses about the usefulness of certain properties of an artifact in a scientifically controlled, empirical way (Constant 1980: 21). It transforms differences in the interpretation of usefulness into technological controversies. Technological controversies are controversies about the truth (!) of hypotheses about usefulness. Or to say it in MacKenzie's word again: "all the issues that recent sociology of science has raised about *experiment* in science can be raised about *testing* in technology" (MacKenzie 1989: 411). MacKenzie puts emphasis on the fact, that there is a tester's regress which is analogous to the Collins' experimenter's regress (cf. MacKenzie 1989: 424). He is right because the tester's regress as well as the experimenter's regress is a regress of truth.

5.2 Interpretative Flexibility of Relevance and the Controversy of Gravitational Waves

In the 1980's, the gravitational waves controversy was reopened, turning into a controversy related to relevance. In 1982, about seven years after the closure of the controversy described above, Weber published new results. He claimed to have found the explanation as to why his measuring apparatus had been able to detect gravitational waves. Following his argument, he had not detected the huge amount of gravitational waves, which he thought he had and which did not correspond with the scientific consensus. Instead, his apparatus was vastly more sensitive than previously assumed.

Figure 4: A Weber Bar (cf. Collins 2004)

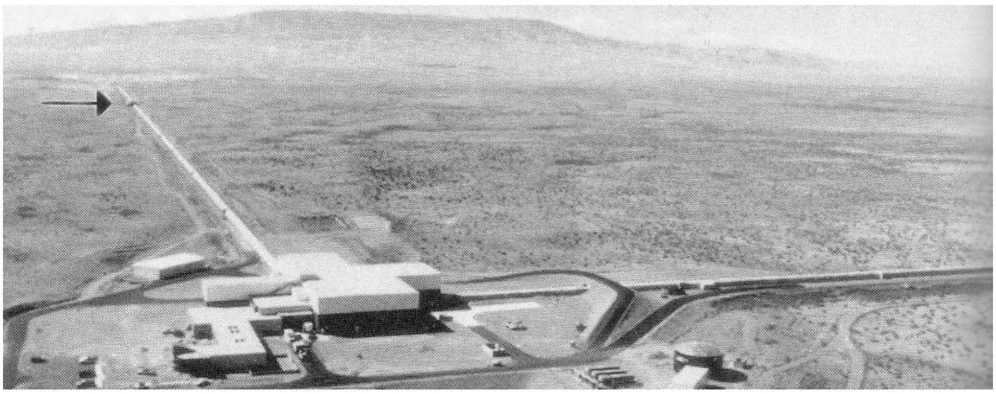
Based on his new theory, Weber calculated the sensitivity of his sensor to be one million to one billion times higher than he had thought. As a consequence, the detected gravitational waves intensity would be a million to a billion times smaller than calculated. This would mean no conflict exists between the data and the established theories regarding the structure of the universe. Weber explained his new estimation of the sensitivity of his with a specific characteristic of the metal bars he uses as detectors He argued that in order to properly describe how the metal bars inner structure responds to gravitational waves, quantum theory must be applied. The quantum-theoretical effects, which Weber assumed to be active in his bars, caused the higher sensitivity to gravitational waves.

Weber published this line of argument at first in 1982 in the journal *Physical Review*. This paper was ignored by the scientific community. After the closure to the controversy in the mid-1970's, this was the usual reaction to Weber's publications. The controversy was closed, further discussion was not necessary. The scientific community's exclusion mechanisms worked well (cf. Collins 2004: 364-366). However, after he had published in 1989 another article on the same topic his line of argument became massively criticized by established researchers in the field. Although this article was published in a smaller journal (*Il Nuovo Cimento*) and contained no new arguments, the scientists reacted to this article. What had happened?

Research Institutes at MIT had developed a new technology for detecting gravitational waves. Based on lasers, this technology was expected to be much more sensitive than the metal bars.

The newly contrived detector consists of two laser measurement sections, which where positioned orthogonally to each other. With the help of the lasers, the exact length of the section is measured at every given moment. If a gravitational wave hits this detector, the lengths of the detector's two "arms" change. This change is different in each of the detector's "arms," depending on the angle in which the gravitational waves hit the detector. This change in the relation of the length can be measured and serves as a proof of gravitational waves. Around the world, a few of these detectors where planned. The biggest two, the Laser Interferometer Gravitational Observatories (LIGO), were planned for construction in the USA. For each of the arms, the laser measurement section measures a length of 4 km.

Figure 5: LIGO (cf. Collins 2004)



The costs for building these facilities were an estimated 300 million dollars. The US government was expected to fund this project. At this time, when the negotiations over LIGO funding were taking place, Weber renewed his claims about being able to detect gravitational waves using a much cheaper and more sensitive apparatus than LIGO. In addition to the article from 1989, Weber wrote numerous letters to the decision makers of the funding of LIGO. In these letters he accused the LIGO-Project to be an enormous waste of tax money compared to his own measuring apparatus. (cf. Collins 2004: 360-361). By doing this, he tried to involve actors into the debate which were not part of the core set of the scientific controversy regarding gravity waves. Thus, Weber transformed the controversy about scientific truth which he had already lost into a controversy about the relevance of alternative research directions of detecting gravity waves. This explains the harsh reaction to his paper from 1989. His new arguments and the opponents' responses did not revive the scientific controversy. This controversy remained closed. Weber's findings were not treated as worthy to be discussed scientifically. Weber argued, that, based on his evaluation criteria, his approach was more suitable to measure gravitational waves – because of the quantum-effects within his bars – and much cheaper than laser-based experiments. His opponents did not agree on his criteria. For them, his argument based on quantum theory was pure nonsense. From their point of view Weber's bars where not able to measure gravitational waves at all and – as a consequence – his cost-argument was insignificant.

So, the goal of Weber's opponents was to show the decision makers of the funding organisations that Weber and his work should not be seen as belonging to the core of research on gravity waves, that is view was not shared by anybody within the scientific community and, thus, that his objections concerning the relevance of their new research approach should not be taken seriously.

5.3 Interpretative Flexibility of Truth and the Development of the Bicycle

The reconstruction of the bicycle development, as Pinch and Bijker provide it, includes an episode where interpretative flexibility of usefulness becomes transformed into truth-related hypotheses about usefulness. According to Pinch and Bijker, the safety bicycle's victory over the high-wheeler was a victory captured in bicycles races. The success of the safety bicycles in these races were seen as a proof that their air tyres have a better performance with respect to the purpose of riding as fast as possible than the solid tyres of the high-wheeler. Thus, these bicycle races provided a situation of testing the functional feature "air tyre" against alternative solutions to the speed problem. Admittedly, it is not a very scientific sort of testing, but it is testing. According to the reconstruction by Pinch and Bijker, these races resulted in the safety bicycle being superior became widely accepted as true. Scientific controversies occur because of interpretative flexibility of experimental results. In the same way, these testing results could have become the subject of a technological controversy. In both cases the underlying problem is or would be the regress of truth. There would have been plenty of opportunities for the advocates of the high-wheeler to question the validity of the bicycle races as tests. An overview over possible reasons for challenging the results of tests is given by MacKenzie (1989: 413-414). Critics could have argued "that existing cycle races were not appropriate tests for a cycle's 'real' speed (after all, the idealized world of the race track may not match everyday road conditions, any more than the Formula-1 racing car bears on the performance requirements of the average family sedan)" (Pinch/Bijker 1987: 46). They could have argued that it is not the average speed of the race, but the maximum speed which is important or that the race proves the superiority of the air tyres, but does not reflect the superiority of the low-wheeler and so on. Arguments of this kind illustrate that technological controversies are about truth-related issues. McKenzie's analysis of the technological controversy about the accuracy of intercontinental missiles shows this very clearly. According to Pinch and Bijker, the proponents of the high-wheeler forwent the option to start a technological controversy. They simply accepted the test results. The bicycle races transformed a situation of contested usefulness into a situation in which it would be now a question of truth to challenge the claimed superiority of the air tyres. Since nobody started a technological controversy about this issue, the transformation immediately led to a closure of the debate.

5.4 Interpretative Flexibility of Usefulness and the Controversy of Neural Networks

The case of Neural Networks serves as an example of a controversy about relevance that revived but took a new direction after questions of usefulness were included. Especially in the 1980's, the discussion about the usefulness of certain methods became crucial for the outcome of the renewed debate. A distinguishing feature of this controversy is that after the debate was closed in the late 1960's, it was reopened at the beginning of the 1980's. Many of the same actors used mostly the same arguments to debate whether Neural Networks or Symbolic AI is the more promising approach. But this time, the result was completely different. The research on Neural Networks, which was announced to be of no avail in the late 1960s, experienced a furious revival. By the end of the 1980's, it became an established and well funded part of the research on artificial intelligence. This is due to more than one reason (cf. Meyer 2004: 97-107). However, one central aspect was that the controversy was enlarged by the question of the usefulness of specific products resulting from the research on Neural Networks compared to research on Symbolic AI.

As indicated above, though having lost the controversy of the 1960's, some Neural Networks research groups were able to survive by "hiding" in other scientific disciplines like biology and physics. Due to the work they conducted there, these groups presented first applications for Neural Networks in the 1980's. In 1987, it was a sensation, when a computer program was presented, completely based on Neural Networks that was able to transform written text in spoken language. Based on successes like this, proponents of Neural Networks tried to shift the focus of the controversy. Instead of a theoretical discussion about the long-term prospects of Neural Networks research, like in the 1960's, they promoted a debate concerning the usefulness of certain existing solutions to problems. Of course, they focused on topics which proved problematic for Symbolic AI, e.g. pattern recognition. As a response to this attempt to reopen the controversy, Minsky and Papert republished their book "Perceptrons." (cf. Minsky/Papert 1988) Because it worked so well then, they just added a new introduction, extended the final chapter, and left the rest of the book as it was. They wanted to show that their mathematical proofs still support their assessment of the nearly non-existing potential of Neural Networks. Thus, Minsky and Papert tried to force the revived controversy into the direction that in the 1960's had proven to be successful in promoting their research approach They argued that all solutions presented by Neural Networks research still rely on overly simplified models which are also subject to the restrictions they claimed to have demonstrated in their book. Applied to the complexity of the real world, they would fail to keep up with the promises of their creators. Because Neural Network research was located in research areas different from computer science and due to the availability of first applications that demonstrated their usefulness, these theoretical arguments could not develop the power they had 20 years before. Minsky and Papert lost the debate concerning the usefulness of the Neural Networks approach because they focused their argumentation on the level of theoretical long term evaluation. Their opponents, on the other side, connected concrete problems with concrete solutions. In doing so, they where able to establish their perspective of the usefulness of Neural Networks. Consequently, research on Neural Networks became an attractive research option for scientists in the field of Artificial Intelligence as well as for funding organisations.

6 Conclusion

The concept of three forms of interpretative flexibility (3FiF) as presented here relies on two strands of argumentation. First, by tracing back interpretative flexibility to three different forms of infinite regress, we focus on differences between phenomena related to interpretative flexibility. Second, we wanted to show that in controversies or debates concerning the meaning of a certain scientific fact, technological artefact or research approach, concurrently or consecutively different forms of interpretative flexibility may play a part. Combining and extending previous considerations regarding interpretative flexibility in this way serves two objectives: we hope that in identifying differences in interpretative flexibility and corresponding differences in handling interpretative flexibility, we will contribute to a better theoretical understanding of the dynamics of the social construction of scientific facts and technological artefacts Additionally, we are confident that our approach is useful for empirically analysing the course of development of scientific or technological controversies in a more appropriate way. Shifts between and transformations of the respective reason of interpretative flexibility (contested truth, contested usefulness, contested relevance) become observable as well as situations of their coexistence. This helps to explain why closure in debates about the meaning of technological artefacts occur although there is no inherent need to come to an agreement; why on the other hand scientific controversies remain open although, here, an imperative to closure exists; how scientific controversies become closed for other than truth-related reasons; or why, as in the case of Neural Networks, an already closed controversy becomes reopened again.

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Deliberative Constructivism

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Abstract

The paper proposes to expand the constructivist view from empirical analysis to pragmatic advice. Its main thesis is: The fact that methods and concepts in the production of knowledge and standards for justifying truth claims are culturally bound does not preclude these bonds from being observed and also controlled and adjusted. Knowledge work imports scientific methods and concepts into virtually all segments of society. Whether knowledge is well manufactured and trustworthy is no longer the sole concern of scientific communities but of clients, stakeholder groups, political bodies, and other actors. The paper begins with reconsidering the symmetry principle of the 'Strong Programme' from a methodological point of view. It argues that excluding justified beliefs from the realm of independent variables is unwarranted. Even if it is impossible to introduce truth as a cause, it is possible to accept justifications of beliefs as causes. In a second line of analysis, this paper explores that the concept of cultural relativity of knowledge has an internal instability. Every lesson in cultural relativism is a lesson in designing cognitive strategies to transcend it. The better the social construction of scientific knowledge is understood and even causally explained, the better reflexive abstraction opens up possibilities to operate with this causality and loosen or tighten the cultural bonds. Examples demonstrate that crossing established boundaries and aiming at higher degrees of cultural independency are as meaningful as value based restrictions to smaller domains. It is in this context that constructivism has a future as a frame for deliberative forms of knowledge construction and justification.

[1] I wish to thank the journal's two anonymous reviewers for helpful comments, Kai Buchholz, Justus Lentsch, and Malte Schopphaus for a fruitful discussion, and Peter Lenco for his attempt to put my German thought style in English words. The paper was written in my English idiom, which Peter tried to remediate. The remaining deficits are my fault. Peter also suggested substantial improvements which I tried to adopt.

1 Introduction: 'Good' and 'Bad' Constructions

This paper aims at recovering a normative stance for the social studies of science which were lost through the constructivist approach. The principal question is rather simple: How can we, as scientific observers of scientific enterprises, distinguish between good and bad constructions of knowledge? Of course, this question presupposes that we should do so. Nevertheless, we should be aware of the conceptual problems involved in contaminating the empirical sociological analysis with normative claims. There is no easy return to any Mertonian position that would declare as more or less self-evident the effectiveness of a set of institutional rules directing scientific practices toward true and valuable knowledge. On the other hand, it seems odd that those who have been so successful in reconstructing the social framework in which knowledge claims and trust in knowledge are constituted declare themselves unable to pronounce any judgement with respect to the acceptability of such knowledge. The counter intuition is that the careful observation of anything made and used by human beings enables us to evaluate its quality and reliability insofar as the observer has turned into an expert. Usually, expert opinion somehow combines the knowable and the valuable either in careful if-then clauses or by blending 'is' and 'ought'. I would advocate such a professional expert position that is based on social studies of scientific knowledge construction and that aims at giving advice in the context of knowledge society. However, this paper's concern is to deal with conceptual and methodological problems raised by the attempt to conjoin normative and descriptive aspects of knowledge analysis.

The normative shift from not only asking why certain constructions of knowledge are actually accepted in certain social settings but also claiming to determine the conditions of acceptability is induced by the following motif. Knowledge production and its application become increasingly interconnected in recursive dynamics of social change. There are already different models constructed to understand this new institutional arrangement. These include – presumably among others – the mode II model (Gibbons et.al. 1994; Nowotny et. al. 2001, dt. 2004); the co-production of science and society (Jasonoff 2004); the variants of actor-network models (Latour 2005); and the real world experimentation approach (Groß/Hoffmann-Riem/Krohn 2005). These models raise new questions concerning the legitimacy and responsibility of scientific work embedded in non-scientific enterprises. But they are – with the exception of the last one – reluctant to suggest answers. The self-reflexive question is: given the competence in the empirical analysis of new arrangements of knowledge production in knowledge societies, what follows with respect to critically evaluating the appropriate set-up of such arrangements? Take as an example regulatory experiments concerning the deliberate release of GMOs as defined by the Genetic Engineering Act and EC Directive 90/219/EEC.[2] Are the design, responsibility distribution, and involvement of actors in a well ordered state? Science researchers are presumably not well equipped with a cognitive and institutional repertoire suitable to giving advice in these matters. And if asked – luckily we are not – how

[2] The responsible agency in Germany is the Robert Koch-Institute in cooperation with the Federal Environmental Agency (Federal Ministry of Environment), Federal Biological Research Centre for Agriculture and Forestry (Federal Ministry of Consumer Protection, Food and Agriculture) and the Federal Research Centre for Virus Diseases of Animals (in cases of using genetically modified vertebrates or genetically modified micro-organisms that are applied to vertebrates; Federal Ministry of Health). Information from http://www.oecd.org/-document/30/0.

in these matters a social construction of knowledge production should be arranged according to the findings of past observations, the sociological observer would politely insist on being nothing else than a careful observer. Admittedly, social scientists would overstress their status if they planned to gain power in defining the correct institutions and procedures of co-production, experimental recursive learning, and robust research. On the other hand, it would seem odd if in matters of legitimacy, reliability, fairness, and efficiency of knowledge production everybody had something to say except for the sociologists of science whose professional self-understanding restricted them to observe, but not to shape, knowledge production.

The stance taken in this paper is different. The focal point is that precisely because constructivism has theoretically, methodologically, and empirically invalidated (almost) all claims of unconditioned universal and objective knowledge, and just because it has disclosed the dependence of acceptance criteria on interests, prejudices, status, values, and world views, it enables us to critically correct this kind of dependence. From a philosophical point of view, one could say that empirical observations of such relations between knowledge and context tend to be generalized to a universal relativism. From a pragmatic point of view, they contribute to a toolkit which can help to construct *more* or *less* objective and universal knowledge claims. Both strategies – on the one hand to generalize and objectify knowledge, on the other to bind its scope and validity to cultural locales - have their merits and costs. Deliberative constructivism is about understanding and making use of these strategies. The main thesis to be developed and justified in the following is: precisely *because* our methods and concepts in the production of knowledge and the justification of truth claims are culture bound, their relatedness can not only be observed but also controlled and adjusted – at least to some degree. To speak of grades is important here. Rendering some knowledge more or less general or objective does not presuppose a belief in (the possibility of) universal and objective knowledge. A physicist can speak of degrees of power without necessarily believing in the existence of something theoretical, such as total or absolute power.

The controversy about truth relativism is, of course, as old as the philosophy of knowledge, which was born in the Sophist period of Greek philosophy. Its most important later stages are the medieval disputes between the Church and deviating scholars on the double standard of revealed versus discovered truths; the Baconian analysis of the idols which prevented people of his time from accepting the experimental method; and the sociology of knowledge tearing down the Cartesian dogma of autonomous rationality. It has been the merit of the social constructivist programme to carry the controversy fully into the system of science and fuel it by empirical research. The more 'scientific' the cases to be studied appeared to be, the more far reaching were the consequences of the lesson about the social conditioning of the content and justification of knowledge. However, the prevailing discourse on the role of science in knowledge society makes it necessary to equally emphasize the reversed perspective. Knowledge work imports scientific methods and concepts into virtually all segments of society. Whether knowledge is well manufactured and trustworthy is no longer the concern of scientific communities but of clients, organizations, associations, stakeholder groups, political bodies, and other actors. Controversies between scientific experts and counter experts can only be heated but not solved by demonstrating the relatedness of knowledge to interests and money. Thus it would seem that even commissioned knowledge work is not worth its money if its product cannot be put to proof and test. This implies that knowledge society is in need of that kind of toolkit that gives scientific and science-based knowledge its internal truth value – be this value related to other values, norms, and interests or not. If Luhmann is right in saying that the predominance of knowledge over norms in society is indicated by the acceptance of a cognitive style of learning at the cost of a normative style of cultivating traditions (Luhmann 1990: 138), then understanding the modalities of the social construction of knowledge becomes a project of societal relevance. Willingness to learn depends on the readiness to accept the 'truth' of a lesson to be learned. Truth in this context means to impute the cause of knowledge to the environment of learning, not to the action of learning. At variance to learning through teachers, science is specialized in learning something new which no other agency can know better. Usually nothing other than scientific agencies can control truth claims insofar as they are based on scientific learning (even if in certain cases local experience beats scientific expertise). In consequence, knowledge society becomes increasingly dependent on trust in knowledge and its agents. This dependency is counterbalanced, at least partially, by additional measures for the control of truth claims which can prevent trust investors from losses of capital, political credibility, health or even aspirations. Liability action can be a harder threat than the displeasure of admitting to colleagues an error or failure.

I return at the end of the paper (section 5) to this relation between standards of justification and kinds of knowledge claims. The next section is purely methodological and tries to develop a framework that allows for the relation between sociological and epistemological references.

2 The Veil of Methodological Ignorance

I will start with making a strong methodological argument for the social constructivist symmetry principle as it was announced in David Bloor's classic "Science and Social Imagery" (1976). I plan to go beyond it, but nevertheless it is a point of departure to be taken seriously. The symmetry principle states that for the sociological explanation of why some knowledge is socially accepted, its quality of being true or false is irrelevant. I want to throw some light on the principle by considering it in a metaphorical setting, one that has been used quite often in giving expression to the human condition: reality as a maze or a labyrinth. The labyrinth metaphor encapsulates the complexity of the world and the experiences of confusion and delusion encountered by those who got lost in it. Real world labyrinths have been constructed for all kinds of exercises. These include the ingenious invention of Ariadne when she rescued Theseus at the Knosses palace labyrinth; the model for salvation given to Christian visitors of cathedral labyrinths; amusement for court people in maze gardens; and the observation of rats in laboratory mazes (see Methews 1922, Attali 1999). The allegorical labyrinth is the metaphor for the world itself in which we are included. Hope does not lie in escape, but in orientation by solving the riddle of its construction. Whether it is of a Platonic order which can be discovered by trial and error, modelling, and calculation, or whether it is determined by a Democritean mess that allows at best some temporal and local solutions, we are never able to decide.

I take up this metaphor in order to construct a thought experiment which can shed light on the connection between methodology and observation in the sociology of science. Let us imagine any simple spatial labyrinth into which at least two actors are thrown – in the sense of Heidegger's *Geworfenheit* - and experience their being in the labyrinth. Step by step they transform their experiences into pieces of knowledge and patterns of orientation. They communicate plans, act by trial and error, interpret outcomes and develop descriptive models. Each can trust and distrust the information the other offers; believe and doubt their theories. By and large they generate a common stock of conventional knowledge which they trust even if they are puzzled by surprises. Incentives to act as researchers can easily be added.

How can one know what they are doing? In our thought experiment they are observed by a sociologist who specializes in doing research on how researchers expand their knowledge base. He or she reports the knowledge he gains to other observers of observers, who can trust or distrust the information. Granted, the scenario is oversimplified and could be enriched in every direction (e.g. different groups of competitive actors, division of labour, different languages causing translation problems), but it is rich enough to pursue a fundamental epistemological question, namely, where do we locate the sociological observer, i.e. the secondary observer? The choice is simple: inside or outside the labyrinth. But the consequences are considerable. If located outside, the secondary observer is in the comfortable position to evaluate progress and error of the primary observers or actors. If located inside, the secondary observer is no better condition than the actors. In fact he knows even less, since he is not involved in the business of discovery, even though he may perhaps contribute other benefits such as keeping records, checking for consistency, or writing down history. In sum: to position the secondary observer inside the labyrinth makes him a cultural relativist doomed to accept the symmetry principle. If the secondary observer is located outside the labyrinth, his position is a realist one. Being in the position to overview the labyrinth he can determine the degree of correctness of knowledge and evaluate the reasoning of the actors and can even observe what in the actors' environment makes successful learning easy or difficult. The best approximation to this realist position is the one of a teacher, who, furnished with superior knowledge, trains students. An approximation to a relativist position is a lay person observing experts in the process of problem solving, e.g. finding the cause of a malfunctioning machine, or the adequate diagnosis of a disease. In such cases the lay person cannot have any justified belief closer to the solution than the experts.

On what grounds can we base the decision between the alternative options of locating the secondary observer? Already in asking the question we are involved in constructing another frame of reference, in which we locate an observer of higher order—a third order observer—who reflects upon the pros and cons of locating the second order observer inside or outside the labyrinth. Surely the third order observer would consider whether the second order observer actually has access to knowledge about the labyrinth independent from the primary observers' reports. At this level of analysis, in which the observation of observers plays a role, methodological controversies within scientific disciplines have their place. Methodology in the humanities and social sciences is a matter of third order observation. Before looking at some examples it should be mentioned that further iteration leads into undecidable philosophical issues. The issues which the observer of the fourth order can raise concern the question as to whether the third order observer has any access to a reality at all, or whether he is doomed to exist in an eternally unknown environment. Since this concern no longer relates to questions of method, it surpasses the scope of this paper. It may be emphasized, though, that philosophies of relativism as well as realism, in trying to address this highest level of reflection, loose relevance with respect to deciding the question of where an observer of observers should locate himself.[3]

Returning to this question it may be interesting to briefly illustrate it with examples taken from fields of the humanities, social sciences, and biology. I apologize for giving these examples more or less freehand; they are not based on an analysis of the present state of methodological discourse. In all fields we meet long-lasting methodological controversies with characteristic dividing lines. Some of these relate directly to the place provided for the secondary or scientific observer.

Ethnography and Social Anthropology have always been divided on the question of how rigorously they should accept the complete 'strangeness' of other cultures. Taken as completely alien, the culture to be studied is an unknown labyrinth with unknown actors. The ethnographer as secondary observer locates himself inside, willing to learn the language and understand the institutions without knowing in advance whether they can be compared with anything he is acquainted with. Quite different is the functionalistic approach. Its most outspoken proponent was Bronislaw Malinowski. He clearly positioned himself outside the labyrinth. He believed in a general functionalistic theory of culture which allows a bird's-eye perspective. Different cultures are "manifestations" of a general schema (Malinowski 1975: 74). Looking from this scientific point of view he believed to possess a theoretical device with which he could decode and thereby understand the basic design of the labyrinth even better than the actors inside. Malinowski would admit, of course, that understanding the specifics of institutions is only possible by deeply immersing oneself into the unknown details. But in principle the situation does not seem to be completely different from research in fields such as astronomy, biology, or geology where all objects differ merely in detail. Malinowski's decoding device provided by a general theory of culture is almost like an algorithm for solving any labyrinth. Just the opposite strategy is adopted by those who believe in the relevance of fundamental differences between all cultures. As scientific observers, they do not want to get completely lost in the labyrinth of an observed culture. This is because it would mean losing one's scientific attitude and becoming socialized as a new member of the culture under study with fading memories to one's original culture. Instead, one has to face the translation problem between two cultures. The ethnologist is an observant actor *there* and a trustworthy reporter *here*, although moving between both positions he has to master the translation problem. Translation can be defined as the attempt to relate observations made inside one labyrinth to those existing in another. From a cultural relativist point of view, the correctness of the translation cannot be examined.

Historians face a different problem. By definition, there is no way to become part of an earlier culture because it is gone. In attempting to do so the researcher would only meet his fellow scientists, also studying texts and material remains. Since the so-called historicism controversy, the focal methodological problem of the discipline has been, however, whether the historian should try to virtually localize himself in the presence of the past, i.e. make efforts to observe as if nothing is known to him or her about the future path of development. In doing so the historian would try to assimilate to

[3] This statement does not imply the uselessness of philosophical discourse. The differences between, say, a phenomenological theory, which embeds knowledge in our being present in a world which we do not infer but live with, and a Kantian approach where the world is given as a manifold variety of perceptive impressions from which everything is imagined, are pertinent for a general theory of knowledge. But they are not helpful for a discourse on the different options for framing the observation of observers.

someone invented or at least reconstructed by his discipline – the ideal medieval monk or medieval heretic, an early industrial entrepreneur or proletarian, etc. The paradoxical situation is: historians invent the labyrinths in which they want to get lost in order to test their inventions. Historians also face the translation problem which may be even more severe because the continuity of the languages suggest similarities of meaning which may be misleading.

Cognition research is my third example. It comprises the biological, psychological, and artificial fields which are all very close to the labyrinth scenario. It was Humberto Maturana, in his quarrel with the artificial intelligence research of the 1950s, who tried to develop a methodology that places the observer inside the labyrinth. The idea was to reconstruct the operations of a cognition system completely from the internal perspective of such a system, one that is absolutely unable to compare its reasoning about, imaging of, and interacting with reality with anything like reality. He called such systems autopoietic systems (Maturana/Varela 1980). Reality is necessarily nothing but an observed reality. Maturana's methodological prescript forbids us to use any language that would describe adaptative achievements of a learning system. Any learning interpreted by an external observer as learning about something real exists only in the domain of actual states of cognition. For Maturana, the autopoiesis model was incompatible with the conception of a non-living technical artificial intelligence, which necessarily must start with functional concepts concerning the ability to learn. Such beings learn, so to say, in our world, not in theirs. We can assess their mistakes, because we can compare that what they learn with what they should learn. And we can re-write the program so that they can do better. If, according to Maturana, no living cognition system can import any information from the environment, then observing the operations of observers in a labyrinth (its environment) is only possible from a virtual point within this labyrinth. Maturana's thought model was influential and contributed to the analysis and construction of cognition and communication systems based on principles of self-organization. Still, most researchers would oppose such purist rigor and accept the fruitfulness of a functional language enabling one to observe evolutionary and adaptive learning. In any case, at least Maturana put his finger on the unsolved methodological problems that arise if cognition is partly observed from a causal and autopoietic perspective from within the labyrinth, and a functional perspective from outside the labyrinth. It should be added that Maturana's methodological rigor led him into a rather bizarre epistemology of recursive observation, which no longer informs or attracts empirical researchers.

The purpose of briefly inspecting the methodological problems of some research fields that deal with observing and learning about people, cultures, brains, and artificial systems is to show that those of the sociology of scientific knowledge are quite ordinary. Before turning to this field I propose the general observation that in all these disciplines there are tendencies to undercut the methodological strength imposed by the labyrinth thought model. These tendencies can have different forms. There can be different schools (e.g., functionalist versus anti-functionalist, nomothetic versus ideographic), or the application of different tools (reductionism, integrative modelling, simulation), or the use of 'thick descriptions' which take the liberty of switching between the observation points without too much respect for methodological barriers. Most research fields tend to occupy both places of observation, with or without explicit justification. This can perhaps be defended with Einstein's *bon mot* on the methodological opportunism which is characteristic of every fruitful research. Still, it is desirable to offer an argument that justifies such opportunism in methodological terms.

The strongest claim to restrict the observer's position to one within the labyrinth was articulated by the strong programme sketched in David Bloor's "Science and Social Imagery" from 1976. Bloor claimed that a sociological observer must refrain from making the truth of any knowledge claim an empirical fact if one wishes to analyse the causes of its acceptance in any group, culture, or society. His point is that the observer must avoid a vicious circle, which, of course, is a methodological point. When a cultural group (e.g. a scientific community) is convinced of the truth of a set of beliefs, the causal explanation of this fact obviously cannot refer to the truth of the beliefs. The reasons they have for feeling convinced must be identified independently of the secondary observer's own judgement concerning the truth or the falsity of the beliefs. Take as an example the pre-modern model of geocentric astronomy. The reasons and evidences that convinced pre-modern astronomers of the truth of the Ptolemaic model cannot change based on the secondary observer's state of conviction. Therefore the sociological explanation of false belief cannot differ from that of true belief. This is the veil of methodological ignorance which sociologists of science constrain themselves to look through.

This tenet is powerful indeed, even if its price is high. Its strength is the unmistakably clear positioning of the second order observer within the labyrinth. Whatever he knows about the truth or falsity of a knowledge claim is forbidden knowledge within these methodological limits. The price to be paid is to associate all scientific knowledge completely with any kind of belief system. The attempt to explain the causes of a belief may lead from individual evidence or collective trust into authority or social bonds of solidarity. However, whether any of these sources are reliable cannot be tested by checking the truth-value of the belief. This price does not seem too high when the sociological observer looks at contemporary knowledge in the making. Take as an example Harry Collins investigation into gravitation wave research (2004). Even if he tried to be as comprehensive as possible he could not claim to solve the riddle by comparing the actions of his observers with the structure of their labyrinth. (Otherwise he could well be the first sociologist who wins the Nobel Prize in physics by deriving new and accepted knowledge about gravitation waves by observing observers of measuring instruments). The situation becomes less comfortable if the second order observer is interested in studying ideologies, betrayal, and deceit in science. And his position is completely helpless if asked to give advice with respect to the question of what would make a knowledge claim more reliable or trustworthy. A distinction between good and bad constructions based on knowledge gained by comparative studies would not be possible – given the veil of methodological ignorance.

Critical objections against the strong programme have not invalidated its methodological strength. Ethnomethodologists criticized the simplicity of the causality concept (Knorr 1988). In fact, a strict model of a law-like relationship between scientific beliefs as effects and social events as causal conditions has never been offered. But the methodological directive to look for causes that make scientists believe a given claim can be stated independent of an available theoretical model. The most frequently used conditioning factor has been the concept of "interest" which can be associated with social background and organizational bonds. While the occasional suitability of the concept is beyond doubt, it was not successfully elaborated toward an analytical framework (Woolgar 1981). Philosophers of science questioned the self applicability of the 'Strong Programme', even if Bloor announced this as one of its axiomatic points of departures (Laudan 1980). Additionally, massive criticism was raised by Latour (1999). The strong programmers' belief in the scientific accessibility of the social conditioning of beliefs is by no means stronger than the natural scientists' belief in the natural causes making these beliefs true–; all points of criticism are well made but they do not affect the methodological kernel. It may well be that the 'Strong Programme' will never be transformed into an empirically founded theory, and it appears that no one is interested any more in doing so. My attempt has not been to defend the 'Strong Programme', but to emphasize its strength with respect to the methodological foundations of the social studies of science. On its basis, social constructivism of scientific knowledge means no more and no less than this: from the point of sociological observation of the formation of knowledge-claims, reference to the truth of these claims is methodologically excluded by the veil of ignorance, which needs to be accepted if the sociological observer has decided to operate within the labyrinth. This minimal statement is consistent with the criticisms mentioned. It avoids the considerable philosophical controversy between realism and constructivism yet at the same time declares the search for social construction mechanisms a disciplinary sociological task.

But why should an observer so strongly be restricted by methodological boundaries? Before trying to answer I want to point at an interesting asymmetry in the labyrinth thought model between the internal and the external position of the observers. Any external observer possesses the capacity to move inside and try to ignore the additional knowledge about the labyrinth. In doing so, he faces problems such as the (im-)possibility of unbiased observation and the translation back into the context of his culture. Still there is this asymmetry, which virtuously and opportunistically is taken advantage of in all disciplines which study knowledge production and communication in different historical periods, cultures, biological species, or even robots. Do the methodological binding forces as outlined by the 'Strong Programme' put the sociology of knowledge in an exceptional position? Certainly, there is no way whatsoever to leave the position of a participant observer inside the labyrinth if processes of contemporary knowledge production are observed because the observer cannot be more knowledgeable than the observed scientists. But the majority of case studies do not completely prevent the secondary observer from knowing something about the issues that have been at stake.

3 A Sociology of Truth?

This section will look more closely at the causality mechanism relating truth claims to social conditions. It was already the basic idea of Popper's falsificationism to circumnavigate the vicious circle implied by using truth as cause but nevertheless hold up a normative stand. If there is no access to controlling the truth of a knowledge claim, then there are at least possibilities to check their resistance against refutations. A knowledge construction that proves stable against organized sceptical testing cannot be too bad, be it true or not. A society that cultivates the construction of new knowledge as well as procedures to deal with them is a culture ready to learn in the double sense of being quick and being careful. Even Thomas Kuhn approved the following quote from *Conjectures and Refutation*: "Assume that we have deliberately made it our task to live in this unknown world ... and to explain it ... with the help of laws and theories ... then there is no more rational procedure than the method of conjecture and refutation." (Kuhn "Criticism and the Growth of Knowledge" 1972: 22) But as is well known, the rationality of the model is not sufficiently in line with the history of science (recall Lakatos' nice phrase: "history falsifies falsificationism"), nor did it survive epistemological objections concerning the independency of testing from theory. Still, Popper's idea to define an institutional procedure to control and evaluate the quality of a knowledge construction by its capacity to survive critical testing was pioneering because it handed over to the secondary observer within the labyrinth an instrument of his own.

I shall not follow Popper's social epistemology of organized criticism, but rather take over the notion of indirect truth relatedness of second order observation. My argument is the following: Even if it is impossible to introduce truth as a cause, it is possible to accept justifications of beliefs as causes. 'Justification' is taken here as comprising all communication about the potential evidence related to a knowledge claim aiming at its acceptance or disapproval. Justifications vary from traditional epistemic concepts to more institutional ones such as trust in peer review or acknowledgement of licenses. Taken very generally, justifications can refer to a broad variety of instances for the fixation of beliefs, among which are conventions, habits, norms, fate, authority, or revelation. They all can serve to answer the question "why do you believe *p* to be true" with a "because ..." clause. Epistemologically relevant are, of course, those justifications which claim to 'refer to truth'. They comprise the announcement of having been an eye-witness, possession of data and documents, presentation of calculations, or a description of an experimental setting. An indicator of truth-related justification is openness for continuing the communication about claims with further "but why ..." questions. But presumably they end rather quickly when some basics are touched upon. Normally, nobody is prepared to answer questions such as "why do you rely on the data produced by your instrument, on the outcome of a calculation?" Rational justification also ends in or merges with conventions, habits, norms, and authority. At least, it seems difficult to assume that justifications referring to truth form a set of standards or criteria which are tailored for science and distinguish scientific justification from other forms of belief management. But this is not my point. The point is that truth related justifications can be taken by a second order observer as an *explanans* without entering a vicious circle or contesting the symmetry principle. Still, one still could say that truth cannot play an explanatory role. But by means of justifications it can play a regulatory role in science as well as in the social studies of knowledge (Goldman 2001). This is important because the correctly stated symmetry principle was incorrectly used for guiding sociological explanations of truth claims toward all possible *explananda* except truth-related reasons. Let us imagine a parallel construction of the symmetry principle for other areas of society such as political power and economic wealth. The explanation of power could be found in anything except power; wealth can originate in anything than wealth. Seemingly, it would be acceptable to explain wealth by power and power by wealth, just as it is possible to explain knowledge by power/authority or by wealth/patronage. Consequently, it would even be allowed to explain knowledge by power, power by wealth and wealth by knowledge. The violation of the vicious circle is only hidden in a merry-go-round.

I have mentioned that one of the critical objections to the 'Strong Programme' was directed against its causality concept. Why should some variables (e.g. authority, class interests, or other features of culture (Bloor 1976: 3) be accepted as independent, while others (in this case instances of evidence) are considered to be dependent? Or more precisely, why, in a sociology of science, should the set of explanatory causes comprise an almost unlimited variety of variables such as carrier, professional standing, money, religious background, social adherence, but precisely exclude truth—truth taken in its sociological meaning as justified belief? From an epistemological point of view we face the following alternative: Either any causal explanation in the social sciences leads into a vicious circle based on the careful assumption of equal rights for all variables with causal explanatory power, or the 'Strong Programme' must be based on an equally strong sociological theory that enables it to distinguish theoretically between the basic and the dependent variables and allows lineal causal explanation of empirical findings by reducing the latter to the first.

Giving 'equal rights' to variables which can be more or less influential in shaping social change does not imply the return to truth as an unconditioned and freely accessible criterion. Rather it implies taking the institutional rationality of science as relevant in itself – as something that can be explained as well as something that can assist in explaining something else. In Luhmann's language it means taking truth seriously as a medium of society. A medium needed for what? "The truth medium serves societies blind flight" (Luhmann 1998: 252). Blind flight is another metaphor for orienting oneself in an unknown reality. And, of course, the second order observer is on the plane. Blind flight depends on numerous technical installations and the competences of trained experts. Whether or not the flight is successful depends on many factors, even perhaps on advice given by authorities of power and money. However, the most important share of independent variables refers to knowledge partly materialized in technology and partly embodied in competences. According to this metaphor, a sociology of truth cannot return to an external second order position and directly observe the fitting of blind flight to reality. However, it should not recoil from the circularity structure which persists if the acceptance of justified belief in the operation of scientific instruments depends on the acceptance of justified beliefs in a theory-based calculation. Perhaps the systems theory of Luhmann goes too far in giving the institutional rationality of functional systems an absolutely closed structure. But the important point of the blind flight argument is that sociological explanations can correctly refer to truth (justified belief) as an independent variable or cause. If this leads into a circular explanation, then either circularity is unavoidable or explanation is impossible. The labyrinth metaphor aimed at avoiding the circularity trap by fixating the secondary observer either inside or outside. We shall see how the model needs to modified in order to incorporate truth as cause.

4 The Cultural Relativity of Justification

Adherents to the 'Strong Programme' may object that the last paragraph elaborated the obvious, namely that justified beliefs can of course function as causes of justified beliefs, provided they are restricted by the valid conditions of a given cultural labyrinth. Just as some forms of authority or heredity are accepted as sources of justification in one culture and not in others, personal evidence may count in one case but not in another. The value of a witness can depend on his social status in one culture or on his withstanding cross examination in another culture. Therefore, the counter argument runs, the attempt to include truth claims via justifications of beliefs into the set of explaining causes ends where it started. The forms and values of justifications depend on social institutions, of which scientific institutions are just a subset. "There are no context-free or super-cultural norms of rationality" (Barnes/Bloor 1982: 27). Thus, just as there are different cultures there are different knowledge cultures. For example, different knowledge cultures must not necessarily be very distant (Chinese versus Western science; cultures of wisdom versus cultures of technology); the differences can exist between neighbourhoods in the same community (mathematical versus experimental physics; quantitative versus qualitative sociology).

Taking the cultural embeddedness of justification modalities for granted, of what value could it then be for the second order observer to refer to them as explanatory causes? First of all, since justification is always addressed to an audience, it is a completely communicative affair even if embedded in conventional institutions. This is the reason why in the labyrinth thought model two primary observers are active. The second order observer witnesses communication between actors about potential common knowledge. He unavoidably becomes part of the communicative social structure, whereby his role can be more the passive listener or the active questioner. The institutional framework in which justification is embedded and specified equips the carrier of knowledge with possibilities to substantiate the quality of his knowledge and make it a validity claim. The communicative structure of justification has two poles: reasons that warrant a claim and reasons that warrant acceptance. It is certainly not incidental that the institutional framework of this structure was derived from the juridical language of the courts. Francis Bacon and Immanuel Kant touched upon the similarity between evidence production in legal and in scientific contexts. The analogy is even more inviting from a constructivist point of view. It goes as follows. (a) In a court of law some of the essential facts remain hidden forever. (b) Witnesses are instructed to render their evidence communicable and make their status as witness reliable. They thereby transform remembrances of experience into information for an audience. The information can intentionally or unwillingly be misrepresented and misleading. (c) Prosecutors, defenders, and experts present indications adding trust or distrust into the witnesses' reports. These may include checking the credibility and competence of the personality as well as testing the solidity of information. (d) The jury is supposed to draw a commonly shared picture of 'what was the case' on the basis of questionable reports of the witnesses, a patchwork of expert information, and the strategic interests of lawyers. The mismatch possibilities are twofold: unwarranted trust as well as exaggerated distrust can lead to misjudgement.

The difference between knowledge relevant in science and knowledge important in a court was traditionally seen in the reproducibility of scientific evidence for sets of almost similar events against the interest in court in reconstructing the evidence for an individual event with irreproducible singular traits. At first glance the dependence on testimony is less dramatic in science because mistakes, errors, and deceits can be disclosed by testing the experimental reproducibility and checking the conceptual consistency, or by observing inconsistencies in using knowledge and knowledge-based products (e.g., a new instrument, medicine). Without playing down the significance of replication, there is danger to overstate its regulatory relevance. Case studies have provided ample evidence that in many fields of research control by replication is not cultivated so that the ratio between disclosed and undisclosed errors as well as their lifespan is unknown (see Broad/Wade 1992, Weingart 2001: 292 ff., EWE 2004). Second, the dependency on trust in testimony is even higher in science than in the legal system. In a court of law the investment in trust ends with every case. The witnesses in different lawsuits are usually independent of each other. In science every piece of knowledge is produced in a systematic dependency from previous and surrounding empirical knowledge, theoretical concepts, scientific instruments and methods. Trust in information which cannot be checked by personal evidence accumulates over time. Even if here and there pieces of received and accepted knowledge are re-examined it would be futile for every researcher to start from scratch. The immense web of trust has caused Martin Kusch (2002) to talk of 'communitarian epistemology' and give trust in testimony a centre stage position. Trust in testimony does not only (and usually not at all) depend on personal impression, but is based on institutions which control the risk of trust. There is a third reason for being sceptical regarding control by replication. Several authors have emphasized the increasing capacity of science to address problems in their specifity, complexity, social and ecological embeddedness (Böhme et. al. 1973, Novotny 2005; Carrier 2004). The increased solution-power of disciplines can become integrated into inter- and transdisciplinary projects. The scientific challenges here are quite different from the traditional interplay between experimental findings, which can be generalized, and the application of laws, which can be specified. In these cases trust becomes even more important. It covers not only trust in actors who contribute knowledge from other disciplines, but in many cases trust of lay persons in the ability of scientists to model complex real world projects. (Groß/Hoffmann-Riem/Krohn 2005).

If trust in testimony is so essential for the working procedures of science and especially for the justified belief in scientific information, then the thesis of the cultural bonds of scientific rationality can be taken for granted. Even if there are science-specific institutions of trust – just as there are those of the legal system – it does not follow that they have a status as independent institutions of rationality. Just the opposite seems to be the case. Culture dependent institutions of trust in science can become a basis for the construction of culture dependent research fields and bodies of knowledge. I shall come back to this point later in the paper.

Summing up the argument: At variance with the 'Strong Programme', I suggest that sociologists of scientific knowledge should give up the exclusion of truth-related justifications from the set of explaining causes in the analysis of scientific knowledge claims. Of course the advantage of including them is to give science the same societal position as any other institutional system of modern society. From this point of view a second order observer is entitled to analyse the formal structure and evaluate the quality of justifications independent of any judgement about the truth value – or the 'real' evidence – associated with truth claims. But as I have shown this justification is predominantly based on institutional trust in testimony – and therefore culturally bound to relying on the validity or rationality of scientific institutions. This is not far from what practitioners of the sociology of knowledge have maintained for a long time. They never claimed that justifications do not play their cultural roles, but rather that their validity is relative to the culture in which they are anchored. Whether or not the commonly shared background convictions are taken as causes or as effects of more deeply rooted social structure variables seems to be a minor point.

The next step of my analysis refers directly to the concept of the cultural relativity of validity claims.

5 The Instability of Cultural Relativity

The concept of cultural relativity has an internal instability. It is strong as long as it is directed against propositions of a culturally independent rationality which would lead to objective knowledge. Today we are in the possession of so many philosophies making the essential point that there is no such thing as unbound rationality or rationality in an absolute sense. The list includes, for example, the tying up of the concept of rule to life forms

(Wittgenstein), the insoluble translation problem between languages (Quine), the untenable concept of natural kinds (Quine), the theory ladeness of observation (Hanson), the under-determination of theories (Kripke), and the interpretative flexibility of all classification systems (Barnes/Bloor). These concepts join forces against arguments still defending the possibility of conceiving rationality as culturally independent. If the validity of justifications is restricted to specific cultures then there is no path left to qualify any proposition as, in Kantian terminology, universally valid. After having made the distinction between perceptive judgements (which are, of course relativistic) and causal judgements, Kant said:

Therefore objective validity and necessary universality (for everybody) are equivalent terms, and though we do not know the object in itself, yet when we consider a judgment as universal, and also necessary, we understand it to have objective validity. (Prolegommena § 19).

If this is right, then the authors just mentioned would hold that these concepts of objective and universal validity are not available.

However, the attempt to turn this negative result into a positive statement about the cultural limits of justification leads to almost equally problematic difficulties. From a scientific point of view, it should be expected that these and other authors show what relativity means in terms of the construction, demarcation, and observation of the limits set to rationality by a given culture. However, a sociological theory which coherently and precisely specifies the limiting conditions seems to be no less available than the epistemology of unbound self-contained rationality. The essential reason I propose is: Any attempt to determine the limiting conditions of a culture provides already cognitive options for transgressing the limits. The argument can be analogically applied to the other examples of cultural limitations of objectivity, e.g. translation. From the impossibility of a 'perfect' translation it does not follow that it is impossible to distinguish between better or worse translations. Instead, the better the limiting conditions of both languages are known, the fairer can the search for an improved translation be guided including options for slightly changing certain language features. A similar argument holds for the justification of truth claims. From the impossibility of defining a universally valid method of justification it does not follow that it is impossible to distinguish between more general and more idiosyncratic forms. I develop this argument in two steps.

I *first* admit the existence of fixed cultural couplings between institutions and justifications (or justified trust in testimony). The variety of these couplings is great. It comprises all kinds of authority, acceptance of special access to sources of knowledge by witchcraft, sorcery, priesthood, wisdom, as well as professional training and expertise. Last but not least it also comprises scientific institutions, which vary between research fields, disciplines, and the natural and social sciences. We call all scientific forms of justification rational in so far as they are organized by argumentation and evidence as opposed to any other forms of legitimacy. Still they are bound to cultures which give argumentation and evidence their institutional effectiveness.

Second, it is possible that individuals or groups discover the institutional relativity of arguments and evidence that stabilize beliefs. The discovery either expands the margins of acceptable beliefs or it leads to dogmatisation with the consequence of making membership dependent on the acceptance of a belief system. Or it leads to a process which Jean Piaget called a 'decentering' strategy. Decentering is based on a reflexive abstraction concerning the binding forces of cultures. It basically consists in developing a new frame of interpretation that enables one to develop an argumentation acceptable from different points of cultural views. The standard example of such processes is the shift from geocentric to heliocentric astronomy. It was already the philosopher Nicolaus Cusanus (1401-1464) who speculated in his book *De Docta Ignoratia* (*On Learned Ignorance*) on the possibility of observing the astronomical world from different positions: "Since it occurs to everybody, whether his position is on earth, on the sun or a another star, that he is positioned on an unmovable and fixed central point, and that everything else is moving, therefore this somebody, if he were on the sun, the earth, the moon, the mars, etc. would form everywhere new poles. The fabric of the universe is therefore so, as if it had its centre everywhere and its periphery nowhere." (*De Docta Ignorantia* II, 162). Cusanus calls the earth a 'noble star' among other stars, on which there might live other intelligent beings. They would have their own perspective of the fabric universe, their centre, top and bottom. Now we repeat the question of the labyrinth: Where do we locate the observer of all these observers? Simplifying Cusanus' universe to our planetary system, there would be a geocentric view, a venocentric one, another from Jupiter, Mars, etc. These culturally bound views cannot all be universally true though every one would provide equally good evidence. As a parable of cultural relativism the episode could end here. The moral would be not to believe too strongly in your own position from which you observe, measure and model the world because there may be other equally good perspectives but incompatible with yours. But then there is the Copernicus solution. It implies asking the question, what, exactly, somebody from another planet would observe and believe to be a valid empirical basis. From this he developed a model capable of deriving the apparently contradictory views from one single source, that is, a virtual point of reference for all points of empirical observation. This virtual point of reference Copernicus located at the Sun. It would be equally demanding to all planetary observers and represent a fair solution. Furthermore, it is an attempt to switch observer's position from inside the labyrinth to the outside. It is this switch for which Piaget has coined the term 'decentering'. Decentering denotes the ability to find a cognitive point of analysis, in this case a geometrical frame of reference, which allows one to correlate different points of view. Decentering is also invitation to others to share the cognitive explanation of the differing views and their compatibility. To be sure, in terms of epistemology the real progress is not in the empirical gains but in the intellectual manoeuvre of being willing to look for a point of reference that reconciles different points of view. In the times of Cusanus and Copernicus the switch – in Kantian terminology – to a more objective and more general frame of reference was virtual; the escape from the labyrinth was only imagined. Today, we cannot seriously doubt that a re-examination of the heliocentric interpretation of the planetary motions is in principle possible insofar as the second order observer is able to observe from the outside the primary observers. But it does not achieve – again in Kant's language – a complete objective knowledge warranted by a universally valid justification. It is merely a move toward a more objective view, one potentially valid for people with different perspectives. And it is an invitation to participate in a more flexible framework. It does not start with a Kantian *a priori* construction of a transcendental epistemic subject, but with a communication between different actors, belonging perhaps to different cultures. And it says: There is no potential stopping rule for an attempt to develop a more general, more flexible frame of reference. At this point a second moral can be drawn from the Cusanus-Copernicus parable: Every lesson in cultural relativism is a lesson in designing a cognitive strategy to transcend it. To understand and explain cultural relativity of knowledge implies the ability to work on decentering frames. The same knowledge that makes cultural relativism empirically strong weakens it pragmatically. The better the social construction of knowledge is understood, especially if explained in a causal model, the better reflexive abstraction opens up possibilities to operate with this causality and loosen the closed ties.

In his essay on "Solidarity or Objectivity?" (1985), Richard Rorty has posed the question of which epistemological standpoint should be reduced to the other. The realists' basis is objectivity, the relativists' (he prefers the term ethnocentrism) is solidarity. Rorty admits that a solidarity basis cannot have the rigor of an axiomatic system. "Cultures are not so designed, and do not have axiomatic structures. To say that they have 'institutionalized norms' is only to say, with Foucault, that knowledge is never separable from power – that one is likely to suffer if one does not hold certain beliefs at certain times and places. But such institutional backups for beliefs take the form of bureaucrats and policeman, not of 'criteria of rationality'" (Rorty 1985: 9). Nicely said, but it is an ambivalent message. Although it emphasizes the institutional ties of beliefs, it introduces at the same time the necessity of completely different regulatory mechanisms in order to suppress and erase unacceptable beliefs. If the causal determinist model of cultural relativism were correct, the omnipresence of censorship could hardly be explained. This is what I have called the inbuilt instability of the social construction of knowledge. Every understanding of the factual coupling is a possibility of dissolving it in the direction of a more loose coupling. This result applies also to the sociological analysis of scientific knowledge. Its reconstruction of the relativity of knowledge is a potential contribution to expand its irrelativity.

6 From Social to Deliberative Constructivism

In two aspects I wish to go beyond Piaget's evolutionary epistemology. One is to emphasize that all decentering strategies have their price. The other is to understand that strategies to restrict, rather than expand, validity claims are equally important. By reflexive abstraction they become manageable in both directions. In other words, the aspects are linked.

Interestingly, the last twenty-five years have witnessed an increasing number of programs and paradigms which counteract the tendency of making claims more general and objective. They offer epistemologies which attempt to particularize validity claims and institutions of trust. Or they offer self-descriptions of cultures which fit certain epistemologies. They are not guided by a pre-constructive dogmatism, but by turning constructivism into a tool for manufacturing epistemic cultures. Furthermore they do not principally criticize abstractive reflection and decentering processes. However, they do maintain that every move toward a culturally more independent justification is a movement in a certain direction with gains at the costs of alternative directions. Because decentering is not unidirectional, there is an element of choice involved.

As an example I take feminist epistemology as it is developed by Donna Haraway (1995). Her focus is not feminism in particular, but what she calls "embodied objectivity and situated knowledge", a concept that is certainly opposed to a disembodied objectivity as strived for by the Copernican virtual observers. Haraway builds her epistemology on the concept of vision. Scientific cognition, as it is usually declared but not practiced, aims at perceiving the world from potentially everywhere (universal perceptibility), and in this attempt it tries to imitate or simulate 'God's Trick': to see everything without being seen and to see everything from everywhere: omnipresence and omnivision. Donna Haraway calls this the ideal of masculine science. In another chain of arguments she calls it confessed irresponsibility. One is responsible only for insights which depend on the point of view one has chosen. Omnivision has no point of view. Here is her alternative: "Only partial perspective promises objective vision". "Perspectives are active perceptive systems building on ways of life, each with a detailed, active partial way of organizing worlds" (Haraway 1995: 181). They unavoidably lead to different world views. Haraway takes her most important epistemological step when she specifies what is needed to understand and to acknowledge the specificities and differences of these views. It is "the loving care of people who are ready to learn how to perceive the world from a different perspective."(181) This argument obviously leads back to a an epistemic decentering strategy, though a quite different one. It is not guided by the rational construction of a cognitive system, but by loving care, which I take to be something like a sympathetic strategy. "To understand how these visual systems work – technically, socially, psychically – this should be the pathway for embodied feminist objectivity."(181) Obviously Haraway is looking for of a new decentering strategy that allows the feminist perspective to exist among several others. *And* the strategy is directly derived from a feminist perspective. The new epistemological feature is the element of choice with respect to decentering options. Even if scientists are asked to give reasons for making choices, they remain choices nonetheless. Here are Haraway's reasons: "I am arguing for politics and epistemologies of location, positioning, and situating, where partiality and not universality is the condition of being heard to make rational knowledge claims."(186) One can call this an argument for a pragmatic relativism. The irresponsible omniperspective is no longer accepted, but the relevance of other perspectives which are able to present their different value bases and embodiments is acknowledged. It should be clear by now how completely different the future path of the development of scientific knowledge should be constructed according to Haraway. The striving for universal objectivity should be abandoned in favour of knotting together values and knowledge – toward valuable perceptions of the world. The unwillingness to present such a perspective is a sign of irresponsibility and should give rise to scepticism.

Donna Haraway's argumentation is far from being idiosyncratic. Similar reasoning can be found in Richard Rorty. In his "Solidarity or Objectivity?" he pleads for the primacy of social values over truth claims. "To be ethnocentric is to divide the human race into the people, to whom one must justify one's beliefs, and the others" (Rorty 1985: 13; 1988: 27). Here it becomes even more clear than in the feminist context that in every society – and of course between different societies – there is a manoeuvring space which leaves it open to determine the cultural entity to which a person wishes to address validity claims. The quote should not be taken to advocate decisionism. The context makes it clear that there should be talk in society on what kind of values, ideals, live forms, and environments people wish to base their rational commitments – science included.

It would be worthwhile to consider further challenges to decentering strategies by new forms of centering knowledge to values, experiences, and even interests. I only mention in passing the deep ecology epistemology and other environmentalist approaches which aim at a physiocentric positioning of epistemology. According to Meyer-Abich (1997), the conception of universal justification of objective knowledge turns out to be in fact a very anthropocentric reading of the world. Meyer-Abich outlines anthropocentrism as the belief in the moral right to understand the world as something put to the disposition of knowledgeable subjects. Within this belief system one does not even think to justify knowledge claims and their technological derivates by recourse to anything else than human beings. In the eyes of Meyer-Abich, Michel Serres (1994), and Bruno Latour (2001) physiocentrism is an alternative path of decentering. Human beings have to understand that their privilege is not a special place in the world from which they are able to have an objective point of view. Their privilege is their responsibility to care for the rights and values of the other inhabitants of the world.

I hope to have sufficiently substantiated the point that loosening the fixed couplings between cultures and scientific belief systems does not amount to entering a one-way road to more general justifications of validity. I return to the main argument: The foundation of the sociology of knowledge is not solid but rather like quicksand, at least in a society where sociology of knowledge (and its precursors in philosophy) is present. To be sure, there are always binding forces between social institutions and rational strategies of justification. But these forces do not establish fixed and tight couplings between the institutions of trust and the strategies of knowledge. Options toward more general as well as more specific relations come up and can be realized if they are supported.

The 'Strong Programme' departed from the search for the institutional causes that turn beliefs into accepted knowledge. As Francis Bacon stated long ago, knowledge of causes gives options for action. Certainly the concept of cause in the social domain cannot be taken in its rigid meaning (as necessary and/or sufficient condition of effecting something according to a time independent causal law). But doubtlessly new insights into social mechanisms provide new spaces of action. It is in this context that constructivism has a future as a frame for deliberative forms of knowledge construction and justification.

The scope and impact of deliberative constructivism cannot easily be assessed. Admittedly, there are fields of science where profound changes are unlikely. But the areas of knowledge production are increasing, where agenda setting, goal-orientation, problem solving, and real world experimentation are important. Nowotny, Scott and Gibbons (2001) speak of the contextualization of science and distinguish between weakly and strongly contextualized knowledge. They expect science to move into the direction of increasing contextualization. They introduce the term *agora* to denote a new public space or institutional framework in which knowledge production is shaped. It is in this contextualization of science where deliberative constructivism will play an important role. The keywords feminism, ethnocentrism and physiocentrism and the ideas of situated, embodied, holistic, contextualized, and robust knowledge indicate how value patterns and ideals of knowledge invade the received self-description of science. Furthermore, the increasing relevance of experts in politics and economics indicate the dissolution of the institutional separation of interest and knowledge. The increasing impact of agenda setting procedures for many research fields indicate the influence of relevance criteria on the flow of research money.

It is in these fields that politics, interests, and values partake in negotiating frames for developing new knowledge. These frames determine the institutional conditions of research, participation, justification, acceptance, and use of results. Sceptical scientists certainly fear a decline and corruption of standards, but at this point it is necessary to remember the first lesson of the sociology of scientific knowledge. There are no such standards which are independent of cultural conditions. Even more important is another consideration. Contextualization may well go along with rising standards of justification. Lay people, interest groups, political bodies, and firms can behave much more sceptically than scientists among themselves. It is precisely here where decentering validity claims beyond the institutional limits of the disciplinary cultures can be expected. In fact, they are already visible. An example is the impact of the precautionary principle on trial research concerning the introduction if genetically modified organism in the European community. The legislative means regulating the treatment of uncertain risks associated with new knowledge goes far beyond the standards of justification common among scientists. Or put in the terminology of trust, contextualized science is much more challenged to earn and maintain trust. In the opposite direction, the lowering of justification standards can be observed as well. An example is the advance of non-standard medical knowledge and its acceptance by concerned patients. Here justification of knowledge is restricted to a smaller cultural domain. Something similar can be observed when experts are expected to give advice in complex action fields. The span to be bridged between science-based knowledge – drawing a complete picture of the situation – and suggested measures may be wide, but the necessity to act lowers the standards of justification. Related fields are those where research and social change merge. A prominent example is research on and adaptation to climate change. Here the negotiation of standards is especially visible because a board of researchers has made it its policy to speak with one voice. Cases of less dramatic scope have been considered under the name of real world experiments. Here the standards of validity can come very close to what in science is associated with hypothetical reasoning and recursive learning. Confidence does not primarily refer to the applied knowledge, but to the science based process of getting stepwise closer to a satisfying solution. The variety of fields where the negotiation of standards of justification and the readiness to invest trust in knowledge can be observed is great. It increases the more science penetrates all areas of society. In turn, modalities of forming specific cultures of knowledge and research increase as well.

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The Politics of 'Actor-Network Theory'

What Can 'Actor-Network Theory' Do to Make Buildings More Energy Efficient?[1]

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Abstract

This text discusses controversies surrounding theoretical, practical, and political implications of 'actor-network theory' ('ANT'). Since its inception around 1980, 'ANT' has been applied in an immense number of empirical studies, both within and outside the field of science and technology studies. But it was also rejected as radical chic without substance and/or as theoretically and politically unacceptable in perhaps as many instances as it was accepted. Implicit in both the application and critique of 'ANT' is the assumption that it can be treated as a 'black-boxed' set of notions and rules containing certain strengths and weaknesses. Proposing to treat black-boxed 'ANT' as useful provocation, I discuss what this kind of 'ANT' can and cannot do for me in my own empirical research on energy efficiency in buildings. In the second part of the text I turn from 'black boxed' and well-defined 'ANT' to 'ANT in the making'. In recent and ongoing work Bruno Latour, John Law, Annemarie Mol, Vicky Singleton, and others (in alphabetic order) answer to critiques of 'ANT's' political implications. The authors share an interest in the development of a non-essentialist foundation of politics, which neither turns into crude functionalism nor into hollow relativism. Concluding this text, two of the proposals made here, 'political ecology' and 'ontological politics', are compared and discussed in the context of my own research.

[1] This text would not exist without the constant inspiration and support of my colleagues at the Department of Interdisciplinary Cultural Studies in Trondheim. I am deeply grateful for the hospitality which I have the privilege to enjoy here. Earlier drafts of this paper were presented at the 2004 4S conference in Paris and at the 2004 annual meeting of the Gesellschaft für Wissenschafts- und Technikforschung in Berlin. The underlying empirical research was funded by the Research Council of Norway and The Norwegian State Housing Bank. Finally, I would like to thank the two reviewers for useful corrections and comments and Chris Hassenstab for her friendly help with the English.

There is life after constructivism –
constructive STS studies contributing
to a better society.
(Bijker 1993, 132)

Only a Sith deals in absolutes.
Obi-Wan Kenobi

1 Choosing the Genre

One of the founding fathers of 'actor-network theory' ('ANT') teaches us that it is not by coincidence that academic writing usually starts by evoking a 'well established fact'. This fact, if really 'well established', according to Bruno Latour (1987), acts as black box, which is built on other 'well-established facts', and so on. The more boxes there are stapled onto each other the more difficult it becomes for the dissenter to disconnect them all and to expose their inner workings. And, Latour maintains, if there is no dissenter there is truth.

In this text, although I will also describe how 'ANT' is used as a black box, I will not naively treat it like one. I cannot trust that this box will remain sealed. This is partly due to its lack of general acceptance, even in my field – Science and Technology Studies (STS). But this is also very much in line with what those who are usually treated as proponents of 'ANT' say about what 'ANT' is and what it can be. Along with them I am equally interested in 'ANT in the making' as in 'ANT' as a tool, which can be applied to understand the world.

Through use of one of the more notorious principles from 'ANT' I will observe 'ANT's' "world-building activities" (Latour 1999b: 21) when and where they happen. And there are, of course, many more occasions where they happen than just when the accredited proponents of 'ANT' talk or write. I will start with those who perform 'ANT' and who do so claiming to know what it is they are performing. I found such accounts of what 'ANT' is good or bad for in its 'applications', such as in empirical studies – mostly from outside STS – which in fact use 'ANT' as a 'black box'. But even more sure about what 'ANT' can do and particularly about what it cannot do are the critics of 'ANT'. Their descriptions are the second source I draw on for looking at 'ANT' as a properly bounded object. All these accounts – be they angry or sympathetic – have one effect: they stabilise 'ANT' as ANT (without single quotes), as a network of people, techniques, and material institutions, which is able to travel unchanged through time and space, also outside STS. Only if there is ANT, can it be applied or criticised. This particular version of 'ANT' is what I will explore in the first part of this text, which will be concluded by discussing what it can do for me in my research on energy efficient buildings.

Then, I will turn to those who are criticised and held responsible for 'ANT'. It is a very different 'ANT', which we encounter here. These are scholars who attempt to publicly 'bury ANT in its coffin' (Latour 1999a), declare 'ANT's' adoption to be 'optional', and its perspective to be 'multiple', 'mobile', 'mutable', 'contingent', and 'ambivalent', to quote Vicky Singleton's answer (1998) to the critique (Radder 1998) of one of her analyses (1996). Again I will ask what this unstable 'ANT' can do for me, but since I am dealing with 'ANT in the making' then, this cannot be an application or critique from outside any longer. Here I accept the invitation to partake in the creation of 'ANT' and will try to find out what it could mean to me in my work.

Part of this trial has already begun. It should be clear by now that from the very beginning I was using an Actor-Network perspective on 'ANT' itself. I invite the reader to see which kind of insights such a perspective can render.[2]

2 Criticising and Applying 'ANT'

The most prominent example for 'ANT according to its critics' is probably the so-called "epistemological chicken debate".

According to the work of Collins and Yearley (1992) 'ANT' is only 'seemingly radical'. They describe the extended symmetry principle[3] of the 'The French School' as requiring great daring, but as actually being "essentially conservative – a poverty of method making it subservient to a prosaic view of science and technology" (Collins and Yearley 1992a: 323). In a footnote (note 14 on pp. 315-6) they take a couple of quotes from Callon's classic text on the scallops and fishermen of St Brieuc Bay (1986) and rephrase them in 'less radical' wording. They state that there is nothing new in it but the vocabulary. In a later article, which seemingly closes the debate (Collins and Yearley 1992b), they seek a middle ground and present themselves as pragmatic scholars more interested in changing 'the relationship between science and technology and other cultural endeavors' and understanding 'what can and cannot be delegated to machines' (ibid.: 388). 'ANT' – according to them – is much more interested in establishing a consistent system, which once established would not make a difference at all (ibid.: 384). Though more related to what Latour or Callon actually write, this also alludes to 'ANT's' pied piper-like attraction, stressing the emptiness behind the daring attitude.

Bruno Latour is often introduced as brilliant raconteur, whose style is "extremely entertaining and creative, but it does not always bear close scrutiny when rigour is sacrificed for repartee" (Scott 1991: 11). In invectives like this, the object 'ANT' becomes an instrument in the cunning hands of seductive Frenchmen, who seduce through 'sparkling writing' (Collins and Yearley 1992b: 384, note 10) with a 'French accent' (Fuller 2000: 8), promising conceptual unity which – stripped of its rhetoric – leads to nothing. The emperor has no clothes and nobody, according to 'ANT's' critics, is able to see that.

So let's turn to the seduced. In a quick and dirty survey of 18 recent (1998-2004) applications of 'ANT' from outside STS, I encountered a limited number of reoccurring themes and patterns of the use they make of 'ANT'.[4] The most obvious use of 'ANT' in these papers concerns the notion of networks. Here, we find a tendency to focus on *social* networks so that the analyses turn into stories of how an actor was included in or excluded from a social network, and how technologies and other non-humans were involved in these struggles (e.g., Colwyn Jones

[2] Unfortunately, due to the approach I have chosen here and also to restrictions of space I cannot provide a proper introduction to 'ANT'. Therefore, I have to assume at least some knowledge about 'ANT', especially in the second part of this text. As introduction I usually recommend John Law's easily accessible 'Notes on the theory of the actor-network' from 1992 (http://www.lancs.ac.uk/fss/sociology/papers/-law-notes-on-ant.pdf). A slightly outdated annotated bibliography of 'ANT' can be accessed here: http://www.lancs.ac.uk/fss/sociology/css/antres/ant.htm (last update 2000)

[3] Latour (1993) extends Bloor's symmetry principle, which states that 'all beliefs are on par with one another with respect to the causes of their credibility' (Barnes and Bloor 1982, 69; Bloor 1976) to also comprise a symmetric treatment of humans and non-humans.

[4] This is and cannot be an exhaustive study of 'ANT's' reception. I invite the readers to compare their own encounters with 'ANT in the wild', with the observations presented here.

and Dugdale 2002; Davies 2002; Harrison and Laberge 2002; Pouloudi et al. 2004; Zackariasson and Wilson 2004). Another, but related storyline I encountered in these applications, works the other way around. Here 'ANT' is used by technologists dealing with technical networks to include humans – the users – into their research (e.g., Atkinson 2000; Braa and Hedberg 2002; Dunning-Lewis and Townson 2004/2005).

This is, mind you, not to say that they apply 'ANT' in a wrong way. The argument is rather that we can ask along with Collins and Yearley whether this kind of research, which uses 'ANT', can be rephrased using more traditional sociological language without losing anything but the radical chic.

Is, thus, 'ANT's' most important contribution its use of the buzzword 'network'? Is it really just rhetoric? Not every critic rejects 'ANT' as a whole. Dick Pels, for instance, seeks a middle-ground between 'ANT's' position and other more traditional conceptions. He tries to define where 'ANT' has gone too far, and where exactly it might be better not challenge established notions about the world. Pels, thus, ascribes the object 'ANT' the role of an useful agent provocateur, who has to step back after "the Great Wall is levelled in order to make room for many lesser fences" (ibid.: 296-7). According to him it "was the radical demarche advocated by Callon and Latour concerning the dualism of Society versus Nature", which opened up room and which now can be filled with 'lesser fences' when we "settle with a weaker asymmetry, or a weaker notion about the permeable boundary running between humans and nonhumans." (ibid.: 297)

Pels' project is of interest here, because it makes explicit another use of 'ANT', which can be found in applications from outside STS. Using 'ANT' social scientists are allowed to talk about technologies and technologists likewise become entitled to talk about humans. The Great Walls between the social and the technical, but also other dualisms, such as the one between macro and micro, between agency and structure are levelled by 'ANT' resulting in openings for crossing those boundaries. And indeed, turning again to applications, 'ANT's' anti-essentialist critique of dualist thinking is most often mentioned. Typically, this is evoked in order to correct a perceived one-sidedness in the respective field. David Featherstone, for instance, studying an embargo enforced on shipping on the Thames in 1768, uses 'ANT's' rejection of the global-local divide to "unsettle a tendency to confine subaltern politics within bounded spaces and open[s] up possibilities for following more dynamic trajectories of subaltern political activity" (see also Jenkins 2000, 308; Featherstone 2004, 702). The structure-agency dualism is critcised by way of 'ANT' by Anna Davies (2002, 190) and Jaquelin Burgess and her colleagues (Burgess et al. 2000, 123) add 'nature or society', expert or lay knowledge', and 'science or culture' as dualisms which they plan to overcome by means of 'ANT'.

The critique of any kind of essentialism is of course the very definition of constructivist thinking. Also more specifically 'essentialist dualisms' have been criticised at length within other theoretical approaches. For the 'science or culture' chasm one could refer to almost all of STS theory including Collins and Yearley. For rethinking the 'expert or lay knowledge' dichotomy Bloor's (1976) symmetry principle works well enough. The 'agency or structure' problem can be solved for instance with the conventional sociology of Giddens' structuration theory, and the literature trying to overcome the global-local divide is abundant.

So, what explains 'ANT's' appeal to be used against dualist notions? I propose that it is its promise to get rid of all of them – at once – while providing a tool which can be applied pragmatically and intuitively. Particularly 'ANT's' network metaphor guarantees its applicability, making it an anti-dualist 'Swiss Knife' useful as agent provocateur in a broad variety of settings – from 'wet-land agri-environment schemes' (Burgess et al. 2000), 'inter-networked after-sales service' (Zackariasson and Wilson 2004), 'computerized medical record systems' (Lehoux et al. 1999), 'Australia's country towns (Herbert-Cheshire 2003) to 'meat consumption and meat production in the U.S.' (Gouveia and Juska 2002), and 'reflex anal dilatation' (Collins et al. 1998).

For now we can conclude that 'ANT' – when used as black box – provides handy tools, which help to criticize dualistic thinking. But where does that lead? Exactly what type of descriptions of the world are we capable of making with this kind of black-boxed 'ANT'?

3 Making Buildings More Energy Eefficient – Black Boxed 'ANT' in Action

At this point I leave applications and critiques by others and turn to my own research. I have a particular problem in my work and I want to know what the anti-dualistic 'ANT' can do to help me.

Since 2002 I have worked in an interdisciplinary research project aiming at improving energy consumption in office buildings. The research group consists of architects, engineers and STS scholars, all in all some 30 researchers of which a majority has already worked as consultants. The starting point of this project was the recognition that energy consumption for basic services in buildings is high and still increasing, despite the fact that there exists technology which could contribute to dramatic savings. There is hardly any reason not to implement these technologies, given considerable cost-savings, political considerations and overall environmental benefits of decreased energy usage.

One main focus of the project is to improve technologies like solar cells, CO2 heat pumps, insulation, energy storage, and better use of daylight through better building envelopes. Additional to the research on individual technologies, the project deals with missing integration, missing user acceptance, and lack of implementation strategies. As a STS researcher I am responsible for two work packages, one about users and one about implementation. Though the bulk of work is confined within the individual work packages, which are organised along disciplinary boundaries, there are regular common activities like workshops and presentations. I made it a point to attend as many as possible of these and visited 13 of my colleagues in their offices interviewing them about what they thought what the actual problem was. At these occasions it soon became clear that there are at least two very different types of experts (see also Berker 2005).

On the one hand there are those who were principally open to a broad inclusion of every other relevant group. One of the actual techniques promoted in this group is called 'integrated design process' (Larsson w/o year), which calls for a more thorough planning with the inclusion of several different groups in an early phase of the building project. Technology is involved in this as a tool for collaboration as well as through a couple of normative notions about how a more energy efficient building should look like, when it is designed in an integrated way. Particularly the building envelope and the physical location of the building is relevant here, both in terms of energy consumption and in that it is a variable, which is difficult to change at a later date. According to my colleagues, 'integrated design processes' can lead to buildings that hardly need any heavy HVAC[5] installations. Ventilation and cooling is taken care of by an intelligent setup of the building from the very beginning. One underlying set of values evoked in this group concerns 'natural' ways of building. This means above all that fewer technologies should be involved. Examples for these 'natural' technologies are 'natural' ventilation[6] utilising natural draught and the more efficient use of daylight.

The other group is much more hesitant when it comes to including other groups into the design process. In a set of techniques bundled under the label 'continuous commissioning' they hope to improve building automation using computerised real time surveillance of every single parameter which is relevant to energy consumption and comfort in the building. These systems are 'intelligent' in that they learn from the occupants and adapt the parameters, continuously guarding the optimal state of the whole system. Manuals describing the process (e.g. FEMP 2002), recommend some limited inclusion of local technicians, but the actual 'continuous commissioning' is done by a specially trained engineer. Technological choices following from this, favour HVAC systems which are as reactive to new target parameters as possible, so that the theoretical optimum is reached quickly. The notion of 'natural' as it is present in the first group, is missing completely here.

A lot of discussion between both factions circles around the question of how much technology is necessary in order to reach a 'good', i.e. comfortable, functional and energy efficient building. The rift between two groups, I was told, is common in the building sector and can be held responsible for poor integration between technical installations and the rest of the building, which should be aligned in order to provide for optimal energy efficiency. The usual way of dealing with different goals is strict division of labour, and to a certain extent this was also what happened within our project. When the project, after three years, entered its final phase this current year, the project leader decided that money should be provided as extra incentive for interdisciplinary work, which lead to pragmatic collaborations for instance around how 'continuous commissioning' and 'integrated design' could complement each other.

To conclude the description of my participant observations: Technologies and techniques which could help to save energy do not travel well from the laboratories to a building's everyday life. Additionally there are two visions of how these technologies and techniques are to be implemented in real life buildings and building projects. Thus, we have two different visions of energy efficient office buildings.

So, what can 'ANT' do for us in this situation? According to the key tenets of 'ANT', the two visions present in the project are political in that they reflect an attitude of relating the environment, technologies, experts, janitors, building owners and occupants to each other. More specifically described in the jargon of 'ANT': these visions each consist of notions of how the 'actants'[7] present in the building (and also: being the building) should be 'translated'. Put this way, the antagonism between

[5] HVAC=Heating, ventilation, air conditioning

[6] Natural ventilation is hardly ever 'natural' in the sense that it does not involve mechanical ventilation; the correct term would be 'hybrid' ventilation, but 'natural' is used equally often.

[7] 'Actants' are humans and non-humans provided with their agency by the relations in which they exist. This term was introduced to replace the notion of 'actor' which is usually imagined to be human.

the two groups within the project (and presumably also beyond it) cannot be about 'nature' versus 'technology'. The second group 'enrolls'[8] CO_2, how quickly it heats up and how long it stores energy; the first group tries to capture draught's cooling powers. No difference here in the eye of 'ANT', but different strategies whose outcome will depend on whether the 'enrolment' of a sufficient number of human and non-human entities will succeed or not. And this is actually the way my colleagues deal with the conflict as well. In those interdisciplinary groups, which were installed recently, pragmatic negotiations take place about which human and non-human entities 'enrolled' by one group can be useful for the other.

In 'ANT's' terms the goal of establishing new and more energy efficient technologies and practices is the same as establishing new irreversible translations of as many heterogeneous 'actants' as possible (building owners and CO2 and draught and end users and janitors, etc). Here lies one contribution of 'ANT', to remind the engineers and architects of what they are doing anyway i.e. relating a broad set of things and people to each other. And this is also the first use of 'ANT', which was presented above as strategy found in applications from outside STS. If we refuse *a priori* distinctions like the one between nature and technology, social science is no longer forced to impose categories onto the practice of the actors. This is, in the situation given here, particularly useful for taking part in the project's everyday work. Since there is nothing wrong with more energy efficient buildings – quite the opposite – together we now can build a brighter future.

[8] In 'ANT's' jargon this word is used to describe a crucial stage when a new 'actant' is included into a network (see Callon 1986).

But are we not loosing something here? Is this not a 'poor method' which looses any specificity from a social science approach? Are we not giving up valuable distinctions, such as for example nature and technology? Where is the critical edge?

In fact, maybe there is actually not much to be said at all without taking at least some dualisms for granted. Latour's writings are full of modest gestures pointing into this direction:

> "ANT does not tell anyone the shape that is to be drawn – circles or cubes or lines – but only how to go about systematically recording the world-building activities of the sites to be documented and registered" (Latour 1999b: 21).

This conceptual modesty suits the theory very well, which, first and foremost, promises to follow the actors, but it is also one of the most criticised aspects of 'ANT'. Here, my doubts are shared by critics of 'ANT' to which I will now turn again, preparing the ground for the second part of this text:

Steve Fuller identifies 'ANT's' modesty with "the Mode 2 conception of policy-driven 'postdisciplinary' research, which welcomes the university's permeability to extramural concerns." (Fuller 2000: 9) He is not exactly fond of Mode 2, which, according to him, serves 'more centrally located clients' and delivers 'on a platter those on the social periphery' (ibid.). He says:

> "Under such a regime [of Mode 2 contract research], if researchers do not provide quality information about their subjects to clients, they will be quickly replaced by someone more willing and able to do so." (ibid.: 11)

The argument that 'ANT', with its disregard for 'broader patterns', always has to take 'the winners point of view' (Radder 1992: 161) has two aspects. First, it is part of a larger group of political objections, which are only then valid if shared political goals can be assumed, e.g. not to betray 'those on the social periphery', not to take 'the winners point of view', and instead to take an openly "evaluative stance towards social consequences of technology" (Winner 1993: 368), for instance against the "militarization of science and technology, especially in this century" (Radder 1992: 151; see also Winner 1993: 370-1). Second, it accuses 'ANT' to impose restrictions on the researcher, which render the research irrelevant, because s/he can only know what the actors know, and has no presumptive categories which could help to see the researched in a new light.

Turning to 'ANT' and how it is performed in its applications also supports this kind of critique. The majority of the texts that were presented above have 'management', 'planning' or 'organisation' in its title or in the name of the journal where they were published. I have insisted on the productive aspect of these efforts to make management of humans and non-humans more effective, which I found in the deconstruction of dualisms. However, this does not help in the situation in which I find myself in the Smartbuild project. Ironically, the only dualism I can think of here, which may be worth deconstructing, is the one between the technical and the social. It can be said that particularly in 'continuous commissioning' there are fantasies of managing people through 'smart' technology and only through technology. The humans within the building then become reduced to being a problem, dubious delegates, who should be controlled by 'smart technology'. To reveal this one-sidedness is a line of reasoning I have used before (Berker 2005). Overall, however, we have to accept that the heterogeneity of energy efficiency in buildings is sufficiently acknowledged in the work of my colleagues. The relative strong presence of social scientists (including myself) shows that besides the technology there is a genuine interest in non-technical aspects. This hardly comes as a surprise, since 'ANT' has taught us that successful engineering is always about managing both humans and non-humans. And finally, as a researcher who works at a STS department with the name 'Center for Technology and Society', I am hardly crossing disciplinary boundaries, when I do research on technology *and* society. The anti-dualistic vigour of 'ANT', thus, cannot contribute much for me in the Smartbuild project.

Is helping to organise, to plan, to manage energy efficiency more efficiently through engineering the only thing we can learn from 'ANT'? And if so, what do I have to contribute which is different from the knowledge how to engage as many humans and non-humans as possible to support the diffusion of my colleagues' favourite technologies? Do I find the black box empty after others have opened it before? Where is my own vision between or beyond the vision of architects and engineers? This is the problem I am facing and it seems that black boxed 'ANT' does not lead me closer to a solution.

4 'ANT' as Critique

Judging from their reactions to this kind of critique – maybe from the fact that they react at all – we can assume that those who are criticised – Latour, Callon, Law, and other scholars held responsible for 'ANT' – are not content with this version of black-boxed 'ANT'. In the second part of the text I turn to recent (and not so recent) developments of 'ANT', which answers the critiques presented in the first part. 'ANT' is shifting here; it becomes less clear what it actually is and when it is revised it is also becoming polyphonic. I will focus on two of these versions of 'ANT in the making', and try to make clear where they are different, and where they agree.

4.1 Political Ecology According to Due Process

First there is Bruno Latour's project of a political ecology (Latour 2004). When things and animals and humans all have the same ontological status then two directions are possible. First, humans are treated like things, which is the negation of any politics. Or secondly, human rights could also be extended to non-humans. The latter option is exactly what Latour's political ecology does, extending of the liberal right of representation to everyone and everything (Lee and Brown 1994: 788).

According to Latour we already live in the age of political ecology:

> "Not many years ago, when we were contemplating the sky above our heads, we thought of nothing but matter and nature. Nowadays, when we look above our heads, we watch a sociopolitical imbroglio, because, for instance, the depletion of the ozone layer brings together a scientific controversy, a political dispute between North and South, and gigantic strategic moves inside industry." (Latour 1994: 796)

For Latour the task is now to deal with these socio-political imbroglios without taking shortcuts following outdated divisions between the social and the natural, values and facts, and of course humans and non-humans. Latour (2004) describes a new parliamentary order in which those representing nature and arte/facts (scientists) and those representing humans and values (politicians) work closely together. Together with other groups like economists, diplomats, and also sociologists they have different tasks in this common enterprise, whose goal is to include facts and artefacts in an open way into the 'collective'[9]. This basically means to assign them a place, meaning, and value after an evaluation done in consultations by the members of the collective. Latour insists that all this has to be done according to 'due process'[10]. This is what distinguishes his vision from the status quo, and could therefore also be called his political message: Neither politicians, nor scientists, nor economists, nor any other group should be allowed to make decisions on their own as to which fact or artefact will have which place, meaning, and value in the world we share. All these different groups become involved in a process, which he calls the 'collective'.

Is this the answer I was looking for? In Latour's political ecology, new and more energy efficient technologies would have to go through the same 'due process' as any other object. According to Latour, there are constantly new challenges to the 'collective'. Those challenges in my example would be the threat of climate change as well as the general depletion of natural resources. All of this is closely related to energy consumption among others in buildings. The question now is what this means for the 'collective' or whether it should meaningful at all. There are many different versions of what to do and all of them ground in a particular understanding of what is 'actually' happening. Within the Smartbuild project we have seen two different proposals, but there are of course, many more. Latour calls this stage the stage of 'perplexity'. 'Due process', now, is the method with which the 'perplexed' collective 'in due course' finds ways to deal with them, discussing options, risks, and value hierarchies, and finally building institutions

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[9] Latour defines the 'collective' as the *process* in which associations between humans and non-humans, facts and artefacts are collected (cf. Latour 2004: 238).

[10] This term is borrowed from the Anglo-Saxon judicial tradition (cf. Hyman 2005 for an introduction).

fixing what it means to live together with climate threat and no oil left. A process is legitimate, according to Latour, when it does not leapfrog over important steps, like the one he calls 'consultation', where relevant other 'actants' are heard, and 'hierarchy', where the new 'actants' are ordered according to their value for the collective.

In the light of Latour's 'due process', the Smartbuild project's task cannot be to foster the new objects from the cradle to the grave. It would be exactly what Latour describes as 'undue' process if the group of scientists gathered and, provided with laboratories and other more or less powerful tools, would seek shortcuts excluding other relevant entities like existing buildings and their installations, other routes to energy efficiency developed at other places, janitors, end-users and so forth. These have to be included and altogether the collective will decide which of the new objects proposed by the engineers, social scientists and architects of the Smartbuild project will finally be implemented. This is 'ANT' turned politically democratising the business of 'heterogeneous engineering' (Law 1986).

Thus, the members of the Smartbuild project are once again reminded that they are dealing with a broad set of entities, and so far there is nothing new in this. The good news however, is that now they are not alone, that they are relieved from the duty to do all the things that they are not trained to do. Instead they are supposed to do what they are good in, using their instruments to continuously displace and change their point of view (Latour 2004: 138) in order to see and record those new things and relations, which the rest of the collective is not able to see. Scientists, maintains Latour, have the power to discover new entities before they are well-defined members of the collective. Their task is to be the spokespersons for these entities and to present them to the collective, which then through consultations has to come to grips with how to proceed. In this model a social scientist's task is not to know what, for example, energy efficiency is or should be in lieu of the actors,

> "[b]ut to inquire into what binds us, we can count on the human sciences' offering the actors multiple and rapidly revised versions that allow us to understand the collective experience in which we are all engaged." (Latour 2004: 225-226)

In this sense, besides producing these 'rapidly revised' versions, which will help 'us' to understand how 'we' can build and live more energy efficiently, it could also be my task in the Smartbuild project to remind my colleagues not to give in to expectations which drive them to *build* the houses which they are now *envisioning* closing the debate all too early.

4.2 From Warm and Light Reversibility to the Margins

The journey is not yet at an end, though. We have seen that Bruno Latour, in line with his model of a political ecology, invites us to revise his proposition. Drawing on other versions of 'ANT's' political project, proposed by Bruno Latour, Michel Callon, John Law, Vicky Singleton, Annemarie Mol, and others (in order of appearance), I will use the reminder of this text to do exactly that.

The common starting point for these alternative propositions is a different understanding of the relation between reversibility and irreversibility of translations and, thus, about stability of associations and their change. Half of the 'due process', which Latour is advocating, is about destabilisation of established associations in the stages of 'perplexity' and 'consultation' and thus about destabilisation of the collective itself. But the other half is about re-ordering and stabilising the collective and the appealing entity, which in

'ANT's' relational logic is one and the same thing.

The question of how change and stability come about is already an important topic in the early life of the object 'ANT'. In 1981 Michel Callon and Bruno Latour place themselves (the sociologist, that is):

> " [...] in the warm, light places where black boxes open up, where the irreversible is reversed and techniques return to life; the places that give birth to uncertainty as to what is large and what is small, what is social and what technical." (Callon and Latour 1981: 301)

This is the foundation of 'ANT's' anti-dualistic perspective: when entities are not yet fixed, nothing can be taken for granted about their essence, and those who are dealing with them, heterogeneous engineers, but also sociologists and others, make a mistake when they treat them as if they were already stabilised as nature or as technology or as social, and so forth. A text by Callon – published ten years later – moves this description of the theorist's place over to a distinction between different types of networks distinguished by the degree of their reversibility. He states that some networks contain more of these 'warm and light' places than others, while some networks are more stable than others:

> "[...] the more numerous and heterogeneous the interrelationships the greater the degree of network coordination and the greater the probability of successful resistance to alternative translations." (Callon 1991: 150)

This kind of irreversibility, according to Callon, is always accompanied by standardisation and normalisation of interfaces which enable the heterogeneous associations to resist alternative translations (Callon 1991: 151). There are highly standardised networks in which a great number of heterogeneous actors are completely and thoroughly acted by the network. Not much to see for 'ANT' scholars here but a lot of tightly locked deep black boxes and powerful 'immutable mobiles' which enable 'action at a distance' from powerful 'centres of calculation'. But Callon (1991: 152) maintains that there are also networks where translations are constantly done and undone. These networks are characterised by "strategy, the negotiation and variation of aims, revisable projects, and changing coalitions." (Callon 1991, 154).

This is the background for Susan Leigh Star (1991) when she criticises 'ANT's' politics in a text published in the same collection. She argues that irreversibility is never reached for every node of a network, that "[s]tabilized networks seem to insist on annihilating our personal experience, and there is suffering." (Star 1991: 48) In this quote Star, who is representing symbolic interactionism in STS (cf. Clarke and Star 2003 for an overview), reintroduces the human subject through the container of personal suffering. The quote, however, also contains a notion, which can be turned critically against 'ANT' without leaving its premises. If no network is ever stabilised for every 'actant', then there are always groups of entities at the 'margins'[11], which then have to deal with the black boxes, which were closed in ways that do not allow them to become 'proper' entity.

The political question now is, how 'ANT' deals with these unstable regions within stable networks. The process which Latour calls the 'collective' is kept moving by exactly the tension between those already included into the collective and those outside, which are 'appealing' to the 'collective'. 'Due process' means that there is a time in

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[11] For more background on Star's critique see also her work on boundary objects, which *per definitionem* are marginal (Star and Griesemer 1989).

the early life of a new member ('actant') of the collective, where it is not yet fixed. But later on there is also a time – the time of 'consultation' – where the 'actant' becomes translated into something more fixed and then, after it has been placed in a 'hierarchy' is embedded in an irreversible way ('institutionalised'). 'Due process' means exactly that no shortcut is taken from reversibility to irreversibility (or vice versa), that the transition between these two states takes place in an open and politically legitimate way.

Here Star's objection is valid. What about those members of the collective that are neither outside the process called the 'collective', nor inside, but systematically and continually stuck between reversible and irreversible translations? In other words: what about the places, where associations neither yet exist nor are successfully stabilized, which are not beginning, but not ending either?

4.3 On being Allergic to 'Continuous Commissioning' and 'Integrated Design'

Susan Leigh Star uses her own allergy to onions as an example. This constantly causes trouble for her since she is forced to live in networks which are stabilised around *not* being allergic to onions. We can now ask who and what is excluded by the visions of my colleagues. Who and what would be incommensurable to the versions of networks they suggest?

Both engineering visions, which were presented above, are equally about 'human/nonhuman mingling', but there are conspicuous absences of certain humans and non-humans.

'Continuous commissioning' does produce a lot of data from sensors all over the building. To analyse this data and to draw the right conclusions, special expert knowledge is necessary, which my colleagues have, but no one else has. Particularly absent is local knowledge owned by building managers and occupants and which usually is difficult to access by external experts. In 'continuous commissioning' this is replaced by data, which is suitable for advanced methods of calculation.

The architects' idea to focus on thorough planning in early stages of the process ('integraded design') excludes systematically all those groups, whose possible contribution is based on daily experience within the building, again mainly maintenance personnel and occupants. This is firstly because early in the building process it is not yet clear who exactly will move in. But there is also a more fundamental problem having to do with the different character of knowledge needed early in the process. For instance, the literature in participative design (e.g. Kensing and Madsen 1991; Greenbaum 1993) notes that it is difficult for lay people to read and understand abstract representations, such as construction drawings, without special training. And the more decisions which are taken early on, the more the building will be the domain of experts and less controlled by those living and working in the building at a later date.

All this is perfectly 'normal' in terms of 'ANT's' description of how translations are stabilised. Both visions of energy efficient buildings allow experts who reside in 'centres of calculation' to control basic parameters of the building, a control which was before in the hands of locals such as for example janitors or the occupants.

That this can be a source of tension and conflict became particularly clear when 'continuous commissioning' was presented at a workshop earlier this year, where a large group of facilities managers and janitors were present. It was obvious that they felt threatened, because their expertise of working with today's HVAC systems would be rendered worthless when these new systems were introduced. When they understood that 'continuous commissioning' is not yet ready for broad implementation their reaction was a mixture of relief and malice. This kind of passive resistance is, in fact, something architects and engineers complain about a lot. Those janitors in the accounts of experts become an inert mass which is resistant to *any* change.

The solution, according to Latour's political ecology, is to engage them, to make them the local associations, which are needed to build the larger ones. If that does not succeed, then *either* they *or* the 'continuous commissioning' and 'integrated design process' have to leave the 'collective'.

But with Star we can now ask, what about those janitors, who live on in the niches and gaps that exist between the newly established facts and artifacts, after 'continuous commissioning experts' have replaced their function? Even though they may be too old to learn the new routines they do not just disappear. Or, if 'continuous commissioning' and 'integrated design' are successfully obstructed by the resistant locals: what about innovations which never fully succeed, but which do not vanish either? More generally (and more solemnly): what about those who hardly survive at the margins, those who suffer from mysterious diseases which do not appear in treatment schemes and never will, those who would love to be proper members of the 'collective' (even as a patient in one of its institutions) if they only could manage to fit in? Political ecology à la Latour only knows of them that they are 'failed' objects, which are encouraged to try again: "It is sad, but in 'due process' it was decided that energy efficiency is a greater good than your expertise, you see?"

Those failed objects may vanish from the collective's (bad) conscious but they do not stop to exist and 'there is suffering', which is 'othered' in political ecology once more. Political ecology in its desire to pacify inclusion and exclusion – to say it mildly – does not understand these objects at the margins very well.

4.4 New Objects: Fires and Fluids

This critique of 'ANT's' difficulties with the Other is not new, its most poignant version perhaps being Lee and Brown's lucid analysis of 'ANT's' inner workings from 1994. I tried to argue in the previous sections that political ecology's inclusion and exclusion in 'due process' cannot be a satisfactory answer.

But there are other answers. Referring explicitly to Star's argument, John Law (2000) suggests that early 'ANT' followed its research objects, 'heterogeneous engineers', too closely, so that those excluded by these system builders would become excluded one more time in the description made by the social scientist. He concludes that 'ANT' indeed was too much interested in functional networks, which then only can be analysed as a success or a failure.

The question he asks then is whether this kind of crude functionalism is a necessary result of abandoning fundamental categories like nature, society, and so forth. His answer is "no". He finds visions of a "non-foundational but material relationality that is not functionalist" in Donna Haraway's work (1988; 1991b; 1997), and also in Annemarie Mol's description of multiple bodies (Mol 2002). This has since become the starting point for a quest to find a way to describe other kinds of objects, which – according to 'ANT's' relational materiality – also means to describe other kinds of associations.

In terms of reversibility/irreversibility, the task is to find a way to understand translations which are reversible, but which are irreversibly so, constituting objects which are neither stabilised, nor fractioned into an arbitrary multiplicity. John Law and Vicky Singleton (2005) call two of these object types 'fluid objects' and 'fire objects', leaving open the question if there are more types. They are best described by example. There is first a 'fluid' technology, as exemplified by the Zimbabwean bush pump, which was analysed in depth by Marianne De Laet and Annemarie Mol (2000). The pump was designed by an engineer, but he has written its adaptation to its surroundings into the apparatus. Its parts are easily replaceable and can be patched with other unforeseen parts. Additionally, the pump is designed in a way so that the respective local community is actively involved in every implementation and in maintaining the pump. The engineer stays actively involved in the development and includes improvements he observes. In 'ANT's' terms this pump is not an 'immutable mobile' but still traveling while adapting its shape to the surrounding. This is its fluidity, which gives it a certain degree of multiplicity, but not in a way whereby it looses its shape completely. It is not *one* but neither *many*.[12]

The other kind of objects, called fire objects, also travels. But it does so in unpredictable, disruptive, discontinuous ways. The example for such an object, which is used by Law and Singleton (2005) is alcoholic liver disease, which they found to be defined by a couple of 'generative absences'. They found that alcoholic liver diseases in practice are constituted by absent alternatives imagined by the practitioners (e.g. abstinence or hard drug abuse, etc). Another generative absence is that the therapy depends on absent conditions outside the reach of those who want to help, for instance, a satisfying social life or work. And finally there is the absence of alcohol itself, which is generative in practices surrounding this disease. All this makes alcoholic liver disease a 'messy' object (Law 2004), which is difficult to study and understand, because not only the practitioner but also the researcher deals with absences s/he cannot know about, but which are constitutive nevertheless.

The difference between fluid and fire objects is that fluidity presupposes that the absent Other is smoothly included in a controlled way (the engineer is still there somewhere), whereas in the case of the fire object the Other is taking control over the object in an unpredictable way.

Turning to my project for the last time, energy efficiency can be described as fire object, because it is generated by the *absence* of energy consumption. Therefore, efficient practices 'in the wild' appear unpredictably here and there. Both versions of energy efficiency, which were presented above, exclude the locals and their specific knowledge trying to replace them by technical and organisational means. According to them, the same technology, the same strategy should be applied in every building.

However, the uses of energy are maybe too multiple, too uncertain, too open-ended, to be thoroughly controlled from afar. We may therefore ask if fluidity would be a 'better' way of pursuing energy efficiency? In terms of calculable efficiency the answer is probably "no". A perfectly aligned network of all relevant 'actants' will in fact be energy efficient. Then, energy efficiency is turned into a proper object and other entities, such as for instance the local janitor and his/her knowledge are excluded from the associations which constitute the building. Those low-energy or even zero-energy buildings, which already exist today, in which everything revolves around energy efficiency demonstrate exactly this: that cases where energy efficiency is aligning the building's contingencies and

[12] 'Fluid objects' share this feature with 'boundary objects' (cf. Star and Griesemer 1989). The focus, however, is not on how these entities relate social worlds to each other, but on how their fluidity allows them to travel through ever-changing associations.

multiplicities are – energy efficient. In real world buildings this is not the case and therefore a more fluid approach may have its virtues, dealing better with the fire object energy efficiency, which slips through the fingers of those, who wish to make it a constant, definite, and unambiguous entity.

5 A World of Bastards?

In the ontology of Latour's political ecology, failed objects are expelled during the course of the 'due process'. Distinguishing between different kinds of 'failed' entities and elevating them to the state of 'proper' objects with defined traits – such as fire objects and fluid objects – we postpone their eviction.

Fire objects, despite the difficulties one encounters trying to account for something which is defined by an absence, do not have to be a bad thing. Near the fire, where bastards of reversibility and irreversibility thrive, there may also be 'warm and bright' places still to discover. Fluid and fire objects may be special cases, which call for special attention. John Law, however, argues through reference to his own and to Annemarie Mol's (2002) research, that the inconstancy, multiplicity, and indefiniteness (Law 2004: 145) of the Other can be found everywhere in real-life practices, which are, therefore, in principle 'messy'. According to him this 'messiness' a by-product of Euro-American metaphysics, which defines 'proper' objects as definite, constant, singular, or in other words tries to convert the bastards to legitimate children and does exclude those who won't fit into the picture. This leads him to call for new methods of scientific work that are able to deal better with this kind of Otherness without trying to extinguish it from both method and the real world.

To account non-inclusively, non-exclusively, and non-instrumentally for the often surprisingly robust bastards of the reversible and the irreversible gives us access to a whole new world of objects, which were invisible before. When we are looking for them, we suddenly encounter a host of bastards, such as *ad hoc* improvisations, which sometimes last longer than any carefully crafted 'immutable mobile' (and nobody fully understands why) or 'zombie objects' which should be dead but do live on, because they are just too monstrous to die.

Whether we want a world ruled by definiteness, constancy, singularity or – on the contrary – a world, which is filled by indefinite, multiple, ever-shifting bastards, all this becomes a question of 'ontological politics' in the end. This is Annemarie Mol's (1999) term describing the relation between ontology and politics, which becomes a relation of mutual constitution if constructivist non-essentialism is taken seriously. John Law calls a politics which aims at definiteness, constancy and singularity a "class politics of ontology which is bad" and continues to say that "[g]reater permeability and recognition of fluidity and all the rest, overall this cannot be a bad" (Law 2004: 149).

Can it be a bad thing? To be sure, there is a whole host of fears which is be fuelled by fluidity[13]. Put more generally, the endless struggles for stable national, group and individual identity in modernity have all their indefinite, multiple, dissolving Other, be it 'eternal Jews', overflows of migrants, or other forms of overwhelming difference. And they sometimes fight these Others to their last breath. At the same time, in modernity more bastards of the known and the unknown were created than in any historical period be-

[13] For me, the most vivid description of what fear of fluidity can do is Klaus Theweleit's classic psychoanalytic study of male fascist torturers' fantasies (Theweleit 1987).

fore. The restless urge of the moderns to meet the unfathomable Other, to reach the boundary of the kn/Own and to cross it, is what makes them tick.

Bruno Latour offers a way to deal peacefully with the Other by including and excluding it according to an open, political, and thus 'due' process. Law's, Mol's, de Laet's, Singleton's and others' political option is different. They want us to find new methods of living together peacefully with the Other without 'including' it at the price of excluding others.

Both versions of 'ANT's' politics share the same goal, to help us to live together without referring to essences and dualisms. They differ in the place they situate themselves, which gives them a different vision[14]. From the very beginning and extending to today[15], Latour places the social scientist at the warm and light sites where all is reversible and where therefore 'rapidly revised' suggestions of how we can live together can be proposed and discussed. Other theoreticians have moved from there to the margins, where they found suffering, but also new insights into ontological consequences of non-essentialist thinking.

So, what can 'ANT' do for us? It can help to dissolve dualisms of all kinds, but it also has accepted the challenge to help us to live in the resulting world of fluidity. Whether it succeeds is subject to the efforts of all those who are willing to collaborate.

And on a very final note: What has using 'ANT' methods and concepts in this text done for me (and hopefully also for the reader) in order to get to grips with 'ANT'? I think I have succeeded not to privilege either the applications of 'ANT' or 'ANT in the making'. The traditional way would be to use the applications against the theory or criticising the application for the (wrong) use of theory. The uses of 'ANT' which I found in the applications were anti-dualistic, while the uses of 'ANT in the making' were about living together in a world in which all kinds of essentialist dualisms are already gone. I guess both belongs together.

[14] Situated and therefore 'partial vision' as opposed to 'the view from nowhere' is discussed by Donna Haraway (1991a).

[15] In his 'Introduction to Actor-Network-Theory', traditional sociology (i.e. all non-'ANT') is given the task to work "with what has been already assembled" (Latour 2005: 12), while 'ANT' takes care of the rest.

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The Situated Materiality of Scientific Practices:
Postconstructivism – a New Theoretical Perspective in Science Studies?[1]

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Abstract

For about 20 years, a rather wide range of conceptual approaches to the social study of science and technology have emerged which have occasionally been labelled "postconstructivist". Although these conceptions differ in various respects, they have in common a twofold opposition: against traditional representationalist realism as well as "classical" social constructivism established by the "sociology of scientific knowledge" (SSK). In order to escape the pitfalls of both these views (and to overcome the familiar, yet unfruitful opposition between them), postconstructivist perspectives understand and study the sciences primarily in term of their situated material and discursive *practices*. The present article starts with a brief retrospect on why and how since the mid-1980s postconstructivist trends have questioned not only rationalist and realist accounts but also the conceptual foundations and background assumptions of SSK's claim to explain sociologically the content of science. Subsequently, the central features of a postconstructivist perspective in science studies are outlined, referring to the key concepts of "knowledge", "practice", and "performativity". The fruitfulness of a theoretical approach focusing on scientific practices is illustrated using the example of the increasingly important issue of scientific non-knowledge: In the same way that knowledge is not to be comprehended as simply the mental "possession" of a knower, non-knowledge is not merely the lack thereof but an (unrecognised) implication of materially and socially situated research practices. Finally, it is emphasised that postconstructivist science studies should not be misunderstood as claiming (as do realism and constructivism) to provide a meta-theoretical explanation or legitimation of science. Instead, postconstructivism should be conceived as a situated critical effort to challenge one-sided accounts of scientific knowledge and foster more self-reflective research practices.

[1] I would like to thank two anonymous reviewers as well as the editors of the Special Issue for their helpful comments on an earlier draft of this article.

1 Introduction: Why Postconstructivism?

Introducing another "post"-term into social science debates (after postmodernity, poststructuralism and so forth) will doubtlessly raise a lot of well-founded objections. Nevertheless, in the present article I hope to successfully establish my thesis that the hitherto only sparsely used concept of "postconstructivism" is appropriate and important, if not indispensable, in order to denote a new and distinct research perspective that has emerged in science and technology studies over the last two decades. Although, in some cases, the "boundaries" might not appear to be clear-cut, this perspective on a conceptual level differs significantly from the "classical" social constructivist sociology of scientific knowledge, but no less, for instance, from the so-called "operative constructivism" of Niklas Luhmann's theory of social systems. In addition, postconstructivism might turn out to be a more promising approach to the empirical study of important issues in the area of science studies than social constructivism.

In order to substantiate these claims, I would first like to briefly review how and why it is that postconstructivist views have been emerging within science and technology studies for around 20 years or so. Second, I shall explain in greater detail the general outlines and central features of a postconstructivist perspective in science studies, focusing primarily on the work of the feminist theorist and physicist Karen Barad, the sociologist of science Andrew Pickering and the philosopher Joseph Rouse, all of them as yet not very broadly perceived within the German-speaking debate.[2] Thirdly, using the example of scientific ignorance or non-knowledge with special attention to what I have elsewhere termed "unrecognised non-knowledge" (Wehling 2004), I would like to illustrate that a postconstructivist perspective is able to provide new and fruitful approaches to both scientifically and politically relevant issues. Finally, I would like to explain briefly how the postconstructivist claim to move beyond the well-established opposition of realism and constructivism should be interpreted and justified. As a result, it might become clear that the somewhat artificial and perhaps only provisional term "postconstructivism" demarcates important differences to both (social) constructivism and (representative) realism and contributes to clarifying and developing the conceptual foundations of science studies.

2 The Emergence of Postconstructivist Perspectives in Science Studies

Why did postconstructivist interpretations emerge in the mid-1980s in implicit or explicit opposition to the constructivist "sociology of scientific knowledge" (SSK) that itself had become established as a new and quick-to-dominate paradigm in science studies only ten years before? In his book *Scientific practice and ordinary action*, published in 1993, Michael Lynch, from his ethnomethodological point of view, stated a crisis of the constructivist and relativist sociologies of science and assumed that one could

[2] For pragmatic reasons, I concentrate in this article primarily on science studies. Nina Degele has recently reclaimed the importance of a postconstructivist view for technology studies as well, in order to bring the "materiality of things" (back) to the fore (Degele 2002: 127). Yet, by speaking of "realist postconstructivism" with regard to Bruno Latour's and Michel Callon's *actor-network theory* (ANT) (ibid: 126) she invites two possible misunderstandings: *either* it is suggested that postconstructivism ultimately is a renewed and extended form of realism *or* that there might also be a "constructivist postconstructivism" as opposed to a realist variant. By contrast, what I shall attempt to show is that postconstructivism aims to question and transgress the entrenched dichotomy of realism and constructivism (cf. Barad 1996; Rouse 2002b; Asdal 2005).

observe, as a consequence, the emergence of "postconstructivist trends" (Lynch 1993: 107-113).[3] These various trends amounted to questioning the key terms of "the strong program's agenda to give sociological explanations of the content of science" (ibid.: 112): how can, for instance, "social" factors be discriminated from "cognitive" or "natural" ones, and what should be considered the "content of science"? According to Lynch, "the most radical and interesting of the postconstructivist sociologies of scientific knowledge" (ibid.: 111) at that time appeared to be the *actor-network-theory* (ANT) which, some years before, based on the work of Bruno Latour, Michel Callon and others, had developed into a novel and independent approach in the area of science and technology studies. With regard to the conceptual foundations of science studies, Latour (1992) had argued for "one more turn after the social turn" and criticised the "complete asymmetry" in David Bloor's famous symmetry principle: "Society was supposed to explain nature." (Latour 1992: 278) In their exchange with Callon and Latour, Collins and Yearley quite straightforwardly expressed this explanatory strategy by giving the following methodological advice to their adversaries: "We provide a prescription: stand on social things – be social realists – in order to explain natural things." (Collins/Yearley 1992b: 382). As Rouse convincingly argues, this "prescription" results in both "the reification of natural scientific knowledge as a determinate explanandum and the reification of some aspect of the social world as a potential explanans" (Rouse 2002a: 136). It was the implicit asymmetry in SSK as well as this twofold reification that ANT sought to overcome by its "extended symmetry principle" which ascribed the same explanatory power to non-human "actants" as to humans and refused to make any a priori distinction between them.

Presumably, ANT is – particularly within the German-speaking scholarly discussion – still the best-known and most prominent conceptual effort to get beyond certain shortcomings of SSK's social constructivism (cf. Krohn 2000; Degele 2002). It nevertheless remains highly contested, not least with respect to the proclaimed "symmetry" between human and non-human actants.[4] However, within the field that might be characterised as "postconstructivist", there is, besides ANT, a wider range of different, yet no less important and perhaps even more sophisticated, approaches to the social study of science that have emerged during the last 15 years.[5] Some of those, as for instance Pickering's "pragmatic realism" or Karen Barad's "agential realism", label themselves "realist" in order to make still more explicit the conceptual difference from (social) constructivism. Against this background, the philosopher of science

[3] A few years later, in his outline of "a constructivist genealogy of social constructivism", Lynch (1998: 18) referred to those conceptual developments in terms of a "post history" of social constructivism in the course of which "hybrid constructivisms" proliferated and a loose consensus emerged that practice "is the heart" of the social study of science. Remarkably, Lynch (1993: 91) did not integrate the studies of laboratory research inspired by ethnomethodology (e.g. Lynch 1985) into the "constructivist line". In fact, those studies show at least as many conceptual intersections with "postconstructivist" accounts of science and technology as with classical social constructivism..

[4] For rather different objections see, for instance, Collins/Yearley 1992 a, b; Pickering 1995: 13-15; Weingart 2003: 76-77.

[5] The same applies for technology studies: apart from ANT, one could mention here, for instance, the so-called "workplace studies" which have emerged in recent years; these draw on ethnography, ethnomethodology, and conversation analysis and show a lot of overlaps with postconstructivist accounts of scientific practices. In workplace studies, technical work is conceived as both socially and materially "situated practice in which the context is part of the activity" (Orr 1996: 10). A survey of this field has been given by Knoblauch and Heath (1999).

Joseph Rouse in a recent review essay has re-adopted the term “postconstructivism” (without making any recognisable reference to Lynch) and stated that particularly “the work of cultural historians, anthropologists, and feminist theories of science has taken postconstructivist science studies in important new directions” (Rouse 2002b: 62). In his essay, nicely titled “Vampires: Social Constructivism, Realism, and Other Philosophical Undead”, Rouse mentions as proponents and promoters of postconstructivism, among other scholars, Donna Haraway, Evelyn Fox Keller, Peter Galison and Hans-Jörg Rheinberger and considers the book *Science as Practice and Culture*, edited by Pickering (1992a), a “benchmark for sociologists’ shift away from social constructivism and its underlying humanism” (ibid).

What precisely is at issue in the postconstructivist criticism and shift away from SSK’s social constructivism? Despite the well-known multiplicity and heterogeneity of constructivist approaches within science studies, answering this question requires at least a preliminary, minimal definition of social constructivism to be offered. I borrow such a definition from Rouse’s book *Engaging Science,* in which he characterises social constructivist science studies by the following two features: “First, all scientific beliefs must be accounted for by *social* factors, whatever that analytical category turns out to include; second, any adequate interpretation of scientific knowledge claims must be neutral with respect to their epistemic or political legitimacy and hence to that extent is committed to some form of epistemic relativism.” (Rouse 1996a: 9 – original emphasis) Given this background, the objections raised by Latour, Pickering and others were directed against a tendency towards a sociological reductionism in science studies, accompanied by what Collins and Yearley had termed “social realism” which inclines to reify certain aspects of social life (interests, power relations, cultural identities and so forth) into a stable, self-evident and uncontestable reality. It was argued by SSK’s critics that the exclusive focus on (supposedly) “social factors” tends to marginalise or even (almost) completely negate the importance for the establishment of scientific knowledge of non-social, material factors and objects. A striking example of this tendency can be seen in Collins’ programmatic statement “that the natural world must be treated as though it did not affect our perception of it” (Collins 1983: 88). Consequently, Collins pleads (1981: 3) for an “explicit relativism in which the natural world has a small or non-existent role in the construction of scientific knowledge”.[6]

While the “postconstructivist trends” were highly critical of such claims, they nevertheless refused to return to any form of “traditional” representative realism. Instead, the objective was to articulate a more adequate alternative to representative realism “while avoiding antirealism”, as Pickering (1989: 279) has put it. The various postconstructivist approaches thus started to tentatively develop theoretical conceptions which, explicitly or implicitly, aimed to overcome the realism-constructivism-divide. I would like to illustrate this move referring to Pickering’s aforementioned paper “Living in the material world”, published in 1989, in which he describes his own view, maybe for the first time ever, as “pragmatic realism” in order to demarcate it from both social constructivism and representative realism. According to Pickering, on the one hand, “it is clear that material practice – interaction with the material world – can play a constitutive role in knowledge production” (ibid.: 280). Yet, on the other

[6] It is no coincidence that Niklas Luhmann (1990: 37) affirmatively quotes this statement. In spite of all other differences, the denial of a significant role of the “natural world” indicates and constitutes a remarkable affinity between Luhmann’s “operative constructivism” and Collins’ “Empirical Programme of Relativism”.

hand, the resulting connection "between knowledge and the material world has (...) to be understood not in terms of fixed correspondence but rather in terms of local, potentially unstable coherences achieved between material procedures and conceptual models" (ibid.: 281). What should therefore move to the fore of science studies is "the *making* of coherence" (ibid.: 279 – emphasis added) in an open and contingent process of mutual "interactive stabilization" of cognitive expectations and the effects of experimental practices. In this manner, Pickering (and others) argued, the objectivity of scientific knowledge and its constitutive relation to material phenomena could be re-integrated into the social study of science without falling back into representational realism according to which nature has always existed "out there" exactly like it is depicted by the sciences. Thus, with regard to realism, the decisive shift leads from "representation" or "correspondence" as an abstract philosophical idea (in the sense of an adequation between things and concepts, between reality and theory) to the sociological study of various representational *practices* in science (Lynch/Woolgar 1990), or from "representation of" to "representation as" (Rheinberger 1997: 103).

Yet, to consistently sketch out a post-constructivist perspective requires not only conceptual transformations in the traditional realist philosophy of science but major revisions of the constructivist sociology of science as well. There can be little doubt that these revisions will have to go even beyond the two above-mentioned objections to sociological reductionism and epistemological relativism. For, if one widens the critical stance on social constructivism, one will become aware that the sociology of scientific knowledge – despite all its criticisms of rationalistic philosophies of science – inadvertently shared the premises and hidden assumptions of the latter (in particular a cognitivist focus and fixation on scientific *theories*) to a much greater extent than is usually acknowledged. Before I refer to this point in greater detail in the next section, I would like to briefly draw two rather preliminary conclusions from this brief historical retrospect.

First, it has become clear that postconstructivism should not be understood in a merely temporal sense, as something which simply comes "after" constructivism. Instead, it primarily includes a conceptual dimension which *presupposes* and *builds upon* constructivist science studies and their objections to ahistorical realist and rationalist explanations of scientific knowledge. Accordingly, postconstructivism means and implies a self-reflection of (social) constructivism, not a return to any kind of "pre-constructivist" realism and objectivism. At most, one could speak of a "re-entry" of realism into constructivism, as does Wolfgang Krohn (2000), using the vocabulary of Luhmann's systems theory, with regard to Latour's work. One should, however, not fail to see that such a re-entry does not leave unchanged the two seemingly contradictory and incompatible views. For postconstructivism ultimately aims to overcome the rigid and highly polarised opposition of the "philosophical undead" (Rouse) realism and constructivism by questioning the supposedly self-evident premises and hidden assumptions on which this opposition is founded.

Secondly, within the area of science studies there is no *single* established and consistent postconstructivist theory or approach but a rather wide range of theoretical perspectives and research programmes which might be labelled "postconstructivist", though these themselves use quite different terms for their self-description. Besides Latour's and Callon's ANT, these perspectives include, for instance, Pickering's "pragmatic realism" (Pickering 1995), Rheinberger's "epistemology of experimentation" (Rheinberger 1997), Rouse's philosophical "naturalism" (Rouse 2002a), or

Barad's "agential realism" (Barad 1996) and other feminist accounts of science. Nevertheless, in spite of all the differences between theoretical backgrounds, disciplinary contexts, and so on, these perspectives have one crucial and fundamental feature in common which, in a more general sense, allows postconstructivism to be spoken of in terms of an emerging new perspective in science studies: science is conceived and analysed primarily in terms of *practices*, especially of material and performative practices. An important impulse for this shift "from science as knowledge to science as practice" (Pickering 1992b) has doubtless been given by the re-discovery and revalorisation of the experiment and its significant role in the production of scientific knowledge during the 1980s in philosophy, history and sociology of science (see for instance Hacking 1983; Gooding et al. 1989).

In the following section I would like to illustrate some of the theoretical implications and consequences of those developments in greater detail by sketching out the central features of postconstructivism (or, rather, the various postconstructivist perspectives mentioned above). As I indicated at the outset, I shall refer mainly to the work of Barad, Pickering and Rouse. Rouse's sometimes rather pointed reflections and statements are particularly well-suited to making explicit the characteristics of postconstructivism and its theoretical differences from social constructivist sociologies of scientific knowledge.

3 Outlines of a Postconstructivist Perspective in Science Studies

It has become clear that – beyond "bringing back in" material factors and their importance for the production of scientific knowledge – postconstructivism implies a critical, self-reflective evaluation and revision of the premises and background assumptions on which SSK, more implicitly than explicitly, is based. I would like to illustrate the characteristics and the reach of such a self-reflective turn with regard to the following three key concepts and issues: *knowledge*, *practice*, and *performativity*.

3.1 A "Deflationary" and "Non-Reifying" Conception of Knowledge

What "is" knowledge, and in particular scientific knowledge, and how can it be conceived of in a theoretically appropriate and productive manner? Usually, it is understood as something that is "possessed" and "applied" by a knower and transmitted by communicative interaction (cf. Rouse 1996b: 406). Contrary to these common-sense notions Rouse has developed a "dynamic" and "deflationary" account of knowledge, drawing on deflationary conceptions of truth: "In both cases, truth and knowledge, the deflationary move is a shift from thinking about a putative object that a concept could describe to thinking about the practices in which the concept is used." (Rouse 1996a: 199) According to this "practice turn" (Schatzki et al. 2001), knowledge is not to be understood (and "reified") as an independent and coherent entity or object which is discovered by science and thus explains and justifies scientific practices. As Rouse has put it: "There are many appropriate ascriptions of ‚knowing' within the multifarious practices of assessing, attributing, relying upon, or contesting understanding and justification, but there is no *nature* of knowledge underlying these ascriptions." (Rouse 2002a: 179 – original emphasis).[7] Instead, (scientific) knowledge "consists" of nothing but those prac-

[7] In deflationary theories, the same applies for truth: in this case "the truth predicate and the capacity to use it are recognized as indispensable to linguistic and epistemic practices, even though no underlying nature of truth unifies or reifies the instances of its appropriate application" (Rouse 2002a: 179).

tices of generating, attributing, and justifying knowledge, and "the historically situated and contested development of the practices themselves suffices us to understand them" (Rouse 1996a: 200).

From this non-essentialist account of scientific knowledge three far-reaching conclusions can be drawn:

1) First, knowledge is an effect and an implication of situated practices and can only partly and "artificially" be separated and isolated from them: "Knowledge is embedded in our research practices rather than being fully abstractable in representational theories." (Rouse 1987: 24). This insight not only implies that scientific knowledge is far more intransparent, ambiguous and "intrinsically open to multiple interpretations" than rationalistic philosophies of science usually admit (Rouse 1996a: 25-26). In addition, scientific knowledge frequently or even regularly encompasses what Collins (2001: 72), in an illuminating attempt to discriminate different forms of tacit knowledge, has termed "unrecognized knowledge" and "uncognized/uncognizable knowledge". With these categories, he refers to cases when scientists are able to successfully conduct an experiment without being fully and explicitly aware of why and how it works. One should therefore always take into account the possibility of unexpected, unrecognised or only partly recognised effects and implications of research or the technological implementation of its results. I shall refer to this point more broadly below.

2) Given this background, it would be severely misleading to conceive of knowledge primarily or even exclusively as a "possession" or "property" of individuals or social groups such as certain scientific communities. Instead, the attribution of knowledge is "more like a characterization of the situation knowers find themselves within rather than a description of something they acquire, possess, perform, or exchange" (Rouse 1996a: 133). As Rouse emphasises, this does not mean simply rejecting our ordinary ways of speaking and thinking about knowing. "It can be perfectly appropriate to ascribe knowledge to a knower, so long as we understand that correct ascription of knowledge depends on how the knower is situated within ongoing practices rather than simply on whether the knower 'possesses' the right beliefs or skills (...)." (Ibid.) For better understanding, one should be aware that Rouse's concept of practices, following Donald Davidson, includes linguistic or discursive practices as well (cf. Rouse 1996a: 205-236; see also Section 2.2). "Knowing" certain theories or "understanding" certain scientific concepts thus implies and means competently participating in discursive practices of connecting those theories and concepts to other theoretical models and/or phenomena and situations in the world. To illustrate this point, Rouse uses the following example: "Biologists (...) employ a rich terminology to articulate structural features of living cells: nuclei, ribosomes, mitochondria, membranes, Golgi bodies, and so forth. The application of these terms was regularised and is now learned through the use of multiple ‚models'. Schematic diagrams depict these components in structural relationships. These diagrams are connected to cells through various laboratory manipulations (...). With these models available, it is perfectly straightforward to learn to understand (and to utter understandably) sentences employing terms like ‚mitochondria', whose truth conditions encompass events taking place in unexamined cells outside the laboratory setting." (Rouse 1996a: 229).

3) The deflationary, non-reifying account of knowledge results in a significantly modified and extended conception of what should be comprehended as the "content" of science. This point is crucial in order to fully realise the "postconstructivist" objections to social constructivism. As is well-known, the

so-called strong programme outlined by David Bloor in 1976 aimed to explain sociologically "the very content and nature of scientific knowledge" (Bloor 1976: 3). In this context, however, knowledge, or the content of scientific knowledge, were conceived of primarily in terms of cognitive beliefs; this becomes clear when, for instance, Bloor's famous "symmetry principle" claims to explain "true and false beliefs" by the same type of causes (ibid.: 7). The same applies for systems theory which de facto reduces knowledge to communication and the employment of distinctions of "true vs. untrue" (which obviously are qualifications of beliefs or propositions) in the medium of meaning (Sinn), while (material) research practices do not matter in Luhmann's sociology of science. In this manner, constructivist sociologies of science implicitly and inadvertently share the same cognitivist reduction of scientific knowledge to theories, systems of belief and true-or-false-distinctions as did their "counterpart", representative realism (cf. Rouse 2002a: 136). The main reasons for this specific framing of SSK's object of study are to be found in the history of the field, especially in the strong programme's explicit opposition to Mannheim's and Merton's approaches. As is well-known, both of them had ultimately exempted the content of scientific knowledge from sociological study, thus leaving its explanation to traditional rationalist accounts. To sociologically challenge this exemption would seem to be facilitated, as Rouse argues, "if the contested turf were described commensurably" (ibid.: 143).[8] In contrast, a postconstructivist perspective gives rise to a completely different account of the "content" of science: "Is the content of a science its verbal representation of the world, or the reconfiguration of the world itself through practical engagement with things, people, and prior patterns of talk? The more radical post-constructivist claim is not that the content of a science can be explained by social rather than material or rational 'factors', but that the only coherent notions of content or meaning incorporate the social, material and discursive setting of a science." (Rouse 2002b: 73).

Such a reflective, postconstructivist interpretation of the content of scientific knowledge not only demarcates a crucial difference to social constructivism in terms of theory but also has significant consequences for the analysis of empirically relevant issues, as I will demonstrate later, referring to the example of scientific non-knowledge. Apart from this, there is another important implication of Rouse's claims that I can mention only briefly here. If the "content" of a science not only consists of beliefs and theories but of the entire (i.e. also institutional) setting of scientific practices, then the opposition and presumed incompatibility of an "institutionalist" and a "sociology of knowledge paradigm" that has emerged within German-speaking science studies in the mid-1990s (cf. Schimank 1995a, 1995b; Amann 1995) turns out to be based on questionable premises and appears to be misleading (cf. Böschen/Wehling 2004: 22-25).

[8] It is in this context where Rouse locates the most important and fruitful contributions by scholars of feminist science studies (as for instance Donna Haraway, Evelyn Fox Keller or Karen Barad) to overcoming the tacit continuities of constructivist sociologies with traditional philosophical accounts of science. In particular, "feminist science studies shift their primary object of study from the semantic content of knowledge or belief to a concern with relationships (...) between knowers and known" (Rouse 2002a: 146-147). Their interest in the materiality (and accountability) of these relationships led feminist scholars to oppose also the forms of relativism and "detachment" which seem to be constitutive for SSK's explanatory programme (cf. ibid.: 151-159).

3.2 The Situated Materiality of Scientific Practices

Over the last years, "practice" has become one of the most important but also most strongly contested concepts in contemporary social theory and sociological research (cf. Schatzki et al. 2001; Reckwitz 2003). Given the multiplicity of perspectives from which practices are studied and theoretically understood, "it is not surprising that there is no unified practice approach" (Schatzki 2001: 2). How, then, is the key concept of (scientific) practice or practices to be comprehended within a postconstructivist framework? First, it is important not to misunderstand scientific practice (for instance in narrow terms of experimentation) as opposed to and strictly distinct from theory. Leaving out of account the widely acknowledged "theory-ladenness" of observation and experimentation, one should better conceive of scientific theories "in terms of *theoretical practices* of modelling particular situations or domains; articulating, extending, and reconciling those models and their constituent concepts and techniques; and connecting theoretical models to experimental systems, rather than in the classical sense of *theoria* or through more recent analyses of theories as axiomatic or model-theoretic systems" (Rouse 2002a: 163 – first emphasis added). Against this backdrop, it becomes clear that scientific practices may not be reduced and narrowed to *material* practices but necessarily encompass *discursive* dimensions as well. This understanding of scientific practices as inherently discursive is opposed to both a representionalist account, according to which language simply expresses the given "objective" meanings of things, and a presumedly "materialist" underestimation of the significance of scientific language, reducing it to "mere" rhetorics or literary technologies. As Rouse (1996a: 153) rightly remarks, "(s)ignification in scientific practice (including metaphors and models as well as supposedly 'literal' discourse) is too rich, inventive, and important to be adequately understood in these terms". The eminent role of discursive practices in the sciences as well as their mutual interactions with (if not inseparability from) material, experimental practices are highlighted in Lily Kay's illuminating account of the history of the genetic code. "Encompassing activities such as naming, describing, interpreting, analogising, and signifying discursive practices have formed the conceptual framework guiding molecular biologists in their theorising, experimental design and interpretations (...)." (Kay 1999: 15). Discourses are therefore "a way of thinking and doing" (ibid.: 16).

A second key element of the postconstructivist account of scientific practices is even more crucial, and presumably more unfamiliar and contested within social theory: from the reflections on knowledge and the content of science portrayed in the previous section it follows that practices in this context may by no means be reduced to the doings of social actors (e.g. scientific researchers) "as distinct from the material setting of what they do" (Rouse 2002a: 163). Instead, an adequate conception of (scientific) practices has to encompass the material "configuration of the world" (Rouse 1996a: 133) which makes the activities of individual or collective agents become significant, coherent and intelligible. In explicit contrast to widespread notions of practices as rules and regularities of social actors' doings, Rouse stresses that "practices are not just patterns of action, but the meaningful configurations of the world within which actions can take place intelligibly, and thus practices incorporate the objects that they are enacted with and on and the settings in which they are enacted" (ibid.: 135).[9] This claim is not to be interpreted in terms

[9] In the wider context of the above-mentioned *workplace studies*, a similar conception of situated practice has been outlined by Suchman (1987).

of an extended and radicalised "symmetry principle", as suggested by Latour, but more in the sense of a "priority of the situation", whereby "situation" is understood as "the relational complex of embodied agents in meaningfully configured settings for possible action" (ibid.: 150). By contrast to Latour, Rouse is less concerned with ascribing symmetrical explanatory power to human and non-human "actants" than with explaining what renders the performances of human actors meaningful and intelligible. As he argues, "one cannot engage in skillful activity without the right sort of equipment in the right surroundings" (ibid.), whereby skills are not fixed once and forever but develop and change in interaction with the material setting.

One a more general level, this emphasis on the situated materiality of practices has far-reaching implications for epistemology as well as social theory which are diametrically opposed not only to realist and representationalist assumptions of independently given "natural" objects of cognition but also to Luhmann's "autopoietic" model of operationally closed observing (social) systems.[10] The profound differences between these conceptual approaches and postconstructivism are highlighted by the following statement: "If the post-constructivist tradition denies that there is any role for 'unreconstructed nature' in our understanding of science, it is not because we are unable to get 'outside' of a relatively self-enclosed social world, but because we have never been 'inside' one in the first place. The question is not how we ever get from our social world to a transcendent nature, but how meaningful language and other practices are sustained as part of the ongoing reconfiguration of a reliable and meaningful environment." (Rouse 2002b: 69)[11] The basic and fruitful idea behind this seemingly extravagant claim is not some kind of metaphysical monism but the rejection of understanding scientific practices, both material and discursive, in terms of representation or mediation. Practices (or representations as their stabilised results) are themselves configurations *in* and *of* the world; they neither represent a given "natural world" supposed to exist "behind" those configurations nor mediate it with a distinct "social world" (cf. Rouse 1996a: 150-151, 2002a: 173). Scientific understanding, according to Rouse (2002b: 69), is "not 'inside' minds or cultures, but embodied in worldly phenomena, skills, equipment, institutions, and situated discursive exchanges that cut across the traditional bounds of natural objects and social or cultural meanings". This reflection leads to a third important feature of postconstructivism: an account of scientific practices in terms of their temporality and performativity.

3.3 The Performativity of Scientific Practices

Perhaps even to a greater extent than practice, "performativity" has developed over the last years into a very prominent and widely used but equally ambiguous and contested concept, particularly in philosophy and cultural or

[10] Something that systems theory and postconstructivism doubtlessly have in common is a shift away from representationalism. As defined by Barad (2003: 804), representationalism is "the belief in the ontological distinction between representations and that which they purport to represent; in particular, that which is represented is held to be independent of all practices of representing". However, Luhmann, somewhat paradoxically, seeks to escape the pitfalls of that belief by entirely cutting off any epistemologically significant relationship between an operationally closed observing system and its environment (cf. Luhmann 1990, 1995). In a way, he thus even radicalises the representationalist background assumption of a clear-cut distinction between the "knower" and the "world" (for critical discussion see Christis 2001; Wehling 2002).

[11] In such reflections one will find the reasons why postconstructivism is considered to be a fruitful and promising conceptual approach in areas such as environmental history (see for instance Asdal 2003).

gender studies (cf. for an introduction Wirth 2002). If one tries to pick out one feature that (almost) all of the different references to performativity have in common, then the best candidate might be its non- or anti-essentialist impetus: performativity is not concerned with substantial things but rather with the (temporal) effects of "doings" and "performances", of repeated actions of some sort. It is this basic idea of performativity that has been attractive for that branch of science studies which seeks to move beyond representionalism (Barad 2003: 805):[12] the objects studied by science are not independently given, stable "things" awaiting discovery but instead *temporally emergent phenomena* that are produced (or co-produced) in their specific forms by and within the scientific practices themselves. According to Pickering, a performative account of science is one "in which the performances - the doings - of human and material agency come to the fore. Scientists are human agents in a field of material agency which they struggle to capture in machines. Further, human and material agency are reciprocally intertwined in this struggle. Their contours emerge in the temporality of practice and are definitional of and sustain one another." (Pickering 1995: 21)

I would to like illustrate the fundamental differences between a "traditional", representionalist approach to science (the basic assumptions of which are at least partly shared by social constructivism) on the one hand, and a performative (and "postconstructivist") account on the other, by referring to Pickering's critical discussion of the concept of "constraints". Usually, constraints are conceived as some kind of external (social, institutional, technical, natural, etc.) condition that objectively limits scientific activities as well as pushing them in certain directions.[13] Pickering criticises this widespread notion of constraint for drawing too static a picture of the relationships between scientific practices and their objects, and also their cultural and institutional contexts and surroundings. Constraints, as he argues, are traditionally understood as "temporally nonemergent", thus "preexisting practice and enduring through it": they "are always there" (ibid.: 65-66). In contrast, Pickering proposes introducing the concept of "resistances" in order to adequately take into account the temporality of the relationships between science and its various contexts. Contrary to constraints, resistances are "genuinely emergent in time, as a block arising in practice to this or that passage of goal-oriented practice" (ibid. 1995: 66). Against this background, scientific practice consists of a performative intertwining of emergent resistances on the one hand and repeated efforts to overcome them on the other. Pickering speaks of "a dialectic of 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.: 22).[14] Under happy circumstances, this dialectic may result

[12] While the concept of performativity as yet has only sparsely been used in an explicit manner in science studies (cf. Pickering 1995; Barad 2003; Kroß 2003), there is nevertheless a wider range of scholars propounding performative understandings of science without making express reference to the concept. Barad (2003: 807) names among the latter Haraway, Latour and Rouse.

[13] However, I do not agree with Pickering's (1995: 65) statement that constraints are usually restricted to the distinctively human realm. Frequently, also technical or natural conditions of scientific activities are conceived as objectively given "constraints".

[14] One should note that Pickering here does not employ any idea of a "symmetry" between human and non-human actants, or between human and material agency.

in "interactive stabilisations of machinic performances and conceptual strata" (ibid.: 182), whereby the latter include an "interpretive account" of how the involved apparatuses of observation and measurement work, as well as a "phenomenal account" of the aspects of the material world under consideration (cf. ibid.: 68-96). In cases of successful stabilisation and alignment of these three elements, one could speak of the generation of new, objective knowledge, with the passage trough the "mangle of practice" (Pickering) defining "a rather severe criterion of objectivity" (ibid.: 195). In order to avoid realist or representationalist misinterpretations: this knowledge does by no means "reveal" what and how an independently given object "really is" but is a local and temporal achievement due to the "constitutive intertwining (...) between material and human agency" (ibid.: 15). The stability and validity of this knowledge therefore depends on the maintenance and repeatability of those practices (in Rouse's broad sense) that had both coproduced the respective "machinic performances" and allowed them to be connected to cognitive expectations and theoretical reflections.

It is important not to misunderstand this performative account of scientific knowledge and practice as if phenomena or resistances which are temporally emergent from and within practices were less "real" or "material" than stable things that are "always there" (see for instance van den Belt 2003: 209). Instead, the concept of performativity results in a different understanding of what Barad (2003: 815) has termed the "primary epistemological unit". This "unit" is no longer to be found in independent objects with supposedly inherent properties but rather in *phenomena* which indicate, as Barad argues following the physicist Niels Bohr, the "inseparability of 'observed object' and 'agencies of observation'" (ibid.: 814). Thus, while doubtless being a relational term, "phenomenon" signifies "relations without preexisting relata" (ibid.: 815). The "observed objects" neither are accessible "outside" of their constitutive relations to agencies of observation, nor do they exist "behind" or "beyond" these relations. This by no means denies the materiality or reality of the phenomena, insofar as, according to Bohr, phenomena necessarily involve "things" which ultimately admit of the observation but may not be reified as existing independently of the (material) practices of observation (cf. Barad 1996: 176). What is observed is, in other words, "not a property of the object in isolation but of the phenomenon as a whole" (Rouse 2004: 148). Or, as Barad has put it: "Reality is not composed of things-in-themselves or things-behind-phenomena, but 'things'-in-phenomena." (Barad 2003: 817) One should add "that material resistances are only manifest relative to prior expectations; they have no existence in the absence of such expectations" (Pickering 1989: 281).[15] Temporally emergent resistances or phenomena are recognisable only when they can be captured and connected, within material-discursive practices, to the cognitive expectations of an individual scientist or a scientific community. This points to the question of how the issue of scientific non-knowledge might be comprehended within a postconstructivist framework. As I shall argue in the next section, postconstructivism offers new ways of adequately understanding this important and contested issue, in particular its most difficult aspect: unknown or unrecognised non-knowledge.

[15] This claim might appear to be misleading; yet, again, it does not deny the materiality of the setting from which resistances might emerge. But in the absence of cognitive expectations, resistances do not become manifest, they have no manifest existence.

4 The Embeddedness of Scientific Non-Knowledge – A Postconstructivist Account

For about 15 or 20 years, the novel and unfamiliar issue of scientific ignorance or non-knowledge has increasingly gained attention, both in the (social) sciences and in the general public (cf. for instance Wynne 1992; Luhmann 1992; Wehling 2001). Moreover, the focus has shifted to what Jerry Ravetz (1990) has termed "science-based ignorance", that is non-knowledge *generated* by science itself. The media researcher Holly Stocking had therefore suggested a few years ago the project of a "sociology of scientific ignorance (SSI) to complement and expand the existing sociology of scientific knowledge (SSK)" (Stocking 1998: 173; cf. also Wehling 2004). In this section, I would like to substantiate my thesis that a postconstructivist conceptual approach is most appropriate to grasp the full range of the processes of generating scientific non-knowledge and especially to adequately understand the key phenomenon of "unrecognised non-knowledge" or, as it usually is termed by British and American scholars, of "unknown unknowns" (cf. Kerwin 1993; Grove-White 2001; Wynne 2002). By this notion, situations are characterised in which the sciences don't even know what they don't know (cf. Wehling 2004: 71-72). The almost "classic" example of this state of complete unawareness is the depletion of the ozone layer by CFCs which, even more than 40 years after mass production of those substances was started around 1930, remained entirely beyond the scope of scientific expectations and cognitions (cf. Böschen 2000: 41-104). In recent social conflicts over new technologies, for instance over genetically modified organisms (GMOs), questions of the possibility, probability, or even unavoidability of unknown unknowns are highly contested and increasingly coming to the fore (cf. Grove-White 2001; Wynne 2002).[16]

Can this strange, double negative notion of unknown non-knowledge or unknown unknowns be sociologically (or philosophically) understood in a meaningful and consistent way at all? For, contrary to what Robert Merton (1987) has coined "specified ignorance", unrecognised non-knowledge is by definition *not* present and observable in the form of a certain individual's or group's explicit recognition of what they don't know. But how to explore what is completely absent or, at least, appears to be completely absent (cf. Weinstein/Weinstein 1978)? At this point, the postconstructivist, non-reifying and non-representationalist account of knowledge outlined above proves to be fruitful with regard to the following three closely related aspects: First, if Rouse's claim is right that knowledge is "embedded" in situated research practices and not "fully abstractable in representational theories" (Rouse 1987: 24), then the same applies for non-knowledge. If, secondly, it holds true that knowledge is only poorly understood as the "possession" of certain knowers, for instance a group of scientists, then non-knowledge may not simply be reduced to the mere "absence" or "lack", individual or collective, of such a possession. Thirdly, identifying the "content" of scientific knowledge not simply with its verbal (or mathematical) representation of the world but instead with the "reconfiguration of the world itself through practical engagement with things, people, and prior patterns of talk" (Rouse 2002b: 73) gives rise to a

[16] In order to avoid misunderstandings, one should emphasise that talking and debating about the possibility of unknown unknowns does not necessarily mean that one becomes aware of what is not known (or of what eventually might happen when GMOs are released to the environment). The crucial point in social conflicts is that, by definition, the occurrence of unknown unknowns can neither be proved nor refuted in advance.

more comprehensive understanding of scientific non-knowledge: no less than knowledge, non-knowledge is embedded and inscribed in practices conceived as material reconfigurations of the world. Thus, the (more or less) explicit recognition and scientific "specification" of what is not known in terms of theories or hypotheses is only *one* dimension of the problem which certainly is important but at the same time extremely dependent on highly contingent and precarious preconditions. To put it differently: unknown unknowns, or unrecognised non-knowledge, are inherent in the situated materiality of scientific practices; they are elements and (possible) effects of the material settings which nevertheless are not manifest (or do not exist, as Pickering has argued) in the form of temporally emergent "resistances" or interactively stabilised "phenomena".[17] For unknown unknowns to become manifest, above all appropriate material and discursive practices are required, including the formation of adequate cognitive expectations as well as technical equipment of observation and measurement. There is, however, no guarantee of the successful alignment and interactive stabilisation of "on the one side, captures and framings of material agency, and, on the other, regularized, routinized, standardized, disciplined human practices" (Pickering 1995: 102). On the contrary, one can by no means rely on the assumption that the various elements of the configurations in which scientific or technological practices are enacted will, sooner or later, "manifest themselves" due to their "sheer" materiality and therefore be fully transparent and controllable.

The importance of such a non-reifying, postconstructivist account of non-knowledge which is not centred on knowing (or not knowing) minds and subjects immediately comes to the fore if one understands the technical implementation of scientific knowledge primarily in terms of an "extension of scientific practices beyond the research setting" (Rouse 1996a: 131). What is crucial in this regard is "the reconstruction of the surrounding world to resemble the laboratory in important respects. Objects and substances created in and for the laboratory are introduced into other settings. Partitions and enclosures are built to prevent unwanted or unaccountable mixtures. Actions and events are more carefully sequenced and timed. Instruments to register and interpret the signs first elicited from objects in laboratories become standard equipment elsewhere." (Ibid.). The issue of non-knowledge, in particular of unknown unknowns, becomes relevant here in two respects: first, the strategies of partitions and enclosures to prevent "unaccountable mixtures" will always tend to be limited and incomplete; the complex social or natural world can not really be made into the controllable "micro-world" of the laboratory. The metaphor of "society as a laboratory" (Krohn/Weyer 1989) therefore remains a metaphor, if, of course, an illuminating one; unforeseen and/or unrecognised effects can certainly not be entirely prevented. Second, if one takes into account that scientific practices even *within* the laboratory are not always fully transparent and recognisable (cf. Collins 2001), then unknown

[17] Alexander Bogner's criticism misses this point by confusing the postconstructivist emphasis on the embeddedness of (non-)knowledge in material configurations with a realist and representationalist notion of reference. In a postconstructivist view, (unrecognised) non-knowledge does not *refer* to "more or less objectively knowable phenomena", as Bogner (2005: 23) suggests, but *is embedded in* and *incorporates* a setting of material entities, agencies of observation, established spatial or temporal "horizons" of attention, and so on. Within this setting it is of course a crucial question whether (and when) at least some of its elements can be "captured" and connected to cognitive expectations by situated practices, both experimental and discursive. But this is by no means a retreat to representational realism. By contrast, Bogner again traps the sociological analysis of non-knowledge in the ritualised dichotomy of realism and constructivism.

non-knowledge embedded in the research setting will inadvertently be "exported" into the surrounding natural and/or social worlds with possibly unforeseeable consequences. Given this background, it comes as no surprise that the hitherto uncontested authority of science over the definition of ignorance and non-knowledge is increasingly challenged by social actors, resulting in a remarkable and far-reaching "politicization of ignorance" (Stocking/Holstein 1993) which includes above all the questioning of the dominant framings of scientific non-knowledge (see for instance Grove-White 2001). An attempt, as made for instance by van den Daele (1996), to restrict the "relevant non-knowledge" to the *known* unknowns, that means to the "specified ignorance" of the experts in the respective fields, is therefore not only dubious in terms of risk regulation and public policy (cf. Wehling 2003: 129-131). In addition, it sticks to exactly that narrow, representationalist conception of knowledge (and non-knowledge) in terms of a "possession" of a certain scientific community that postconstructivism seeks to overcome. What follows from a postconstructivist account of scientific (non-)knowledge, is, in contrast, the demand to extend the accountability of the sciences *beyond* what is explicitly known or not known, thus encompassing the material configurations in which scientific practices are enacted.

With respect to this demand, different scientific "cultures of non-knowledge", understood as practices of generating, recognising, defining and communicating non-knowledge, move to the fore.[18] As Karin Knorr-Cetina (1999) has shown convincingly in her study on the "epistemic cultures" of high-energy physics and molecular biology, the sciences differ widely in their ways of "making knowledge". Drawing on Knorr-Cetina's findings one can suppose that these epistemic cultures do not only encompass "cultures of knowledge" but also, at the same time, cultures of non-knowledge, i.e. specific practices and routines of dealing with what is not known. Whereas, according to Knorr-Cetina (ibid.), high-energy physics inclines to actively search for "liminal" or "negative" knowledge, that means knowledge of the limits of its knowledge, molecular biology employs an epistemic strategy of "half-blind variation": if experiments fail or show unexpected and unexplainable results, the scientists usually do not have much interest in carefully exploring the reasons why but vary some of the elements of the experimental setting until it works and delivers explainable and usable results. The study of such routines, mainly tacit, of dealing with (self-generated) non-knowledge might offer fruitful perspectives for initiating more self-reflective research practices, especially when such contrasting scientific cultures of non-knowledge are confronted with each other in public arenas, as in the controversy over GMOs.

5 Conclusion: Beyond Realism and Constructivism?

In his discussion of how to deal with material objects and experimental practices in science studies, Henk van den Belt maintains that postconstructivism as outlined by Rouse, in spite of its "deceptive label", is "really just another version of radical constructivism" (van den Belt 2003: 216), which, according to him, "makes the existence of an object depend on human knowledge" (ibid.: 209).[19] Other critics might

[18] The exploration of such "cultures of non-knowledge" (*Nichtwissenskulturen*) is the aim of a research project conducted at the Environmental Science Center of the University of Augsburg, using the examples of agrobiotechnology and mobile phone communication (cf. Böschen et al. 2005).

[19] Van den Belt aims to defend a "moderate constructivism", as advocated in particular by the "strong programme", against this "radical constructivism" to which he attributes, besides Rouse, the work of Ashmore,

consider postconstructivism as nothing but a retreat from "strict constructivism" (Bogner 2005) to an at best slightly more sophisticated version of traditional realism. Can the postconstructivist claim to move *beyond* the unfruitful dichotomy of the "undead" realism and constructivism nevertheless be substantiated and justified – or is there no escape from the pitiless rule of being *either* realist *or* constructivist (and from being misinterpreted from both sides)?

Usually it is taken for granted in the ongoing discussions on these issues that there are indeed fundamental differences between realism and constructivism that render the two opponents more or less incompatible. Without denying such differences, one should not fail to see that there are also, more often implicit than explicit, striking continuities and correspondences, beginning with a reifying notion of knowledge which is tied to the semantic and representational "content" of scientific knowledge in the form of theories, propositions, mathematical calculations, and so on (see above, Section 2.1.). It is at this point that postconstructivism intervenes: it does not seek to "overcome" the realism-constructivism divide by successively weakening and playing down the differences between them until they meet somewhere "in the middle" (in the shape of "moderate" versions). On the contrary, the critical strategy of postconstructivism aims at transforming (or at least irritating) the dichotomy itself by questioning the hidden background assumptions on which it is founded.

Callon/Latour, Knorr-Cetina, Pickering and Woolgar (van den Belt 2003: 203). Yet, as I have demonstrated in Section 2, the basic assumption of postconstructivism is almost directly opposed to van den Belt's assertion: according to postconstructivism, knowledge is embedded in research practices and "depends" therefore on material configurations of the world.

Representationalism is, according to Barad (2003: 812), "a prisoner of the problematic metaphysics it postulates". This metaphysics "separates the world into the ontologically disjoint domains of words and things, leaving itself with the dilemma of their linkage such that knowledge is possible" (ibid.: 811). From a non-representationalist perspective, however, one becomes aware that the question of whether scientific knowledge is to be explained by natural or cognitive rather than social factors (or vice versa), of whether it reveals the "objective truth" of independent things or is "nothing but" a more or less arbitrary social construction, only arises if we understand knowledge as a "coherent domain of determinable facts susceptible to and in need of explanation" (Rouse 2002a: 136). If, in contrast, scientific knowledge is conceived as embedded in research practices, in material configurations that cut across the boundaries between the supposedly distinct "natural" and "social" realms, it becomes meaningless to ask whether those practices *either* are determined by the reality of natural objects *or* constructed by social actors and influences. The three postconstructivist key concepts outlined above, namely the deflationary account of knowledge, the notion of the situated materiality and discursivity of scientific practices, and the concept of performativity, therefore challenge and transform the shared background assumptions of realism *and* constructivism - and thus elude and abrogate the dichotomy itself.[20] Moreover, compared to realism and social constructivism, these concepts are able to contribute to a more adequate and empirically rich image of the sciences and their achievements and successes as well as their risks and "blind spots" (cf. Section 3).

[20] Apparently, this does not prevent these concepts from being misinterpreted either as traditional realism or radicalised constructivism.

Yet one should not underestimate the persistent influence and attraction of realism and constructivism as supposedly coherent world views. It is in this sense that Rouse has ironically termed them “vampires” or “philosophical undead” which, in spite of all critical objections that have been raised, “still haunt our concepts and interpretations of nature, culture, and science” and “continue to function even when the explicit positions and arguments have become otiose” (Rouse 2002b: 63). Against this background, postconstructivism may not be misunderstood as itself being or claiming to be a coherent philosophical or even metaphysical account “above” or “outside” of the practices of generating, justifying or contesting knowledge. As I would like to suggest, it should instead be conceived as a self-reflective and critical discursive strategy that aims to continuously question and undermine reifying, one-sided interpretations of scientific knowledge (cf. Asdal 2005: 259). As a consequence, realism and social constructivism might lose their position of meta-theoretical certainties and guarantees: scientific knowledge can no longer be explained and legitimised (or de-legitimised) with reference to either “nature” or “society”. What remains is “merely” the socially situated study of the scientific practices themselves and their reliability and accountability, for instance in terms of relationships between “knowers” and the “known”, risks and benefits, or known and unknown unknowns.

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