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The Internal Environment of Knowledge Claims: One Aspect of the Knowledge-Society Connection

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ABSTRACT: In the sociology of knowledge, the relationship between society and knowledge — or rather what separates them — remains an unsolved problem. A critical analysis of various solutions that we must look for to this problem suggests the plausibility of a passage between social groups, styles of argumentation and objects of knowledge. An empirical model of "*decision displacements*" is proposed on the basis of a corpus of texts and of observations derived from concrete analysis of a laboratory situation.

KEY WORDS: Argumentation, decision displacement, epistemic/extra epistemic, sociology of knowledge

Perhaps the most significant event in the social study of science since the emergence of the sociology of science pioneered by Robert Merton is the rise of a genuine sociology of scientific knowledge in the 1970s. For the first time in the history of sociological thought there exists a vigorous and systematic effort to subject natural and technological scientific knowledge to the same scrutiny which has long been brought to bear on other systems of belief. Inspired by wider developments in philosophy, history and sociology, recent perspectives on social studies of science have redefined traditional concerns in the study of science to include the content of science and the very core of scientific activity, technical work (Knorr-Cetina and Mulkay, 1983: 2ff.).[1] Along with this concern there emerged another. We witness a revival of interest in the hoary old puzzle of the social conditioning of knowledge, which, in Mannheim's terms (1954), is the constitutive question of the sociology of knowledge. The attempt to include in the social study of science the cognitive activities of scientists, that is scientific work, appears to be an honorable and altogether innocent move forward in the history of our specialty. If anything needs to be explained, then it is why it came so late. The move toward studying the content of science with a view to its potentially social conditioning is less innocent. It raises the question of the very foundation of our most authoritative, esoteric and faithfully accepted system of belief.

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Moreover, it raises the ghost of Mannheim's relativism, of a hyperdemocratic ideology of 'helpless surrender' (Lucacs, 1974) which cannot discriminate between different forms of knowledge.

Now some of these fears may be clearly misplaced, as the perennial anxieties which the slightest semblance of relativism seems to inspire. Others have simply been premature. For worthy as our fresh pursuit of questions of the sociology of knowledge may be, it is unclear to which degree the quest has been successful. Like Achilles racing the turtle in Zeno's paradox, we seem to get nearer and nearer to our object of concern, without in most cases, getting near enough to attain the elusive goal. This, to be sure, is in itself a feat of scientificity; unlike the hard sciences, sociology is usually not thought to be capable of losing its phenomenon (Garfinkel et al. 1981). And does not physics itself provide a precedence for Zeno's paradox? Its quest for the elementary constituents of matter has led to a seemingly indefinite dissolution of matter into ever more intermediary particles, with no end in sight.

Thus, the sociology of scientific knowledge is in prominent company, and presumably on the right track. In this paper, I offer another gloss on how to reduce the distance between sociology and knowledge. But first, how far have we come along, what distances have we covered? Evidently, this question does not have an easy answer. Many studies in recent sociology of scientific knowledge are surprisingly ambiguous in regard to what exactly they demonstrate. Take the example of grid-group analysis developed by Douglas (1975) and Bloor (1978), which proposes a marriage between a style of scientific work and a pattern of social organization. Douglas defines social organization in terms of the degree of social stratification ('grid') and the degree of coherence and boundary maintenance one finds in a group ('group'). From this she derives four basic forms of social organization which range from those which load high on the grid-group dimension through intermediary high-low combinations to societies low on both. No such conceptual clarification exists for "styles" of scientific work. In recent writings, these have been variably interpreted as styles of argumentation exemplified by scientists' characteristic responses to anomalies, as forms of scientific thinking, forms of knowledge (all in Bloor, 1978), and as paradigm-like research traditions (Rudwick, 1981). But much depends on this definition.

Assume individual scientists do indeed adapt their style of argumentation to what they think feasible under given grid-group conditions. Such matches between individual and organization may well remain accidental to the development of knowledge: individual forms of argumentation may cancel each other out or add up to a mix in which no one style dominates. Competitors from the outside may reject an argument precisely because they suspect it to be connected to the social circumstances from which it originates. Scientists may decide that the form and content of an argument are different matters, and may turn to evaluating knowledge claims on other than stylistic principles, and so forth. On the other hand, assume we are talking about research traditions which win out *because* they are sustained and enhanced by a particular kind of social structure. Plainly, correlations of this sort could not be considered accidental to the development of knowledge. If we could plausibly argue that a given theory or 'paradigm' survived because it embodied strategies of reasoning sustained by a specific form of social organization, we would indeed have identified a powerful source of the social conditioning of scientific knowledge.

But grid-group analysts have shyed away from any strong conclusions of this kind. Rudwick, for example, explicitly asserts that 'the scarcely deniable cumulative element in geological knowledge has no simple historical association with any one style' (1981). And Bloor points out the difficulty that the same scientists apparently adopt different 'methodologies' of argumentation in different circumstances governed by the varying demands of expediency (1978: 272). This, of course, jeopardizes the whole theory, for it suggests that there also exists no simple historical or geographical correlation between forms of social structure and any one style of argumentation.

The fruitfulness of grid-group analysis depends on whether we can find a plausible passage from the grid-group of social structure to scientists' patterns of argumentation, and from there to the objects of scientific knowledge. Grid-group theory solves this problem by fiat, that is by assuming from the outset that there exists a correlation. Analysts concede the difficulty, but have not, to my knowledge, investigated it any further. Until we have clarified the problem of association and decided on what is entailed by a style of scientific work, the meaning of results from grid-group theory remains uncertain at best.

Now take another example. Barnes (1977), Shapin (1979), MacKenzie (1981), Pickering (1984) and others have offered to answer the question of the sociology of scientific knowledge by a revived and revised 'interest' explanation. Their work allows for a number of interpretations of the role of interests in scientific practice, only some of which are pertinent to the present question:

(1) Scientific results may be used to *legitimate* collective social goals. For example, certain theories of evolution may lend themselves to a support of the idea of controlled genetic improvement of human races. This can easily be granted, and examples abound; but it is hardly pertinent to the question whether and how a theory of evolution is itself socially conditioned.
(2) Scientific results may be *pursued and developed* in order to achieve collective social goals. For example, some theories of management can be employed to attempt to make industrial organizations more effec-

tive, and they have been developed with exactly this purpose in mind. This interpretation is compatible with all notions of science as a problem-solving activity which admits that problems may have their origin in larger societal concerns. If the mechanics of the heavens were worked out in the 16th and 17th century in order to serve the purposes of navigation, so much the better. But social problems which stimulate scientific work in an area function as a *trigger* and not as a blueprint for the substantive results of this work. They may explain why a particular piece of knowledge was generated without in the least tackling the question whether and how it may itself have been molded by social concerns.

(3) A scientific result may be developed *in a certain way* in order to make it compatible with or relevant to collective goals. A correlate of this formulation is that a *particular kind* of result may be preferred by a scientist *because* it is compatible with or relevant to collective goals to which the scientist submits. Did Pearson insist on treating nominal data like interval data *because* this made the resulting tetrachoric coefficient compatible with or useful to the eugenic cause? Would Yule's coefficient have been less compatible, or less useful, or both?[2] The present formulation of an interest explanation differs from the previous in that it is more concerned with the *structure, composition, or content* of the result than with its pure adequacy in regard to a goal. I shall return to this argument below.

(4) Finally, a scientific result may become *generally accepted* because it was compatible with, supported by or supportive of collective goals of the group to which the majority of the respective scientists of a dominant subgroup among them belong. In MacKenzie's example (1981), this would amount to saying that statisticians in general accepted the tetrachoric coefficient because it suited the purpose of the class which they or their scientific opinion leaders represented. On a general level, version (4) demands that the distribution of scientists pro and con a given result somehow reflects, at least probabilistically speaking, prevailing group divisions and the goals sustained by these divisions. This is trivially true when what we have in mind are specialist groups divided along the lines of their cognitive interests in specific results. The non-trivial thesis that any distribution of proponents and opponents of a scientific result generally reflects prevailing social class divisions has not, to my knowledge, been seriously proposed in recent interest explanations. Nor has it been proposed that those knowledge claims will as a matter of course become accepted in the natural sciences which reflect the interests of a dominant social group. Hence, if version 1 and 2 can be ruled out from further consideration because they are irrelevant, version 4 can be ruled out because it has not been seriously proposed.

To conclude, then, only one of the interpretations suggested by current interest explanations proves relevant to the question of the social conditioning of knowledge (version 3). It says that knowledge objects are developed or preferred in the way they are with a view to (that is at least compatible with, at most directly useful to) collectively sustained goals by scientists who share these goals. This is a reasonably clear teleological explanation which links collective goals to individual scientists' choices. Assuming that potential difficulties like multiple or conflicting goals can somehow be accomodated, the form of this explanation appears unproblematic: it is built upon the accepted assumption that actions are goal oriented, with the possible qualification that we are talking about collective and sustained goals. Hence, the general acceptability and the specific relevance of such interest explanations in regard to the social conditioning of thought will depend exclusively on the goals imputed to scientists. In the past, interest-aficionados have paid for relevance with acceptability: to many, goals identified as the interests of a class seemed unacceptable as a primary and unplausible even as a secondary factor in explaining technical choices. The problem, I think, is in many ways similar to the difficulty encountered by grid-group analysis. The plausibility of the analysis depends on whether we can find a point-able connection between the social interests espoused by a collectivity and the laboratory site, between the rationality of a class and the reasoning of a science. I do not, of course, wish to suggest that both are logically or methodologically different. But neither are they substantively the same or causally short-circuited.

Finally, consider a third approach that may possibly contribute to the sociology of scientific knowledge. This is the political approach to scientific behavior, a version of which was first proposed by Bourdieu for the social sciences (1975). Latour and Woolgar (1979) and Latour (1984) offer an account of the political strategies which Guillemin in the 1970ies/1980ies in California and Pasteur at the turn of the century in France pursued in and through their natural science work. By analyzing these strategies, Latour and Woolgar wish to determine how scientific facts are socially constructed (1979: ch. 3) — a pursuit which, one assumes, answers directly to the question of the social conditioning of scientific knowledge. But does it really? As one reads the above accounts one finds that scientists political stratagems do not enter and shape scientific knowledge in quasi-automatic ways, though they may determine, to paraphrase Latour and Woolgar's description of their intent, where, why (because of what personal goals) and with the help of what strategic means (work tactics, financial calculations, techniques of persuasion etc.) a knowledge of claim was created (1979: 127). Previously we thought that perhaps the advancement of knowledge is served best when scientists adhere to the virtues of universalism, communism, disinterestedness and organized scepticism. Now we may conclude from studies such as the above including Mitroff's (1974) and Wade's (1981) that perhaps science is served better when scientists act like hard headed entrepreneurs, ruthless maximizers that is, of their scientific success and reputation. It is after all perfectly conceivable that scientists may pursue political success strategies in and through their work without in the least affecting the content of their knowledge claims. Indeed, both the Latour and Woolgar and the Wade study of the race between Guillemin and Shally for the isolation of the thyrotropin releasing factor (TRF), a hormone synthesized in the brain, leave TRF unexamined from a sociology of knowledge perspective, and instead focus on how these scientists succeeded in outclassing other competitors and attempted to outmaneuver each other. Similarly, Latour's study of Pasteur treats the microbes on which his scientific theories relied as naturally occurring agents whose behavior and effects can be clarified in the laboratory rather than as entities which themselves result from scientists' (social) constructions. Thus, portrayals of scientists as skilled political actors (science is politics conducted with other means, says Latour (1984)) do not appear to bring us nearer to sociology of knowledge goals unless they make clear what difference political stratagems make with respect to the internal make up and the acceptance of a knowledge claim.

Some sociologists of knowledge (notably Chubin and Restivo, e.g. 1983) have not proposed a general model but rather work on the variable historical connections between forms of knowledge (mathematics) and social structures. Others (Collins, e.g. 1985) have taken a totally different approach: instead of pointing out mechanisms that connect knowledge and society they demonstrate that empirical observations and 'rational' argument do not in themselves conclusively 'prove' a scientific interpretation and hence by implication other (social) factors must play a role in the establishment of a knowledge claim. But for those approaches which want to propose a general model in the sociology of knowledge it would appear to be essential to specify what these factors are and how they operate. The political action model of science points out the social (political) character of scientific pursuits but fails to indicate its relevance to and impact upon the form and content of scientific knowledge. Grid-group analysis specifies a mechanism of connection between knowledge and social structure (styles of scientific argument) but still needs to account for the variability of this mechanism in similar social circumstances. Clearly, the most complete and coherent account to date of the social conditioning of natural scientific knowledge if offered by the interest model; yet it appears to be unclear to what degree the model can be generalized to cases other than those studied (do class interests *always* shape the content of knowledge claims?), and furthermore, the model admittedly rests upon imputations of common goals by the analyst to collectivities whose nature as a social category and collective actor is itself problematic. Plainly, not everyone agrees that such large scale entities as social classes exist in modern society as collectivities identifiable by their common pursuits and shared characteristics.[3] And even if the collectivities referred to are of a more tangible kind, as with Pickering's specialist groups whose members have similar investments in interconnected problem domains (1984), we may not be justified to assume that these similarities suffice to trigger among members similar attitudes and actions.

Yet can we avoid such imputations of shared goals and behaviors? I think we can, but at the cost of giving up the broader — and perhaps more interesting — claims that refer to large scale sociological categories such as social classes. In this paper, I want to explore one connection between 'knowledge' and 'society' that is routinely established by participants (scientists) themselves. Thus I want to adopt a strictly 'participant-centered' perspective (Edge, 1979), and to see where one gets if one limits oneself to the analysis of those 'trans' scientific connections (Knorr-Cetina, 1981: ch. 4; 1982) which are formulated by participants in technical work. I shall first present a version of scientists' laboratory reasoning which specifies decision displacements in technical decisions as the points at which the relevant connections are made. Second, I shall draw upon a recent case study of a series of experiments involving the biopolymer chitin to give a more elaborate example of the various transitions between 'knowledge' and 'society' found in scientific inquiry.

THE DECISION-DISPLACEMENT MODEL OF SCIENTIFIC INQUIRY

On a general level, scientific 'findings' can be seen as the results of complex processes of production. How can we characterize these processes in more detail? Processes of production, if they are not routine or automatized, involve technical decisions. In fact one might say what constitutes the specifics of a complex technical process are the kind of selections embodied in its course of events. Surely in this sense scientific work is decision-impregnated work, and scientific products can be seen as highly internally structured in terms of the decisions and selections through which they were brought about. And surely in this sense at least, scientific results can be said to be 'constructed' in scientific inquiry rather than to be merely 'encountered' or 'produced' in laboratory research (Knorr-Cetina 1981: 5ff.).

I want to propose that the selections embodied in scientific inquiry serve as a kind of anchoring point for the 'knowledge'-'society' connection: in making technical decisions, scientists tie together different provinces of meaning relevant to their work. Specifically, the connection runs through *decision displacements* and the trails which link different selection frames to a single laboratory problem. The notion of a decision displacement starts from the observation that the selections found in scientific inquiry can only be made on the basis of further selections which we commonly call *decision criteria.* For example, when choosing between two electron microscopes of which one operates with laser beams scientists may consider the sharpness and enlargement of the resulting pictures, the accessibility of the instrument and the know-how required for its operation. These are second order choices as is the option between the degree of depth penetration and surface representation of the particles scrutinized which the two instruments offer. Following Tversky and Kahnemann (1981),[4] I shall use the term 'decision frame' to refer to a scientist's conception of the acts, outcomes and contingencies associated with a particular technical choice. Second order choices can then be seen as different decision frames. In the process of working out the consequences of the choice between the two electron microscopes, scientists may decide that the 'real' question is what they want to do in the future, for example, whether they want to continue their work in the direction of an exploration of particle structure which requires the laser beam microscope or not. The original question and the question of future work constitute different decision frames, and decision frame 1, D(F1), has been displaced by decision frame 2, D(F2):

D(electron microscope 1 vs. electron microscope 2)
↓ decision displacement (frame translation)
D(particle structure vs. surface structure)

It appears that subtle differences in expression may be informed by different decision frames. We know from experiments that small differences in the formulation of a choice can influence a decision. Work on the use of heuristics in decisions under uncertainty consistently shows that changes from a positive to a negative formulation can lead to reversals of preference, even though the respective outcomes may have equal expected values. Typically, the certain death of 400 people is less acceptable than the two-in-three chance that 600 will die, but the certain survival of 200 people is more attractive than the one-in-three probability that 600 will be saved (Tversky and Kahnemann 1981: 453). Apart from the formulation, these problems are identical.

Now one suspects that a person who formulates an option in terms of a loss rather than a gain has 'something in mind', and that this 'something' has acted as a reference conception relative to which the choice was formulated in negative rather than in positive terms. For example, a scientist who formulates an option between working alone and involving a colleague in his/her work in terms of a potential inflation of his/her credit rather than in terms of an opportunity for cooperation may be guided by a perceived status discrepancy between the prospective collaborator and him/herself. In the beginning I argued that choices are made with respect to other choices by which the original options are replaced. While this is in principle an indefinite (though not infinite) process, in practice chains of explicit decision displacements appear to be quite limited. We might perhaps use the notion 'reference decision' or 'reference frame' for those taken-for-granted selections which, for the time being, adopt the role of structuring the formulation of a decision problem. If the formulation shifts from positive to negative, a displacement may have occurred in the reference decision. Implicit reference frames can overrule and reinterpret explicit decision frames. For example, participants in the well-known Milgram experiments appear to have taken the authority and responsibility of science as an absolute reference point from which they reinterpreted explicit experimental instructions. As a consequence, they were ready to administer potentially fatal electric shocks to alleged fellow participants in the experiment, seemingly undeterred by the subjects agonizing screams and other signs of disastrous consequences (Milgram, 1973).

Now my argument is that decision problems involve decision displacements, and that these create passages between different domains implicated in laboratory work. Concretely this means that scientists place themselves in situations potentially influenced by a particular technical choice, and consider the choice from the vantage point of their new position. A series of decision displacements may be called a *decision trail.* Decision trails connect specific laboratory selections with a variety of domains which are thereby identified as potentially implicated in present pursuits. Phrased differently decision trails provide the routes on which scientists move back and forth between a variety of settings whose features they import into inquiry work. As a result, scientific 'findings' thrive upon an internal environment which, though not visible on their surface, nevertheless sustains these results' successful realization. Exhibit 1 illustrates the connection between decision trails, decision displacements and first and higher order selections.

The circles in this diagram represent different decision frames some of which add up to different arenas of action and provinces of meaning

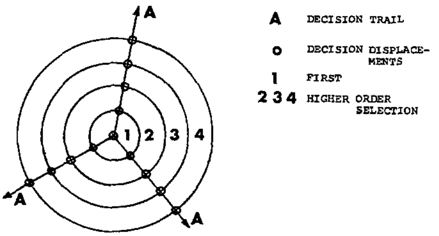

Exhibit 1: Decision displacements in scientific work.

(discourse) in which scientists are involved either through structures of relevance or through personal participation. The number of decision frames invoked in scientific reasoning appears to be indefinite: the number of different arenas of action and provinces of meaning in which scientists participate seems more limited, and one can attempt to distinguish between these according to Schutz' (1945) or some other classification. For the present purpose of examining the 'knowledge-society' connection as it appears in scientific technical decisions we must somehow distinguish between arguments that pertain to the advancement of knowledge and those which strictly speaking do not.[5] I propose to use the notion 'epistemic' to refer to considerations which can be presumed to promote the 'truth'-like character of scientific results. Hence 'transepistemic' arguments would be those which traverse and transcend such considerations. Previously I have suggested that arguments implicated in scientists' technical decisions appear to be transepistemic, and that they point to reference groups which are smaller in size and broader in regard to the scope of their influence than specialist groups (Knorr-Cetina 1982). In the following, I want to illustrate these claims in terms of the decision displacement model introduced above as applied to a sample of decision problems encountered in a laboratory science.

THE STRUCTURE OF DECISION PROBLEMS IN SCIENTIFIC WORK

The examples I shall offer draw upon a series of ongoing experiments in an area of biology/biotechnology. The research is university based; interviews with scientific informants, laboratory protocols, scientists' research reports and on occasion direct observation are the source of my information.[6] The experiments analyzed here focused on the binding mechanism of the biopolymer chitin, a cellulose non-digestible by human beings. The question was to what degree and under what circumstances chitin could be used as a carrier for other substances that have certain functions in the body but should not be absorbed by it, or as a carrier for the controlled release of drugs etc. which have negative consequences or are less than optimally effective when taken in great concentrations. The resulting papers report on three in vivo studies and one in vitro study. The in vivo studies were based on experiments with Mongolian gerbils, a variety of desert mice. To test the binding properties of chitin scientists choose a red dye. Both substances were mixed into the gerbils' diet and the excretion rate determined by colorimetric methods. The rate served as an indicator of the degree to which chitin had successfully bound the substance and prevented its absorption.

From what little I have said about the experiments a number of technical decisions embodied in this work are already apparent. For example, why did researchers choose the biopolymer chitin? Why three in vivo feeding studies compared with one in vitro study and why Mongolian gerbils instead of rats? Why did they choose a dye rather than other substances to be bound by chitin and why had the dye to be red? Consider for a moment the last mentioned choice which, as judged from scientists reasoning, displays a fairly simple structure. Why did Richard, who ran most of the experiments, pick red?

> I picked red . . . I discussed it with Tom and I just said what should I do, and he said the feces are usually greenish and so it made sense to use something very bright (but) you wouldn't use yellow or something. So . . . somehow for me *red* was a good choice because you could see it (best) in the diet as well as in the feces, which was an assumption I made without having any proof.
>
> So I was looking for the brightest possible colour, which would show best, that was the reason.

It was plain from this and other comments that if scientists had picked a dark colour, this could not have been easily differentiated from the dark greenish colour of the feces. If they had chosen white or yellow, the results would be dubious at best. Confronted with the question of choosing the colour of the dye, scientists moved to a consideration of the colour of the feces. The decision was displaced to a domain (colorimetric measurement) whose characteristics in turn had implications for the resolution of the original decision problem:

D (colour of dye to be used)
↓
D (colour that can be seen (measured best))
↓
D (colour of feces)

Thus, many laboratory decisions appear to be made by imagining the consequences and preconditions of a choice in an area implicated in the decision, and by reconsidering the choice from the vantage point of the troubles and opportunities offered by that context. This implies, I think, that the common notion that scientists draw upon a specified set of decision criteria may be unhelpful at best. In fact, from scientists' reasoning it appears that they do not draw upon 'decision criteria' at all. Rather, they seem to follow a procedure best expounded by G.H. Mead (1967) with respect to a different issue: they place themselves in implicated situations, and evaluate their problems in terms of the *concrete* characteristics of that situation. It is these concrete characteristics which determine which selections are made and consequently, what the results of scientific inquiry will look like. Decision 'criteria' are post hoc and ex ante generalizations triggered by a different problem: that of accounting for and reporting selections.

As an example of what a more complete[7] decision tree for one research question may look like consider now the question why scientists decided to use gerbils rather than rats in the in vivo experiments. Exhibit 2 presents a number of reasons offered at various occasions. The Exhibit summarizes the structure of scientists' spoken reports and comments; it does not represent a history of their thinking over time, or of the occurrence of actual decision problems in the laboratory. I want to illustrate the claim that the conjunction of domains connected by decision trails in scientists' technical reasoning transcends epistemic considerations. Consider first reasons collected in Exhibit 2 beginning at the top of the page.

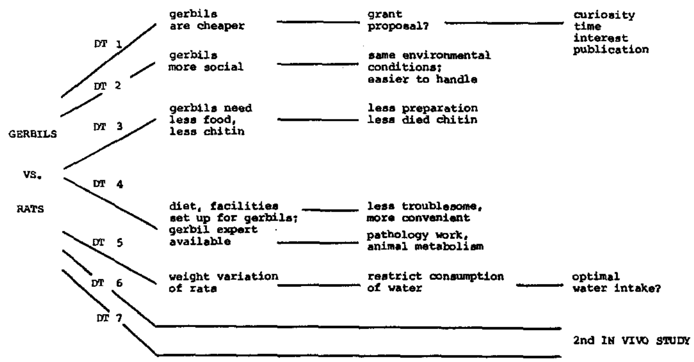

Exhibit 2: Decision displacements (decision trails, DTs) regarding a laboratory choice.

Several comments scientists made in the context of interviews and discussions included a reference to the higher costs involved in using rats rather than gerbils for the in vivo study, and to the notion that what grant money they had available might better be used for other purposes. The question of gerbils versus rats had been transformed into 'a question of money'; it had been displaced, it seemed, by what were obviously non-epistemic considerations. I had assumed that the transepistemic connection of inquiry would not become apparent from inspecting scientists' first order selections. I thought I would find these embodied in higher order selections which would lead us progressively farther away from the technicalities of experimental research. But upon examining scientists' decision rationales, one finds that non-epistemic considerations spring up thick and fast in the course of technical work as exemplifed in the first decision displacement in Exhibit 2. But one also finds that more epistemic arguments may lurk behind what seem to be obviously non-technical considerations. When I asked why scientists did not apply for research grants and wait with the study until 'the question of money' had been resolved, curiosity and the (cognitive) interest of the problem were cited repeatedly as the main reason why one did not want to wait. There was also the issue of invested and available time: time had already been invested in the first in vitro study and should, it was hinted, somehow pay off. Furthermore it was implied that time was available *now* to complete the study whereas at a later point (because of continuing teaching obligations) this might not be the case. Finally, there was an interest in publishing a paper on this topic which was considered to be new as soon as possible, which meant before the competition got wind of it.[8]

Now one might argue that curiosity and the (cognitive) interest scientists indicated in the studies tie directly into epistemic considerations. What this exemplifies, I think, is that decision displacements do not necessarily occur in one direction (science → society) only: implicit reference frames behind seemingly non-epistemic considerations may lead us back into epistemic questions. A similar argument can be made in regard to decision trail 2, that is with a view to scientists' notion that gerbils are 'more social' than rats. One question which arose in connection with the issue of gerbils vs. rats was that of the problems to be expected in handling the animals. It was said that gerbils would be less aggressive and less violent and neurotic than rats. Hence several of them could be put together in one cage and handled simultaneously. Rats would have to be kept in separate cages and handled separately. But this was not only a question of the ease of care; it was also translated into a question of standardizing the environmental conditions for the animals. Scientists felt that standardization would be greater if all replications (4-6 animals) would share their food, water, light etc. by being put in one cage. Once again, pragmatic considerations (ease of care) tied into epistemic considerations (standardizing experimental conditions).

Now it might appear that by emphasizing that non-epistemic arguments are sustained and supported by epistemic considerations I am undermining my claims about the transepistemic connection of research. I think I am not, because the reverse that epistemic considerations are sustained and informed by non-epistemic considerations is also true. Recall that in decision trail 1 scientists also referred to the need to publish (a hot topic) quickly and to their personal shortage of time as reasons for their choice — both on the face of it considerations of a non-epistemic kind. In terms of our simplifying analytics of distinguishing between epistemic and non-epistemic reasons, decision trail 1 suggests that scientists move back and forth between considerations which can be associated with the two poles, thus producing arguments which appear to be more or less balanced mixtures of both:

Epistemic
(gerbils vs. rats)
↓
Non-Epistemic
(question of Money)
↙ ↘
Epistemic (curiosity; interest) | Non-Epistemic (time; need to publish)

In fact, it is often difficult to establish and very likely insignificant which kind of reasons lie at the end and which at the beginning of a decision trail. Accordingly, at least some of the lines in Exhibit 2 could have been pointed in both directions. For example, in decision trail 4 the expectation of doing pathology work and potential problems with the metabolism of the animals may have led scientists to choose the species for which an expert was available, and conversely, the availability of a gerbil expert conceivably influenced their even considering pathology work. Similarly, confronted with the question of choosing gerbils or rats scientists may have called to mind (or physically explored) the conditions in their immediate laboratory environment, and asked the question for which of the species existing facilities would be better suited. Or they may have put themselves in the situation of actually doing the work of preparing the diet and feeding the animals, of collecting their feces and of measuring them. This may have prompted them to ask whether there existed a pre-established routine in their environment for one of the species, which would greatly facilitate their work.

In sum, in making technical decisions, scientists move back and forth between different provinces of meaning and relevance depending upon what appear to be the varying demands of circumstances and expediency, and it does not seem to matter where they — or we — enter the path. Arguments rooted in different relevance structures mutually sustain each other, and the sum of these linkages creates a web of significances which constitutes the internal environment of technical results. There is no need, I take it, to inspect the decision trails in Exhibit 2 any further (decision displacements not discussed so far can be found in Appendix 1). However, I want to add a word about the arenas of interaction relevant to these patterns of argumentation. Some arguments scientists invoke can be traced to the reference groups implicated in scientific inquiry, that is to agents and groups by which technical work is informed and to whom it is directed. Previously I have suggested that these reference groups are both smaller in size and broader in regard to the scope of their influence and the roles and institutional affiliations of their members than the specialist groups which we commonly associate with scientific work (Knorr-Cetina 1982). I have also maintained that they constitute arenas of interaction in which many decision frames invoked in scientists' technical decisions originate, and in which relevance structures and associated decision displacements may be actively negotiated.

I want to illustrate these claims by briefly pointing out some of the agents implicated in the above decision. Clearly Tom the gerbil expert figures prominently in the accounts analyzed here; not only was his presence on the spot, his expertise, routines and willingness to cooperate an almost constant factor in considerations of the choice between gerbils and rats, but a large percentage of the relevant considerations were worked out and negotiated in discussions with Tom. Two other agents importantly implicated in the decision, particularly with respect to the second and later in vivo studies, were mostly referred to as institutional actors: the Food and Drug Administration (F.D.A.) and 'the company', meaning the company from which scientists had obtained the red dye (see the sample arguments in Appendix 1, DT 7). In the first in vivo study scientists ran into a severe problem in that their gerbils inexplicably lost their hair and also lost weight after they had been on the dye diet for a few days. The experiment had to be terminated after thirteen days of feeding the animals: the gerbils were put to death and frozen for pathology work lest they would die off by themselves under uncontrolled circumstances. The red dye which had been used was approved by F.D.A. as suitable for human beings. How F.D.A. arrived at this decision (it does not usually release its information to the public) and what its counter-arguments would be to the claim that the dye might be the cause of the rapid deterioration of the gerbils became a major issue in scientists discussion. They assumed that the same problem had not occurred in F.D.A. related research which presumably used rats, or F.D.A. would not have approved the dye. Had they discovered something F.D.A. had missed because it used rats? Were the gerbils simply more sensitive to a compound that was slightly toxic and was this sensitivity limited to this species? Scientists rang up the company to ask whether they had any evidence regarding the problem, but were shut off completely. Yet arguments related to the company and F.D.A. continuously entered the decision process. For example, the fact that F.D.A. was likely to have relied on experiments with rats was seen as an argument in favor of continuing the in vivo studies with gerbils — presumably, it was only by continuing to work with gerbils that one could come up with something 'extremely interesting', some new 'discovery' regarding the effect and operating of the dye in conjunction with other variables. At the same time, the argument was seen as a reason for avoiding any direct attack on those promoting the dye, including F.D.A. Scientists would say, for example, that they would run into major problems using gerbils if they wanted to attack 'the dye people', and would refer to an earlier experience with the Sea Grant Office to support this expectation.[9]

Now what I have said so far about agents that figured prominently in considerations regarding the choice between gerbils and rats sufficiently illustrates I think the varying groups and institutions which these agents may represent. It also indicates that epistemic *and* non-epistemic considerations may be relevantly discussed with or influenced by agents in any of the professional categories; there is no one-to-one relationship between the profession or institution which an agent represents and the kind of argument in which he or she is taken into consideration (for example, Tom the scientist and member of relevant specialty groups appears in as many epistemic as non-epistemic considerations). Finally, it is plain that not all considerations invoked in technical choices can be directly linked to a scientist's current and concrete social relations. Universes of discourse and argumentation also feed upon the literature and upon memory and hence are partially independent of changing networks of interaction. Some arguments are difficult to place with respect to sustaining interactions because relevant agents are not mentioned explicitly. For example, it might appear from decision trail 1 in Exhibit 2 that grant agencies relevantly figured in scientists' arguments once the question of gerbils vs. rats had been translated into a question of research money. But they did not. Instead, the argument was translated further, and the concrete agents of greatest concern at this stage as judged from additional informations were students: those they would have to teach next term and to whom they would have to allocate research time, and those who might, as graduate students, be recruited (or required within course projects) to do some of the research work.

SUMMARY AND CONCLUSION

To sum up this paper I want to emphasize three characteristics of the relationship between 'knowledge' and 'society' explored in the last sections: First I have argued that the selections embodied in scientific inquiry serve as an anchoring point for one kind of 'knowledge-society' connection: in making technical decisions, scientists tie together different provinces of meaning and relevance implicated in their work. Some of these meanings can be classified as 'epistemic'consideration: at least on the face of it, they appear to promote the 'truth'-like character of scientific results. Others appear to be concerned with different matters, such as the convenience of obtaining or the proper time to publish certain results. The point is that the respective decision displacements are made by scientists themselves; it is *their* constant worry and concern to recognize, work out and negotiate appropriate connections. The 'knowledge-society' connection explored in this paper is routinely established by participants. This gives the analysis a methodological advantage but it also accounts for its particular limitations (see below).

Second I want to emphasize that the different meanings and relevances which scientists tie together in technical decision making mutually sustain and inform each other. More specifically this means that non-epistemic considerations may play an important role in guaranteeing the ongoing continuation of technical work. Even in the best of all worlds it would hardly be conceivable that non-epistemic arguments could be completely eliminated from scientists' technical reasoning. In fact one might go one step further and speculate that it could be the excursions into non-epistemic provinces of meaning through which technical success is built into some scientific products: through these excursions, scientific products may be pre-adapted to conditions of applicability and use within and without science regardless of the 'verisimilitude' of the respective results.

Third it is plain that if epistemic and non-epistemic considerations mutually sustain each other in technical decisions and are in fact necessary for the continuation of research it makes no sense to attempt to demarcate science from other areas of social life in terms of the objectively 'rational', 'cognitive' or 'truth-promoting' character of scientific procedure. Those who wish to pursue such demarcations must first show that a purely epistemic approach if adopted by scientists would indeed be feasible and successful in scientific practice.

To conclude let me stress that the 'knowledge-society' connection explored in this paper is limited in its implications: for example, it appears to have little relevance to debates concerning relativism which have been of key importance to the sociology of knowledge. Second it is also clear that we pay a price for the participant-centered model we have adopted. In scientific inquiry, agents will indicate their reasons in the manner exemplified above but rarely give us a clue as to how their preferred considerations relate to larger social concepts such a social structures, class interests or class relations. Thus without additional analysis on reconstructive evidence such as historical or contemporary macroscopic data and without further imputations, the analysis remains limited in scope from a sociology of knowledge perspective. Its greatest advantage is perhaps that the relationship explored are tangible and concrete. But it is clear that these relationships constitute but one of several conceivable 'knowledge-society' connections.

NOTES

[1] A summary statement of relevant approaches and a collection of essays by some of the main authors in sociology of scientific knowledge can be found in the same book (Knorr-Cetina and Mulkay 1983).

[2] This example is taken from MacKenzie (1981).

[3] See R. Harré (1981: 148ff.). See also Woolgar's methodological critique of interest models (1981).

[4] See also E. Goffman (1974), M. Callon (1975) and M. Callon, J. P. Courtial and W. Turner (1979).

[5] The distinction is introduced here as a tool to clarify and summarize the structure of decision arguments in the analysis that follows. As far as I can see, such distinctions have no ontological reality for participants themselves.

[6] The data analyzed in the following were collected in 1982—1983 at an East-Coast University Department in the United States. Experiments on the biopolymer chitin have since continued and in fact been expanded.

[7] 'Complete' as far as my data go. Presumably, no decision tree can ever be said to be fully complete.

[8] These and other interpretations in this analysis are based on scientists reports and comments such as those exemplified in Apendix 1.

[9] The report ran 'When I submitted a grant proposal . . . I would suggest gerbils, they came back to me immediately saying "Why do you use gerbils?" — which I did just automatically, I did not even think.'

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APPENDIX 1

DT 1 *"Gerbils are cheaper"**

R: . . . (gerbils) are also cheaper, it's also simply a question of money, or for us it was
I had money for some chitin work, but not for this kind of work. I used the money, I did buy the gerbils out of the grant, but I wouldn't have had enough to buy 200 rats . . . and I needed a number of other things-

K: You didn't want to apply for a grant and wait with the study?

R: I didn't want to *wait*, the main thing was I was just curious, I mean I was interested, and I wanted to do it!
It was also availability of time, I did the in vitro study myself when I was able to work in the laboratory during winter session, and then feeding studies followed, so I was just curious, since I had done the in vitro-

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R: But there was certainly a reason for me to be interested because if you show that there are significant differences between gerbils (and rats) in the sensitivity toward an approved dye that would have been something worth while-

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R: Well, the other reason was I was promoting (the use) of chitin and I felt that the dye binding properties had not been considered so far,
so I really wanted to publish a paper on the binding properties of the compound

K: This was a new thing-

R: It was a new thing, and chitin was a new thing, so to me it was something which doesn't necessarily need to wait around.

DT 2 *"Gerbils are more social"*

R: (. . .) they are more social than rats, you can keep them in a cage while you can't keep rats in the same cage

K: Oh, several-

R: Yeah, as a group it's easier because you would have the same *environmental conditions*-
somehow it seems to be easier to handle if you have five replications in one cage-

DT 3 *"Less food, less chitin"*

R: (. . .) and, I mean, the reason why we selected gerbils was also you need less food, you need less chitin, you need to go through less preparation problems, you have to produce less dyed chitin (bind dye to the polymer) . . .

DT 4 *"Diet, facilities and environment set up for gerbils"*

R: T., the person I worked with in the in vivo study had a lot of experience with gerbils, so the diet, the facilities and the environment was set up for gerbils-

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R: The diet was available and it was just less troublesome and more convenient to go through the gerbil *routine* . . .
What I mean is since he had been doing all this work with gerbils he knew exactly the diet requirements, I just needed to take his recipe, and he had even some of the- , like vitamin mixtures were *pre-mixed* already . . .
I thought it would be helpful, the more he knows about the animals the better it is if anything strange happens to explain or track down why it happened

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R: (. . .) I knew he (T.) wouldn't be too happy if I suddenly say I want to use rats (you) see he has a routine method developed, if you want, . . . he uses gerbils because they fit his experiments better . . . I just know this is his routine, and I was assuming I (would) need a lot of convincing to do just to-
And I was assuming if I do *pathology* like cutting them up I would definitely need him, and I was assuming I need him if something happens in the metabolism of the animals. Since he had a lot of experience, I think the main reason was being on the safe side, having somebody who knows, I mean I needed an expert so I decided on the gerbils because he was a gerbil expert.

DT 5 *"Weight variation of rats"*

R: I came across a study 2 or 3 years ago, they show that depending on the intake of water, the weight (of the rats) can vary up to 200%. Gerbils, since they are basically desert mice, they consume extremely little water, that means you cut down the potential variability to a rather low level.
For me this was an argument because I felt it is rather ridiculous to use experiments in which you have this high a variability-

One way would be to restrict the consumption of water but I don't know whether one knows the optimum water intake for rats under given conditions-
So I thought if they drink as little as possible this would reduce this variability . . .

DT 6 *"Follow Up"*

R: Then the second time it was logical to use gerbils again because it was a follow up on the first study

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R: The other reason was since the first study had been done with gerbils it made no sense to change the species-

DT 7 *"Gerbils more sensitive than rats"*

R: (. . .) I was curious since all the work has apparently been done with rats- I talked to N. who did quite a lot of work with gerbils and I asked him whether he came across anything which makes gerbils more susceptible to the kind of substance I am using in comparison to rats-
Because that could have been a reason, I mean something F.D.A. or the company missed, something to which gerbils are extremely sensitive, which would have been extremely interesting to show.

R: (. . .) F.D.A. would use rats instead of gerbils, it is known that different animal species behave different (when given) toxicants, so the question was whether we had discovered anything which had not been found when they fed the dye to rats. It didn't occur as a problem, and it occurs now because we are using gerbils . . .
I didn't follow up (on this) because data are usually not available to the general public and the company didn't release its information.
I just can't imagine that if all these same things occurred with *rats* that they would say its OK. Because it was not only the hair loss it was also the loss in weight-

* R: Researcher K: Interviewer