Science, Technology & Innovation Studies,
Special Issue 1, July 2006

ISSN: 1861-3675

STI Studies
www.sti-studies.de

Deliberative Constructivism

Wolfgang Krohn[1] (Department of Sociology, University of Bielefeld)

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 im-

plies 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 laby-

rinth. 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 ob-

server 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 desir-

able 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 Laka-

tos' 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. (Prolegomena § 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 basi-

cally 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 under-

stand 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 con-

sideration. 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.

7 References

Asendorf, Dirk, 2004: *Tauwetter am Nordpol*. In: DIE ZEIT, Nr. 47, 11. Nov., S. 46.

Bloor, David/ Barry Barnes/ John Henry, 1996: *Scientific Knowledge. A Sociological Analysis*. London: Athlone Press.

Böhme, Gernot/ Wolfgang van den Daele/ Wolfgang Krohn, 1973: Finalisierung der Wissenschaften. In: *Zeitschrift für Soziologie* 2, 128-144.

Broad, William/ Nicholas Wade, 1982: *Betrayers of the Truth*. New York: Simon and Schuster.

Carrier, Martin, 2004: Interessen als Erkenntnisgrenzen? Die Wissenschaft unter Verwertungsdruck“. In: Wolfram Hogrebe/Joachim Bromand (eds.) *Grenzen und Grenzüberschreitungen. XIX. Deutscher Kongress für Philosophie. Vorträge und Kolloquien*, Berlin: Akademie-Verlag, 168-180.

Collins, Harry, 2004: *Gravity's Shadow. The Search for Gravitational Waves*. Chicago: University of Chicago Press.

Goldman, Alvin, 2001: Social Epistemology. In: *Stanford Encyclopedia of Philosophy*. http://plato.stanford.edu.

Groß, Matthias/ Holger Hoffmann-Riem/ Wolfgang Krohn, 2005: *Realexperimente. Ökologische Gestaltungsprozesse in der Wissensgesellschaft*. Bielefeld: Transcript.

Haraway, Donna, 1995: Situated Knowledge. The Science Question in Feminism and the Privilege of Partial Perspective. In: Andrew Feenberg/Alastair Hannay (eds.), *Technology and the Politics of Knowledge*, 176-194.

Janich, Peter (ed.), 1981: *Wissenschaftstheorie und Wissenschaftsforschung*. München: Beck.

Jasanoff, Sheila, 2004: *States of Knowledge: The Co-Production of Science and Social Order*. London: Routledge.

Kant, Immanuel. 1783: *Prolegomena to Any Future Metaphysics*. Transl. Paul Carus, 1902.

Knorr Cetina, Karin, 1988: Spielarten des Konstruktivismus. Einige Notizen und Anmerkungen. In: *Soziale Welt* 40: 86-96.

Kuhn, Thomas, 1972: Logic of Discovery or Psychology of Research. In: Imre Lakatos/Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.

Kusch, Martin, 2002: *Knowledge by Agreement. The Programme of Communitarian Epistemology*. Oxford: Oxford University Press.

Latour, Bruno, 1999: For David Bloor ... and Beyond. A Reply to Bloor's Anti-Latour. In: *Studies in the History and Phil osophy of Science* 30: 113-129.

Latour, Bruno, 2001: *Das Parlament der Dinge. Für eine politische Ökologie*. Frankfurt am Main : Suhrkamp.

Latour, Bruno/ Steven Woolgar, 1979: *Laboratory Life*. Beverly Hills: Sage.

Laudan, Larry, 1980: Views of Progress: Separating the Pilgrims form the Rakes. In: *Philosophy of the Social Sciences* 3: 273-286.

Luhmann, Niklas, 1998: *Die Wissenschaft.der Gesellschaft*. Frankfurt am Main: Suhrkamp.

Meyer-Abich, Klaus Michael, 1997: *Praktische Naturphilosophie. Erinnerungen an einen vergessenen Traum*. München: Beck.

Serres, Michel, 1994: *Der Naturvertrag*. Frankfurt am Main: Suhrkamp.

Rorty, Richard 1988: *Solidarität oder Objektivität? Drei philosophische Essays*. Stuttgart: Reclam.

Rorty, Richard, 1985: Solidarity or Objectivity. In: John Rajchman/Cornel West (eds.), *Post-Analytic Philosophy*. New York: Columbia University Press, 3-19.

Woolgar, Steve, 1981: Interests and Explanation in the Social Study of Science. In: *Social Studies of Science* 11: 365-39.