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LATOURIAN CONSTRUCTIVISM UNFOLDED INTO EPISTEMIC HISTORY

1. Results and troubles

One of the most interesting trends during the last fifteen years is the emergence of sociologically inspired case studies targeting at the contents and context of science. Historians, philosophers and sociologists have made science studies a joint project. At least that was the promise. After a burst of studies using a common heuristics and some interrelated approaches, science studies have met with inner differences and even contradictions. There is some confusion concerning the achieved results and the future approaches. (See Callon & Latour, 1991).

Latourian constructivism has given highly interesting, original descriptions and explications of modern scientific and societal dynamics. It has not only explicated the origins of practical change, but also the creation of new realities in a laboratory. It has focused on the simultaneous formation of science and society.

Constructivist case studies have provided us with a "sociologics" (Latour) of **modern**

scientific research. As for the analysis of practice and society, sociologists feel confident that there are results. Even sociologists, however, are doubtful, whether the promise concerning the contents and contexts of science has been fulfilled. It is problematic, whether there is, in spite of a common discussion, any actual association or contact between the different approaches of historians, philosophers and sociologists on technoscience and society. In the course of my presentation I will identify some material and topics which in my view may be used to determine the relationship between the approaches. We will start by asking, what has fallen out of sight **due to** the assumed constructivist approach.

The focus of scrutiny has been on "Science in the Making". Latour (1987: 4) wrote: "our entry into science and technology will be through the back door of science in the making, not through the more grandiose entrance of ready made science". It is an unbroken theme of Latour's "Science in Action", that science is Janus-faced. But it is essential to note that the two faces of science do not make up just two aspects of science or two

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optional starting points of a study. Instead, "Science in the Making" is the locus of creation and "Ready Made Science" is the outcome of that creation.

Natural scientific theories, the entities of our reality and in the last instance Nature as a whole, are all taken as an outcome or result and cannot accordingly be used to explain how and why the controversies in scientific research have been settled. (See Latour, 1987: 99). It is an essential commitment of the constructivist approach to keep all the "settled parts of science" in the position of an outcome (see Latour, 1987: 258). Accordingly, also societal relations and entities are seen as outcomes.

2. Problem of shared and prior knowledge

Constructivism allows and assumes a historically open creation of realities and entities. Ready made science is at every moment a temporary outcome which is transformed through science in the making. We may confront the constructivist approach with Descartes, Kant and large parts of later philosophy of science which regard science primarily as ready made science and as a fixed resource of ongoing research. Change of theories and principles is managed in two different ways. That which, from the point of view of constructivism is a transformation of temporary outcomes, was seen by classical intellectualism and many later philosophies of science as shifts of scientific world-views and rational resources.

A constructivist approach cannot take any entities or contents of theories for granted because this would mean closing the process of creation and history. Open history must not be replaced by any dynamic system of the Hegelian brand nor by any other implicitly teleological definitions of rationality and reason.

It is, however, not entirely out of scope for constructivism to regard ready made science as a given entity. This may and will happen, when discussing the resources of a scientist,

the implementing of a research program or the so-called acting at a distance in practical control situations. Such granting of frozen entities or theories is possible for constructivism inside the thematic concerning the way outcomes act as resources of further research. At this point there is a link to the way philosophy of science looks at science. But let me first regard science as an outcome and show some genuine problems induced by the constructivist approach itself.

One appearance of the difficulties is the so-called problem of "shared knowledge" or "prior knowledge". Constructivism has been accused of ignoring the role of prior knowledge in scientific research. In an early criticism of the constructivist approach Freudenthal (1984: 294) stressed: "in order to understand scientific activity it is necessary to postulate the existence of shared, non-local knowledge.". He takes a double approach according to which both observations and prior knowledge constitute new knowledge: "However, logical positivism, through a process of severe self-criticism, long ago modified its original platform and has evolved into logical empiricism: it has recognized the central importance of the systematic interconnectedness of science, and it has adopted the idea that new knowledge depends on both prior knowledge and new observations." (1984: 293).

Hardly anybody would oppose Freudenthal as for the "dependence" of new knowledge both on prior knowledge and on observations. Because the constructivist approach keeps the settled parts of science in the position of outcomes, it cannot "postulate the existence" of prior knowledge and accordingly assume prior knowledge as an independent ground. But other "dependences" can be found. (I now use Freudenthal's somewhat problematic expression which applies the notion of knowledge for the purpose of discussing knowledge).

Freudenthal was not totally wrong in maintaining in his early criticism of Knorr's work that constructivism disregards "the systematic, trans-local and inter-subjective aspects of scientific knowledge" (1984: 293). Even

though early constructivism, when focusing on science in the making, did regard prior knowledge as precondition of the creation of new knowledge, it often was at this point very implicit and ambiguous. Prior knowledge was treated through particular, spatially and temporarily delimited research activities. It has also been typical to consider the preconditions of knowledge-creation as heterogenous and optional resources rather than as a single world view of a research program or a single set of theoretical expectations.

It is another over-interpretation to maintain that for constructivists, entities are "arbitrary" or "conventional" social constructs (see Roth&Barret 1990: 596-599, 604 note 48). This interpretation has found some grounds in the weaknesses of the whole sociology of science. As long as society was supposed to explain Nature, sociology as a fixed resource had a privileged position. Social explanations of science imply that the entities of nature are of a social character. Both of these weaknesses, namely leaving the contents of science, especially the prior knowledge, to internalists, and taking sociology as a privledged resource of science studies, gave rise to vigorous attacks on the sociology of science after "Bloor's turn" (Latour). Not only the strong program, but also constructivism have been among the targets of the criticism.

The critiques saw meaningful cultural realities, on the one hand, and natural realities, on the other, as mutually exclusive and as bases for two separated, independent worlds (see Oldroyd 1990: 641-642). This old dichotomy makes up exactly the dilemma to which Latourian constructivism has tried to find a solution by stressing that science and society are created at the same time, or by stressing that natural sciences and social sciences must be treated symmetrically (Latour, 1987; 1988b). Constructivism should be able to challenge the entities given by science. Simultaneously it should challenge society as an entity. Both should be seen as outcomes.

Constructivists confront over and over again the temptation of considering some results as absolute **resources** instead of regarding them as **outcomes**. It would be parallel to the ef-

forts of philosophy of science to postulate independent grounds. But it would mean rejecting the constructivist approach. Identifying sociology as a privileged resource of the earlier sociology of science has been an important step.

In my view the shared or prior knowledge is a genuine and burning problem for Latourian constructivism, which perhaps has achieved the most proximate grip of the contents and context of science among science studies. The loose, partly metaphorical formulations should, in the long run, be replaced by formulations which challenge both philosophers and historians by revealing the contact.

3. The settled parts of science

In Latourian constructivism the settlement of controversies raised by counter-laboratories trying their strength defines the settled parts of science or the temporarily closed blackboxes, which become a precondition of further scientific research. The transformation of the preconditions of research is explicated and explained in a seemingly historical manner, because there is a sequence of transformations of the preconditions. Although an important mechanism of modern scientific transformation is explicated, the whole dynamics of counter-laboratories and new research programs is all but clear. Even though we may assume that the settled parts of science are a result of some previous trials of strength, we should not assume that some particular controversies or translations will give us the settled parts of science. Taking science in the making with particular local and temporal delimitations does not necessarily cover the contents and context of ready made science. To say it in Latourian terms: we may not assume that the particular translations in a laboratory should exhibit the whole of the translations of the networks, even though the particular translations would be most important in creating some new realities and even though the whole networks should be reorganized because of those translations.

A laboratory should be thought of in the

context of some history. Otherwise we have no means but an arbitrary postulate for the introduction of the arsenal of the contents and contexts of theories. There is still a gap between the local episodes concerning science in the making and such a history of science which really comprises the transformations of the settled parts of science. After all, constructivism will be confronted with the problem of prior knowledge. How should constructivism deal with prior knowledge, if postulating its existence is excluded?

In a particular sense it is completely allowable to introduce things into a problem situation by a postulate. I mean introducing things that are not a topic of the study, as results of some other studies, into the problem. Often it is the shortest and most effective way in a study, in which only some things make up the topic and the rest are non-topic. Actually Latour has introduced the settled parts of science for Pasteur's laboratory, when interpreting the creation of particular new realities and the consequent theoretical transformations. (Latour 1983; 1988a: 79-100). But they have not been introduced as outcomes. Latourian constructivism ignores the fact that entities lacking contents and context of their own were introduced. These entities exhibit themselves merely as resources of Pasteur, instead of as outcomes. They make up potentially heterogeneous grounds and endanger the coherence of the constructivist approach. It is not a question of whether there is or is not any prior knowledge in the episode, but of how a constructivist approach can live with it. (In Latourian terms you should, instead of "prior knowledge", read earlier and other "networks".)

The settled parts of science, by whichever terms we might express them, act as theoretical preconditions of further research. As a resource of further research we might call the settled parts of science somewhat metaphorically "theoretical a priories of research".

Many theoretical a priories are present and available for research. We see this clearly in cases of optional analyses of realities, whereby researchers shift between contexts. For instance, it was not at all clear if the familiar reality of every kitchen, yeast, should be analyzed into a chemical or microbiological phenomena. Both analyses were possible, as well as physical and mathematical analyses. Liebig's chemical analysis of yeast was not impossible but it was uninteresting and fruitless compared with Pasteur's microbiological analysis. Theoretical a priories of research are present as optional contexts of analysis or optional networks of translation. We are not obliged to assume their existence in any essentialist sense. We may assume them as previous translations and networks fully in Latourian terms abandoning only the spatial-temporal simultaneity.

We usually consider one analysis of the options as the right one, while other possible analyses look trivial or inadequate. Taking the kitchen yeast as a solid particle would be an adequate analysis for cases of slinging the yeast through the kitchen space. Actually in every research situation we have optional theoretical expectations for the analysis of particular realities. Only after removing the realities into one particular context and translating the realities into certain phenomena have we fixed our theoretical expectations concerning the realities. Considering our theoretical expectations as resources of research provides an entirely different scope compared with considering realities removed into one of the optional contexts of analysis. The first way is typical of philosophy of science.

Pasteur implemented many theoretical expectations as his resources. The constructivist approach has not given an answer to the question of the means by which these theoretical a priories may be kept in the same position as the settled parts of science, which originate in the transformative episode.

Latourian study is confined to a "sociologics" of modern scientific research. It illuminates the events of a laboratory busy in transforming the implicitly introduced entities. No larger historical context is given by Latour for the theoretical a priories than the laboratory of Pasteur. The actual laboratory activity does not cover the whole contents and context of the theoretical a priories. The contradiction is so mild only because microbiological theories

are born every moment. It would be much harsher for the cases in which theories are present in a more developed fashion. Even though I agree with Latour in that we should never "cut off the abstractions or the theories from what they are abstractions or theories of", I see no reason for the assumption that the schemes or realities of a particular laboratory should represent the whole contents of a theory. (See Latour, 1987: 241-243). The theories are theories not only of those realities, but of a larger domain of realities in historical depth. Sooner than letting the whole networks be defined by one temporally and spatially particular episode, I would prefer the rejected, conventional idea of an actualization of the theoretical a priories. Latourian constructivism should be augmented in order to challenge shared or prior knowledge. Hereby it should become genuinely historical instead of a mere transformational dynamics.

4. Old misconstructions by sociology of science

Kant formulated the dualist subject-object constellation for modernity not only as an aprioristic conception of mathematics and logic, but also as an aprioristic philosophy of science in general. Kant's "Metaphysische Anfangsgründe der Naturwissenschaft" interprets Newtonian dynamics as a resource of a research program. Kant himself recognized that physics and all other theoretical sciences contained "synthetic judgements a priori, as principles". (Critique of Pure Reason, Introduction: V.2.). That Newtonian physical theory is the contents of the new scientific world view is unveiled also in the demarcation towards the theoretical a priories of the young chemistry.

Kant turned the Newtonian synthesis not only into a resource of successive research, but also into the sole and fixed intellectual resource for changing any experience into a posteriori knowledge. In my mind the core of apriorism is the turning of theoretical intermediations of realities into a research program with theory-induced expectations. The

external discourse between rationalism and ancient modes of reason was a second level of the transformations that created the distinctions and dilemmas typical to modernity.

In the course of research on the basis of the Newtonian paradigm and after the new theoretical a priories of chemistry, electricity, biology, and philosophy of science had to be cleared of synthetic judgements a priori. It was molded into empiricism with a minimal arsenal of analytic judgements a priori. Theory, taken as a resource, was seen to be in need of empirical justification. Philosophy of science became, in a way, interested in theories as outcomes, and formulated the problem of induction.

Later sociologists also met the a priori functions of theories which were turned into research programs. There are numerous sociological attempts in the tradition of sociology of science to clear away the experienced a priori power of theories acting as seemingly given resources of research, which are not results of sociological analyses but must be postulated as a faculty of human intellect. Perhaps meeting various ways of life and experiencing cultural relativity made all assumed invariants intolerable for sociologists.

Sociologists tried to re-contextualize the theoretical a priories belonging to the subject. Durkheim and later Sohn-Rethel wished to identify the categories with societal relations or entities. Scheler and Habermas developed the idea that natural-scientific theory could be interpreted as a nexus of work or instrumental action. The finalization-theorists thought that practical interests could be constitutive of theories.

All the sociological efforts to find an immediately practical context for theory and to solve the problem of a priori entities have failed. They have remained heuristic ideas and externalist interpretations of various practices which are related to, but not a part of, the contents of science. They have had no special impact on the explication of the mechanisms of scientific transformation. The old question concerning a social explanation of the categories, of the theoretical a priories of natural sciences, and of the entities given by

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metaphysics, was dissolved because of the wanting fertility of the sociological research program.

The latest sociology of science has emerged as a continuation and a new introduction of old sociological ideas and heuristics. The sociological heuristics and other preceding intuition has been: science cannot be entirely a-practical, a-social or a-historical. With this heuristics in mind modern sociologists of science have analyzed cases of scientific research. Social context and social explanation have had a similar significance in the sociology of science as the idea of aether had in physics at the turn of the century (see Latour, 1988b: 30). The result seems to be: there is no hidden social construction outside the "technical" contents of science. In this sense we may almost agree with Slezak, who maintains that social factors are "irrelevant" to the content of theories (1989: 564-6, 570, 572). There is no special disagreement between Slezak and Pinch (1990: 659), Oldroyd (1990: 640-43), and even Pickering criticising retrospective realism.

Both the externalist sociology of science and most of the sociology of scientific knowledge have been a blind alley as for the contents of science. In spite of initial aims of studying science, they have remained sociology of scientific institutions, sociology of scientists and research or presentations of ready made results in macro-sociological contexts. The only remaining place to look for a historical, practical and societal nature of science, is in what so far has been called the "technical" contents of science.

Sociologists have sometimes got a grip of the contents of science but often at the cost of cleavages or of leaving sociology altogether. Sociologists have combined internalism and the social frame. When social studies of science really were concerned with the contents of science, they seemed to turn into internalist studies. In the best case studies, internalist studies were "sandwiched" (Latour) between sociological explanations. A closed definition of the social entities and making them into a fixed resource for explaining Nature has justly been called "social realism" by Latour in the

recent controversies with Collins and Yearley (see Callon&Latour, 1991). Considering sociology as a fixed resource for the social explanation of science while letting sociology itself fall outside the reflexive relations means an asymmetrical approach and a presumptive irrefutability of the resources. We have good reasons for abandoning all privileges of sociology in science studies. Social entities must be taken as outcomes, too.

5. Sociology may learn from science

Instead of considering sociology as a fixed resource, we should "transform the definition of social until it is *on a par* with the very content of the science studied, and exchanges properties with it." (Latour, 1988b: 26). Latour really has for a long time tried to follow the symmetrical approach for both the entities given by natural scientific theories and the entities making up what we call "society". This is explicit in his "Science in Action". In the recent reformulations of the constructivist program he has made this even more explicit by introducing it as his "second principle of symmetry". (I mean Latour's discussions with Collins and Yearley, and his contributions at Science Studies Program-UCSD, San Diego).

Bloor applied one sociological approach in a symmetrical way when trying to give social explanation both to the beliefs accepted as science and to the rejected beliefs as well. His approach left society and the resource of sociology in a privileged position. Science studies were carried out letting sociological theory provide the a priori functions of research. Realities were automatically changed into sociological phenomena. In other words: science studies were actualizations of sociological paradigms. Latour has denied the privileged position of sociology and also the privileged a priori functions of any other theories in science studies. He makes a return to the origin of modern a priories and of apriorism, that is, to authors first considering theories as resources of research.

For treating Nature and Society in the making in the same terms Latour needs a

"counter-Copernican" revolution against the aprioristic and dualistic constellation of the modernity first formulated by Descartes and Kant. By Kant, the phenomenon is reduced into a meeting point of two sets of fixed resources. Instead of two opposite transcendencies, Nature and Society, Latour wishes to rely only on one transcendence and origin of reality. He gives a new meaning to the term "fact" by assuming an approach which redistributes the ontological statuses. (I would prefer to speak of realities of our reality). The Kantian a priories were dropped from the primary, ontological status in favor of more particular realities. This one transcendence is given by Latour by means of various nominations, descriptions and exemplifications. As a source it is the "activity of nature/society making". It may be termed "history of societies *and* things" or "collective things". It may be expressed through the human and non-human actors or actants, thus redistributing ontological activity. It may be taken as that which is to be explained, as nominated "facts" awaiting analyses into phenomena, "matters of fact", or Serresian "quasi-objects"(see Latour, 1990a; 1990b).

The most important difference between philosophy of science and constructivism is: philosophy of science makes theories into its topic as a resource of research, which science studies regard theories primarily as outcomes. But clearly the redistribution of ontological statuses is parallel to the way philosophy of science molded Kantian apriorism into empiricism. Accordingly Latourian constructivism is or should be interested in completing the reappraisal of apriorism.

Assuming and allowing no privileged resources renders some problems for the reception of the Latourian approach. From the point of view of persons using some present resources for research, the Latourian approach should be reduced into some existing discipline like sociology, or philosophy of science. But from the point of view of Latour, science studies cannot rely on any fixed resources (which would become privileged resources), because they must be taken as outcomes. The starting 'point' is a plurality of actants or realities and their translations, networks, associations etc. The new transcendence is not beyond the analysis and intermediation, but before it.

Assuming no privileged resources need not mean no resources at all. Perhaps the negation of privileged resources should, however, be reformulated to exclude only the privileged position and fixed nature of resources, but allowing the use of all resources in science studies. If sociology may learn from science, could science studies not learn from both, and not only from the more exotic networks of anthropology, literature or semiotics?

Some years ago, Latour tried to follow symmetrical intuitions in studying the case of Einstein's theory of relativity. Latour had earlier (1987: 62) written about so-called technical literature: "This literature is so hard to read and analyse not because it escapes from all normal social links, but because it is *more* social than so-called normal social ties.". The contents of scientific theories exhibit a more general societality, a more general practicality and a more profound historicity than practices in general.

Latour's studies (1987; 1988a) on the simultaneous making of science and society in the case of Pasteur's laboratory and French farming are well-known. Latour has also tried to give an extreme example of the simultaneous transformation of Nature and Society by interpreting the generalized societality of Einstein's theory of relativity: "He (Einstein - SR) is interested only in the way in which we send *any* actor to *any* other frame of reference. Instead of describing laws of nature, he sets out to describe how any description is possible." (1988b: 9). Einstein's theory deals with the elementary spatial and temporal relations between any actors, and with turning them back into internal referents. The practical domain or the 'society' defined by the intermediation is as universal as possible. A theory makes up a unity between the events or particular realities by the chains of intermediations. Einstein is the master of spatial and temporal intermediations of this **society** of actors.

We could also say, that Latour has given a

rational reconstruction of the practical and social meaning of Einstein's natural-scientific theory. Society has been created anew at the same time as the entities of Einstein's theory have been defined by the networks of intermediations. Not only the notion but also the entity of society have been transformed by Einstein's works.

As for Einstein's theory or any other natural-scientific theory there are, however, some problems concerning the historical contents and the uses of theory as a resource and a world view. Latour has been busy in giving a systematic and reconstructive explication of the Einsteinian theory. The Einsteinian theory has, however, experienced enlargements as for its domain, contents and context all the time. Reading Einstein's book in the actual historical context is a difficult task. Sooner, we are prone to read it through the contemporary contents of the theory of relativity or even through the theoretical expectation induced by the theory, when turned into a research program. For this purpose we have numerous explanations of so-called "meaning" of Einstein's theory which hereby has sooner been taken as a resource than as an outcome. The actual contents of the theory disappear and we see only the theory-induced expectations.

Strong rationalizing effects arise when the natural-scientific theoretical nexus of realities is after its formation regarded as a resource of the scientist, i.e. as a research program. The entities given by the theoretical synthesis begin to function as theoretical expectations of a newborn research program. In the course of implementing the research program the contents of the theory are transformed. There is a danger of taking the theoretical expectations of a research program as identical with the actual contents of the theory. Latour's universalizing, logical rationalization need not be wrong. At least it is illuminating in giving a rational reconstruction of the creation of the new science and society. World-views and later especially the forms of a priori rationality are the most salient appearance of theories after they have been turned into resources of research. But in science studies it is impor-

tant to move along with the actual contents of theories.

6. The settled parts of science as outcomes

In Latourian constructivism there are problems caused by the temporal simultaneity and spatial locality of the starting point. Firstly, it is difficult to regard the whole networks as an outcome. Secondly, the a priori function of theories remains unclear, because the settled parts of science must be introduced implicitly. Especially, it is very ambiguous as for the optional theoretical expectations. More constructivism is needed in order to get the settled parts of science, that act as theoretical a priories of research, into a position of outcomes. They become an outcome only in historically large and deep networks. By treating the settled parts of science and their subsequent function more fundamentally as theoretical a priories it is possible to challenge the historian and philosopher of science, and make science studies a joint project.

The counter-Copernican revolution sketched by Latour may be helpful in augmenting the scope of constructivism, because it allocates primary ontological statuses to experiments, singular events, and natural and experimental histories opening the historical domain. Without an augmentation of constructivism to include the settled parts of science at least as far as they act as theoretical a priories of research, neither philosophy nor history of science can be challenged. Philosophy of science has made its topic the a priori functions of the settled parts of science in research. History of science moves in such large domains of intermediations that all the settled parts of science become outcomes.

Constructivism cannot rely merely on the logical constructibility of the practical meaning of a theory or on the constructible meaning of theoretical a priories of the research program in order to find a solution to the problem of shared or prior knowledge. At this

point constructivism must transform itself, if it wishes to take the challenges laid down by the philosopher or historian of science. It must interpret as outcomes and in a historical contents and context, the theoretical a priories which are present in the role of preconditions of the scientific transformation. I think this can be done without rejecting the approach.

The closed black-boxes which a scientist considers as his/her theoretical a priories when creating new realities in a modern laboratory may be removed back into the contexts where they initially got closed and into the later transformed contexts in between. Thereby we may at last see them with their historical contents and contexts in a way which makes sense for the constructivist approach. The settled parts of science acting as theoretical a priories of research exhibit themselves as networks and translations of a historical domain and accordingly as outcomes in history. "History is back in the centre" (Latour). Maybe even more so than Latour meant.

The settled parts of science acting as theoretical a priories of contemporary physicists are Galilean-Newtonian or Einsteinian networks. In order to live with what philosophers of science call the prior or shared knowledge, constructivism must assume a historical creation of realities and a formation of subsequent historical outcomes. Hereby we meet translations between different laboratories and through large temporal spans. Most difficulties stem from the enlarged contents and contexts rather than from the constructivist approach itself. If we start from the contents of a theory, we get a historical context rather than a particular spatial unit or a sphere of temporal simultaneity. The simultaneity and unity created by a theoretical intermediation of realities are of a very curious quality. We should be prepared for temporal and spatial transfers back over centuries or even millennia in order to gather the contents of many theories and exhibit their domain. No doubt scientists also sometimes move through the intermediations of the same range in order to open the closed black-boxes or to adopt new theoretical expectations for the analysis of some realities.

7. Maintaining the constructivist approach in epistemic history

Augmented constructivism has to assume that every theoretical a priori stems from theory turned into a world view with a set of theoretical expectations. A prior knowledge interfering from outside of such settled parts of science would be a heterogeneous, independent ground breaking the approach which regards science as an outcome.

We have no special difficulty in removing many of the physical a priories of today into functions of the Galilean and Newtonian syntheses, or the many chemical a priories of today into the modern system of chemistry of the nineteenth century. It is more difficult to see that the numerous theoretical a priories of today were initially closed already in the practices of the ancient Greeks. If we wish to redefine the modern entity of nature we may be obliged to open not only the Galilean-Newtonian concepts but also the concept and entity of PHYSIS. It intermediates realities in the domain of natural histories. Modern entity of nature, instead, comprises the artificially created realities, too. PHYSIS is a different nexus for different realities compared with the entities given by the more modern natural scientific theories. In the analyses of subatomic realities and analyses of cultural realities the ancient notion of AITION will presumably also be re-opened and re-defined.

There are no settled parts of science closed for ever, nor any absolute entities. Accordingly there are no absolute resources of research. Every intermediation has a historically open contents of intermediated realities and a corresponding, changing context. Sometimes it is assumed that mathematics, logic and modalities are absolute a priories instead of being temporarily closed black-boxes. If it were so, apriorism would have an opportunity as well as **logical** empiricism. But even these settled parts of science may be seen as outcomes. New operations on the basis of the existing entities, augmenting the domain of things and uniting descriptions give rise to changes of contents, context and the intermediations themselves. The stability of so-

called formal disciplines is stability only in relation to the sciences which implement formal disciplines as their resources.

Because of huge historical distances and ranges it has been difficult to see the initial contents and contexts of age-old mathematical theories and logical disciplines. Many generations have seen them only through the theory-induced expectations, i.e. through the function of the intermediations as a resource of research or of practical control. A lot of work is needed in order to determine the actual, ever changing contents and context of a theory whose functions as an a priori we have learned to know. Similar efforts are needed, if we wish to see the very beginnings of theories and disciplines.

Studies in the origins of some type of nexus (theory, networks, structure, intermediation) provide one important argument for discussions on the approach. Because a theoretical a priori emerges as a result of newborn theoretical intermediation of realities, it should in the last instance be regarded as an outcome of something else than the uses of the intermediation as a resource. The successful removal of some ancient, seemingly independent mathematical a priories into a system and practice of clay token operations by Schmandt-Besserat (1979) changed one intellectual a priori into a function of a system of translations. A seemingly independent rational nexus turned into a historical result of systematizing practical activities.

Even the modal notions are open to historical intervention and are transformed. The principle of plenitude has been transformed as a regulative, categorical structure or as an equation between realities and their most elementary structural intermediation (See Lovejoy, 1971: 244; Hintikka and Kannisto, 1981: 6). In order to see such nexuses as outcomes we have to assume exceptionally large historical contents and contexts. The creation in a laboratory may be sufficient to make visible theoretical transformation or even the origin of some theory. But making visible the origin and transformation of modal categories and taking them as outcomes is possible only by dealing with the largest historical creation.

Contemporary laboratory creation may provide important exemplars rendering transformations of modalities. But the domain of realities intermediated by modal categories is all-encompassing.

Even the most stubborn rational nexuses may, however, be removed into their initial contexts and dissolved into their practical ingredients and practical contexts. It is pleasing to see, how small and unimportant islands the first rational nexuses were in the context of ancient practical discourses. (See Szabo, 1969).

Accordingly, we have good reasons for maintaining one and the same approach to all the settled parts of science (and cultural disciplines) as outcomes and to their turning into resources of research with functions of a priories. To be a theoretical a priori is a function of the nexuses. Theoretical a priories are neither independent entities nor independent grounds, but a function of completed intermediations towards particular realities belonging or taken to the domain. I call "epistemic history" studies in the origins and transformations of theories and other intermediations. For epistemic history theoretical and other nexuses of realities are outcomes.

Theoretical nexuses are open to historical interventions through practical creation. There is no single mode of transformation of those networks. In a recent analysis Thagard (1990) has described the transformations of the conceptual networks in the supersession of the phlogistic theory by the oxygen theory. In the analyses of realities, objects may be removed from one category to another. Conceptual relations may be redefined. Whole networks may be restructured. There are many modes of change for the networks. For example, as a result of research we may get transformations of the contents without major theoretical reorganizations and on the other hand changes of the paradigm as well.

When taking the settled parts of science as an outcome, we have no reason to presuppose that there is similarity between the birth contexts of theoretical a priories or that there is similarity between the nexus forms. Instead, finding out the actual contents and contexts

of intermediation for theoretical expectations (a priories) which are present as a resource, takes us into very different contexts. For the origin of chemistry we find a contents and context of art through the range of centuries. The classical structure of science consists of histories and their nexuses or ex-structures. Let me only mention the contributions of Lepenies, Foucault, Diemer and Koselleck. When moving in historical depths we may neither presuppose modern contents and contexts with specialized experimental creation, nor scientific research in a modern sense. Experimental creation and modern scientific research are agents only for the most recent transformations of the nexuses.

Many ex-structures are present prior to the specialized, experimental creation of new realities. If we follow the transformative emergence of a theoretical intermediation of realities, we often encounter antecedent intermediations which appear as transforming preconditions and which have been used as resources. But a certain kind of theoretical nexus may also dissolve into a philosophy of histories, a group of practical, interrelated arts, or some practical discourses of various contexts. By deconstructing a couple of modern transformations we already see "the modern constitution of truth" itself at its own formation (See Latour, 1990a: 154-156).

Francis Bacon considered the realities of practical experience and those of experimenting trials as almost identical. They all belonged to "histories natural and experimental". Only astronomy and mathematics had by then developed theoretical nexuses of the type which would become the trademark of modernity.

Philosophies provided loose syntheses of the material of histories. Even today histories may be used in order to transform old nexuses or to originate new nexuses and sciences. The end of natural history is not total even though there have been essential changes in its general role.

Bacon, whose text I read as an explication of the evolving modern context, noted that causes were most effectively illuminated not only by the intermediation of histories stemming from practical contexts but those stemming from an experimenting which is made free of immediate practical appraisal. Accordingly experiments which were of "no use" were recognized to be valuable in discovering causes, i.e. in transforming the nexuses of the realities (see Bacon, Preparative .., VI). It is well-known that since Galilei, artificial realities have been included in the domain of theories, i.e. Nature in the modern meaning is an intermediation both of natural and artificial realities.

Bacon described also the origin of the so-called "science push" and the indirect strategy of practical change which is so essential of modernity. "Science push" may be delineated in epistemic history by means of a priori functions of theories and other intermediations. If a theory or some other intermediation of realities is turned into a resource of research, we have a science push. In this sense mathematics and astronomy have provided a science push (or a logic push) for thousands of years, whereas natural-scientific science push is typical of modernity. The practical importance of science push is not now our topic.

8. The settled parts of science as resources

Theories are nexuses of certain realities. They are a priori towards each particular reality of their domain. We may say: to be a theoretical a priori is a function of the provisionally closed back-boxes when they are taken as the resource of a research program. A theoretical a priori is the relationship between a nexus of realities and an exemplar of particular realities which belong to or are taken into the domain of the nexus.

The metaphysics and theories of Newtonian physics gave rise, when turned into a resource of research, to Kantian apriorism. Afterwards theory and metaphysics were made transformable by means of experiential realities, actually by means of history. This was achieved at what appears to philosophy of science as the cost of an irresoluble problem of induction. Sociologists have reacted on

Kantian apriorism and on the functions of the settled parts of science as a priories of research in their own way trying to show that the settled parts of science are transformed by social facts. It is curious that sociologists have largely ignored the solutions of philosophy of science and applied only the general ideas of the origin of social facts for the entities given by science.

There is no need for the essentialist question of a priori entities. Instead, we should deal with the a priori functions of existing theories or networks, that are turned into resources of research. For example, theory-specific ontology is one of the resources of research programs. The a priori function is a relationship between the networks and some particular realities. Even in these terms we may discern possible transformations: we may recognize a particular reality to be a case of the nexus, we may subsume a particular reality in the domain, we may transform the reality by analysis into a phenomena defined by the nexus, and we may transform the nexus itself so that new realities may be included and old ones excluded.

Every new nexus will provide a new theoretical a priori by functioning as a theoretical expectation. A theory offers an optional analysis and context for particular realities. Terms like identification, subsumption and analysis refer to the various relations of realities to the theory or other intermediation. Analysis may originate transformations in the networks itself.

We return to the Latourian case of modern scientific transformation rendered by the creative activity in Pasteur's laboratory. What was the prior knowledge of Pasteur; or in other words what were his theoretical a priories? We mold the question into the following: which theories had a function of a theoretical a priori in studies concerning the realities of Anthrax-disease or the curious behavior of yeast? We see that there were options. Eminent scientists, such as Liebig, usually moved the realities of yeast into the context of chemistry and made them into chemical phenomena. Diseases were subsumed in various theoretical nexuses. Even after the stabilization of reali-

ties into phenomena defined by one theoretical nexus the realities were subjected to many other analyses. Pasteur analyzed the realities of disease in the light of physical, chemical, and mathematical a priories. It is likely that many uninteresting or wrong subsumptions and vain trials have also been forgotten.

In research the theoretical a priories need not be pre-given. Theoretical transformations may in principle be rendered in any theoretical contexts, where new realities are being subsumed and analyzed as cases belonging to the domain of the theoretical nexuses. Entirely new intermediations of realities may be created. The most intensive transformation should take place in the theoretical context that is actively used as a resource.

If we wish to reorganize a present nexus, we must deconstruct the theoretical transformations up to the initial formation of the nexus in order to see the actual contents of the theoretical nexus and its context. If I today wish to redefine, for instance, the realities of AIDS as a result of spiritual guilt, I am obliged to open and redefine at least all the closed black-boxes starting from Pasteur and Koch, because they offer microbiological intermediations of the realities instead of a plurality of separate moralities for the realities.

Formulating the settled parts of science into resources of research has since Descartes and Kant been the task of philosophy of science and of methodology. An important part of it was at first the formulation of scientific world views defining the entities of the world. Today research programs offer a plurality of scientific world views and the main thematic of philosophy of science consists of more generalized questions.

Philosophy of science is concerned with the justification, confirmation and appraisal of theories, with proofs and truth of theories, and with structures between theory and observation. Philosophy of science regards today the settled parts of science as a changing resource. At this point where theories are turned into a resource of research, Latourian constructivism should in my view make explicit the links to philosophy of science. The metaphorical and descriptive language using

terms "settled parts of science", "closed black-boxes", "networks", "actors" should be redefined by means of the developed vocabulary of "entities", "theory specific ontology", "scientific world view of a research program", "paradigm", "event", "normal-scientific research", "theoretical transformation", etc. But also additions and reformulations are needed in order to include the intermediations of realities which surpass the definitions of theory in the ordinary meaning of the word. Building links instead of ambiguity of metaphors would better contribute to the joint venture. Of course science studies, constructivism included, are especially interested in the settled parts of science as **outcomes**. But even for this reason science studies should learn from philosophy of science, because implementing the settled parts of science as a resource is a most important fact of contemporary research in regarding the settled parts of science as an outcome.

9. Resource, potentiality, and philosophy of science

Many theories originate as nexuses of some particular realities. Even though we think that, for instance, experimental realities are representative of whole classes of realities, we may see that theories have at first a very limited actual contents and context. We may think of the origin of DNA-theory, Einsteinian exemplars of physical realities, the contents of the oxygen-theory of Lavoisier, the actual contents of the Galilean dynamics or the contents of the first ancient rational theories of musical harmonies. The actual contents of theories defined by the realities which actually are being intermediated, is an entirely different thing compared with the potentialities of the research programs.

Every theoretical nexus may function as a theoretical a priori towards particular realities. As an a priori the theoretical nexus turns into a research program and defines by its entities some theoretical expectations and a closed world. One of the main efforts of philosophy of science has been to articulate the theoretical expectations, the closed worlds defined by the entities turned into a research program, and to manage the field of different and ever changing scientific world views. Kant's study, "Metaphysische Anfangsgründe der Naturwissenschaft" is the most salient example. Perhaps Kant improved the conceptual coherence of Newton's physics, but the main thing was, of course, taking the theoretical nexus as a new a priori, as a potentiality of a research program or as a resource. Latour remarked of the philosophers of science: "epistemologists, like generals, are always one war too late." Certainly this does not depend on any failures of philosophy of science, but on the functions of philosophy of science in considering the present nexuses as **resources**. Today we have, instead of Platonism and Cartesian world-views or Kantian synthetic judgements a priori, more recent worlds of theoretical expectations provided by quantum physics, biological theories, information approach, a priories of irreversibility, or a priories of fractal mathematics and chaos theory. But we also have many a priories stemming from what Rickert called cultural or historical sciences according to the distinction "Naturwissenschaften" vs "Kulturwissenschaften".

Every research program meets its utmost limits in the so-called process of actualization of its potentiality. The paradigmatic presuppositions will sooner or later be transformed so much, that we no longer can speak of one world or problematique. From this perspective we may assume that the kaleidoscope of scientific world views and scientific problematiques based on theoretical nexuses must be enormously rich, because the respective theoretical a priories are numerous and ever transforming.

In many cases the potentiality of the first natural scientific theories was tried for all realities, whether these were non-human, human or spiritual. The first natural-scientific theories, by functioning as theoretical a priories, resulted in a mechanization of our whole world view. Later times have shown us many other effects of theories functioning as paradigmatic expectations, i.e. as theoretical a

priories and definitions of the temporarily closed problem worlds of a research program.

Philosophy of science has been active in articulating the potentiality of various research programs and the respective scientific world views. This task is a very specialized one and not at all identical with the study of the actual contents of theories. The kaleidoscope of philosophies of science may be opened as a discourse of various scientific world views stemming from an articulation of the paradigmatic expectations of the research programs based on modern theoretical nexuses. The discourse of potentialities continues to the present day.

For instance, the idea of irreversibility which had such an important role in the criticism of classical natural scientific world views may turn into a natural scientific world view of its own. At least Prigogine&Stengers may be read as a new natural scientific world view with theoretical expectations of irreversibility, active matter, and entropy barrier (1984: Chapter IX). On the other hand their discussion on open science may refer not only to the time dimension of an evolutionary process but also to a historical creation of novelties. Every theoretical nexus may receive the function of a theoretical a priori. Articulating this expectation means defining a world and prolematique. By making us take the settled parts of science as a resource of research philosophy of science provides us with ever new natural-scientific world views, i.e. theoretical expectations. In a generalized mode this has been expressed as rationalism, methodology of research or as other discourses of the philosophy of science.

History of science has in my view been written mainly through the changing world views and less through the actual contents of the theoretical nexuses. We need a sharper distinction between theory taken along with its actual contents of intermediated realities on the one hand, and theory taken as a resource of research on the other. Articulating the potentiality of a research program is, of course, very important especially for research. But for science studies the actual contents of theories are most important. Many problems may be caused by taking scientific world views as our reality or as resources of research fixed for ever. Maintaining certain fixed theoretical expectations and modes of rationality has sometimes implied suppressive measures in scientific research.

10. No sin to be a realist?

Elkana (1981) was among the first to stress that scientists hold a realist attitude towards the entities expressed by theories, whereas historians hold relativist attitudes. If we accept that all entities may be seen as an outcome of some kind of practical or historical creation, relativism is a more fundamental attitude towards those entities. Why should scientists retain realist attitudes as they seem to do?

The answer is rather simple. Inside a research program you have no choice but to assume the theoretical a priories, if you wish to use the theoretical intermediation of realities as a resource. It is reasonable to maintain the paradigmatic expectations given by theoretical nexuses. A change of research program and adoption of other theoretical a priories is always possible, but then you only have moved from one research program into another. However, it is a difficult question, whether scientists are realists in any absolute sense or in any philosophical meaning of the term, i.e. beyond the temporary taking of theories as a resource of research.

Verronen has in his Kuhn-study shown that a paradigm and the respective family of actualization theories G(P) are closed as for the problems which appear amenable to solution, but simultaneously they are open in the following sense: "it is not possible on the basis of a paradigm to define *in advance* sufficient and necessary conditions demarcating the family G(P). If such a definition were possible, empirical science (..) would become a calculator by means of which in a given situation it would be possible by fixing additional conditions to calculate the details of a result, in principle already known." (1986: 149). Nor-

mal scientific research, and perhaps especially that, renders a transformation of the contents of theories. "Normal-scientific research is thus not in the main (..) the *application* of a given theory (an engineering job) but, in the best sense of the word, basic research.."(1986: 167).

Normal scientific research is enough for the transformation of the contents of theories. But is it possible to maintain frozen theoretical expectations and to be a realist in some absolute sense of the word until a total reorganization occurs. Some studies like Laudan (1984) have suggested that subsumption of new realities into the domain of a theoretical synthesis or analysis of particular realities need not give rise to conceptual transformations. For instrumentalism theory may be a grammar, i.e. the rules of the game.

However, even in these cases we can see transformation of theory. The rules are rules of different contents and accordingly different rules. Only by putting into brackets the change of the domain and the contents do we get provisionally frozen entities.

Similarity of theoretical a priories may also be only relative similitude in front of major transformations. As Thagard (1990) has shown, there are many optional modes of change of the networks themselves. A theoretical nexus with a different domain of realities must be considered as a different nexus, because there are no nexuses without a respective contents. We may assume that even scientists who never get involved in a change of their paradigm or who never study conceptual history may, however, experience and remark the transformation of the entities. Scientists remain realists only as long as they consider the existing theories as a resource of the research program.

Realism is a legitimate and necessary attitude towards the entities expressed in a research program, if we wish to use the theories as resources. Outside this context the attitude of realism should find some other ground in order to become valid. In science studies straightforward realism would be the same as reasoning from first principles. Taking theories as a resource freezes their trans-

formation and may thus, in science studies, be only a provisional standpoint for the explication of the resources of research in line with philosophy of science.

The limits of a realistic attitude may also be illuminated by showing that an experiment is not "a zero-sum game" but an event. Latour relies on Stengers when remarking that an experiment should not be explained by the principle of sufficient reason. Assuming the principle of sufficient reason means that novelty cannot be accounted for adequately. "But, nothing proves that an experiment is a zero-sum game. On the contrary, every difficulty (in explaining the outcome of the experiment — SR) suggests *that an experiment is an event.*" (1990b: 65). There is more in the experiment than was put in it. There is a history of things, not only of science. (See Latour, 1990b: 65-66).

Only discovery or some ideas concerning it seem to fulfill the preconditions of realism. In discovery we need not presuppose a transformation of the theoretical entities. Today we should discover the structure of HI-virus. We do not suppose that our main microbiological expectations will be recast at the moment of discovering the structure. We expect to find a distinct micro-organism with some molecular structure of known elements of life. If our theoretical a priories, that is our resources, are sufficient, we can speak **only** of a discovery of the structure. But if they were to change, we would have to discuss other modes of scientific change.

11. Speculations on some old problems

I have called a constructivism, augmented into a history of experiments and of other practical creation rendering theoretical transformation, "epistemic history". In the context of epistemic history we may re-interpret some old problems.

Many contradictions concerning the nature of discovery and scientific research stem from adequate studies of factual transformations of science. Their only fault may be the universalizing demand. Starting from a case

and then universalizing the domain of statements has caused many struggles and confusions. There are many different events in the history of science. The events range from subsuming new realities into the domain of a theoretical nexus to redefining the entities of reality. Extending the interpretations of the cases beyond their contexts necessarily causes confusion.

Let us assume for the sake of argument along with the realists that finding out the "right" planetary relations by Copernicus and Kepler was a solution of a puzzle or of a particular case without any major transformations of the entities. Then the case could really be described by considering some theoretical a priories as fixed resources of the scientist. (See Lehti, 1990). Accordingly these events should not be described in terms of a shift of "thinking caps" (Butterfield, 1957) or in terms of transforming the entities of nature. Let us assume that Einstein, instead, sometimes transformed the theoretical entities without experiments of his own or without the help of new experiments. Taking his transformations as a universal mode of scientific change would mold our view of the aims of scientific creation. Instead of using some resources for running a research program we should only add to the arsenal of resources. In any case we have good reasons to assume that there are many modes of transformation.

There are often optional theoretical a priories for the analysis of some realities. For a scientist the choice of theory may mean the choice of resource. In the context of epistemic history the choice looks different because theories are taken as transforming results instead of as resources. The scientist actually transfers the particular realities into the domain of a pre-given theoretical synthesis. For epistemic history it is the realities that move first. The reader should keep in mind that theories are all the time thought of as nexuses of some realities, instead of thinking of them as intermediations without contents. In subsuming some realities or analyzing them we first meet the theoretical expectations of a research program. But after successful completion of the subsumption or analysis the actual contents of the theory have been transformed.

By referring to epistemic history we may give contextualizing interpretations to the so-called "intellectual steps" or shifting of the "thinking caps" (deSolla Price, 1984); Butterfield, 1957). Let us take theories according to their actual contents and context and not as worlds of possibility and potentiality defined by the entities of the theory when turned into a research program. Now we have good reason to interpret the intellectual steps as transitions from one network of intermediation to another, both of which are nexuses of the realities. The mode of change of the networks need not be anything evolutionary. Sooner, the change consists of redefinitions of domains, hierarchical relations, predicate relations, and other intermediations. (See Thagard, 1990). As for the mode of change we do not disagree about the ruptures. The restructuring of nexuses or intermediations need not at all have the same mode of change as the change of the realities that are being intermediated.

The practical degrees of freedom in modernity cannot be adequately treated without considering the functions of the settled parts of science. We are used to thinking of practice as action on the basis of knowledge produced in a laboratory. And this is the factual situation when we follow an indirect strategy in order to render practical change. Our reality is created as an outcome of many particular creations. Theory as an outcome means following an indirect strategy towards changes in practical contexts. We should not underestimate the role of this outcome as a resource of research **and** as a resource of so-called acting at a distance in various practical contexts.

From Latour we learned that in the cases concerning Pasteur's laboratory some realities were first removed into the laboratory. After the analyses and theoretical transformations some micro-biological technological realities were created on the farms as extensions of the laboratory. In an ongoing study concerning the problem patterns of doctoral

dissertations I have found a similar scheme. The starting point of so-called applied research was often in practical problem situations or evaluations. The practical value context was bracketed out and some realities were taken to be molded. Mathematical or natural scientific analyses of the realities revealed the degrees of freedom in the intermediation of the realities. The mathematical or natural scientific contents and contexts were taken for granted as far as possible. But in the last instance analyses of the realities rendered theoretical transformation as in natural scientific research.

It is an important idea that there is an inherent relation between the realities of a laboratory and those of a farm or some other practical domain. But it is very important that the realities of the starting point have no privileged position. Instead, the whole transformed contents of theories is ready for extensions into various practices and value contexts. For example, if our starting point were analyzing means of communication for the handicapped, we might have to transfer the realities into the context of a theory of signals. But the transforming theory of signals may have among its practical contents not only the communication of the handicapped, but also the flight of a fighter. Our analyses may, by transforming the contents of the theory, benefit warfare. The spin-off of military research is a well-known example of opposite changes of value contexts. Instead of speaking of the neutrality of theories towards practices and instead of assuming a finalization of theory, I would like to stress that the contents of scientific theories are, as for their practical value, genuinely multi-valued according to our contemporary and future practices. Typical of modernity is the indirect strategy through natural-scientific theories.

Pasteur had many practical problems as his starting point. He analyzed and transferred the realities into the domains of some theories, changed them into controlled microbiologically based art and returned them as an extension of the laboratory into practical value contexts. There is an immanent relation between the realities of a laboratory and those of practical contexts. One thing remains in the darkness. Pasteur's microbiological theoretical nexuses were transformed, and the whole contents of the transformed theory was in principle available for a practical evaluation. Only if we have the case of an origin of the nexus we may assume that the actual contents of a theory is given by the exemplars of the case (See also Verronen, 1986: 150).

12. Positive logic and ontology — for passing moments

Scientific transformation has many "logics", or more adequately, modes of change depending on which transforming events we are looking at. In the last instance the transformation is rendered by some kind of historical creation. I would like to say that there is no positive logic of scientific change for the change as a whole. Instead, the creation of novelties takes place in the domain of negations of old realities. Accordingly, even the most free creation aiming at theoretical transformation is constrained by the existing realities, and by the "negative logic" of creating new realities in the domain of negations of old realities. We may foresee the new things in a general way as the negative domain of existing realities, but we cannot give any novelties in an unequivocal way before their origins. In the last instance we must admit that even the modes of change are provisional or open to historical interventions.

We get a positive logic of research, discovery or theoretical development only by provisionally freezing our theories or other intermediations into resources of research. Such a positive logic may be tried for the episodes of theoretical transformation and for history of science, too. Some results have been achieved. In recent studies modes of change of networks were extracted and fixed as a resource for the interpretation of cases of scientific development. The trial resulted in an interesting description of the changes of networks. An unexpected link between artificial intelligence and the history of science

was established (see Slezak, 1990; Thagard, 1990). Accordingly, the mode of transformation may be turned as a temporary positive logic into a resource of research.

Freudenthal (1984) maintained that constructivism bases on idiosyncratic and contingent products and may not explain simultaneous discovery or anticipation. We have seen that there is a basic difference between the approach of philosophy of science, on the one hand, and that of constructivism, on the other. The former regards theories as a resource of research, while the latter is interested in science in the making and takes theories and other intermediations as outcomes. For constructivism the assumption of a positive logic is always provisional. However, constructivism should better take into account what Freudenthal would call the functions of prior knowledge. Especially, it should take into account the Kuhnian normal-scientific research, because it is a most relevant instance of **historical** creation of the present day.

So far we have had no reason to use any entities as merely postulated and accordingly independent grounds. All entities may be seen as outcomes. No doubt, constructivism augmented into epistemic history will meet the old torture by means of abstractions, like "existence as such" or "objectivity independent of our aims". Old misconstruals should not be imported by assuming the dilemmas of Cartesian or Kantian philosophy of science, or the kaleidoscopic circus of scientific world views of modern research programs.

Maybe constructivism has something new to say as for the problem of universal nexuses of events and the problems of the existence of everything. Until then we may assume for instance that "objectivity" is a function of the whole epistemic history. As a provisional answer to the old dilemmas Latour has redistributed the ontological statuses. Instead of thinking of facts or phenomena as a meeting point between the resources of the object-pole and the subject-pole, in the counter-Copernican revolution they receive a more important ontological status. Latour does not give an unequivocal definition of this immediate "tran-

scendence" which is prior to intermediations but describes it by means of the hot agents of making Nature and Society, through the ideas of "quasi-objects" (Serres), "actor-network" (Callon), "forms of life" (Shapin&Schaffer), and "experimental practice" (Lynch).

I have chosen to speak of particular realities of our reality which in the last instance may only be nominated, i.e. referred to so far lacking a special analysis of them. As for some classical formations and transformations of science we have good reasons to call the entities in question "histories". I would also like to bring up the optional concepts used by Whitehead. I mean "events" and "actual entities". We should also keep in mind Rickert's way of seeing our reality through singulars or historical events. Our reality becomes nature if we take it in a universalizing manner and history if we take it in an individualizing manner. All ontology is theory specific or in some other way historically open. We have no absolute, positive ontology.

13. For a joint project of historians, philosophers and sociologists

I hope the above said gives a view of the consequences of the necessary augmentation of the constructivist approach. When augmenting its field of problems, constructivism should retain its strong points. Especially, the empirical grounding should not be weakened in the course of augmenting the field. The Latourian redistribution of ontological statuses should not mean ignoring the settled parts of science or favouring some naive empiricism.

In my view there are opportunities for new links between the studies of historians, philosophers, and sociologists. What philosophy of science turns into a resource of research is actually historically open, or if you prefer, a temporarily closed black-box. Laboratory experiments as non-zero-sum games, events as singular things, Kuhnian genuine normal-scientific research, particular realities of our reality, histories in the classical meaning of the word and irreduced experience all introduce history into an agent of theoretical trans-

formation. They make every intermediation historically open. Constructivism is especially interested in this kind of science in the making.

The openness of philosophy of science in front of history has been acknowledged in various ways in philosophy of science. Firstly, it has been admitted as a criticism of so-called received view which neglects the dynamic aspect of theoretical change and gives history no theoretical significance. Secondly, it has been admitted in the elimination of absolute metaphysics and synthetic a priories, in the dead end of apriorism and classical intellectualism, in establishing positivism, in moving from justificationism to falsificationism, and in the relativization of scientific world views. Thirdly, it has been admitted in front of the fact that no positive logic of induction, no positive logic of discovery or theory change has been found. The category of novelties and anomalies means the same openness. Lastly, Kuhnian philosophy of science has tried to introduce history as a regular part of philosophy of science through normal-scientific research.

Constructivism concerning the simultaneous making of science and society, philosophy articulating the research program, and history as epistemic history may be linked to each other. Division of labor, differences of scope and approach remain, but the separateness can be overcome.

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