

## **Experimental Systems, Objects of Investigation, and Spaces of Representation<sup>1</sup>**

### *1. Introduction*

Ian Hacking opened his 1983 book on "Representing and Intervening" with the cutting phrase that for too long, philosophers had transformed science into a mummy. This criticism was meant simultaneously as a gesture and an offer toward his own philosophical community. Hacking urged his colleagues to set their mummies aside and to enter into a dialogue with the living body of science (Hacking 1983). It would be an interesting question to investigate whether the majority of philosophers of science followed this invitation. The present paper does not claim to answer this question. Moreover, we do not want to deal with the problem, whether one can entertain a fruitful philosophical study of science today that does not take the impact of cultural and social configurations on scientific knowledge into serious consideration. There is no doubt, however, that the practical turn in science studies over the past fifteen years has changed the old disciplinary and cognitive boundaries between history, sociology, and philosophy of science. Science studies have come into years, and so it seems not unlikely to look at the merits and shortcomings of this enterprise. One of these shortcomings seems to be the strict polarisation between 'nature-out-there' and 'society-out-there,' leading to a view of science as being exclusively shaped by construction and interest, manipulation and negotiation. Whereas philosophers sometimes have the tendency to regard practice and the cultural context as mere epiphenomena, sociologists of science have often neglected the epistemological dimensions of scientific objects. Claiming that scientific facts are socially constructed is as uninteresting as it is to insist on the objectivity of scientific facts without acknowledging that objectivity is itself a historical category (Daston and Galison 1992). An understanding of scientific practice that transcends these dichotomies – as developed especially by

---

1 This essay is based on the following publications: Rheinberger and Hagner 1993; Hagner, Rheinberger, and Wahrig-Schmidt 1994; Rheinberger, Hagner, and Wahrig-Schmidt 1997. Michael Hagner acknowledges the financial support of the Deutsche Forschungsgemeinschaft, granted to him in the form of a Heisenberg Fellowship.

Bruno Latour (cf. Latour and Woolgar 1986; Latour 1987; Latour 1988) – does not simply reorient the attention on instruments and experiments. The claim is rather that the complex conditions of engendering scientific objects and phenomena (elementary particles, genes, neuronal networks, enzyme complexes, the immune system, and so on) are to be treated as historical ensembles that resist to being classified as either natural or artificial, objective or socially construed.

In the following paper we want to suggest that experimental systems, i.e., the empirical set-ups in which scientific objects take shape, serve as prisms in focusing the sum total of these conditions (cf. Rheinberger 1997). The paper is divided in three sections. First, we give a brief and, admittedly, coarse reconstruction of the major changes from theory-first views to what could be called the primacy-of-practice view on science. Second, we take a closer look at experimental systems and try to characterise certain major features of their synchronic as well as their diachronic interactions. Finally, we argue that the investigation of such localised systems can yield clues for a history and epistemology of science that might come under the heading of "cultural spaces of knowledge."

Practice constitutes a major focus of contemporary science studies, but there is a continuing debate over the scope of its context. "Social studies of science," "science in context," "new experimentalism," or "practical reasoning" are only a few of the more prominent slogans under which experimental practice is thematised.<sup>2</sup> What is common to all these approaches? The growing scepticism against the leading role of theories in scientific development has already been mentioned. Prominent twentieth century philosophers of science, Pierre Duhem and Willard Van Orman Quine, Rudolf Carnap, Carl Hempel, Ernest Nagel, Karl Popper, and Thomas Kuhn, Imre Lakatos, and even Paul Feyerabend, have been accused of having concentrated their attention on large theoretical changes.<sup>3</sup> Logical empiricists, critical rationalists, and even their critics basically treated experimentation as an element of an overarching inductive or hypothetico-deductive model. Experiments were seen as rather unproblematic instances of testing hypotheses.<sup>4</sup> In the last resort, truth or at least

---

2 Actual compilations can be found in Pickering 1992; Pickering 1995; Buchwald 1995.

3 Hacking has stated: "History of the natural sciences is now almost always written as a history of theory." (Hacking 1983, 149). Today, this qualification can no longer be upheld.

4 See the critical overview in Galison 1988, 207-208. The famous formulation of Karl Popper reads as follows: "The theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive ans-

evidence was thought to be organised by theory, models, and mathematical reasoning according to the laws of logic and of nature. This firm belief explains the shock that reverberated through the community when Kuhn claimed that scientific paradigms contained more, and categorically different, elements than ratio, logic, and proof. Kuhn did not, however, explore the whole space he had opened with concepts such as "incommensurability." Investigating a possible incommensurability between theory and practice was, for example, not on his agenda.

Similar trends can be observed in history of science. To put it somewhat simplified: after 1945, historians of science oriented themselves toward a history in the tradition of Arthur Lovejoy, based on the assumption that the "longue durée" of a single idea would shape various fields of knowledge over a long period; or they varied Alexandre Koyré's thesis of the fundamental Platonism of modern science in eagerly attempting to underpin every scientific innovation with a philosophical foundation.<sup>5</sup> Neither the intrinsic dynamics of modern scientific experimentation, nor the relationship between laboratory bench work and instrumentation, nor the broader context of technological, industrial and social development came to the attention of these historians of science who, beyond all divergence in detail, completely agreed among each other and with the philosophers on this point. "External" influences were left graciously to the Mertonian type of sociologists of science.

This division of labor was still prevalent in the 1970s. How and where did the new orientation take its starting point? Hacking's philosophical manifesto and Latour and Woolgar's early laboratory study have been mentioned. Around the same time, decisive impulses for changing the situation came from the Edinburgh and the Bath schools of sociology of science. David Bloor, Harry Collins, and others developed a long argument for conceiving the whole of science as a social construct.<sup>6</sup> The experiment as a practical activity served as a litmus test for this approach. Just as with any prototype of the first generation, it would be easy to enumerate the deficiencies and shortcomings of the "strong program." Indeed, the practical turn just mentioned quickly emancipated itself from the programmatic framework of investigating the social constitution of the sciences. Coming from widely different methodological and discipli-

---

wer to these questions, and to no others. All other questions he tries hard to exclude." Popper 1935[1968], 107.

5 To mention *pars pro toto*: Lovejoy 1936; Koyré 1968.

6 From Edinburgh, compare, e.g., Bloor 1976. Second edition 1991; Barnes and Shapin 1979. From Bath, see Collins 1974; Collins and Pinch 1982. Useful in this context is also Barnes and Edge 1982.

nary backgrounds, researchers have discovered practice as an object of inquiry in its own right and have focused on the epistemological status of experimentation including, of course, its social dimension. Among others, the following questions have been raised: Under which circumstances does manipulation acquire the status of an experiment? How are experiments performed? Who decides what counts as a successful experiment, and what as a failed experiment? How are experimental data produced, how are data transformed into facts? Why is it that modern scientists have settled on the experiment as the source of scientific success since the seventeenth century (Shapin and Schaffer 1985, 3)?

It is evident that questions about the nature and the status of experimental practices and their theoretical as well as social consequences have to be framed within the specific context of an epoch. It is equally evident that an answer to such questions depends, at least in principle, on the possibility of reconstructing often complex experimental activities. For a long time, historians of science have restricted themselves to the interpretation of published data. In order to reconstruct experimental investigations, however, unpublished sources such as laboratory notebooks, letters, grant applications, research reports, and interviews are gaining more and more attention (Holmes 1992b and 1993a; Steinle 1995). As a consequence, the practical reproduction of historical instruments and experiments, originally developed for pedagogical reasons, has become increasingly valued.<sup>7</sup> Of course, the reproduction of an experiment does not show irrevocably how it 'really' was enacted in history, as might be assumed naively (Holmes 1992a). Work on experimental reproduction does, however, open a space of knowledge that is not accessible to historical reconstruction at the level of texts. Experimental designs and research chronologies are likely to reveal themselves as published fictions, and from a sufficient historical distance, local constraints and limitations of experimentation are likely to disappear in the bright light of scientific success stories. Furthermore, the question arises as to whether experiments are privileged elements of deliberate strategies, whether the corroboration of facts operates from a secure Archimedian point, or whether experiments exhibit an inherently exploratory and groping character that may be proper to any conquest of new territory.<sup>8</sup>

---

7 Falk Rieß of Oldenburg University is one of the pioneers in this respect. See also Sibum 1994; Sibum 1995.

8 For the notion of exploratory experimentation see Steinle, this volume.

## 2. *Experimental Systems*

Our own work in this field has focused on those functional research units which in the everyday language of the life sciences figure under the rubric of "experimental systems," a notion which we have explored in a number of case studies.<sup>9</sup> In doing so, we have sought to contribute to the growing discourse on experimental practices mentioned above. But we have also sought to accentuate the materiality of research processes in the more specific sense of this notion.

Experimental systems are hybrid arrangements: in a permanently fluctuating and varying pattern, they mix up elements which many historians and philosophers of science, and sometimes even scientists (at least in their semi-popular essays) wished to have properly separated. This desire for separation is due to a vision of purity that has no counterpart in the process of science in the making. Research objects, theories, experimental arrangements, instruments as well as disciplinary, institutional, social, and cultural *dispositifs* (Foucault 1978) add up to amalgams of every conceivable gradation. It was a frustrating enterprise to sort out elements of this entangled ensemble, to dichotomise it into external and internal factors of scientific development and to characterise their relation in terms of relative autonomy, influence, dominance or dependence. Instead, we propose to take experimental systems as the units for pursuing the fine structure of such intricate textures from within, assuming that such systems contain all the conditions necessary for a research process in its entirety. In addition, an analysis of how different experimental systems interact – how they overlap, and how they delimit, exclude, or supplement each other – should provide insight into the developmental dynamics of broader fields of science.

The question of how far the notion of experimental system will take us remains open for debate. Is an experimental arrangement adequately described in terms of a "system"? Certainly, there is a friction between the concept of a system as a large functional totality and the idea of experimental systems as tinkered and hybrid research settings. We deliberately use the notion of system in a rather loose fashion, and certainly not in the Luhmannian sense of "science as system" (Luhmann 1990). The concept of experimental system appears to be justified only if a "system" is allowed to encompass heterogeneous elements that can be recombined

---

9 Compare, e.g., Rheinberger 1992a and 1992b; Hagner 1993. See also Hentschel's attempt to classify different elements of experimental systems in a more systematic manner, this volume, and Hentschel 1995.



at any time; and, moreover, if it is seen as remaining open in the course of its history for discarding old components and for incorporating new ones. Historical scenarios that operate with a "brick model" of the intercalation between relatively autonomous traditions of theorising, experimenting, and instrument building also break with the oversimplified unitarian view of science (Galison 1988). Nevertheless, they have to cope with the problem of generalising the conditions under which advanced modern physics is practised, and they are confronted with the basic problem of permeability between theoretical, experimental, and instrumental traditions. The decisive question is how scientific objects become displaced, transported, reiterated, and boosted in experimentally, instrumentally, and theoretically integrated ensembles endowed with epistemic, cultural, and social power.<sup>10</sup>

Viewed from a historical perspective, particular experimental systems have contributed decisively to shaping disciplines. Molecular biology with its plethora of *in vivo* and *in vitro* systems from biophysics to biochemistry to genetics is a prime example of this process (Morange 1994). On the other hand, experimental systems time and again have been the levers with which classical disciplinary boundaries have been transcended, perforated, displaced, and even dissolved. Following the dynamics of such systems, history of science is no longer concerned with dichotomies such as extrinsic versus intrinsic factors of scientific development, basic science versus technical application, and biographical versus historical reconstruction.<sup>11</sup> In this perspective, history of science – beyond a history of ideas and persons, of disciplines and institutions –

---

10 Lynch and Woolgar tackle the problem with the formula of "representational practice in science," including the following elements: "graphs, diagrams, equations, models, photographs, instrumental inscriptions, written reports, computer programs, laboratory conversations, and hybrid forms of these." Lynch and Woolgar concede that from such bricolage, a certain "heterogeneity of representational order" follows, thus lending considerable vagueness to the concept of representation. See Lynch and Woolgar 1990, 1-2. For the use of the notion of representation in contemporary science studies see also Hagner 1997.

11 That history of science is a latecomer in this respect is documented by the French history and archaeology of the human sciences. Georges Canguilhem has shown that the stabilization of the relation between normality and anormality in medicine was the result of a long transformation process throughout different scientific disciplines (Canguilhem 1943 [1991]). And Foucault reveals what he calls the "positive unconscious of knowledge" through a comparative analysis of natural history, economy, and grammatology in the eighteenth century. He shows that the commonalities between them – disciplinary boundaries notwithstanding – were far more prominent than the respective continuities with their purported heirs in the nineteenth century, that is, biology, political economy, and philology. Foucault 1973. To this point, compare also Lepenies 1977, 135-136.

becomes also, and above all, a history of things (Latour 1990 and 1996). Concepts such as experimental system, epistemic thing, space of representation, resonance of representational forms, and medial materiality testify to the effort of giving voice to the cultural impact and force of these things. Traditional ideas about the conscious choice of the scientist, popular (auto)-biographical anecdotes about lucky intuition, or the intervention of chance in discovery, are all interesting anthropomorphisms which provide material for a cultural history of the scientist. They have been evoked inaccurately and instrumentalised, however, in order to reduce the complex processes of scientific inquiry to the orderly measure of an individual. In contrast, we focus our attention on how scientific objects are produced and reproduced, stabilised and destabilised, deformed and reformed.

It would be a misunderstanding to equate the emphasis on experimental systems with a sort of aestheticisation of the experiment, transforming the laboratory into a closed chamber in which completely autonomous events happen. We do not plead for a new autonomy of the sciences, transposed from the rationality of scientific thinking into the intricacies of experimentation. One thing appears to be clear, however: the deliberately social constructions of the experiment are simply insufficient to account for the incredible economy of the scientific enterprise, which at every turn comes up with new surprises and changing prospects that no individual or social imagination has ever been able to anticipate.

There is another tradition worth revising that has made its way into science studies – the notorious focus on the physical sciences. Many scholars who highlighted the role of the experiment have taken their examples from physics: Galison, Hacking, Shapin and Schaffer, Wise, Pickering, Cantor, Gooding, and many others.<sup>12</sup> The strong focus on and the continuing argumentation from the physical sciences is certainly due to the fact that the physical sciences have been regarded, from the eighteenth century onward, as the most developed among all scientific disciplines. In addition, quite a number of twentieth century philosophers of science had a fairly extensive training in physics. This disciplinary restriction has not remained without consequences. Even Hacking's revised approach to the experiment, culminating in the pronouncement that experimentation has a life of its own, is mainly based on examples from the history of physics (Hacking 1983, 149-166). Galison's model of autonomous traditions in theorising, experimenting, and instrument building

---

12 It is revealing in this respect that Gooding, Pinch, and Schaffer's (1989) important essay collection does not contain a single piece featuring the life sciences.

explicitly refers to physics, with its socially and epistemically consequential division into an experimental and a theoretical realm. It is less convincing when he suggests the same model for microbiology, even if he concedes that the divide is much more subtle in this case. Galison is certainly right in warning of the "unwarranted assumption – shared by both positivists and anti-positivists – that there is a universally fixed, hierarchical relation between experiment and theory" (Galison 1988, 208). Especially in the biological sciences, it is evident that such hierarchical relations can scarcely be discerned, and that well-established autonomous traditions of theory and experiment do not exist. In contrast, we observe a permanent reconfiguration of heterogeneous components which crystallise in experimental systems – and which frequently dissolve again after a certain period.

Promising epistemological concepts and models deriving from the history of the biomedical sciences scarcely existed until fifteen years ago. If they existed – Ludwik Fleck is an example (Fleck 1935) – they were received only after a considerable delay. This situation has changed with the anthropologically motivated laboratory studies in the late 1970s and early 1980s, when anthropologists and field sociologists invaded biomedical laboratories.<sup>13</sup> These studies did not remain without consequences for the larger context of history and philosophy of science. Little later, the experimental character of nineteenth century physiology was highlighted in its many facets (Coleman and Holmes 1988; Cunningham and William 1992), and historical studies concerning the recent development of molecular biology and immunology gained momentum.<sup>14</sup> Only the smaller part of this development is due to the long-standing efforts of a 'history of biology' that oriented itself on the standards of the history of physics, whose predilections for theory and concept development it took for granted.<sup>15</sup> Rather, it appears that this development is largely due to the fact that biomedical crafts, and the knowledge derived from them, have become so prominent during the last two decades. It is not by chance that many historians engaged in these issues have a laboratory background. Although speculative, the prognosis that the life sciences of the twenty-first century will succeed physics in its role as a cutting-edge science is

---

13 Compare the pioneering studies of Latour and Woolgar 1986, and Knorr-Cetina 1981.

14 Compare, e.g., the different contributions in Le Grand 1990, as well as in Lynch/Woolgar 1990, comprising physics, chemistry, biology, anthropology, geology, and medicine.

15 This general conclusion notwithstanding, there have been important developments in the history of biology. See Olby 1985.



not unreasonable. Apart from this speculation, experiment and theory in the life sciences are so intricately interwoven that the function of the experiment as an instance of testing hypotheses appears to be largely marginal. As a rule, scientific innovation in this field is in the exploratory realm and beyond disciplinary boundaries (Burian 1997; Steinle, this volume).

### *3. Objects, Differences, and Conjectures*

In the first part of this paper, we argued that the shaping of experimental systems is a contingent process. It is embedded in instruments, apparatus, technical procedures, materials at hand, and model objects on the one hand, and it is closely linked to local crafts, research traditions, and wider epistemic as well as practical interests on the other. The decisive question is how these particular segments get articulated, how they condense to a structure that finally develops a dynamics that was not inherent in these parts per se, and therefore serves as a crystallisation point for unprecedented knowledge. If it is correct that the analysis of particular components of an experimental system is a necessary, but not a sufficient, condition for understanding its dynamics, then we have to look for relevant features that characterise such an ensemble as a whole. In the following section, we will concentrate broadly on three such features. Objects, differences, and conjectures are three notions we deem necessary for an understanding of the dynamics of experimental systems, of their precarious existence between repetition and change, and of the synchronic concatenation and the diachronic chains they form.

#### **Objects**

Historians of biology have emphasised repeatedly that the choice of objects of investigation has played and continues to play a decisive role in the life sciences. Particular organisms become reference points for 'problem packages', and in turn begin to shape these packages. A telling example is the sweet-water polyp hydra that served as a focus for the debates on the existence and efficacy of the so-called "formative drive", or "life force" in the eighteenth century. Questions of bioelectricity and – in a more general sense – of nerve and muscle physiology in the late eighteenth and early nineteenth century centered around one particular animal species – the frog (Rothschuh 1973; Holmes 1993b). A closer analysis of such examples shows that the uses and scientific 'careers' of certain kinds of organisms are bounded by a complex set of boundary conditions, com-

prising ethical and financial considerations as well as disposability, aptness for laboratory work, and easy manipulation. At this point, social and epistemological aspects of the investigative enterprise intercalate. The scientific dignity of an object depends on its suitability for being technically embedded in an experimental system. If revolutionising biomedical techniques and representations at times depend on highly complicated and expensive apparatus, at other times they result from deceptively simple inventions. In both cases, new methods and strategies for manipulating objects are decisive. Yet, history of science has shown repeatedly that it is the broader context which decides about whether such arrangements are to develop an innovative potential in the long run.

A natural object can become attractive for many reasons, but it only becomes productive in an epistemic sense if it fits into specified spaces of representation. In turn, such spaces receive their particular shape from these objects. As already mentioned, whole experimental systems and investigative programs accreted around particular animals, plants, or unicellular organisms in the late eighteenth and the nineteenth centuries. This situation has become even more accentuated in the twentieth century. Historians of molecular biology have shown that the choice of a suitable organism has inaugurated entirely new fields of research.<sup>16</sup> Bacteriophages, *Escherichia coli* bacteria, *Drosophila*, the Weaver mutant of a mouse, all these investigative objects have shaped laboratory research in molecular genetics, neurophysiology, immunology, virology, and cancer research to a considerable degree. That does not mean, however, that these organisms can be equated with experimental systems. At best, they are components of such systems with a high potential for configuring those systems. Certainly, the transformation of natural objects into scientific model objects is one of the crucial procedures in the experimental sciences. Analysing such transformation processes in specific laboratory situations is worthy of detailed historical reconstruction, but the exclusive focus on organisms disguises the role of apparatus, inscription devices, and visualisation techniques whose epistemic impact is not the organism per se. Otherwise, such organisms would not function as models. They fulfill their role only if they can be 'loaded,' that is, if they serve, qua scientific entities, as a material basis for the construction of epistemic things. Flies, mice, and bacteria are not epistemic things by themselves, they are the matrices into which epistemic things such as memory, oncogenes, messengers, and brain centers are inscribed in the process of ex-

---

16 Compare the articles collected by Lederman and Burian 1993. See also Kohler 1991, and Kohler 1994.

perimentation. It is precisely at this point that Bachelard's concept of the "scientific real" has to be placed, with its two faces of "reified theorem" and "epigraphy of matter," respectively (Bachelard 1934 [1968], 170-172). Scientific objects do not represent nature as such, but are always subjected to manipulations that may separate them from their natural environment. What is ultimately important is the 're-scription' of these objects as models for something that escapes immediate comprehension. Consequently, we need to understand in detail how these inscriptions and traces are put on stage. We will come back to this question in the last section of the paper.

### Differences

One of the central aspects of an experimental arrangement is that it must display sufficient reproductive coherence. Reliability and stability belong to the foundations of the authority with which scientists justify their activity, to each other as well as to the public. Historically, this has not always been a hallmark of science. In late seventeenth century's erudite circles, for example, the existence of certain monstrosities was accepted solely on the basis of the reliability of a single witness, although nobody else had seen that specimen. Harry Collins and Trevor Pinch have convincingly shown that with respect to central scientific events of the twentieth century, too, reproductive stability is not always achieved. Experimental corroboration of hypotheses and theories, replicability, and the generation of coherence are by no means the only criteria for the acceptance of a scientific fact. Acceptance, they claim, is the outcome of a complex system of relations and negotiations (Collins and Pinch 1993).

Where Collins and Pinch argue that research results are ultimately justified and stabilised through the interaction of participants and critics, we assume that the very attraction, and with that, the historical force and reason of experimental systems, lies in their ambiguity toward stabilisation, in their ability to produce differences. Collins and Pinch seem to maintain that experimental instability and uncertainty is necessarily a case for negotiation outside the laboratory. Such a view underestimates the productive dynamics of the experimental process. Differences are new data whose very novelty renders them non-corroborated at the point of their emergence. On the other hand, such differences must become included in the reproductive background of the system in the long run. Otherwise, they would degenerate to a mere divergence and finally lead to the dissolution of the system. Historical hindsight makes it easy to distinguish between productive differences and degenerative divergence. At the research front, however, this distinction is not easy to make. It is one of

the central issues scientists negotiate, debate, and often fight over passionately. The production of differences often has been dismissed as a generation of artefacts, of noise, or of epiphenomena, or it has been taken out of the realm of the epistemological and placed into the psychological and contingent context of discovery. We maintain, on the contrary, that new knowledge – in its experimental reality – emerges from an oscillation between stability and breakdown. Just as objects come to embody epistemic things that were not written in plain on their surfaces, experimental systems can generate epistemic things that did not belong to the initial makeup of the system.

### Conjunctures

Experimental systems may become functionally autonomous for some period of their existence. However, this is not necessary, and does not generally remain so. Instead of being intrinsically autonomous, experimental systems display a tendency to become connected. In other words, they are susceptible to conjunctures. Conjunctures are characterised by connections, co-operations, and transfers that are the result of unprecedented events originating from experimental systems. They often make the systems appear in a new perspective. Such repositionings displace the boundaries of possible scientific co-operation at a given time, and give rise to renegotiations of scientific interests that result from the introduction of new apparatus, materials, and know how perceived to be useful in contexts other than those of their locally constrained initial development. Conjunctures form nodes or attractors for the spontaneous creation of informal scientific communities, which serve as mediators of experience, or keep the game of the possible going beyond institutionally-sanctified disciplinary boundaries. From an epistemic point of view, conjunctures belong to the same category as scientific objects and differences: they describe the emergence of scientific novelties, and help us understand how new fields of research acquire social reality, as transient co-operative ventures, as research projects, and sometimes even as Max Planck Institutes.

### *4. Spaces of Knowledge: Representation*

In recent years, science studies have increasingly borrowed from cultural anthropology, social history, and discourse analysis. This trend toward transcending existing barriers in the perception of its subject matter also implied that the boundaries of what counts as history and philosophy of

science have become porous. This has led to a state of productive insecurity, which is not due to a lack of reflection, but is part of a broader change in the historical conceptualisation of modernity we are witnessing currently. The seeming paradox of globalisation on the one hand, and contingency, incongruence of time, and fragmentation on the other hand are elements of this change. While we are not yet able to draw its contours in a sufficiently distinctive manner, it is clear that the traditional boundaries between history of science, cultural history, social history and the history of media have become blurred. Horizontal fields of discourse have come into view, and as modes of narration and strategies of objectivation are questioned, new, transdisciplinary problems come to the foreground. One of these problem complexes accretes around the concept of representation, in the sciences as well as in other cultural activities.

The interest in the history of media and the relationship between representing and intervening in scientific practice is part of a movement that retrospectively has been baptised as "semiotic turn".<sup>17</sup> On the other hand, the attention paid to laboratory inscriptions and to the material semantics of research technologies is a genuine tendency of the new science studies. Science, so the argument goes, is no longer to be opposed diametrically to other forms of knowing, of conceptualising, and of meaning. The semiotic movement following Ferdinand de Saussure (Fehr 1995; Saussure 1997) as well as the more sociologically-oriented science studies imply – widely differing methodological premises notwithstanding – a growing sensibility for the fabrication and hence the historicity of symbolic spaces and cultural systems of meaning. It is not an accident that historians of the natural sciences have resisted this type of historical and discursive analysis so vigorously. The modern sciences have long been considered as the cultural system par excellence for disclosing truth, granting validity, and conferring power; in short, a system constituting the vanishing point of modernity *tout court*.

The comparison between semiotics and science studies is as appealing as it is problematic, at least from the perspective of their pretenders. The persistent discourse on the irreducibility of 'writing' has led to the crisis of a key concept of traditional metaphysics: the *logos*. The epistemological barrier between representation and its referent has become permeable. It would appear that the transfer of the "crisis of representa-

---

17 Latour 1993, 62-65, cf. also Pavel 1988. The "semiotic turn" in French philosophy should be sharply distinguished from the "linguistic turn" in analytical philosophy. In the historical distance of 30 years, it might be interesting to compare these two crucial events despite categorial differences between them.



tion" to the sciences might come as a relief. One might gain the impression that with respect to the question of representing scientific knowledge, nothing more than the execution of a testament might be required, a testament deposited long ago by art history and literary criticism. In contrast to such expectation, the field has yet to be charted. To take just one example, the problem of the relationship between science and literature, and scientific writing as a literary genre, has received attention only very recently.<sup>18</sup> These changes in the study of the sciences give the issue of representation a refreshing touch, but if history of science is to succeed with a concept of representation that articulates itself beyond the perennial correspondence-theory-of-truth quarrel, revision is necessary on two points. The first point concerns the model of language, and the second point concerns the very issue of representation itself.

As long as critical and historical reflection does not question the paradigmatic role of linguistic structures, representation inevitably remains linked to theory. Analysing representation then continues to mean historical reconstruction of symbolic systems. In a practice-oriented analysis of representation, however, the experimental, instrumental, pragmatic, and discursive aspects of scientific symbol production will be predominant; representation will be treated as a materially mediated cultural activity. This should not be taken as a capitulation before theory. Our century has witnessed such far-reaching events as the revolution in subatomic physics and the introduction of the information concept in cybernetics, computer science, and molecular biology, after which we can duly ask whether the work of the sciences amounts at all to something like an "image of the world" (Wittgenstein 1921 [1968], 19). The molecular turn in biology since the 1950s seems to imply a complete reversion of the traditional relation between representation and referent. The molecular code itself no longer can be envisaged as the representation of something, but has to be conceived as that primordial procedure that generates representations of itself. On this fundamental level, it can rightly be asked whether concepts such as matter, message, or life can be treated as representations any longer, at least in the traditional sense of the word. What, then, could representation still mean? That "representation" reveals itself as a many-layered metaphor should not ban its use, but it should keep us aware of the fragility of its meaning (Hagner 1997; Lynch 1994).

---

18 Interesting views on a cultural history of scientific knowledge are developed in Rouse 1993; Rouse 1995. With respect to the relation between literature and science, see the series "Writing Science" of Stanford University Press, directed by Hans Ulrich Gumbrecht and Timothy Lenoir.

On the level of scientific practice, representations are realised in quite different forms. They come as experimental designs, data configurations, symbolic schemes, diagrams, graphs, formula, pictures (from drawings to X-ray films), charts, statistics, computer simulations, holograms, to mention only a few historical and actual manifestations. The activity underlying these forms of epistemic rendering can be generalised in terms of a production of traces. But where do traces come from? What do they refer to? What transforms a material mark into a trace? Instead of repeating the game of invoking nature or else society as the ultimate points of reference, we ask: what is it that lies in between? Could it be that our conceptual distinctions themselves – the thing-in-itself and its image, nature and society, text and context, representation and referent – successfully prevent us from understanding what could be called, with Latour and even Hegel, the "work of mediation"?

It is only by comparison with other forms of production in art, literature, architecture, media, and music that the epistemic specificity of science-based representations, with their multiple forms of translation, can be delineated more clearly, provided there is any such specificity. For can we assume that epistemic traces, their cultural, technical, and social embodiment notwithstanding, display some characteristic irreducibility that distinguishes them from economic, musical, or other traces? If that is the case, what could serve as sound criteria of demarcation? Are the representational devices of the sciences manifesting a historical continuity, if not a general pattern of cognitive development? Are the procedures characterising the practice of research at the frontier between what is currently knowledge and what is not yet knowledge to be measured with the same yardstick as the consolidated results of this process that we dignify as objective, rational, and logically stringent? Even if we accept this latter distinction, there is no guarantee that these co-ordinates provide us with the means of assessing the scientific enterprise in its spectacular innovative efficacy. A comparative analysis of the modes of representation in different epochs of the history of science is as indispensable as is the comparison of science with other forms of cultural activity in this respect.

All scientific, and more broadly conceived, all artistic representation circles around rendering novelty possible, and yet maintaining former achievements (Kubler 1962). Take the example of visualisation in the sciences – there would be no pictures or photographs without an agreement that they, in one respect or another, represent an already identified object. While this claim is constantly renewed, everything else constantly changes. The material of which the pictures consist, their technical production, the "moral economy" that guides their use, and last but not least,

the objects themselves undergo profound alterations in the very process of representation. We do not intend to reduce these processes to technological determinism; we argue instead for looking at how the various elements of the space in which these representational novelties – our perpetually changing rendering of the world – take shape. In view of the 'death of reference' at the end of the twentieth century, models of knowledge spaces that remain rooted in isomorphic worlds and that do not account for the basic features of openness as well as recurrence remain of historical interest at best. The idea of 'one' science, as opposed to a fundamental plurality of the sciences, can be placed in the same category.<sup>19</sup> The sciences develop from different starting points, sometimes they bifurcate, and leave the traces of an unending work of disclosure. Even in a constructivist perspective, the problem of representation remains underdetermined if it is not understood as the problem of how novelty is produced. At this point, the sciences as well as the arts come into contact with the physico-chemical and biological phenomena of self-organisation – they all walk and work, to use the words of Friedrich Cramer, on the ridge between chaos and order. The trace *is* the ridge, the precarious borderline between mere turbulence on the one hand and frozen pattern on the other. The trace is the cutting edge where the acts of science and art eventuate, where novelty dawns. Such a 'dawning,' the sudden or almost imperceptible surfacing of contours, can be seen as the basic feature of representation.

In the end, there may be good reasons to question whether the concept of representation *bona fide* is still able to illuminate the spaces of knowledge at the very point of closure of an age that Michel Foucault has characterised as the age of the modern *episteme* of representation (Foucault 1973). In search of a framework that may help us to understand what happened in these spaces, the order of the day is to fathom the boundaries of the concept and to point to the connotations that accompany its stunning omnipresence. We presumably still lack a comparable and unifying perspective to understand the spaces of knowledge that are opened up today, to understand what happens in these spaces, to understand the forms of knowledge that come along with the virtual and the hyperreal, and to understand the precession of models and scenarios that define the real as the iterable. Whether the concepts we have brought into play will help us to foster such a perspective awaits further inquiry.

---

19 At present, there is a growing interest in the "disunity of science." Dupré 1993; Rosenberg 1996; Galison and Stump 1996.

## References

- Bachelard, Gaston (1934 [1968]). *Le Nouvel Esprit scientifique*. 10. Hg. Paris: Presses universitaires de France.
- Barnes, Barry, & David Edge (Hg.). (1982). *Science in context. Readings in the sociology of science*. Milton Keynes: Open University Press.
- Barnes, Barry, & Steven Shapin (Hg.). (1979). *Natural order: Historical studies of scientific culture*. London: Sage Publications.
- Bloor, David (1976). *Knowledge and social imagery*. Chicago: The University of Chicago Press.
- Buchwald, Jed. Z. (Hg.). (1995). *Scientific practice*. Chicago: The University of Chicago Press.
- Burian, Richard M. (1997). Exploratory experimentation and the role of histochemical techniques in the work of Jean Brachet, 1938-1952. *History and Philosophy of the Life Sciences* 19, 27-45.
- Canguilhem, Georges (1943 [1991]). *The normal and the pathological*. New York: Zone.
- Coleman, William, & Frederic Holmes (Hg.). (1988). *The investigative enterprise. Experimental physiology in nineteenth-century medicine*. Berkeley: University of California Press.
- Collins, Harry M. (1974). The TEA set: Tacit knowledge and scientific networks. *Science Studies* 4, 165-186.
- Collins, Harry, & Trevor Pinch (1982). *Frames of meaning: The social construction of extraordinary science*. London: Routledge & Kegan Paul.
- Collins, Harry, & Trevor Pinch (1993). *The Golem: What everyone should know about science*. Cambridge: Cambridge University Press.
- Cunningham, Andrew, & Perry Williams (Hg.). (1992). *The laboratory revolution in medicine*. Cambridge: Cambridge University Press.
- Daston, Lorraine, & Peter Galison (1992). The image of objectivity. *Representations* 40, 81-128.
- Dupré, John (1993). *The disorder of things: Metaphysical foundations of the disunity of science*. Cambridge, MA: Harvard University Press.
- Fehr, Johannes (1995). *Saussure: Zwischen Linguistik und Semiotik*. Preprint Nr. 23. Berlin: Max Planck Institute for the History of Science.
- Fleck, Ludwik (1935). *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. Basel: Schwabe & Co.
- Foucault, Michel (1973). *The order of things: An archeology of the human sciences*. New York: Vintage.
- Foucault, Michel (1978). *Dispositive der Macht*. Berlin: Merve.
- Galison, Peter (1988). History, philosophy, and the central metaphor. *Science in Context* 2, 197-212.
- Galison, Peter, & David J. Stump (Hg.) (1996). *The disunity of science: Boundaries, contexts, and power*. Stanford: Stanford University Press.
- Gooding, David, Trevor Pinch, & Simon Schaffer (Hg.) (1989). *The uses of experiment: Studies in the natural sciences*. Cambridge: Cambridge University Press.
- Hacking, Ian (1983). *Representing and intervening: Introductory topics in the philosophy of natural science*. Cambridge, MA: Cambridge University Press.
- Hagner, Michael (1993). Die elektrische Erregbarkeit des Gehirns: Zur Konjunktur eines Experiments. In Rheinberger & Hagner (Hg.), 97-115.

- Hagner, Michael (1997). Zwei Bemerkungen zum Begriff der Repräsentation in der Wissenschaftsgeschichte. In Rheinberger, Hagner, & Wahrig-Schmidt (Hg.), 339-355.
- Hagner, Michael, Hans-Jörg Rheinberger, & Bettina Wahrig-Schmidt (Hg.) (1994). *Objekte, Differenzen und Konjunkturen: Experimentalsysteme im historischen Kontext*. Berlin: Akademie Verlag.
- Hentschel, Klaus (1995). *Zum Zusammenspiel von Instrument, Experiment und Theorie am Beispiel der Rotverschiebung im Sonnenspektrum und verwandter spektraler Verschiebungseffekte von ca. 1880 bis etwa 1960*. Habilitationsschrift, Universität Hamburg.
- Holmes, Frederic L. (1992a). Do we understand historically how experimental knowledge is acquired? *History of Science* 30, 119-136.
- Holmes, Frederic L. (1992b). *Hans Krebs. Vol. I. The formation of a scientific life, 1900-1933*. New York: Oxford University Press.
- Holmes, Frederic L. (1993a). *Hans Krebs. Vol. II. Architect of intermediary metabolism*. New York: Oxford University Press.
- Holmes, Frederic L. (1993b). The old martyr of science: The frog in experimental physiology. *Journal of the History of Biology* 26, 311-328.
- Knorr-Cetina, Karin (1981). *The manufacture of knowledge: An essay on the constructivist and contextual nature of science*. Oxford: Pergamon Press.
- Kohler, Robert (1991). Systems of production. *Drosophila, Neurospora, and biochemical genetics. Historical Studies in Physical and Biological Sciences* 22, 87-130.
- Kohler, Robert (1994). *Lords of the fly. Drosophila genetics and the experimental life*. Chicago: Chicago University Press.
- Koyré, Alexandre (1968). *From the closed world to the infinite universe*. Baltimore: The Johns Hopkins University Press.
- Kubler, George (1962). *The shape of time: Remarks on the history of things*. New Haven: Yale University Press.
- Latour, Bruno (1987). *Science in action*. Cambridge, MA: Harvard University Press.
- Latour, Bruno (1988). *The pasteurization of France*. Cambridge, MA: Harvard University Press.
- Latour, Bruno (1990). The force and reason of experiment. In Le Grand (Hg.), 49-80.
- Latour, Bruno (1993). *We have never been modern*. Cambridge, MA: Harvard University Press.
- Latour, Bruno (1996a). Haben auch Objekte eine Geschichte? Ein Zusammentreffen von Pasteur und Whitehead in einem Milchsäurebad. In Latour 1996b, 87-112.
- Latour, Bruno (1996b). *Der Berliner Schlüssel. Erkundungen eines Liebhabers der Wissenschaften*. Berlin: Akademie Verlag.
- Latour, Bruno, & Steve Woolgar (1986). *Laboratory life: The construction of scientific facts*. Second edition, Princeton: Princeton University Press.
- Lederman, Muriel, & Richard M. Burian (Hg.). (1993). The right organism for the job. *Journal of the History of Biology* 26, 233-367.
- Le Grand, Homer (Hg.). (1990). *Experimental inquiries: Historical, philosophical and social studies of experimentation in science*. Dordrecht: Kluwer.
- Lepenies, Wolf (1977). *Soziologische Anthropologie*. Frankfurt/M., Berlin, Wien: Ullstein.
- Lovejoy, Arthur O. (1936). *The great chain of being*. Cambridge, MA: Harvard University Press.
- Luhmann, Niklas (1990). *Die Wissenschaft der Gesellschaft*. Frankfurt/M.: Suhrkamp.
- Lynch, Michael (1994). Representation is overrated: Some critical remarks about the use of the concept of representation in science studies. *Configurations* 2, 137-149.



- Lynch, Michael, & Steve Woolgar (1990). Introduction: Sociological orientations to representational practice in science. In Lynch & Woolgar (Hg.), 1-18.
- Lynch, Michael, & Steve Woolgar (Hg.). (1990). *Representation in scientific practice*. Cambridge, MA: MIT Press.
- Morange, Michel (1994). *Histoire de la biologie moléculaire*. Paris: Editions La Découverte.
- North, John D., & J. J. Roche (Hg.). (1985). *The light of nature*. Dordrecht: Nijhoff.
- Olby, Robert (1985). A retrospect on the historiography of the life sciences. In North & Roche (Hg.), 95-109.
- Pavel, T. (1988). *Le mirage linguistique: Essai sur la modernisation intellectuelle*. Paris: Seuil.
- Pickering, Andrew (Hg.) (1992). *Science as practice and culture*. Chicago: The University of Chicago Press.
- Pickering, Andrew (1995). *The mangle of practice: Time, agency, and science*. Chicago: The University of Chicago Press.
- Popper, Karl (1935 [1968]). *The logic of scientific discovery*. New York: Harper & Row.
- Rheinberger, Hans-Jörg (1992a). *Experiment, Differenz, Schrift: Zur Geschichte epistemischer Dinge*. Marburg: Basiliskenspresse.
- Rheinberger, Hans-Jörg (1992b). Experiment, difference, and writing. I. Tracing protein synthesis, and II. The laboratory production of transfer RNA. *Studies in History and Philosophy of Science* 23, 305-331, 389-422.
- Rheinberger, Hans-Jörg (1997). *Toward a history of epistemic things: Synthesizing proteins in the test tube*. Stanford: Stanford University Press.
- Rheinberger, Hans-Jörg, & Michael Hagner (eds). (1993). *Die Experimentalisierung des Lebens: Experimentalsysteme in den biologischen Wissenschaften 1850/1950*. Berlin: Akademie Verlag.
- Rheinberger, Hans-Jörg, Michael Hagner, & Bettina Wahrig-Schmidt (eds). (1997). *Räume des Wissens. Repräsentation, Codierung, Spur*. Berlin: Akademie Verlag.
- Rosenberg, Alexander (1996). *Instrumental biology; or: The disunity of science*. Chicago: University of Chicago Press.
- Rothschuh, Karl E. (1973). *Laudatio ranae exploratae*. *Sudhoffs Archiv* 57, 231-244.
- Rouse, Joseph (1993). What are cultural studies of scientific knowledge? *Configurations* 1, 1-22.
- Rouse, Joseph (1995). *Engaging science. How to understand its practices philosophically*. Ithaca: Cornell University Press.
- Saussure, Ferdinand de (1997). *Linguistik und Semiologie. Notizen aus dem Nachlaß*. Frankfurt/M.: Suhrkamp.
- Shapin, Steven, & Simon Schaffer (1985). *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life*. Princeton: Princeton University Press.
- Sibum, Heinz-Otto (1994). Working experiments: Bodies, machines and heat values. In Staley (Hg.), 29-56.
- Sibum, Heinz-Otto (1995). Reworking the mechanical value of heat: Instruments of precision and gestures of accuracy in early Victorian England. *Studies in the History and Philosophy of Science* 26, 73-106.
- Staley, Richard (Hg.). (1994). *The physics of empire*. Cambridge: Whipple Museum.
- Steinle, Friedrich (1995). Looking for a 'simple case': Faraday and electromagnetic rotation. *History of Science* 33, 179-202.
- Wittgenstein, Ludwig (1968). *Tractatus logico-philosophicus*. Frankfurt/M.: Suhrkamp.

Michael Heidelberger und Friedrich Steinle (Hg.)

# Experimental Essays – Versuche zum Experiment



Nomos Verlagsgesellschaft  
Baden-Baden

Die *Interdisziplinären Studien* stellen eine Auswahl der Arbeitsergebnisse des ZiF vor, die exemplarisch für seine Forschungen sind. Sie verfolgen grenzüberschreitende Themen in und zwischen den Geistes-, Sozial- und Naturwissenschaften sowohl im Hinblick auf die theoretischen Grundlagen als auch auf den Vergleich empirischer Befunde aus verschiedenen Disziplinen.

Die Deutsche Bibliothek – CIP-Einheitsaufnahme

**Experimental essays** – Versuche zum Experiment / Michael Heidelberger/Friedrich Steinle (Hg.). – 1. Aufl. – Baden-Baden : Nomos Verl.-Ges., 1998  
(Interdisziplinäre Studien / ZIF ; Bd. 3)  
ISBN 3-7890-5367-8

Gedruckt mit Unterstützung der Universität Bielefeld.

1. Auflage 1998

© Nomos Verlagsgesellschaft, Baden-Baden 1998. Printed in Germany. Alle Rechte, auch die des Nachdrucks von Auszügen, der photomechanischen Wiedergabe und der Übersetzung, vorbehalten. Gedruckt auf alterungsbeständigem Papier.

This work is subject to copyright. All rights are reserved, whether the whole or part of the material is concerned, specifically those of translation, reprinting, re-use of illustrations, broadcasting, reproduction by photocopying machine or similar means, and storage in data banks. Under § 54 of the German Copyright Law where copies are made for other than private use a fee is payable to »Verwertungsgesellschaft Wort«, Munich.