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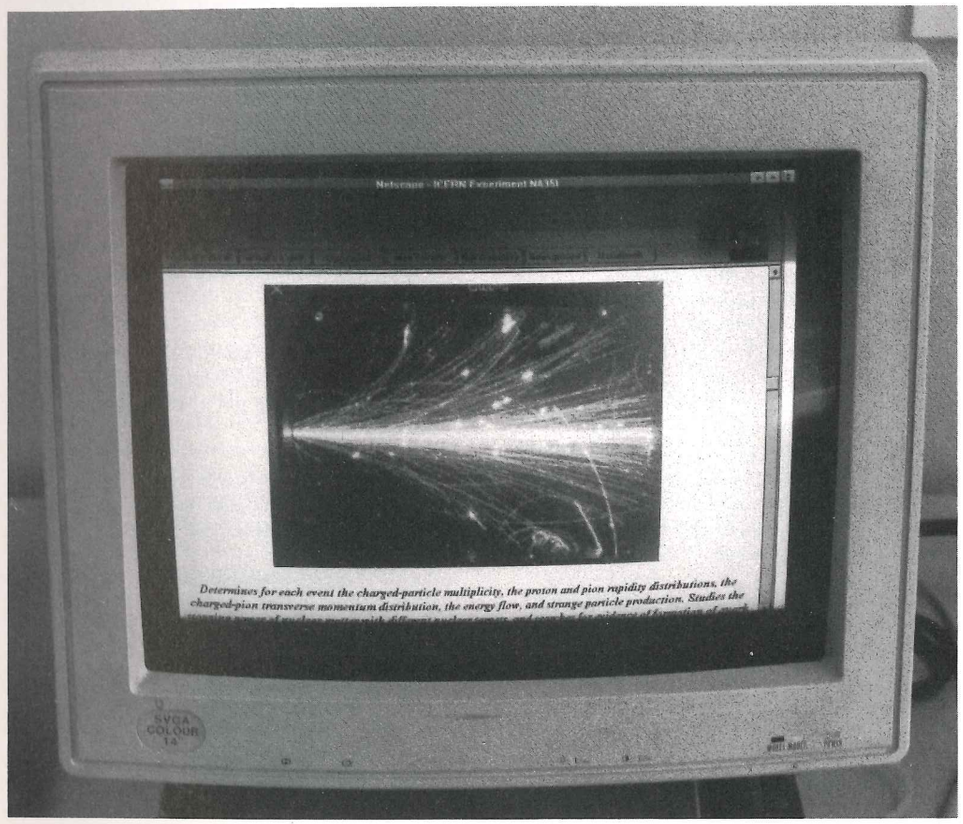

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Sociology of Science - Should Scientists Care?

On Knorr Cetina's Work on High Energy Physics

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At least since the 1976 formulation of the strong programme by D. Bloor (Bloor 1976) there has been an ongoing debate concerning a possible regress when accounts of science and scientific activity are given from the point of view of relativism.[1] In the sociology of science the question of regress is particulary relevant when the sociologist is labelled a 'constructivist' or a 'social constructivist', since relativism is often associated with these positions. This has to do with the enhanced role of 'judging' natural science which enters sociology of science when constructivist positions are acknowledged, i.e. when the sociologist is arguing about truth- or fact-making in natural science - what 'truth' status should one assign to the argument itself. The intension and interests of sociologists of science may not include judging natural science or evaluating its epistemic credibility, but the claim of fact-construction (implying that facts could have been otherwise) - at least - challenges the picture of natural science as dealing with indisputable facts of nature. These aspects of sociology of science have led to heated discussions between sociologists - or rather STS people in general - and scientists (the situation is sometimes referred to as 'Science Wars'). Examples of these discussions include the exchange which took place in August 1994 in the British Association for the Advancement of Science between Harry Collins and Lewis Wolpert (reviewed in *THES* (Collins 1994)) and the debate following *Higher Superstition* (Gross 1994), on the electronic mailing list 'sci-tech-studies'.

This article will discuss an analysis of experimental High Energy Physics (HEP) carried out by a sociologist of science and some of the (philosophical) foundations on which constructivism is based. The discussion is from the physicist's point of view - albeit not the view of an experimental high energy physicist but rather a theoretical physicist who is sympathetic towards the idea of 'construction' in experimental HEP. If there is indeed a war going on between STS people and scientists, I will attempt to remain in a quiet, reflexive, area of the battlefield. A central question in this article is: Given that sociologists make claims about the nature of scientific activity, to what extent should natural science react to these claims?

Social Constructivism or Constructivism

Karin Knorr Cetina is a sociologist of science who has had a major impact on constructivist ideas - a point which is illustrated by many references to her work and position (e.g. Callebaut 1993, Sismondo 1993 and Niiniluoto 1991). Reading her work on High Energy Physics presupposes some background knowledge in order to grasp what is contained by the notion of construction. Below, I will give a short review and some remarks on social constructivism and constructivism.[2]

The central point in constructivism (which Knorr Cetina also labels 'constructionism' in a 1993 article) is that scientific facts are constructed through negotiations, accidental events and interpretations. Scientific reality itself is also constructed by selective and contextual scientific laboratory practices. When one focuses on how the social and political environment contributes to the construction of facts, the programme may be labelled 'social constructivism' (the term 'constructivism' is usually linked to microsociological studies only, for instance

laboratory studies). Acknowledgement of this position is often followed by some remarks about relativism and realism which can illuminate how 'strong' the consequences of the constructivist program are, e.g.:

> We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist. Our point is that "out-there-ness" is the *consequence* of scientific work rather than its *cause*. (Latour and Woolgar 1986, p.180)[3]

It is somewhat unclear exactly what is stated here. It is not a simple relativist position, and the last sentence seems to be of anti-realist origin even though reality is not denied in the quotation. The position may be conceived of as transcending the philosophical debate on realism vs. anti-realism by introducing 'irrealism', a version of anti-realism/relativism where scientists, through their work, create reality which could have been different (see e.g. Hacking 1988). In any case it is obvious that the position expressed in the quotation stands in sharp contrast to a traditional scientific intuition where an objective reality or out-there-ness is the ultimate cause for scientific findings and facts. A milder version of relativism is the 'methodological relativism' where the sociologist simply has a different interest than the scientist, namely to describe *why* the scientific community at a specific historical time trusted a certain result instead of *whether or not* the result was true. Although these questions - context of discovery vs. truth - may be difficult to separate, this form of relativism is acceptable also from a traditional view of science since no scientist would disagree that some results have turned out to be correct despite the fact that they were originally considered wrong or vice versa. The relevance of methodological relativism may be granted also from a traditional point of view when scientists believe that truth will win in the long run. Existence of a final truth or a complete knowledge of how nature works is questionable even among natural scientists. A version of methodological relativism for laboratory studies is incorporated in the frame of reference of constructivism which can be seen from:

> The constructivist program has extended this idea by claiming that the information produced by science is first and foremost *the product of scientific work*, and what we should do is try to describe how scientific work produces scientific information, rather than locating the focus of the analysis between the finished scientific product and the world. (Knorr Cetina in Callebaut 1993)

In this quotation Knorr Cetina argues to shift the problem area of constructivism, to take it away from ontological speculations about reality and concentrate on how scientific facts are constructed. Moving the focus from ontological statements into analyses of science through the notion of construction, however, implies an ontological presupposition, namely that facts can be constructed. In another quotation, Knorr Cetina acknowledges also the stronger epistemic relativism which "asserts that knowledge is rooted in a particular time and culture" and "holds that knowledge does not just mimic nature and insofar as scientific realism wishes to make such claim, epistemic relativism is anti-realist" (Knorr Cetina and Mulkay 1983). It is claimed here that knowledge does not *just* mimic nature. Now *if* scientific knowledge in part mimics nature, for instance with respect to knowledge reflecting the resistance of material objects (e.g. we cannot move through walls - except by using doors), then there are areas of knowledge which essentially cannot be otherwise. Construction of facts are therefore constrained by not only social but natural factors or simply reality. Though Knorr Cetina states that "constructionism holds reality not to be given but constructed..." (Knorr Cetina 1995), she seems to acknowledge some sort of resistance from material reality:

> Constructionist studies have recognized that the material world offers resistances; that facts are not made by pronouncing them to facts but by being intricately constructed against the resistances of the natural (and social) order." (Knorr Cetina 1995)

Thus when a 'resistance from material reality' is granted, the notion of fact-construction should not be so scary to scientists as is suggested when the word 'construction' is associated with hard relativism or an 'any theory goes' position.

Nevertheless, a quantitative question like "to what extent is nature mirrored by scientific knowledge?" remains important to the question of how scientists should react to the claims of constructivism.

Theory-dependent data analysis in HEP

I noted in the introduction that I am sympathetic to the idea of construction in experimental HEP. Let me briefly explain. Experiments in HEP are typically conducted by accelerating beams of particles to very high energies and then bringing them to collisions in a detector. The point is to analyze the reaction products of such collisions to determine the constituents of the original particles and the identity of new particles formed in the collision. From the collisions to 'sensible data', however, is a big step. Raw data from the detector are filtered through a multi-step selection process which aims to cut away data coming from malfunctions of the detector or data irrelevant to the physics one wants to study. Since theory dependent computer simulations play a central role in this data selection process, it follows that the final experimental results can be limited by the theory used in the simulations (which is often the theory one is testing in the experiments). Even though experimental physicists claim that they are able to take this limitation into account it remains unclear to what extent one can still talk of an 'objective' experimental test of the underlying physical theory. Note that these concerns with construction in experimental HEP are aimed at the interpretational and technical aspects of the enterprise and do not focus on accidental events or social interactions in a laboratory such as negotiations among scientists.[4] Thus the question posed here is: Does the technological set-up and the data handling processes in experimental HEP guarantee that physicists extract facets of reality or is it possible that the results are 'constructed' by adjusting the apparatus and data selection criteria, so as to fit a particular physical model?

From a constructivist point of view the experimental results are always dependent on both the theory used and specific microsociological events such as evaluations, negotiations, strategic alliances between scientists, etc. However, whether one accepts a strong or weak form of realism, a necessary condition for natural science, if natural science is to be distinguished from pure religion, is the attempt or norm of objectivity. This holds irrespective of any sociological or historical constraints on natural science since what distinguishes religion from science is - at least - that the reality science operates within offers resistance. As argued above, scientific facts and theories are restricted by nature itself. Phrased differently, social constraints on scientific activity may be granted but these are not the only ones.

Experimental HEP according to Knorr Cetina[5]

I will review some of Knorr Cetina's points in these texts, reflect on them in the light of my own views on experimental HEP and examine how Knorr Cetina's notions correspond to constructivist perspectives as outlined above.

The first point does not relate directly to HEP but illustrates a difference in the underlying assumptions which can be found in sociology and physics texts. In "The Disunity of Two Leading Sciences" Knorr Cetina presents "two stories about kinds of empiricism", and a comparison between the different cultures of experimental behaviour in HEP and Molecular Biology.[6] For a physicist, Knorr Cetina's use of the word 'stories' may seem odd at first glance - is it not an account of the nature of scientific activity? The notion of a story could be a consequence of a relativist approach concerning the regress discussed in the introduction - the claim that facts are constructed and consequently could have been otherwise makes it difficult to offer conclusions on the basis of empirical investigations and even harder to make 'this is the way science really is' statements. Now, this may very well be besides the point of Knorr Cetina and perhaps it is also a trivial conclusion from constructivism that observations and facts in science studies could have been different. Ignoring for a moment the danger of putting too much emphasis on the metaphor of a story - it is interesting to take it at face value. I assume

that the motivation, at least in part, for Knorr Cetina's 'stories' is to pass a message which may be relevant to other members of the community and that the message is based on empirical facts and their interpretations.[7] Texts can of course be relevant to someone without containing any facts, e.g., one may find a particular poem highly relevant, but facts are likely to belong to the characteristics of scientific language. Science - including social science - differs from poetry in its use of logical and/or empirical arguments which include attempts to convince other scientists of a 'best' approach or, at least, that one's own position and/or studies are reasonable. Thus, scientific texts do contain facts regardless of their origin - that is - whether these are objective or a consequence of social practices. (I use the term 'fact' in the sense of either an empirical or a logical fact). Constructivism is an approach to science studies where attention is often paid to complexities and controversies in science. Studying complexities implies looking 'at' the complexities and in some sense the attempt to 'look through' the complexities seeking an understanding of them (for instance by deconstruction). Thus, the choice of constructivism as the theoretical framework for science studies seems to be an attempt to provide knowledge of scientific activity - knowledge which is supported by the arguments and/or empirical studies of the sociologist - and hence not solely a 'story'.

HEP as a closed system involved with 'negative' epistemics

In "The Disunity of Two Leading Sciences" (1994a) Knorr Cetina motivates her work by emphasizing the differences between previous laboratory studies (by herself, Latour and Woolgar, Lynch and Traweek) and the recent ones carried out by herself. According to Knorr Cetina, the old studies assumed that different empirical sciences followed similar procedures in obtaining results (the scientific method). In addition, although they discussed the construction of scientific knowledge with respect to negotiations, accidental events and interpretations, they did not capture the construction of scientific knowledge through the empirical machinery. Knorr Cetina's recent studies aim to investigate what the old ones missed (as I have argued earlier, the machinery in HEP is closely related to the interpretation aspects). Knorr Cetina might understand my concerns for experimental HEP, although, from the constructivist point of view, these concerns are not linked to HEP in particular but are rather a common feature of science in general. Nevertheless, HEP has features different from other sciences and Knorr Cetina sets off by comparing experimental HEP with the brain, arguing that:

> like the brain, particle physics operates within a *closed* circuitry. In many ways, it operates in a world of objects separated off from the environment, a world entirely reconstructed from within the boundaries of a complicated multi-level technology of representation. (Knorr Cetina 1994a p.3)

Knorr Cetina describes experimental HEP as mostly concerned with investigations of itself by studying the apparatus (limitations and malfunctions), and the various 'anti-forces' which complicate the extraction of sensible physics from the data samples (anti-forces include noise, smearing of the signals, and data from physical processes other than the ones the experiment aims to analyze). Another area of experimental work is 'limit analyses' which aims to put limits on possible knowledge. If a search for a particular particle is conducted in an experiment where a certain energy was available and the particle is not found, then it is concluded that the particle in question is most likely heavier (i.e. one has put a lower limit on the mass of the particle by not finding it in the energy regime investigated).[8] It is interesting to note that since experiments are mostly self-analyses and perhaps limit analyses in the above sense, HEP is left with the rather strange situation where the 'good' experiment (which either supports or falsifies a given assumption) is ruled out:

> Measurements in HEP appear to be curiously immature beings, more defined by their imperfections and shortcomings than by anything they can do.
>
> Purely experimental data, as physicists say, 'means nothing by itself'.

and

> Experimental data are wholly, utterly dependent upon a particular detector configuration and on the criteria applied in extracting information out of the detector. Another detector, another set of criteria, yields other measurements. (Knorr Cetina 1994b p. 111)

The experimental strategies and procedures of HEP are referred to as the 'negative epistemics' of HEP due to the complex relations between signs and objects, the emphasis on self or inward analysis and the key role of 'negative' knowledge - "knowledge of errors and uncertainties, of objects *not* of primary interest in a piece of research, and of self-limitations" (Knorr Cetina 1994b, p.101). In the conclusion of her analysis, Knorr Cetina praises HEP for its emphasis on reflexivity. HEP has turned reflexivity into a necessary principle for doing science by devoting many resources to self-understanding, self-observation and self-description. By referring to the problems of reflexivity in 'science studies', Knorr Cetina indicates that this field might have a lesson to learn from HEP and asks the question: "Perhaps it would be time to ask if we have to have foundations, whether we cannot build a theory of knowledge from circular foundations?" (Knorr Cetina 1994a).

It is not clear exactly what Knorr Cetina implies by circular foundations but, most likely, it could refer to the absence of generally accepted criteria and methodological standards for 'how science studies should be done'. As part of the reflexivity in HEP, Knorr Cetina comments on how physicists deal with the problem of having available different theories to describe the same data. According to Knorr Cetina, the physicists invoke the different theories as a source of error, and she indicates that other sciences, including science studies, might benefit from using this approach instead of splitting the field and scientists into different groupings (Knorr Cetina 1994a p.11). The 'different theories' referred to here, however, are in fact different phenomenological models which are not intended to provide any physical explanation of the collision processes, but they do play a key role in the data analysis. Moreover, the phenomenological models all have initial assumptions which are connected to the same overall physical theory - the general accepted 'Standard Model' for HEP. Thus, to label the different phenomenological models as different *theories* is an exaggeration and it is difficult to see how other sciences could benefit from implementing error considerations of the above kind.[9] Most scientists would agree with Knorr Cetina when she notes that "scientificity consists in considering all theories available, provided they are not completely outdated by recent measurements" (Knorr Cetina 1994a p.11) but, as I have indicated, it is not obvious in which sense 'theories' (phenomenological models) can be completely outdated by measurements in HEP.

The reality of HEP

In a passage on the objects that physicists work with (namely elementary particles), Knorr Cetina comments that these objects are, in a very precise sense, 'unreal' since "they are too small to be ever seen except indirectly through a detector, too fast to be captured and contained in a laboratory space, too dangerous to be handled directly" (p.4 in Knorr Cetina 1994a). This can hardly be an argument for the non-reality of physicists' objects. Does a 'real' thing need to be visible (the air)? How fast are things allowed to travel to be 'real'? Can something 'unreal' be dangerous? When Knorr Cetina adds that elementary particles are often very short-lived and so 'always rather history than present', one might ask how long something has to exist in order to be considered real?

Obviously, experimental HEP objects are not as easy to access and comprehend as cups and tables but to note them as 'unreal' on the basis of the above is too drastic an interpretation. As argued by Hacking (1982), one has to be a realist with respect to scientific entities when these can be manipulated in order to experiment on other entities, that is, when one has to rely on the causal properties of the first entities. (Hacking uses the example of the electron.) This is precisely the case in HEP when a beam of particles is prepared to investigate properties of other particles. Hacking's argument, however, leaves open how we should think about particles which are not used to manipulate other particles, because they are very short-lived. Particles in HEP are

usually 'seen' as peaks in certain diagrams (illustrating the outcome of an experiment). The physics argument for the 'realness' of these particles relies on what may be conceived of as their causal properties: 'Something' caused the peak and this something is called a particle. The question then is how can one be sure that the peaks are not just artifacts of the machinery (see below)? One can also ask about the pre-existence of subatomic particles, i.e., did they exist before they were 'discovered' in the experiment? On this point Knorr Cetina writes:

> Preexistence itself is a historically variable phenomenon; what objects are thought to have preexisted changes with these cultural practices and with scientific belief. Thus specific scientific entities like subatomic particles begin to 'preexist' precisely when science has made up its mind about them and succeeds in bringing them forth in the laboratory. (Knorr Cetina 1993)

This is probably as close to scientific anti-realism as it gets since, obviously, if the particles did not exist before experiments are conducted then the assertion of construction is unavoidable. To counter this position one can, aside from again arguing with Hacking, question what is meant by 'specific scientific entities'.

Imagine travelling to an island which has not yet been explored. Now, if you meet a new animal and assign it a name then the name may not have preexisted but what about the animal itself? This example may have little to do with physics but in the light of the above quotation it poses the question of how to distinguish entities which are 'specifically scientific' from those which are not. It is difficult to see the possibility of a position which acknowledges a 'daily-life' realism towards cups and tables but claims anti-realism with respect to scientific entities since the borderline is very hard to identify. If a chair exists as a material object then how about things which are seen only through a microscope? If, on the other hand, constructivism implies a general anti-realism thesis, then, besides the problems of material resistance mentioned earlier, it becomes impossible for Knorr Cetina to claim that constructivism is interested only in the actual scientific work and not the relation between scientific objects and the world. A general anti-realism thesis implies a particular point of view on this relationship. Nevertheless, I do think Knorr Cetina has a point about where it is appropriate to question what is meant by 'reality'. This has to do with the relation between signs (or symbols) and objects in HEP. Are the symbols (the message one gets that a tiny current has run through some part of the detector) pointing back to the fact that the object (an elementary particle) really was in the detector? This is indeed a central problem since - as Knorr Cetina describes - signs from interesting events are smeared and mixed with other signs perhaps from uninteresting physical events or perhaps from malfunctions in the detector. Since contemporary theory is an ingredient in sign processing and data analyses, the possibility exists that the reality of the objects is 'constructed' in accordance with the theories physicists believe. Thus, the machinery in experimental HEP might not just be an enlarged microscope.

How are facts constructed in HEP?

The claim that nature is a result of constructed scientific facts is probably the main point of disagreement between scientists and constructivists. However, a pragmatic interpretation may be agreed upon from both sides, namely that scientists understand nature *through* scientific facts (i.e., when the existence of electrons, protons and neutrons are established scientists say that material entities are composed of these particles). Nevertheless, most scientists would turn back and insist that scientific facts, including facts in experimental physics, are found or discovered in the realm of nature which is the cause and not the consequence of these facts. Even when scientists concede an approach to the history of science where 'discoveries' are anomalies which are later reinterpreted as discoveries, the construction of facts is not implied. Like the positivist tradition which grants irrationality in the context of discovery, the scientist may grant that discoveries are not simply 'stumbling over new facets of nature'. When it comes to the context of justification or the question of validity, however, I assume that most scientists would shy away from the notion of

construction, for instance, by referring to repeated experiments which either support or falsify a given discovery. As described earlier, the constructivist approach focuses on negotiations, accidental events, interpretations and in addition to this, Knorr Cetina takes into account the machinery of HEP (Knorr Cetina 1994a,b). It is not clear in Knorr Cetina's work exactly how all these factors enter the fact construction process in HEP but in any case she labels the experimental strategies and procedures as cultural preferences. At the end of *Epistemic Cultures*, a physicist P comments on the text, and it seems that his main objection is exactly this 'cultural preference' point. Physicist P argues that experimental HEP does not have a choice as to which strategies and procedures one should use:

> But it is clear to me that *we* cannot do our work in any other way. If we did not use *these* methods we would not generate any results. They (other sciences) can obviously do without this obsession with errors, for example. While we could not possibly. There is no cultural choice for us in this.

This comment by P can be read as a defense against constructivism. When P argues that physicists have no methodological choice, then the notion of construction is weakened. If the facts could not have been obtained in any other way, the role of, for instance, negotiations is diminished. Moreover, if the way facts are obtained in HEP is the only possible way, then it looks like it becomes a matter of taste if these facts should be regarded as constructed or indisputable statements of nature since no one would be able to tell the difference. Nevertheless, the position of physicist P seems too simple, especially if P implies that the world of HEP with all its experiments, methods and machinery is completely predetermined. To deny the role of cultural choices in HEP, which may or may not be rational choices, seems radical. Negotiations concerning which aspects of HEP are most important to investigate and which methods and equipment are to be used *do* play a role. Meetings are held where decisions are made from among a number of possibilities. In any case the assumption - that given a certain amount of money and a certain number of physicists with such and such abilities at a given time in history, would definitely lead to a particular way of performing science - places very high bets on rational choices in science. In the light of P's remark, there is an important lesson to be learned from Knorr Cetina's study which experimental high energy physicists could most likely benefit from. This is her focus on the importance of the distinction between the design/ construction of the detector (the apparatus) and the physics analysis. Knorr Cetina points out that this distinction penetrates all experimental work. Participants in the experiments, various steps in the project, and data itself are always either 'detector related' or 'physics related'. This lack of distinction leads to a different way of thinking at the two levels of the experimental procedure according to Knorr Cetina. Even from a traditional realist account of science, this makes the transition from experiments or data to the support or falsification of some theory in HEP very difficult. In this regard, it is worth noting the very complex situation in experimental HEP where hardly anyone has the possibility of understanding all the details in an experiment (400 co-workers in an experiment is not unusual at CERN - the European center for HEP where Knorr Cetina did her fieldwork). This point alone is certainly a problem for experimental HEP since it is difficult to determine completely where the facts came from. Thus, I agree with Knorr Cetina that there are cultural influences in experimental HEP like in any other field but, of course, this does not imply that the facts obtained are culturally or socially determined. As far as I can see, the notion of construction in the work of Knorr Cetina on HEP is somewhat ambiguous. It is not clear how much is implied by the construction metaphor or what the role of a material world independent of science is. (Recall Knorr Cetina's quotation where she notes that scientific facts are constructed against the resistances of the material world.) This ambiguity may be illustrated by contrasting the claim that nature is the consequence of scientific facts with the following two quotations:

> particle physics is perfectly capable to derive truth

effects from its representing operations. (Knorr Cetina 1994a p.3)

and

if one asks a physicist in this area he or she will say that the goal of it all remains to catch the (positive, phenomenal) particles which are still on the loose, to measure their mass and other (positive, phenomenal) properties, and nothing less. All other things are ways and means to approach this goal. There is no doubt that this goal is indeed what one wishes to achieve, and *occasionally succeeds in achieving*, as with the Nobel prize winning *discovery* of the vector bosons at CERN in 1983. (Knorr Cetina 1994a p.9, emphasis added)

Knorr Cetina states that 'truth effects' are derived from replacing the care of unreal objects (only a very limited amount of time in the experimental procedure is devoted to the actual study of elementary particles) with "the care of the self" (analyses of how the detector works) but she does not explain what she means by truth effects. According to constructivist ideas the notion of 'truth' cannot refer to truth outside the detector, or at least not to truth outside the minds of the physicists. But in this case one can hardly talk about success in catching positive, phenomenal particles,[10] or discovery of new particles. It could be that Knorr Cetina is simply using words that a physicist would use in describing what he/she is doing but it is, at best, a very confusing language.

Summing up, moving on

I have argued that Knorr Cetina and constructivism cannot claim disinterestedness in the relation between the scientific product and the world, and at the same time claim that scientific facts are the result only of negotiations, accidental events and interpretations. Thus, the core of constructivism (as I see it) is not reasonable. At the same time, I have focused on the problems of experimental HEP which, once recognised, give credit to constructivism. In this sense the realist/objectivist position of science is not threatened. On the other hand the focus on what goes on in science reveals aspects of this activity (here HEP) which should be taken seriously as limitations for scientific knowledge - not least by scientists.

Steve Fuller (Fuller 1994) and Steven Weinberg (Weinberg 1994) have had discussions in *Social Studies of Science* where, after reviewing Weinberg's book *Dreams of a Final Theory*, Fuller poses the question: "But can scientists and science practitioners go beyond mutual fear and suspicion - and towards public-spirited debate?" Weinberg's reply to Fuller (Weinberg 1994) and to science studies was much too quick, and emphasized misreadings rather than the central issues at stake. Another event which may also be seen in the light of 'Science Wars', took place at a session at the 4S meeting in Charlottesville 1995, where D. Haraway (1995) and the authors behind "Higher Superstition" (Gross 1994) focused almost entirely on the misreading of each others texts.

One reason for the controversy between science studies and scientists may be linked to the distinction between natural and social science as 'hard' and 'soft' science respectively. Though the validity of this distinction may be questioned (a strong social constructivist would probably deny it due to the claim that all science is social), it is difficult to avoid since natural scientists from the very beginning of their careers are taught that what they deal with most of the time are brute facts. If, on the other hand, a career in social science implies professors and books which have a variety of ways of looking at the same subject, cultural clashes such as 'Science Wars' are to be expected. Nevertheless, since there are both natural and social constraints to science, the context and content of science are worthy of study and though these two aspects are mutually dependent it is not only unfruitful but wrong to claim that one determines the other.

NOTES

1. This debate can be found in the literature on the history, philosophy, and sociology of science. Some recent examples are (Niiniluoto 1991) and (Collins and Yearley 1992).

2. A more detailed account can be found in Sismondo 1993. Constructivism has been used in various contexts and with different meanings in the literature but Sismondo attempts to encompass these differences.

3. In an interview with W. Callebaut (Callebaut 1993), Knorr Cetina takes the same position as Latour and Woolgar.

4. Even though interpretations may depend on results of negotiations or social conditioned criteria for success.

5. The following section is based primarily on chapter 3 of Knorr Cetina's *Epistemic Cultures* (1994b), entitled "The Care of the Self: Particle Physics and its Negative Epistemics" and on "The Disunity of Two Leading Sciences" (Knorr Cetina 1994a).

6. In this article I will concentrate on Knorr Cetina's discussion of HEP, leaving out aspects concerning Molecular Biology.

7. Assuming that some distinction between facts and interpretations can be made.

8. In physics mass and energy are equivalent.

9. A defense for Knorr Cetina on this point could be that the notion of 'different theories' seems to be taken from a physicist (Knorr Cetina 1994b, p.126)

10. The word 'positive' refers to the confirmation of actual particles as opposed to the negative knowledge i.e. all the experiments aiming to limit what one can observe.

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Comparing Notes

by Aant Elzinga

A review of Ulrike Felt, Helga Nowotny, Klaus Taschner, *Wissenschaftsforschung. Eine Einführung.* Reine Campus Studium. Frankfurt/aM band 1085, 1995, 322 seiten, 26DM

As the subtitle says, this is an introductory text on science and technology studies. As such it has the virtue of defining and locating the field historically and intellectually in a much broader terrain than what we usually find. Apart from the topics that figure prominently in similar books stemming from Anglo-American academic speech comunities, this one tries to do justice also to a number of additional entries. Chief among these are institutional studies, issues of science and technology policy in a critical perspective, and a discussion of the place and conditons of research in the humanities. The latter is a topic put on par with and treated as part of "science studies". This is natural considering the continental European definition of science as *Wissenschaft*, whereunder is also included *Geisteswissenschaft*. This broadening of the scope of the science studies field is welcome and corresponds with recent trends in the policy domain where the humanities are being played up as important dialogue partners with the natural sciences, medicine and engineering (without, for that matter, being able to expect more funding - on the contrary).

In keeping with their broader perspective the authors, who are affiliated with the Department of Theory and Social Studies of Science, also betray their humanistic ambitions and training by bringing to the subject a politically informed, critical and historiographically reflective approach. Touching upon macro- as well as micro-levels of analysis found in the literature, history and philosophy of science, social studies, gender studies and policy studies of science are all brought together under the common umbrella of *Wissenschaftsforschung*.

This is a term that is difficult to translate. The nearest English-language equivalent might be "Research on research" (which is comparable to the Scandinavian *forskning om forskning*), or perhaps "science of science", or even "Science research". The trouble with the first two is that they are too loaded with technocratic associations of managerial contexts, especially in the former Soviet Union, but also in the West. The third translation is too awkward. The ambition of the authors of this book is moreover anything but technocratic. First and foremost it is one of socially responsible scholarship. Their delimitation of the field is consciously informed by the dual heritage of STS, on the one hand that of the radical or critical science movement, on the other hand that of the more moderate "internalist" and "professionalizing" post-Kuhnian mode of sociology of scientific knowledge and actor network theory with their various conceptual shifts, branches, terminologies and methodologies.

In the very first chapter some of these historical roots and intellectual background features are highlighted, indicating how STS should be understood as part of a progressive, i.e., democratic politico-cultural trend of more than fifty years vintage, especially in Europe but also in the U.S. The historical trajectory charted out goes from Saint Simon, over Marx, Weber, Scheler and Mannheim to a concentration of a number of important events in the 1930s. Included are precursors to scientometrics, Boris Hessen's delivery at the 2nd History of Science Congress, the emergence of an early notion of science of science in Poland 1936, the work of the

Bernalists, and the debate on the steerability of science sparked off by their theses on the social responsibility of and need to plan science. The accent all the way through is on science, not so much on technology. Merton's identification of a system of norms is taken to reflect a liberal democratic tendency in opposition to totalitarian attacks on science qua institution and enlightenment culture. This history is brought forward through the thick of the Cold War and the McCarthy era up to the advent of 4S and EASST in the mid-1970s. Some readers might want to see something on the German finalization debate included, compared for example to the Bernal-Polanyi debate, contrasting the situation and contexts.

The foregoing account, which is much more detailed and multifarious than what has been sketched here, is in keeping with the authors' conviction that the function of STS is to contribute to critical historical self-reflection over and in the sciences. This, it is suggested, may be done through three parallel analytical lenses. One is with a focus on the (among other institutional) interplay of science, technology and society broadly speaking, but also yielding specifics amenable to case studies. A second is with focus on societal and cultural conditions (and conditioning) that facilitate and are constitutive of science and research, all the way from the level of its differentiation(s) and institutional fabrics to the realms of imagery, norms and cultural goods that play into the self-definition and upholding of boundaries or relationships vis-à-vis other domains of societal endeavour, like religion, which historically speaking is seen to have had a heavy bearing on the shaping of science and its multiple discourses, its dynamics, and the posing of questions concerning its authority and/or legitimation at various points in time. A third focal point is the one that has been most prominent of late, i.e., a concern with the nitty gritty of scientific practices, the interplay of materialities and the cultural in the lab, and how scientific knowledge is (socially) constructed at its sites of production.

Thus emerge three broad, partially overlapping domains of STS. These are defined on the basis of a common denominator - the quest for (self-) understanding of (and in) what has become a major cultural force in our time: science/wissenschaft. From this vantage point the so-called "science wars" debate is seen as yet another instance of *Kulturkampf*, the latest in a series of such if one looks back into the past, at least in the continental schools where one has had scrimages around hermeneutics and *Methodenstreit*, where *Natur-* and *Geisteswissenschaften* were pitched against each other, with compromises like the emergence of *Kulturwissenschaft* (Cassirer). The delimitation of the field in terms of *Wissenschaftsforschung* as proposed in the present volume also seems to invite closer participation from scholars in places with stronger disciplinary identities, like political science, history and sociology which also deal with knowledge society, risk and reflexive modernity from critical points of view. Moreover the authors' approach clearly privileges the humanities and cultural sciences, as distinct from more instrumentalist (unthinking?) modes of analysis found at business schools. This does not mean that scientometrics, science policy studies, or other "harder" tools of the trade need to be excluded. In the present text they are certainly included, but in a way that does not let us forget the broader flux of contingencies, be it of institutions, corporate frameworks for industrial growth, state and power politics, or social and epistemic differentiation processes at work in disciplines and the articulation of disciplinary identities and claims. All of these, it appears the authors are saying, are significant at one or another level in the negotiational and truth claiming practices that take place at (and around) the sites of scientific knowledge production.

When it comes to making sense of life at these sites the authors want to enrich Bourdieu's scheme of reproduction of symbolic capital(s) with Latour's and Woolgar's account of credibility cycles. This in turn is overlayered with David Edge's analysis of various types of regulatives and conditions involved in maintaining competition within bounds of the non-antagonistic as distinct from the antagonism that gives rise to and is part of

the substance of science and technology based controversies in our contemporary societies. This seems to be a fruitful framework for controversy studies, a subject the authors are well equipped to elaborate on but have refrained from doing so here. Perhaps in the next edition one might see a specific chapter that takes this (controversy studies as a genre) further, with exemplifications from the many case studies that now exist in the literature.

Gender studies is a genre that is not forgotten. It has a chapter of its own. Among others it is seen as a field where scholarship has been able to show historical and political contingency, not as external contextual determinants but as constitutives right into the heart of cognitive structuration. Considerable space is given to the "first wave" of the women's movement in science studies and the efforts to make visible and reconstruct women's contributions in science, as well as explanations of the economic, social, cultural and academic mechanisms whereby women were excluded or made invisible at various points in historical time.

A second section reviews findings that deal with gender as a determinant in the hierarchisation of social location and the distribution of rank and rewards in (and between) sciences and the humanities. Two further sections take up the question of the social construction of gender difference in science and other issues related to feminist science criticism: standpoint theory, essentialism, and identity politics subsuming "new forms of representation". Similarities and differences of approach and analysis among Evelyn Fox Keller, Sandra Harding, Donna Haraway and others are briefly explored. One of the strengths of this chapter again is its broad reflective historiographical mode of presentation, which tends to add a social epistemological dimension to the trends under discussion.

Another chapter gives an insightful review of concepts and perspectives of the "newer" STS, building on Callon's identification of four models (in the *Handbook*). The tension between philosophy of science and the cognitive (anti-rationalist) turn in Social Studies and with it the discussion centered around the Duhem-Quine thesis and paradgim theories is reviewed as a backdrop to a presentation of essential tenets, topics, shifts of "program" and a few influential case studies within or associated with SSK. (Some of the earlier ones on reciprocity between natural and social order - e.g. phrenology - are unfortunately left out.) This is followed by an outline of laboratory studies and actor network theory, with reference made to several genres in this domain - Knorr-Cetina, Latour and Woolgar, Callon and advocates of the rhetorical or semiotic turn of analysis. Today there are many books and articles that cover this episode; despite the brevity of the treatment in the present book this digest is hard to beat in terms of independence and pointedness of succinct presentation.

Chapter six is something of a novelty in that here the social sciences and *Geisteswissenschaft* are added to the science studies map. We are given an interesting recapitulation of some of the current discussions on the role of the humanities that has emerged in several countries in connection with evaluation exercises. Are the humanities compensation potential to balance technocracy and alienation in science-permeated high tech society? Or do they have some other utility and function? Breakthroughs in the neurosciences (with prospects of neuropharmocometics), genetic biology, nanotechnologies, AI and computerized visualisation techniques prompt questions of identity - what is it to be "human"? - therewith challenging the humanities with some of its age old questions in new and urgent forms. At the same time the conditions of doing research in the humanities are also changing rapidly. To be sure, all of this is a signficant part of making the case for considering "science studies" as having a broader scope both in its objects of research and approaches. Research ethics and bioetchics as topics might have been played up more in this context too.

The final three chapters deal with topics that show how science and technology policy studies, when framed in a self-reflective historiographical perspective, are really integral to STS. For this reader at least the book provides a rationale for paying much more attention to policy relevant issues, science communication (conceived more broadly and critically than PUS - public understanding of science), and interplay between institutions; we are shown how these can be tackled without giving up our science criticist agenda. This applies equally to technology assessment, risk analysis and the role of social movements in the shaping of technologies. In relationship to the triangle drama of macro "actors" - university-state-industry I miss a fourth one, civil society.

All of these topics have become the basis of more limited specialist discourses; here they come together with the topics of the previous chapters in a natural way, without for that matter stranding in some species of Low Church eclecticism. The quality of analysis, the selection and up-to-dateness of the literature, and the synthetic achievement are all commendable. The readability of the book is enhanced by the inclusion of lists of literature with comments after each chapter, as well as a 12-page glossary of frequent STS terms and concepts (we might quible about ones missing too), plus a 4-page list of journals in the field, again with comments. The bibliography is useful, even if I would have liked to see something of the German finalization theorists, controversy study texts like Engelhardt and Kaplan, as well as some bio- and research ethicists included.

In sum, what we have here is "the introductory textbook we have all been waiting for". My only regret is that it only exists in German. Hopefully EASST's Publication Committee can do something about this, so that it will become available in English too, so we can start using it more extensively in our classes.

Also recommended is another publication from Vienna: Ulrike Felt and Helga Nowotny (eds.), *Social Studies of Science in International Perspective* (University of Vienna, Institute for Theory and Social Studies of Science, 1994), 142 pp. It contains the proceedings of a workshop at which several well-known scholars address a few thematic questions (e.g., the new production of knowledge) and gives brief reviews of the state-of-the-art in their respective countries, mostly pointing to who (what groups) are doing what. This report is also useful for those who need introductory materials to provide orientations into STS for their curricula, or to argue for the field in front of their faculty boards, research councils and central university authorities.

Entries refer to Britain, France, the Netherlands and Austria. German, Portugese, Spanish and Scandinavian country studies do not appear here. It is always stimulating to compare notes between countries. Apart from an orientation on the various "turns" in the discourses at hand, one of the things that emerges from this little volume is that STS is still beset with many difficulties when it comes to gaining a recognized institutional space in university landscapes in Europe. Discussion and comparison of strategies for institutional capacity building and interchange tend to be exhausting but are certainly not exhausted.

Mathematics, Quantification and Social Change

by Adrienne van den Bogaard

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Review of Sal Restivo, *Mathematics in Society and History*, Kluwer Academic Publishers: Dordrecht 1992, and Thedore M. Porter, *Trust in Numbers*, Princeton University Press, Princeton, 1995.

Mathematics and number-worlds are very influential in our daily lifes. We all have to learn some mathematics at particular moments in time, so we can buy chicken at fixed prices per kilo, work with grade point averages, organize schedules, understand policies established for noise levels at work places, etc. The use of numbers in the world around us seems so obvious; numbers seem so logical in the way they are established as well as in the way they function that its use doesn't need any explanation or further exploration. Do we ever doubt that 2+2=4? Do we ever check whether a liter of milk bought in the supermarket actually contains one liter? The truth of mathematical representations seems to leave hardly any room for the study of the relation between mathematics and society. This review will show that the opposite is the case. Sal Restivo and Theodore Porter even have opposed views on the relation between mathematics, quantification and social change.

Restivo's and Porter's books on mathematics and quantification are very different in focus, argumentation and style. Restivo is the sociologist, Porter an historian of science. Restivo studies the development of mathematics as a discipline. Porter studies the role numbers play in bureaucratic, political environments and in scientific communities working under pressures from the outside world. Both tell big stories; taken together they ambitiously grasp the whole world except Africa and Australia. Restivo and Porter share a perspective on the making of knowledge: both stress that they are somewhere in the middle between the realists and the relativists. By starting with the statement that scientific knowledge (about objects and processes) is a social process through and through, they both stress that this does not mean that the knowledge is random and completely arbitrary. Knowledge can't be made whichever way scientists would like. Having said this, both focus on the question as to which social processes create knowledge.

David Bloor was one of the first in STS to open up mathematics to social enquiry. He used the mathematical discipline perceived as the ultimate producer of 'truths' to show the possibilities of the 'strong programme' (*Knowledge and Social Imagery*, London: Routledge 1976). Bloor situated the locus of sociological enquiry in the selection of possible mathematical orderings of (physical) objects. This implied some form of 'alternative mathematics' which Bloor exemplified by comparing Greek ideas about 'number' with our own. Oswald Spengler, author of *Decline of the West* (first published in 1918), spent one chapter on the Meaning of Number which apparently inspired Bloor to think about an alternative mathematics (p95).

Restivo presents his book as 'the first by a sociologist fully devoted to a sociology of mathematics.' Restivo has also been inspired by the writings of Spengler on whom he spends his first pages. Spengler has been cited as an important though neglected source of inspiration for sociologists by Randall Collins and Restivo in the "Development, Diversity, and Conflict in the Sociology of Science" (*The Sociological Quarterly*, Spring 1983, pp185-200): "Oswald Spengler did treat science and mathematics as culturally related outgrowths of the historical ethos

("soul") of each civilization. But he had little influence on the emerging sociology of knowledge, in part because he did not link ideas to specific social classes or institutions" (p187). In his book Restivo tries to rehabilitate Spengler by exploring his thesis 'that there are as many number-worlds as there are cultures'. To many (social) scientists mathematics is a unique mode of knowing that has produced universal orderings. Spengler, however, attacked this privileged status of mathematics as an intellectual or scholarly discipline:

> We find an Indian, an Arabian, a Classical, a Western type of mathematical thought and, corresponding with each, a type of number - each type fundamentally peculiar and unique, an expression of a specific world-feeling, s symbol having a specific validity which is even capable of scientific definition, a principle of ordering the Become which reflects the central essence of one and only one soul, viz., the soul of that particular Culture. Consequently, there are more mathematics than one. (Spengler, Cited in Restivo, p8)

In part II of his book, Restivo works out what he calls the weak interpretation of Spengler, i.e., the consideration that mathematics is a social and cultural phenomenon. He aims at developing a sociology of mathematics: 'Numbers are social through and through.' In my opinion he is most successful in chapter 7, 'Conflict, Social Change, and Mathematics in Europe', which is both consistent and humorous.

"The ideals or norms of science do not *cause* scientific behaviour, but emerge from the struggle for individual success under different conditions of competition." This is Restivo's structuring argument in writing his history of the emergence of a European mathematical discipline. The controversy between two Italian mathematicians, Cardan and Tartaglia (in the first half of the sixteenth century), reflects a transition from a situation in which solutions to mathematical puzzles were kept secret to one in which it was normal to share intellectual properties. Cardan's advantage was the result of his decision to publish his solution to cubic equations. The contest between Cardan and Tartaglia was settled by a mathematical duel, which was a traditional way to earn your money among sixteenth-century mathematicians. Cardan came out as the winner because Tartaglia had withdrawn.

The next controversy Restivo goes into is the one between Leibniz and Newton, a priority-dispute about the infinitesimals (second half of the seventeenth century). This controversy, Restivo claims, reflects the shift from informal message centers, where knowledge was personally delivered from by one scholar to another, to organisations (e.g., the Royal Society) with printed knowledge in the form of journals. 'Leibniz must rank as one of the most successful organisation builders in the history of science' (p71).

The chapter ends with a critique on Merton and Kuhn in favour of his own argument. "In no case do we find a mathematical change *centered* in a struggle between rival traditionalists and innovators. Moreover, the long-term trend in Western mathematics has not been towards a single, dominant paradigm, but rather towards rival schools at odds over fundamental questions about methods and knowledge" (p85-86). In the case of mathematics the state of the discipline resembles more the social sciences than Kuhn's image of normal science. Therefore, the dynamics of mathematics can best be analysed in terms of the changes in organisational forms.

Another very nice piece in Restivo's book is his attempt to develop a sociology of 2+2=4. This 'representation' seems to be the exponent of the kind of universal truths mathematics produces. He starts with citing some mathematical realists, one of whom claimed that 2+2=4 accurately described the encounter of two dinosaurs with two others, although no one has ever observed this event. There cannot be any culture-bound answer to the question of 2+2. It's always 4. Considering these attributions of truth to this expression, it is remarkable that within the mathematical community Whitehead and Russel (who are the exponents of the relation between mathematics and logic) wrote a lengthy book of 800 pages to prove the truth of 2+2=4. How true actually is 2+2=4? Restivo argues that there must be more at stake. For him, the fundamental question here is what these kind of number-truths do in social relations. "It is not just that something *is* or *is not* logical in some absolute sense. It

is that logic - and certainty relations in general - are cultural resources that can be used to defend or attack a social order by affirming or denying self-evident statements" (p114). To rephrase this we could say that these kind of mathematical truths function as resources of power.

Restivo goes on by empirically problematizing 2+2=4. "Adding is in general empirically problematic." What happens when we add two cups of rice and two cups of water? The ones absorb the others. This example may look like a misunderstanding, referring to the general truth that we can't add apples and oranges. However, whenever we add we make abstractions and generalizations. If we add two books we could ask questions about the numbers of pages or the authors. When we chose to add two books with the same number of pages, we could ask questions about the sort of paper the book is printed on, etc. To rephrase it in constructivist terms, adding implies similarity, and similarity is problematic.

One of Restivo's main claims is that the competitive structures of the mathematical discipline forced mathematics into an ever ongoing process of abstraction and generalisation. The more mathematics developed, the more mathematics itself formed the basis for new mathematical knowledge.

The idea that mathematical representations are made into certainties by people outside the mathematical community in the first place raises a question which happens to be central in Porter's book, namely "How are we to account for the prestige and power of quantitative methods in the modern world?" (pviii). Porter is an excellent story-teller; his book consists of stories of accountants in the US, engineers in France, statistical bureaus, cost-benefit analysis and many smaller histories like the development of the metric system. All the stories are about knowledge developed by experts under more or less bureaucratic and political control. The moral of these stories is, to put it bluntly, that quantification supersedes elitism. The less experts derive their authority from the institutional context the more their work is organized along strict rules (which Porter calls mechanical objectivity) in order to produce controllable numbers.

The stories carry one main lesson, and serve to illustrate one main point and one main answer: numbers are technologies of *trust*. The development of capitalism and the emergence of national states with one central government resulted in a replacement of face-to-face relations by relations at a distance in the domains of trade and politics. In a period in which personal trust could no longer be the basis for negotiation, numbers were given this role. The amount of trust attributed to and built in numbers is however a matter of degree. Porter distinguishes France and England from the United States on the question where experts derive their authority from. In France, experts were educated at the École Polytechnique which was very close to the French state bureaucracy. Bureaucrats and politicians imputed the value of experts' personal judgments to their *Bildung*; discretion was highly appreciated. In England, it was not so much the education as the 'mobility' of the experts through bureaucracy which had given them their authority. Experience in different domains - instead of specialist experience only - resulted in trustful personal judgments of experts. Democracy in the US resulted in a need for impersonal judgments; expert discretion could always be disputed in court. This provided an impetus (not always to the expert's pleasure) to quantify their knowledge. Porter ascribes to numbers a kind of objectivity which can be characterised as 'impersonal knowledge', the exclusion of subjectivity. Therefore, in Porter's opinion, there is a relation between quantification and democracy, and the expert *Bildung* and technocracy. "The pursuit of rigor flourishes mainly in conjunction with democracy. ... The regime of calculation involves a bid to empower experts who have at most a limited ability to subvert democratic control. Technocracy presupposes relativily secure elites" (p146).

It seems to me that Porter is arguing that quantification tends to break down elite cultures. He mentions quantifrenia in the bureaucratic management of diversity (p76). Quantification and impersonal knowledge go hand in hand; not the blacks themselves but the figures tell us about their oppression.

Porter considers quantification a liberating and emancipating force, supporting social change. Restivo, makes a completely different plea. "The realm of the 'logical', 'rational', 'scientific', 'objective', and 'quantitative' is, among other things, a realm of ideas that symbolize the reigning social order, and inevitably become targets of opponents of that order" (p135).

While Porter claims that striving for a specific sort of objectivity and a changing society went hand in hand, Restivo claims, as we have seen, that objectivity and a stable order go hand in hand. These opposed views seems to suggest that the relation between quantification and social change/stability is a dynamic one. In the late nineteenth century there was a great concern about the extreme poverty of the working class. This led to the idea of measuring nation's national incomes to be able, among other things, to gain insight into the distribution of this national income. In the Netherlands, for example, it was the well-known socialist W.A. Bonger who tried to establish a national income figure. In the 1920s, this led to the phenomenon of the 'budget statistics' that required a huge system of measurement. We could argue, at this point, that the quantification of nation's wealth was at least partly an attempt to show how small laborers could share this wealth. Although the poverty of the working class must have been very obvious in terms of bad clothing and housing, a quantified proof of this poverty made the problem 'objective'. One could no longer deny the problem. Poverty was redefined as a problem of the distribution of national wealth. This part of the history supports Porter.

After the 1940s, these budget statistics were one of the roots in the later development of national accounts and macro-econometric models in the Netherlands. During the second half of this century, government increasingly used economic data in the interpretation of economic events and in the preparation of economic policies. These numbers became part of a consensus on how to think about economic life of the established bureaucracy. One consequence was that left-wing parties and progressive movements had more and more problems getting their different views accepted as at least a possible viewpoint on the economy. All the other parties accepted the economic data produced which increased the 'objectivity' of these numbers. As Restivo argues, "In general, the wider and more diffuse the social interests embodied in a representation, the more it qualifies as objective" (p125).

This Dutch example of the history of quantification in economic policy suggests that questions about quantification need to be historicised as well as sociologised. It suggests that we can't speak about numbers per se, but that we always have to ask 'whose numbers?'. And then we come back to Restivo's point about the limited empirical relevance of numbers. We have to ask ourselves what these numbers embody and for what purpose they were constructed. The problem then becomes who is actually able to deconstruct numbers. Everybody who has ever watched a quiz on television knows how scared people are when a numerical math problem is to be solved.

In my opinion, Porter's argument about the relation between 'openness' and quantification misses precisely the fact that numbers also have a mystifying role. Who is actually trained enough to deconstruct numbers when the need arises? For many people the origins of numbers, let alone when mathematical symbols enter the arena, are highly obscure. People believe them or they don't, but seldom do they themselves check the numbers about the economy's growth. Porter himself must have recognized that it would have cost him quite a lot of readers if he had included complex nineteenth century mathematics used by the Ponts engineers. He tries to verbalise some quantitative arguments, but never writes down any number relation, let alone a symbolic equation. Doing this would have closed his text for those not very educated in numbers and symbols. The same holds for Restivo's book: why does he mention, for example, 'transcendental numbers' without explaining them?

Let me finish with a comparison with technology studies. During its history we have had advocates of "technology as emancipating force" and advocates of "technology as oppressing force". Nowadays

we know that both positions are as true as they are ridiculous. Sometimes technology is emancipating, sometimes it is not; sometimes it emancipates the one group, while it oppresses an other. Sometimes technology emancipates in one period while it oppresses in another period. We, who study the role of mathematics and quantification in society, and the way society and culture have shaped mathematics, should take over these lessons.

Quantitative Measuring or Qualitative Understanding: Is it possible to bridge the divide in STS?

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Report of a Workshop at the University of Amsterdam, 21-24 May 1996.

"Measuring is knowing" is a truism in the sciences. In the interdisciplinary field of science and technology studies (STS), however, a large gap exists between quantitative and qualitative approaches. There are a few exceptions, but most scholars restrict themselves to one of these repertoires. As a consequence, scientometric studies are sometimes sociologically naive while qualitative analyses often neglect possibilities to test their hypotheses. This divide — might it be coined a great divide in science and technology studies? — is visible in methodology sections of articles, in conference selections, in the composition of personal networks of STS scholars, and, lastly, in their citation behaviour.

The quest for more integration between qualitative and quantitative perspectives was central to a workshop organized by the Network of European Centers in Science and Technology Studies (NECSTS). NECSTS has organized an annual series of ERASMUS workshops since 1992 designed to stimulate the vitality, social relevance, and intellectual coherence of the interdisciplinary field of science and technology studies (STS). Each of these workshops focuses on a specific specialty or discipline contributing to the wider field of study. Previous workshops have addressed "university-industry relations," "public understanding of science," "history of science and medicine," etc. In stimulating discussions in working groups, those attending have ample opportunity to reflect on the usefulness of the pertinent approach for their own research.

In 1996, the workshop focused on *scientometrics*, the quantitative study of science and technology. As is well-known, the emergence of scientometrics as a specialty within the STS field is strongly associated with the creation of the *Science Citation Index SCI* in the 1960s. The importance of the *SCI* was immediately recognized by sociologists and historians of science like Derek de Solla Price and Robert King Merton. They contributed to the organization of the citation index (Elkana *et al.* 1978; cf. Wouters 1996), especially in the early years of its existence. The *SCI* is still the dominant data source for many scientometric studies.

During the 1980s, other databases and new approaches to databases became available. Crosslinks have been firmly established between scientometrics and the quantitative study of patent statistics ("technometrics"). Scientometrics has also been influenced by approaches used in other fields focusing on measurement (like informetrics, with its roots in library and computer sciences).

From the perspective of scientometrics, three interfaces can currently be distinguished:

* The interface with theorizing in the sociology, history, and philosophy of science. Problems of probabilistic inferencing, of organizing large datasets, and of testing for the significance of differences are pervasive in these disciplines, and the researcher may wish to turn to the more quantitative side of the field for expertise (cf. Leydesdorff 1995).
* The interface with science and technology policy issues. Increasingly, sophisticated evaluation and forecasting studies are called for by policy makers. Scientometricians have been asked to map the future possibilities in specific fields, e.g., the choices on the agenda for Dutch physics (Glänzel *et al.* 1995). The relation between evaluation and forecasting is a crucial issue in debates at this interface (cf. Irvine & Martin 1984). In methodological terms, researchers focus on questions about the reliability and validity of indicators.
* The increased use of S&T in advanced knowledge-based economies raises a third set of problems. How do these complex systems evolve? Is it possible to model their interactions? Recently, Gibbons *et al.* (1994) have distinguished a traditional "Mode 1" type of science from a new "Mode 2" type at the interfaces. Others have suggested a triple helix model of university-industry- relations (Etzkowitz & Leydesdorff 1995; Leydesdorff & Etzkowitz 1996). Indicators of these developments must develop means to incorporate changes in the social environments of science and technology: for example, what counts as "biotechnology" is different in 1995 from its delineation in 1980.

Furthermore, the spread of the desktop environment, i.e. using the personal computer and workstation in networked enviroments, is revolutionizing scientometrics like many other fields of science. One tends to become less dependent on mainframe computers and established research institutions. The specificities of studying cognitive interactions like in science and science-based technologies, however, require methods different from those readily available in computer software for the social sciences (although there is some overlap). In summary, now is a good time to make these techniques available to more qualitatively oriented colleagues and policy makers so that frameworks for future collaborations can be explored.

The Amsterdam workshop was structured along these intellectual lines. Forty-four participants discussed these issues in both plenary sessions and smaller groups. Most participants were from Western European research centres; additionally, there were participants from Russia, India, Rumania, Hungary, and Australia. Each workshop was devoted to a specific topic: the social conditions of scientometrics, scientometrics and European science policies, scientometrics and STS theory, and scientometrics as a policy instrument. Special attention was also given to Ph.D. projects using scientometric techniques.

Arie Rip (University of Twente, The Netherlands) faced the central questions in his opening presentation by focusing on the relationships between scientometrics and the practices of science. He distinguished three consecutive phases in the development of scientometrics: the times of Derek de Solla Price that Rip characterized as "data-driven"; the late seventies and the eighties when science indicators became "the real business"; and present-day scientometrics.

What might be the difference between present-day scientometrics and scientometrics of the previous period? Rip formulated it as follows: "In my view, the third phase is one in which representation is becoming less interesting; nowadays, the added value of indicators and maps (for various audiences,

scholarly and otherwise) counts." But how should one assess the validity of indicators if they are no longer underpinned by claims of "objective" representations?

Rip proposed to distinguish between "natural" and "artificial" aggregations in science and the study of science for solving this problem. The sciences themselves are systems of communication that continuously organize and reorganize these communications in order to make them available for evaluation. The creation of databases like the *Science Citation Index* has made it possible to confront these "natural" aggregations with aggregations based on "artificial" reflections of the science system. This raises a number of questions, such as the priority of the various aggregations, the congruency among them, and the validity of the one aggregation (e.g., peer review) vis-à-vis the other (e.g., scientometric evaluation). The speaker noted that in some cases the independent evaluation by policy makers using scientometric information has already had a significant effect on the underlying processes of knowledge generation. This feedback loop induces discussions of quality control on the side of scientometrics that require discussions with non-scientometric colleagues in STS and policy analysis (cf. Glänzel 1996).

Rémi Barré (Observatoire des Sciences et Technologies, Paris) analyzed the complexity of the system of mediation between science & technology and policy making in terms of functions that have to be fulfilled in advanced industrial systems. These functions are carried out differently in various national systems in terms of the institutional divisions of labour between bureaucracies, universities, and para-university research. The author emphasized the need for entrenchement in national systems because of the availability of specific statistical resources, while national experiences need to be communicated so that international standards can be developed and codified in order to facilitate comparisons.

Ulrich Schmoch (Fraunhofer Institute, Karlsruhe, FRG) elaborated this perspective with a discussion of the specificity of the technology side of the system. Patents are different from publications since they refer to the capitalization of scientific knowledge. Recent advances in the dynamic modelling of these interactions were discussed. Although they occur along a different axis, Schmoch also emphasized the systemic nature of these developments, and the need to define units of analysis, levels of aggregation, and to hypothesize mechanisms for the transfer of knowledge.

Tibor Braun from the ISSRU-unit in Budapest (Hungary) explained the independent processing of the *Science Citation Index* at his unit. He showed how the data is processed into indicators in various dimensions and time horizons. The trade-off between capturing the complexities of the system and the need to provide policy makers with comprehensive information was discussed in terms of the dimensionality of the representations. Braun suggested that the development of three-dimensional indicators be considered.

Possible applications of the noted development of "desktop scientometrics" were demonstrated by Sylvan Katz from the *Science Policy Research Unit* at the University of Sussex (UK). Katz presented his findings along these lines, using UK-data. He noted that the various national centres are "cleaning the data" for their respective countries. Perhaps one should agree on standards and organize the data internationally using internet and Unix as well as Windows-type programming. The audience agreed that desktop scientometrics provides policy makers with a laptop-model for asking specific questions, while such a system would provide the community of scientometricians with almost unlimited domains to discuss interesting questions concerning the research systems under study. However, intellectual property rights make it necessary to pursue these discussions in gremia other than this workshop.

Katz's suggestions were underscored by a report by Paul Cunningham (PREST, Manchester, UK) about the use of scientometric evaluations in the various EU S&T programs. Although these programmes are committed to high-quality evaluation procedures, their use of scientometric data is not systematic. The effect of independently reached conclusions is sometimes dramatic, but this remains the exception. Thus, a

rapprochement between scientometric discourse and policy discourse is still very much in need.

On the third day of the conference, the issue of differences among discourses was discussed in more theoretical terms. Barend Van der Meulen (Foresight Committee, The Netherlands) suggested that scientometricians are using a naive model of communication. The relevance of a scientometric communication is determined not in terms of its enlightenment of the policy process, but in terms of its usefulness to this process. Anthony Van Raan (Leiden, The Netherlands) accordingly emphasized the essential role of the client in developing this field. Scientometricians should understand their role as a service based on applied research. But in academia the applications have to be firmly rooted in basic research. Thus, the field should develop in terms of these two dimensions: basic as well as applied, customer-driven as well as academic. Additionally, Van Raan provided the audience with a comprehensive state-of-the-art review of the field of scientometrics.

Different dimensions of the communication, such as "utilization", "codification", etc., were distinguished by Loet Leydesdorff (Amsterdam, The Netherlands). He used aggregated citation networks among STS journals to show this effect reflexively at the level of STS as a field. Since 1986, the various core journals of STS (*Social Studies of Science*, *Scientometrics*, and *Research Policy*)1 have grown apart in terms of aggregated citation behaviour to such an extent that they can no longer be considered as a single grouping. The integration of such a (functionally?) differentiated system of communications can only be achieved by making a reflexive step forward. In his opinion, there is a need for models that take into account the highly skewed distributions that are produced over the years, as well as the insights into the underlying mechanisms provided by qualitative theoretical and empirical approaches. Thus, a second function of quantitative approaches in STS can be distinguished in addition to the data-driven approaches presently dominant in scientometrics. Theoretically informed indicators might enable policy makers to distinguish the various dimensions in the complex dynamics of S&T systems.

Wouter van Rossum (Groningen, The Netherlands) had fallen ill prior to the workshop, but his paper was presented nevertheless. He focused on the dynamic overlay of university-industry-government relations which is emerging in advanced industrial countries. This raises new questions about the position of indicators. If the environments change, one has to be clear about what one is measuring if one keeps the yardsticks stable. The discussion at the workshop concentrated on the distinction between relational indicators that capture the operation of the system and positional indicators that try to capture its structure.

On the last day of the workshop, a number of advanced graduate students of the Dutch Graduate School for STS presented their thesis work, using scientometrics for the understanding of science and technology. Gertrud Blauwhof (Amsterdam) presented a longitudinal study of patent and science statistics in the case of "telephone switching systems." Floor Rikken (Groningen) demonstrated the retrieval of developments in a pharmaco-medical setting using the words associated with so-called adverse drug reactions. Ed Noyons (Leiden) presented simulations of the development of neural network research using co-word models. Other, more detailed studies were presented by a number of participants in the various workshops during the conference.

The workshop concluded with a discussion about the future of scientometrics, introduced by Paul Wouters, who presented three possible scenarios: "a glorious future," "impending death", and "muddling through." These scenarios were obtained by varying the development of the three interfaces mentioned above. Thus, the specialty of scientometrics may be expected to develop differently as its environments (science and technology studies, science policy, evolutionary economics) exert differential influences.

One topic discussed in this and several other workshop sessions was the divergent development of the quantitative and the qualitative study of science and technology.

In the early years of scientometrics, a "natural coalition" seemed to exist between Mertonian sociology of science and scientometrics. Since the early eighties, this bond has been severed due to a number of intellectual, practical, and economic developments. The field of scientometrics seems to have moved towards a more intimate connection with the quantitative library sciences and related specialties in information sciences. Consequently, academic communication among science and technology students is no longer as seamless as it was in the past.

Quantitative modelling of the science/technology system, however, is in need of more advanced theoretical underpinning. From this perspective, future research should focus on combining scientometric research with both sociological insights (from the qualitative specialties in science and technology studies) and computational expertise from the various library, information, and data-structural sciences. According to the participants, this workshop has clearly contributed to this goal. An intensive exchange of opinions and insights has occurred among leading scholars in the field, including a non-neglizable Dutch community, European colleages at both the graduate and postgraduate levels, and relevant policy makers. Papers from the workshop will be published in a special issue of *Scientometrics* co-edited by the organizers of the workshop and the managing editor of this international journal.

Acknowledgments
We are pleased to acknowledge the financial support of the Netherlands Ministry of Education, Culture & Science, the European Union DG XII, the Netherlands Organization for Scientific Research, and the Royal Academy of Science. The workshop was co-organized by the European Association for the Study of Science and Technology (EASST) and the Netherlands Graduate School of Science, Technology, and Modern Culture.

NOTE

1. *STHV* has a different position, since this journal has become the journal of *4S* since 1988. In terms of citation behaviour, it can be considered as belonging to the same grouping as *Social Studies of Science*.

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Conference Announcements and Call for Papers

Abstracts are invited for the *4th International ASEAT Conference* to be held in **Manchester, UK**. ASEAT stands for 'Advances in the Social and Economic Analysis of Technology'. Contributions from Eastern Europe, Asia, Latin America and Australasia will be especially welcome. There is an apparent tension between the globalisation of innovative practices and processes, and the persistance of substantial differences in the styles of innovation observed in firms and other forms of research organisation. Globalisation suggests the diffusion of best innovative practice among firms worldwide, while styles suggest that differences in innovative practice do exist among even firms that operate as multinationals. How do we explain these differences? Are they best explained by using the 'National Systems of Innovation' approach which focuses on national innovative resources and national institutions? Are styles best captured as from a regional, sectoral or firm perspective? What's the role of global alliances, and do styles of innovation have a bearing on relative competitiveness?

The conference seeks to identify and discuss various approaches 'styles' and the factors influencing 'difference'. Contributions from all social science disciplines (with the exception of sociology and economics) are welcome. Send 500-word abstracts by 28 October 1996 to Albert Richards, Manchester School of Management (UMIST), PO Box 88, Manchester M60 1QD, UK, fax: 44-161-200-3505, email <albert.richards@umist.ac.uk, or on diskette in Word or Wordperfect format. Full papers due by 3 March 1997. Selected papers will appear later in book form.

The *International Conference on Systems Sciences* will be held on January 7 - 10, 1997 in **Maui, Hawaii**. A portion of the conference is devoted to "Genre in Digital Documents". By genre is meant not just particular technologies or modes of communication or presentation (e.g., hypertext, email, the Web, and so on), but complex communicative forms anchored in specific institutions and practices -- the digital analogues, that is, of print forms like the newspaper, the annual report, the how-to manual, the scholarly journal. The purpose of HICSS is to provide a forum for the interchange of ideas, research results, development activities, and applications among academicians and practitioners in computer-based systems sciences. The conference consists of tutorials, advanced seminars, presentations of accepted papers, open forum, tasks forces, and plenary and distinguished guest lectures. There is a high degree of interaction and discussion among the conference participants because the conference is conducted in a workshop-like setting. The deadline for registration is 15 November 1996. For information on the 'genre' sessions contact:

Geoffrey Nunberg
Xerox Palo Alto Research Center
3333 Coyote Hill Road
Palo Alto, CA 94304
ph: 415-812-471; fax: 415-812-4777
email: nunberg@parc.xerox.com

For information on the conference:
Barbara Edelstein
College of Business Administration
University of Hawaii
2404 Maile Way
Honolulu, HI 96822
(808) 956-3251 FAX: (808) 956-9685
e-mail: hicss@hawaii.edu

EU Publication News

Biotechnology Risk Regulation in Europe: Implementation of Directive 90/220

In 1990 the European Community adopted the Deliberate Release Directive 90/220 as a means to anticipate and prevent potential harm from the intentional release of genetically modified organisms. The Directive linked environmental protection with 'harmonized procedures and criteria' for approving genetically modified products across the European Community, which has since been enlarged to become the European Union (EU). For many commercial products, however, EU member states have disagreed over the basis for market approval.

During 1994-95 the efforts at implementation and harmonization were studied by a network of researchers, co-ordinated by the Open University in Britain, and funded partly by DGXII of the European Commission. Their analysis has been published as the June 1996 issue of the journal Science & Public Policy. Les Levidow and Susan Carr served as guest editors for that special issue, entitled 'Biotechnology Risk Regulation in Europe'. It features a lengthy overview of the European Union plus short articles on eight member states.

To purchase copies of the journal (or subscriptions), please contact the publisher: e-mail:<page@scipol.demon.co.uk>, Beech Tree Publishing, Guildford, Surrey GU1 2EP, fax: 44-1483-567497.

Reprints of the Europe and UK articles are available (in limited numbers) from the project co-ordinators: e-mail <M.McVay@open.ac.uk>, Mary McVay, Centre for Technology Strategy, Open University, Milton Keynes MK7 6AA, UK; fax 44-1908-652175.

Any comments: <L.Levidow@open.ac.uk>

Positions and grants available

Applications are invited for post-doctoral fellowships in the history and philosophy of science, technology and medicine at the **Sidney M. Edelstein Center in Jerusalem, Israel.** Students with a Ph.D. degree or equivalent in the history, philosophy or sociology of science, as well as in the physical and life sciences may apply. Send a CV, publication list, letters of recommendation, the abstract and a sample chapter from the dissertation, and an outline of the planned project.

Major relevant collections in Jerusalem include the Edelstein collections (chemistry, alchemy and dyeing), Albert Einstein archives, Theological collection of Isaac Newton, and the Friendwald Collection (history of medicine). The Edelstein Center is a repository for the Archive of the History of Quantam Physics assembled by the American Institute of Physics.

Applicants should apply directly to the Sidney M. Edelstein Center, The Hebrew University of Jerusalem, Givat Ram, 91 904, Jerusalem, Israel by 1 March 1997. Tel. 972-2-658-5652; fax 972-2-658-6709.

The Chemical Foundation (Philadelphia) - Edelstein Center are accepting applications from established scholars for the 1997-1998 Edelstein International Fellowship in the History of Chemical Sciences and Technology. It runs from 1 September 1997 to 30 June 1998. The fellow will be expected to divide his or her time between the Chemical Heritage Foundation in Philadelphia, Pennsylvania and the Edelstein Center in Jerusalem. The fellow will devote most of the time to research, and will contribute to the work of each center in an appropriate manner. The fellowship may be held in conjunction with other research or sabbatical support, and a travel allowance is also available.

Letters of application should indicate how Chemical Heritage/Edelstein Library resources in chemical sciences are relevant to the applicant's research, and also should enclose a financial statement, a CV, and the names of 3 referees. Send application to Prof. Seymour H. Mauskopf, Coordinator, Edelstein International Awards, Department of History, Duke University, Durham, NC 27708, USA.

The **Edelstein International Studentship** is available for dissertation research and writing in the history of the chemical sciences and technologies. Candidates should have fulfilled all requirements for the Ph.D., excepting dissertation. The studentship provides a stipend and travel and dissertation fee-support for a five or six month stay at The Hebrew Univerity of Jerusalem in the course of the academic year. The resources of the Edelstein Library (especially strong in the history of chemistry) and the Harman Science Library will be available to the holder in Jerusalem. Philadelphia resources include the Othmer Library of Chemical History and the Edgar Fahs Collection.

Applications should include a dissertation prospectus, a brief statement of researh and writing plans for the year of the studentship, a CV, and the names and phone numbers of 2 referees. Applications should be received by 31 October, 1996. Send applications to Prof. Seymour H. Mauskopf, Coordinator, Edelstein International Awards, Department of History, Duke University, Durham, NC 27708, USA.

A *Mellon Foundation postdoctoral teaching-research fellowship* is available in the Department of Science & Technology Studies at **Cornell University**. It is open to citizens of the U.S., Canada or to those holding a permanent U.S. residency card. While in residence at Cornell, postdoctoral fellows hold department affiliation, and have limited teaching duties and the opportunity for scholarly work. Applicants are encouraged from any of the four component fields of Science and Technology Studies: sociology of science and technology; history of science and technology; philosophy of science and technology; politics and policy of science and technology.

The postdoctoral teaching-research fellowship will begin July 1, 1997 and offers a stipend of $28,000. Applicants must have received the Ph.D. degree after September 1991. Applicants who will receive the Ph.D. degree by June 30, 1997 are eligible to apply. All application materials (including letters of recommendation) must be postmarked on or before January 4, 1997. Awards will be announced in February 1997. For more information, see http://www.sts.cornell.edu/CU-STS.html, or contact: Lillian Isacks, Administrative Assistant, Department of Science & Technology Studies, 726 University Avenue, Cornell University, Ithaca, NY 14850-3995. Telephone: (607) 255-6234. Fax: (607) 255-0616. E-mail: li10@cornell.edu.

Send application materials (including letters of reference) to:
Ms. Agnes Sirrine, Program Administrator
Mellon Postdoctoral Fellowships
Cornell University
A.D. White Center for the Humanities
27 East Avenue
Ithaca, NY 14853-1101
Telephone: (607) 255-9274

The Division of Social Science of the **Hong Kong University of Science and Technology** invites applications for a faculty position (rank open) in the discipline of Science, Technology, and Society. At the Assistant Professor level, candidates are required to have a Ph.D. degree and relevant research experience. Applicants for appointment at the Assistant or Full Professor level should be senior scholars in a

professional capacity, with demonstrated academic leadership qualities. Candidates are required to teach interdisciplinary courses on Science, Technology and Society to undergraduate engineering and science majors, with possible future teaching at the graduate level as the program develops.

Applications/nominations, with a curriculum vitae and three references should be sent to:
Professor Andrew Walder
Head, Division of Social Science
School of Humanities and Social Science,
The Hong Kong University of Science and Technology,
Clear Water Bay Road, Kowloon, Hong Kong
tel: (852) 2358 7782. Fax: (852) 2335 0014.
The deadline for applications is 16 November 1996.

The *Center for History of Physics of the American Institute of Physics* has a program of grants-in-aid for research in the history of modern physics and allied sciences (such as astronomy, geophysics, and optics). Grants can be up to $2500 each. They can be used only to reimburse direct expenses connected with the work. Preference will be given to those who need part of the fund for travel and subsistence to use the resources of the Center's **Niels Bohr Library in College Park, Maryland** (Easily accessible from Washington DC.
Contact Spencer Weart,
Center for History of Physics,
American Institute of Physics,
One Physics Ellipse,
College Park, MD 20740,
email sweart@aip.org
tel: (+1) (301) 209-3174.

Two programs at the **National Science Foundation** - *Ethics and Values Studies*, and *Research on Science and Technology*, in the Division of Social, Behavioral and Economic Research - are issuing a call for proposals for their February 1, 1997 target date. Contact program directors Rachelle Hollander or John Perhonis, NSF, Room 995, 4201 Wilson Blvd., Arlington, VA 22230. Tel: 703-306-1743. Fax: 703-306-0485 or 0486. E-mail: rholland@nsf.gov or jperhoni@nsf.gov. These programs have approximately $2 million to support awards from the proposals submitted.

The Ethics and Values Studies (EVS) program focuses on developing and transmitting knowledge about ethical and value dimensions associated with the conduct and impacts of science, engineering, and technology. The Research on Science and Technology (RST) program supports research to improve approaches and information for decision making concerning management and direction of research, science and technology. Projects might address the following kinds of questions. These descriptions are suggestive, not exhaustive.

Target Dates and Program Procedures
All applicants to the program must follow the requirements listed in the NSF Grant Proposal Guide (NSF95-27), which contains the forms needed for submission. The GPG is available from Forms and Publications Unit, NSF, 4201 Wilson Blvd., Arlington, VA 22230, 703-306-1130; and can be requested electronically through Internet: pubs@nsf.gov. Applicants can also find this information in the NSF Home Page: http://www.nsf.gov/
The programs have two rounds each year for consideration of proposals. The target dates are February 1 and August 1. Proposals focused on undergraduate education should be submitted for the February closing date of the program, since their review will be coordinated with that in the Division of Undergraduate Education in the Directorate for Education and Human Resources. Preliminary proposals, giving a description of the project and its suitability for consideration in the programs, can be submitted at any time; the program directors will try to respond with informal suggestions for submission.

Web news

SPEED: AN ELECTRONIC JOURNAL OF TECHNOLOGY, MEDIA AND SOCIETY is on-line at http://www.arts.ucsb.edu/~speed. The most recent edition is devoted to AIRPORTS AND MALLS. SPEED also provides a forum for the critical investigation of technology, media and society. The intention is to contribute toward a democratic discourse of technology and media, one that is always focused upon the material conditions of life that technologies and media constitute and demand, and yet does not lose sight of the power of ideas to change those conditions. To subscribe send an e-mail to _SPEED_@alishaw.ucsb.edu with "subscribe" in the subject header.

Wavelength, an ejournal from the Science, Society and Media program at the University of the West of England can be found at http://science.btc.uwe.ac.uk/~waveleng/home.html. Wavelength magazine carries articles which explore the historical, social and cultural contexts of science. It also carries fiction and more 'traditional' science features. Wavelength is produced in conjunction with the BA degree in Science Society and the Media at the University of the West of England, Bristol. For more information on the course contact the admissions tutor Alex Easton, e-mail: a-easton@uwe.ac.uk, tel: +44 (0)117 9656261 ext. 2491.

The AIP Center for History of Physics mounted a site featuring the history of physics and allied sciences:
http:/www.aip.org/history/
Among many other things the site features an exhibit on Einstein: Image and Impact, using photographs, quotes and text to present highlights of Albert Einstein's life.

For a searchable index of email forums on a variety of subjects, see http://www.tile.net/tile/listserv/index.html. The science lists are at http://www.tile.net/tile/listserv/science2.html. One may find and subscribe to any of the following lists by or about Perspectives on Ethical Issues in Science and Technology; Minority Perspectives on Ethics in Science and Technology; Society for the History of the Social and Behavioral Sciences; Center for the Study of Science in Society Faculty Discussion Group; Center for the Study of Science in Society Seminar List; History of Science Society - Early Science Interest Group; H-Net Network on Science and Culture; Medieval Science Discussion List; Science Policy Discussion Group; Science, Technology, and Society Discussion and Networking Group; Women In Science and Engineering NETwork.

Much of the Web news comes from the science as culture list. To participate, send a subscribe message to <listserv@sjuvm.stjohns.edu>. See also the website:
http://www.shef.ac.uk/~psysc/rmy/sac.html.
North Americans may wish to access its mirror at
http://rdz.stjohns.edu/human-nature/sac/sac.html

Seminars and Workshops

University of Manchester, CENTRE FOR THE HISTORY OF SCIENCE, TECHNOLOGY AND MEDICINE, and WELLCOME UNIT FOR THE HISTORY OF MEDICINE announces the following seminars, held on TUESDAYS at 16.00, Room 3.29, 3rd floor, Maths Tower, Oxford Road (Unless otherwise stated)
Tea from 3.30 pm, Room 3.04

8 Oct Jean-Paul Gaudillere (INSERM, Paris)
Making Biomedical Networks: Mice, Viruses, and Cancer in Cold War United States

15 Oct Keith Tribe (Keele)
Making a Discipline of Economics: the Cambridge Tripos, 1900- 1950

22 Oct Anne Secord (Cambridge)
Observing Differences: Artisans, Gentlemen, and the Work of Nineteenth-Century Botany

29 Oct. University History of Medicine Lecture
*Stopford Prof. Robert Tattersall (Nottingham)
Bldg. *5.30 Understanding Diabetes in Victorian England

5 Nov Simon Chaplin (Science Museum, London)
Invisible Industrialisation: the Centrifugal Cream
Separator and the Manufacture of Milk, 1879-1900

12 Nov. Roger Cooter (Manchester)
The Encounter of Encounters: Malingering, the Great War and the Rise of Defensive Medicine

19 Nov. Joe Cain (University College London)
Intimate Working: Collaborations between Husband and Wife in the Early Scientific Careers of Anne Roe and George Gaylord Simpson

26 Nov. Gerrylynn Roberts (Open University, Milton Keynes)
The Making of the Chemist in England, 1902-1939: A Portrait of the Chemistry Department at University College London

3 Dec. Lara Marks (Imperial College, London)
The Barrier of a Common Language: The History of British and American Policy around Thrombosis and the Oral Contraceptive Pill 1960s - 1970s

10 Dec. Peter Bowler (Queen's University of Belfast)
Myths, Narratives and History in the Study of Human Origins

17 Dec. David Edgerton (Imperial College, London)
History of Technology

Enquiries: Professor John Pickstone, Room 3.31, Maths Tower (0161-275-5926)

THE SOCIETY FOR APPLIED PHILOSOPHY
London Workshops 1996-97

The Society for Applied Philosophy holds workshops on the first Saturday of each month in Autumn and Spring terms. They take place from 2.30 - 5.30pm in room 304, Senate House, University of London, Malet Street, WC1. The nearest underground stations are Goodge Street and Russell Square.

5 October 1996
PHILOSOPHICAL COUNSELLING: WHAT IS ITS FUTURE?
Speakers: Colin Clayton, Face to Face Dasein. and Mike Parker, Department of Health and Social Welfare, the Open University.
Convenor: Colin Clayton, Face to Face Dasein, Shirley Holms, Lymington, Hampshire, SO41 8NH. Tel: 01590 683454.

2 November 1996
FOOD FOR THOUGHT: RISK, KNOWLEDGE AND THE BSE CRISIS.
Speakers: Peter Dickens, School of Social Sciences, University of Sussex and Brian Wynne, Centre for Environmental Change, University of Lancaster.
Convenor: Jenneth Parker, 10 Emmanuel Road, Hastings, East Sussex, TN34 3LB. Tel: 01424 432792.

7 December 1996
EUROPEAN IDENTITY, EUROPEAN RIGHTS
Speakers to include: Matthew Humphreys, Anglia Polytechnic University and Paula Cohen DHSS.
Convenor: Matthew Humphreys, Department of Law, Anglia Polytechnic University, East Road, Cambridge, CB1 1PT. Tel: 01223 63271 ext. 2567.

1 February 1997
MEDIA ETHICS AND REGULATION
Speakers to include: Matthew Kieran, University of Leeds and Stephen Whittle, Director of the Broadcasting Standards Council.
Convenor: Matthew Kieran, Centre for Business and Professional Ethics, Departments of Philosophy, University of Leeds, Leeds, LS2 9JT. Tel: 0113 233 3275

1st March 1997
ETHICS, ECOLOGY AND ELEPHANTS: TENSIONS BETWEEN ANIMAL WELFARE AND CONSERVATION.
Speakers to include: Kate Rawles, Lancaster University.
Convenor: Kate Rawles, Department of Philosophy, Furness College, Lancaster University, Lancaster, LA1 4YG. Tel: 01524 592500. Fax: 01524 592503. e-mail Kawles@lancaster.ac.uk.

REGIONAL WORKSHOPS

LANCASTER
VALUES AND THE ENVIRONMENT
Series 8
This annual series of 'out of town' Saturday seminars will be continuing into its eighth year with six meetings on 19th October, 16 November, 7th December, 1st February, 22nd February and 15th March. Meetings will be held in Bowland College Senior Common Room from 10am to 1pm. Speakers will include Brian Goodwin on Life at the Edge of Chaos and Susan Canney and Kate Rawles on Conservation and Animal Welfare Issues in Africa. Further details from Kate Rawles on numbers above.

LEEDS
MEDIA ETHICS: PRIVACY, PUBLIC INTEREST AND CENSORSHIP
20TH-21ST September.

CARDIFF
Details of these workshops are available from Andrew Edgar, Philosophy Section, SESJP, University of Wales College of Cardiff, PO Box 94, Cardiff, CF1 3XE.

If you would like further information about these workshops or about the Society for Applied Philosophy please contact Mike Parker at his e-mail address which is <m.parker@open.ac.uk>.