# CHAPTER 11

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# PROLEPSIS

## Considerations for Histories of Science After 2000

Anticipation, presumption, and promise are what the term prolepsis conjures up. There is, however, no anticipation without retrospection, no presumption without prior consumption, and no promise without stake. And just as there is generally more than one way of looking at what happened in the past, proleptic excursions also contain an element of choice. Several grand perspectives for the historiography of science have followed each other in the past decades. In the 1960s and 1970s, it was fashionable to talk about revolutions. In the 1980s, revolutions were passé and 'turns' became the order of the day – thus we could have taken the so-called 'practical turn' as our point of departure. We would then have asked how far and where that turn has taken us since and what we might expect from it in the near future. In the 1990s it was fashionable to take issue with that 'relativistic turn' in philosophy and history of science which in the second half of the decade provoked a battle for truth, objectivity, and reason. This was a battle fiercely fought by those who saw the grand scientific project of the occident endangered, against those accused of abandoning these values altogether. We could have asked whether the ensuing 'science wars' did or did not bring about a climatic change in talking about science that will prove lasting and whether, as a consequence, the claims and terms of inter- and transdisciplinarity will be redefined. And indeed, we will come back to all these issues of the 1980s and the 1990s, but we will tackle them from a different perspective.

### STRUCTURALISM VERSUS CONSTRUCTIVISM

Structuralism and constructivism have become key labels for a number of intellectual movements and developments in the humanities and social sciences in the last fifty years. There is perhaps no general agreement about the meaning of the terms structure and construction and some might prefer to use the notions only in the plural. But there is certainly little doubt that the two concepts have been regarded as more or less incompatible terms of opposition. When Pierre Bourdieu characterized himself as a 'constructivist structuralist' some years ago, he could do so only by deliberately abstracting from



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the historical context in which the concepts had been shaped and had taken on radically different connotations. In an effort to explain this apparent paradox, Bourdieu defined structuralism as assuming 'objective structures' in the social world, objectivity meaning a certain independence from the consciousness and will of actors. These structures are assumed to function as possibility horizons but also as barriers, thus setting the margins for shaping practices and conceiving ideas. Constructivism, on the other hand, was defined by Bourdieu as emphasizing the 'social genesis' and thus a certain subjectivity and flexibility of such structures, patterns of perception, practice, and thinking. These structures and patterns are, in turn, taken to be responsible for the constitution of habitus, social relations, group identities, and social classes in particular (Bourdieu 1987, 147).

Bourdieu argued for a reciprocality between the two – and he referred to Hegelian categories when speaking of a 'dialectic relation' between structure and construction. The charming impudence of Bourdieu's approach lies in the fact that his use of both terms is decontextualized and ahistorical. When he talks about structuralism, he is far from positioning himself with respect to, say, structural linguistics in the tradition of Ferdinand de Saussure and the Prague circle or to structural anthropology in the tradition of Claude Lévi-Strauss, let alone Michel Foucault's structural discourse analysis. When he talks about constructivism, he certainly does not have in mind things like radical constructivism à la Heinz von Förster, Ernst Glasersfeld, and Humberto Maturana, or the social constructivism of the Edinburgh school. Rather, and generally speaking, it is Bourdieu's aim to find a way out of the dichotomy between a philosophy *without* subjects and a philosophy *of* the subject.

This is not the place to answer the question of whether Bourdieu succeeded in offering the clues to a deeper understanding of the intricate relation between facts and acts, between objective structures and subjective constructs in society. Nevertheless, it is heuristically tempting to play around with a decontextualization of the sort just mentioned, because doing so might shed light on some methodological and historiographical conundrums in recent history of science. We do not pretend to do full justice to Bourdieu's intentions, but rather take the liberty of decontextualizing Bourdieu himself. Starting from his paradox allows us to draw together various things which are usually kept in different boxes. For example, different brands of social historians, marxists, and discourse analysts may have taken issue with one another in many respects, but they have one thing in common: They tend to avoid basing their accounts on a philosophy of the subject and argue instead that agency, creative dimensions and human actions in general are epiphenomena of social and economic structures, of discourses or even of self-organizing machines; this can justifiably be called structuralism. Against this tendency, there is a movement – by no means a homogeneous one – arguing for the irreducible importance of human agency in circumscribed and local contexts. Many positions in cultural history, those of *histoire des mentalités*, *microstoria*, and *Alltagsgeschichte*, can conveniently be mapped along the line of human agency: constructivism. Social versus cultural, macroscopic overviews versus microscopic insights, structures and laws versus contingencies – these are the frontlines in an ongoing debate in contemporary historiography (Hardtwig and Wehler 1996; Evans 1997; Chartier 1998; Wehler 1998; Daniel 2001).[1]

In history of science, these polarities have only recently become important. For a long time, the main categorizations were quite different. First of all, there was the classic divide between an internalist and an externalist history of science. These two accounts were in perfect harmony, and they practiced a clear-cut division of labour. For the externalist, political, economic and ethical frames constituted the guiding principles and limitations for scientific activity; for the internalist, the lawfulness of nature itself and the universal and historically invariant categories of objectivity, rationality, and truth were seen as the driving forces of scientific progress. This divide, however, is not matched by the opposition between structuralism and constructivism. Yet, both views shared a reliance upon a deliberately de-individualized understanding of scientific activity.



Besides these two accounts, there was yet another, very different approach that had its origins in the heroic understanding of science. One might, with Nietzsche, call it the 'monumental,' or memorial, history of science. This attitude was often promoted by scientists themselves, who incorporated categories like individual mental power, genius, creativity, and the relationship between the teacher and his pupils in their accounts. Wilhelm Ostwald's early-twentieth-century project on 'Great Men' of science is a telling example of this kind of history (Ostwald 1910). Hard-boiled structuralist philosophers or sociologists of science would at best admit that such aspects may, if at all, complete the picture of the scientific enterprise.

The cold world of rational structures and systems and the passionate world of freedom, playful creativity and power-play, anxiety and joy – the science of the day and the science of the night, in François Jacob's words (Jacob 1987; Jacob 1997): one would expect these two extremes to stand in staunch opposition, permanent debate, and competition. Quite surprisingly, that is not the case. This is due to the fact that the two approaches serve different purposes and are often directed toward different audiences and identified with different genres of writing about science. Rationality and objectivity occupy center stage in metascientific reflection, in the hands and minds either of professional philosophers of science, or of scientists themselves as self-made, spontaneous philosophers. The personal dimension, on the other hand, is rarely found in textbooks on philosophy of science or in the public pronouncements of scientists. Here it is the 'context of justification' that counts. The 'context of discovery,' in contrast, is more likely to be cultivated in personal genres of scientific prose such as (auto)biographies, obituaries, or popular histories. This division of labour has effectively precluded the question as to whether and how far these two aspects of scientific activity might complement each other, or, more radically, the extent to which the division might be seen as a contingent historical phenomenon. Are we confronted with two incommensurable conceptions, or is there a way to set aside the categorial differences between structural necessity and contingent construction, the objective and the subjective? What might it mean to be an intelligent 'constructivist structuralist?'

Bourdieu's answer to this question is to be found in his *Méditations pascaliennes*, where he talks about the double face of scientific reason:

> A realistic vision of history forbids a fictive transgression of the intranscendable limits of that history and prompts to examine how, and under what historical conditions, truths can be squeezed out of history which are not reducible to that history. One has to admit that reason did not fall from heaven like a mysterious gift destined to remain inexplicable, and that it is, therefore, historical through and through. But one is not at all forced to conclude, as is usually done, that it is reducible to history. It is in history, and in history alone, that one must look for the principle of the relative independence of reason from history whose product it

> nevertheless is; or more precisely, one has to look at the genuine, but very specific, historical logic according to which those universes of exception have been instituted in which the singular history of reason accomplishes itself (Bourdieu 1997, 130–1).[2]

It is only recently that history of science has even begun to take notice of the historicity of categorial choices such as rationality and objectivity. Historians of science have started to examine the circumstances in which these choices were shaped, reshaped, and gained their historically changing meanings (Daston 1992; 2001; Daston and Galison 1992). In his classic book on the history of the life sciences in eighteenth century France, Jacques Roger did not yet feel compelled, as we do today, to justify a statement such as: "Scientific truth itself is a product of history" (Roger 1993, xxvi). For today's new, self-empowered defenders of scientific objectivity, this statement would certainly count as an example of the fiercely combated "flight from reason" (Gross, Levitt and Lewis 1996; Sokal and Bricmont 1997). To this end, the grail watchers of science have constructed one Enemy to the scientific enterprise, an enemy intent on deconstructing the triple myth inherited from the nineteenth century: that of – again in the words of Roger – the one Scientist, the one Science, and the one Nature. It is an act of intellectual honesty rather than of revolutionary radicalism to maintain that a historicizing analysis of the role of the scientist, of basic epistemological categories, and even of the meaning of science itself is much more an attempt to liberate the concept of reason from its self-chosen chains rather than a flight from reason. We want to remain interested in how these categories become historically embodied in different scientific practices and how they are shaped by the various materialities in which they are embedded. This is by no means tantamount to claiming that these categories are mere epiphenomena of the deterministic power of socially constructed hardware (whether economic conditions, technological systems, or media). We will return to this point.

## PHYSIOGNOMICS OF EXPERIMENTATION

Over the past ten years, our efforts have focused on the role of scientific experiment and experience, of epistemic objects and of spaces of representation in the historical development of the sciences. One of the guiding ideas was that analyzing the historical forms and configurations of experimental systems may provide important clues to an understanding of the dynamics of modern science. We envisaged that such a bottom-up analysis would serve as an alternative to top-down approaches, generally in the structuralist tradition, be they framed in terms of a sociology of disciplines or a philosophy of scientific reason. Instead, we chose to examine the materials and material forms in which scientific activity is enacted and through which it represents its results. In history of science, the exploration of certain objects and phenomena has for a long time been described as a process revolving around core concepts assumed to be historically invariant. Although this does not mean that these concepts were supposed to have been present throughout history, it was taken for granted that once they had been *discovered* or had proven to be successful, they could be regarded as integral parts of the science at issue. In the life sciences, concepts such as organism, species, race, gene, or neuron have been placed in this category. Undoubtedly, they were of tremendous importance for nineteenth and twentieth-century life sciences. Yet none of them had any significance for dealing with the world of living beings in the early modern period. And we do not have



an answer to the question with which François Jacob ends his account on the history of heredity:

> Today the world is messages, codes and information. Tomorrow what analysis will break down our objects to reconstitute them in a new space? What new Russian doll will emerge? (Jacob 1973, 324).

Since the seventeenth century, the successive scientific accounts of living beings have gone from a classificatory *historia naturalis*, an anatomy of visible structures, and a physiology of obvious body functions to the multidisciplinary investigative enterprises of the late nineteenth and early twentieth century, with their research objects located on multiple levels.[3] The dimensions of these objects reach from biodiversity, through the social behavior of organisms, right down to macromolecules. To each of these levels of organization and investigation there corresponds a phenomenology, along with a set of analytical approaches and of ways in which the objects are determined. In a history of science from the top down, one has become accustomed to seeing these developments as a social process in which scientific disciplines become more and more differentiated, or as a succession – evolutionary or revolutionary – of theoretical paradigms in a series of ever more refined representations of nature. Both accounts – history of disciplines, history of ideas – are typically structuralist: They identify and lay bare (in the anatomical sense) key elements and structures with an ultimate appeal to universal recognition. In this view, disciplines develop according to a general pattern of differentiation, although they are a relatively modern phenomenon starting only in the early nineteenth century; and ideas develop according to an infinite approximation toward truth/reality.

While the exclusive interest in ideas always risks degenerating into a matter of academic genealogy that searches for a true origin, disciplines are certainly important categories to understand professional developments and social and economic structures of modern science. The disciplinary framework allows particular actors or groups of actors to raise an authoritative voice. It seems, however, that disciplinarists have a tendency to lose touch with scientific contents. In trying to understand the epistemic activity of science, we find it inevitable to ask how research fields, and ultimately disciplines, aggregate around ensembles of practices and technologies, including methods, concepts, and theoretical conjectures. Such aggregations appear to be socially as well as conceptually underdetermined. If, on the one hand, they lead to relatively stable configurations of epistemic power and explanatory relevance, these configurations are, on the other hand, as a rule also historically transient and prone to disaggregation. It is probably no coincidence that we take our bottom-up approach at a time when a thoroughly molecularized and technologized biology is in the process of subverting and blurring all the disciplinary boundaries that, for the short hundred years between 1859 and 1953, between Darwin's *Origin of Species* and Watson and Crick's DNA model, enabled a friendly coexistence between academic specialists in the diverse field of the life sciences under the roof of an overarching evolutionary perspective.

If a pragmatogonic view of the history of the life sciences is taken seriously, historical assessment cannot begin with the reconstruction of a perceived immanent logic of the development of biological concepts. Rather, it will start from a reconstruction of the practical contexts in which particular concepts and associated theories gained momentum and eventually came to dominate. Here, the design of experimental arrangements for the generation of concepts, generalizations, and explanatory patterns in the modern biological sciences becomes crucial. Over the past ten years, we have assembled a number of studies focusing on the role of experiment in the biomedical sciences (Rheinberger and Hagner 1993; Hagner, Rheinberger and Wahrig-Schmidt 1994; Rheinberger, Hagner and Wahrig-Schmidt 1997). Our case studies of experimental systems have always been cautious about the question of when these systems historically came into being and whether they are of comparable importance in other areas of science (Rheinberger 1997). Whatever the future answers to these questions may be, it remains one of the big tasks for history of science to provide a comprehensive analysis of how experimentation itself became a – historically – variable scientific form of life that has undergone decisive shifts of structure and content from the early modern period to the present. These shifting and sifting boundaries reveal themselves best through broadly conceived comparative examinations, which eventually may lead to a classification of the historical settings, structures and meanings of experiment.

As early as in the seventeenth century, natural philosophers can be said to have 'experimented' with living things. Concerning England, Steven Shapin has argued for a sharp distinction between demonstration experiments, displayed for others and characterized by being successful, and more private trials, in which the outcome remained unclear (Shapin 1988, 400–3). In a more diachronic view, the demonstrative experiment of the seventeenth century differs starkly from the experiment as a more or less systematic extension of the art of observation in eighteenth-century French life sciences (Roger 1993). In contrast, in early nineteenth-century Germany, particularly in what one might call Goethean science – though not necessarily that practiced by Goethe himself, who saw the experiment as a 'mediator' of object and subject (Goethe 1962, 305–15) – observation and experiment were regarded as being radically opposed. The former was viewed as the natural attitude of the scientist, the latter as artificial and at least suspect, if not fallacious. This opposition included ethical and aesthetic aspects which formed an integral part of the question of how properly to investigate nature. Criticism against useless, superfluous and badly organized experiments began at the same time, and has developed its own history to the present.

Later in the nineteenth century, in many areas observation itself became dependent on experimental exploration. In the course of these changes, the practice of experimentation took on different forms and became greatly diversified. Concomitantly with this diversification, methodological reflexivity grew. A brief example from mid nineteenth-century experimental physiology may illustrate the point. In his private notebooks, Claude Bernard repeatedly thematized the difference between his approach and the *école allemande*:

> Science is enriched in two ways: either by adding new facts or by simplifying what already exists. [...] I have been reported as finding what I did not look for, whereas Helmholtz only finds what he is after (Bernard 1965, 145).[4]

Experimentation is offered here as a systematic mode of verification and refinement within a certain horizon of knowledge on the one hand, and on the other as an inventive practice in which the unexpected and the unforeseeable is brought into existence. Bernard's plea for the role of contingency and surprise might be read in the light of another



note, in which he criticizes the practice of excessive quantitative measurements in physiology:

> Ludwig, du Bois-Reymond, etc., are wrong because they believe in certain precisions they cannot attain (Bernard 1965, 174).

In contrast to the German physiologists mentioned, Bernard was convinced that physiology was not merely applied physics or chemistry (Bernard 1965, 188; Mendelsohn 1965; Coleman 1985). However, this does not explain sufficiently Bernard's own views on his strategy of experimentation. This caveat does not amount to saying that experimental practice is entirely disconnected from metascientific statements, but that their relation has to be investigated – instead of assuming that experimental practice is the direct result of a metascientific, that is conceptual, methodological, epistemological or cultural, position. There is no doubt that a number of mid nineteenth-century physiologists discovered precision as a value and attempted to organize their experimental research around this value, among others.[5] Nevertheless, Bernard expressed strong reservations against precision and quantitative measurement in general as a value. How should we historically interpret such an account?

Even if Bernard did not write his notes for the public, we should not take his statements as an immediate reflection of the experimental process itself. It is quite likely that the passages quoted above do not have an immediate addressee, and that they are nurtured by simplifying dichotomies such as German-French rivalry, competing schools, etc., which were part of the physiological reality of the time. It is thus imperative to compare these confessions with published statements as well as with published and unpublished documents that allow some insight into the experimental work and its conduct itself.

More generally speaking, these rudimentary remarks call for something that could be called a historical physiognomics of experimentation. A comparative study of the forms of experimentation is part of a comprehensive historical epistemology of the sciences and their culture. It is a truism that the epistemological and cultural gaze on experimentation leads far beyond the narrow focus of a philosophy of science in the hypothetico-deductive and in the later analytical tradition that have accorded to the experiment the mere, trivial function of testing hypotheses. But various attempts over the last fifteen years to open 'Pandora's box' will only be successful if the full assembly of new methodologies is mobilized (Gilbert and Mulkay 1984). This includes the comparative analysis of different genres of scientific writing such as research notes, lectures, and published texts of various formats such as handbooks, textbooks, and research articles. It will also require a semiotic approach to visualizations from unpublished sketches or drawings to copperplates, photographs, recordings of traces and inscriptions of various sorts. If these are the media within which the scientific discourse organizes itself, the study of the macro-material culture will address historical laboratory structures including their architecture, the technological functioning of laboratories, the role of laboratory objects, their impact on structuring behavior, and the trajectories of instruments.

## EPISTEMIC OBJECTS

In calling for comprehensive comparative studies, one must not overlook the danger of all classificatory enterprises: becoming an endless and fruitless collection of cases. Historical studies of the laboratory do not necessarily have to remain in the realm of microstudies that speak only for themselves. On the contrary, the focus on the materialities of science, on objects and phenomena, opens the perspective for long-range macrostudies of experimentation. A decisive aspect of scientific innovation lies in choosing, shaping, reshaping, and sometimes in abandoning such objects or phenomena. Studying the history of these entities presupposes that they take on variable forms within the different research contexts where they acquire their epistemic virtues and powers. Embryos, brains, mice, or bacteria, for example, are not epistemic objects by themselves; they are things which become epistemically laden insofar as they locate monsters, memory, oncogenes, or messengers in an ensemble of practical and theoretical relations. In contrast to an abstract analysis of pure concepts, we propose an analysis of these impure epistemic entities in their impure settings, half-way between crude pieces of matter and rarefied ideal types: material enough to exert the characteristic resistance and resilience encountered in the exercise of science, and symbolic enough to serve the vested interests of a quest for knowing.

One example of the historical complexity of an object is the transformation in brain research from the 'organ of the soul' to the 'brain' in the years around 1800 (Hagner 1997a). In the early modern period, the organ of the soul played the role of a material medium between the body and the soul. It provided an understanding of man which combined metaphysical and religious aspects with medical and natural historical approaches. The reconceptualization of the brain as an organ in which various mental qualities resided in proximity with each other marked a profound epistemic rupture. Before, there existed a categorical difference between the body and the soul, and between reason and the passions. This accorded with an enlightened, yet strictly hierarchical, view of man. The new development led to brain research as a forceful field within the sciences of man. The previous radical difference now became localized in the brain itself. The brain came to represent an epistemic field of combat for various attributions and oppositions that were fundamental and foundational for the coordinate system of modernity – man/woman, madness/genius, wildness/civilization, and sense/sensibility.

In the life sciences, the choice of organism has always played and continues to play a decisive role in the process of research (Clarke and Fujimura 1992; Lederman and Burian 1993; Kohler 1994). Since the end of the eighteenth century, the meaning of key biological concepts has been shaped by the availability of particular model organisms. Late eighteenth-century vitalism, for example, was by no means a purely speculative endeavor; it is unthinkable without the frog as a model and emblem of bioelectricity. The same animal, however, became crucial in other historical contexts in a quite different manner. It was instrumental in the establishment of physiology as a discipline in Germany around 1830; two decades later, it became the material carrier for the success of the Berlin organic physicists; at the turn of the nineteenth to the twentieth century, it helped to establish *Entwicklungsmechanik*; and it became one of the first higher organisms to enter the realm of molecular biology in the second half of the twentieth century (Rostand 1964a; Rothschuh 1969; Rothschuh 1973; Pera 1991; Holmes 1993).



The eighteenth-century conception of epigenesis accreted around the freshwater polyp *Hydra viridissima* as a model of organic regeneration and, thus, of the vital force of *Bildungstrieb*. We can hardly imagine today the repercussions that were provoked throughout the scientific community by this little animal which Abraham Trembley had retrieved from the ponds of Count Bentinck near Den Haag. For example, the annalist of the Académie Royale des Sciences exclaims in 1741:

> The story of the Phoenix who is reborn from its ashes, fabulous as it is, offers nothing more marvellous than the discovery of which we are going to speak. The chimerical ideas of Palingenesis or regeneration of Plants & Animals, which some Alchemists believed possible by the bringing together and the reunion of their essential parts, only leads to restoring a Plant or an Animal after its destruction; the serpent cut in half, & which is said to be rejoined, gives but one & the same serpent; but here is Nature which goes farther than our chimeras. From one piece of the same animal cut in 2, 3, 4, 10, 20, 30, 40 pieces, & so to speak, chopped up, there are reborn as many complete animals similar to the first (Dawson 1987, 7).

Perhaps to a lesser extent, but significantly nonetheless, the emerging discourse of epigenesis accreted around the incommensurability between the existence of monsters and the doctrine of preexistence. Pierre Louis Moreau de Maupertuis, for example, hinged his arguments against preexistence on this point when he stated:

> Certain monstrosities, either by excess or by default, perpetuate themselves ordinarily from one generation to the other, and through several generations. [...] This phenomenon [...] is inexplicable from the point of view of today's systems of generation [...] supposing that the child is completely prefigured either in the father or in the egg of the mother before the copulation of the two sexes (Maupertuis 1965, 159-60).

Nowhere, however, were monsters used more excessively as an epistemic "instrument of science" (Canguilhem 1975, 179)[6] than in the epigenetic approach of Caspar Friedrich Wolff. He worked for almost twenty years on an encompassing theory of monsters with which he closed his scientific career in St. Petersburg (Hagner 1999). A few decades later, Etienne Geoffroy Saint-Hilaire inserted teratology into the experimental landscape of the early nineteenth century (Rostand 1964b).

The history of genetics, to take another example, could almost in its entirety be written as a succession of model organisms. It started from eighteenth-century observations on human pedigrees of deviant characteristics such as surnumerary fingers. Peas and other plants such as tobacco became favorite objects of breeding experiments as well as for horticulture in the later eighteenth and the nineteenth century. The fruit fly *Drosophila melanogaster* played a crucial role in the establishment of classical formal genetics at the beginning of the twentieth century. The mold *Neurospora crassa* mediated the transition from formal to physiological or biochemical genetics. Without bacteria such as *Escherichia coli* and viruses such as *phage* and the tobacco mosaic virus, molecular biology would have been unthinkable. The molecularized developmental biology of today leads back to fish, mice, and humans. Each of these organisms, in its day, exhibited features that fitted with the means of manipulation available, and they usually resonated with broader concerns of the contemporary life sciences, nutrition regimes, and medicine.

To mention yet another perspective on model organisms, experimental medicine has been epitomized since its beginnings by a debate on what counts as a good animal model for studying human diseases and testing pharmacological substances. A closer analysis of such examples shows that the uses and scientific 'careers' of certain organisms are bounded by a complex set of conditions. These include technicalities such as utility, disposability, aptness for laboratory work, and ease of manipulation. However, they also extend to ethical and economic considerations; forms of everyday life; theoretical presuppositions; and, not least, contingency. These conditions may even include the natural and cultural histories of organisms, diseases, and entire environments. Cultural, social and epistemological aspects of the investigative enterprise are here tightly interwoven and should accordingly be studied within this texture, as Jean-Paul Gaudillière has shown for the example of mice as model organisms for breast cancer in the United States (Gaudillière 1999).

## SPACES OF KNOWLEDGE

A natural object or phenomenon such as an organism or an associated structure or function can become attractive for many reasons. Its epistemic relevance, however, depends on its fitting into historically specified real and symbolic spaces of knowledge acquisition. According to Michel de Certeau, space is a dynamic network of mobile elements. It depends upon different speeds and the variability of time.

> Space occurs as the effect produced by the operations that orient it, situate it, temporalize it, and make it function in a polyvalent unity of conflictual programs or contractual proximities (Certeau 1984, 117).

This is a rather unusual understanding of space. What Certeau has in mind is conceptualizing space as an 'anthropological space' as opposed to a 'geometric space.' Following Maurice Merleau-Ponty in this respect, Certeau understands anthropological spaces as being created by human experiences, perceptions, and practices. What can such a conceptualization mean for an understanding of spaces of knowledge like natural cabinets, botanical gardens, laboratories, the camp and field of the anthropologist or the traveling naturalist, or – today – the vast and ever growing spaces of simulation and virtual reality? At first glance, one might be tempted to say that, for example, a laboratory is a laboratory is a laboratory. It consists of stable structures serving the process of research including architecture, electricity, instruments, apparatuses, etc. But such an account says nothing about the intrinsic variability of laboratory equipment. Laboratories are as variable as the experiments performed in the confines of their walls. It is not sufficient to ask whether or not frogs, as objects of scientific inquiry, are proper model organisms. The question must also be posed how the spaces are shaped within which the objects are located and relocated, established and dismissed. In short, the creation of a space is linked to a history, and that space remains a space of knowledge production only as long as it is variable and has open boundaries. If this is no longer the case, the space degenerates to a junk room, filled with objects no longer needed for a specific research enterprise. Undoubtedly, the history of science is full of spaces like this, late witnesses of past successful endeavors or failures.

In contrast to these abandoned or even extinct spaces, there is another kind of space that is best described with Foucault's concept of *heterotopia*, which he contrasted to *utopia*. The latter term signifies 'unreal spaces,' related to the 'real space of society' by



'immediate or inverted analogy.' According to Foucault, utopias 'present society itself in a perfected form, or else society turned upside down.' Heterotopias are characterized as

> real places – places that do exist and that are formed in the very founding of society - which are something like counter-sites, a kind of effectively enacted utopia in which the real sites, all the other real sites that can be found within the culture, are simultaneously represented, contested and inverted. Places of this kind are outside of all places, even though it may be possible to indicate their location in reality. Because these places are absolutely different from all sites they reflect and speak about, I shall call them, by way of contrast to utopias, heterotopias (Foucault 1986, 24).[7]

The representational power of these spaces is enormous, although they do not necessarily invent, create, order or systematize the objects and phenomena they are filled with, independently and splendidly isolated from their entourage. Foucault illustrates the historicity of *heterotopia* with reference to libraries and museums. Until the late eighteenth century, both institutions were the 'expression of an individual choice.' Very much in accordance with his genealogy in *Les mots et les choses*, Foucault locates the rupture in the nineteenth century:

> By contrast, the idea of accumulating everything, of establishing a sort of general archive, the will to enclose all times, all epochs, all forms, all tastes, the idea of constituting a place of all times that is itself out of time and inaccessible to its ravages, the project of organizing in this way a sort of perpetual and indefinite accumulation of time in an immobile place, this whole idea belongs to our modernity (Foucault 1986, 26).

Of course, one could add here the archive, which in its modern form is also an invention of the nineteenth century, standing for virtual completeness and timelessness, maximized order and preservation (Nora 1984–1992). If one thinks of the many thousands of specimens in nineteenth-century pathological museums, the hundreds of monstrous deformations, or the thousands of ethnic skulls on the archival shelves of a Natural History Museum, the physiognomy of 'unlimited amassing' becomes plainly visible. Usually these spaces were not accessible to the public, but were reserved for the anatomist, the naturalist, or the anthropologist. There are, however, also examples of public exhibitions in late nineteenth-century science museums, in which the showcases and glass cabinets were overloaded with objects. The idea behind such amassments was the 'total impression' that was supposed to speak for itself in its overwhelming abundance.[8] Only a few years later, however, this type of exhibition began to be replaced by more didactically, structured and clearly delineated exhibitions, of which the Hygiene exhibition in Dresden in 1911 became an epitome (Offizieller Katalog 1911). The tendency of today's science museums to become places of entertainment and surprise events is only the most recent step in a long history of the visual disposition of scientific knowledge. The point to be made here is that different shelves, cabinets, visual arrangements and designs tell more about perceptions and ways of characterizing scientific objects than the objects themselves can display in their supposedly natural, naked and de-contextualized form.[9]

These few examples call for a more refined historicization of spaces of knowledge in general, and in particular an examination of the strategies of inclusion and exclusion associated with them. Following Foucault in this respect, one can regard the years around 1800 as a watershed, which is not to say that this divide is sufficient to grasp the full range of spaces of knowledge. Here Certeau's dynamic concept of space enters the stage again. Take the spaces opened up by experimental physiology in the second half of the nineteenth century: The first institute of physiology was built in Leipzig in 1869 and was headed by Carl Ludwig (Lenoir 1998, esp. chapter 5). What was special about this place? In an analysis of Emil du Bois-Reymond's physiological work in Berlin, Sven Dierig has argued that the space into which du Bois-Reymond's mode of doing science became inserted developed from a 'parlor physiology' in the 1840s to a 'workshop physiology' in the 1850s and 60s, and to large-scale 'industrial physiology' from the 1870s on (Dierig, forthcoming). This development was accompanied by the industrial and urban development in nineteenth-century Berlin, including economic and infrastructural systems such as water supply and electricity, as well as aspects related to education, art and mentality. Such detailed analyses will have to be carried out for other cities and disciplines, and for other protagonists and time periods. The historical development of the spaces of twentieth-century molecular biology, for example, may provide further insight into the dynamics of contemporary science and broaden the existing perspectives on experimental systems, epistemic objects, and the instruments and technologies that knot them into the fabric of experimental practice.

## MENTALITIES

Finally, to a particular material culture there corresponds the mental equipment of the scientist.[10] In fact, *the scientist* is itself a figure displaying historically contingent configurations of explicit and implicit skills, of bodily and mental discipline, and of gestural repertoires. As already mentioned, for a long period in the history of science the creativity and the ingenuity of the scientific genius were regarded as the personal, individually unique complement to structural rationality and objectivity, which were taken to be located beyond the scope of the scientist's personality. Apart from psychoanalytically motivated attempts to develop a psycho-history of scientific creativity, the personal factor was hardly ever a subject of closer historical examination. Only with the advent of the history of the body and the application of Foucault's concept of discipline to the scientist was the body also thematized in history of science (Kutschmann 1986; Feher et al. 1989; Lawrence and Shapin 1998).

These accounts mainly focus on the body's contribution in the engagement with science, whereas they give little credit to cognitive and psychological aspects. The predominance of the body seems to be an exaggerated reaction against the long philosophical tradition of separating mind and body and almost ignoring the latter, or else reducing it to a machine. The history of the body has corrected this image, but at times the focus on the body and on 'science incarnate' seems to dwell uncritically in these dualisms, as if they were primordial distinctions. Our guess is that the instantiation of the scientist's self in modern science does not primarily work along the lines of a mind-body dualism. It is rather the scientist's mind-body as a *mixtum compositum* that flexibly and variably becomes incorporated into and in turn embodies different scientific activities. Foucault's rigorous claim, that the mind is a mere result of the practices of body discipline, fails in explaining explorative activities, which do not follow strict rules and necessities. To be sure, there is no mental activity or expression without embodiment or in-corporation. This, however, does not merely mean discipline, control, and restriction, but also excess, transgression, and 'imagination', to take an expression used by the physiologist Johannes Müller (Müller 1834, 3–4).



Certeau proposed an alternative model in *The Practice of Everyday Life*, in which he conceived of the everyday behavior of consumers as an active mode of practice and not mere passive conformity. Although he accepted Foucault's notion of microphysics of power and tacit technologies behind the scenes, by which institutional procedures are shaped and regulated, Certeau observed that individuals and social groups can be quite successful in escaping these structures. What are the strategies and practices capable of building a counterweight to those silent procedures that organize the socio-political order? Transferring Certeau's account to scientists in action, one might arrive at the following scenario: Discipline does not make very much sense without subversion; rigid frameworks are not very effective without spontaneous bricolage, but they do prevent bricolage from leading to mere noise; practitioners mobilize artfulness, skill and imagination in order to redistribute the spaces in which they work. It is important to note that Certeau does not retreat into a traditional theory of the individual subject. Individuality is, rather,

> a locus in which an incoherent (and often contradictory) plurality of [...] relational determinations interact.

In order to examine this phenomenon more precisely, Certeau suggests examining

> readers' practices, practices related to urban spaces, utilizations of everyday rituals, re-uses and functions of the memory through the 'authorities' that make possible (or permit) everyday practices, etc. (Certeau 1984, xi, xv).

Certeau was interested in consumers, and thus did not turn his attention to mind-bodily practices occurring in the scientific arena. There is, however, no reason why these patterns of analysis should not be extended to the minds and bodies of scientific actors.

Before we give an example to illustrate what this could mean, we must consider a problem encountered by any cultural historian dealing with the composite mind-body (mentality, perception, ideas, imagination, experience, etc.) of an actor: the very explanatory power and reliability of these categories themselves. An influential attempt of highlighting individual forces is Michael Polanyi's category of 'tacit knowledge' or 'personal knowledge' (Polanyi 1958). More or less ignored in the 1960s and 70s, when structures, theories and paradigms were dominant, Polanyi's approach has recently been taken up again and has come to include craft and skill, competence and commitment, bodily discipline and gestures, perception and passion; in short, all aspects of scientific activity that resist explicit formalization. Though being mainly a challenge for traditional philosophy of science, Polanyi's account has also been criticized as inaccessible to historical analysis. In a paper on tacit knowledge and school formation, Kathryn Olesko critically remarked that Polanyi's concept of the inarticulable and untractable nature of tacit knowledge had serious historiographical consequences: If tacit knowledge was unspecifiable, then

> the actual mechanisms for acquiring skills and values central to the formation of a school fell outside the domain of direct historical investigation (Olesko 1993, 20).

In contrast to Olesko's, our problem is not so much whether or not scientists in their environment are to be perceived as conscious and self-reflexive actors; following de Certeau, we would rather contend that skills, gestures, preferences, perceptions, etc., can be historically analyzed independently of whether they are explicitly reflected by the historical actors or not.

We are aware, however, that such a claim bears risks and problems. In his study on the history and anthropology of sensory perception, Alain Corbin pointed out that historians often fall into the trap

> of confusing the reality of the employment of the senses and the picture of this employment decreed by observers.

For example, experts in nautical medicine noted about the sensory perception of sailors that their senses of smell, taste and touch, in particular, were dramatically reduced. Except with respect to vision, sailors had degenerated to 'senseless creatures.' Descriptions of this kind, Corbin argues, display the perspective of the authors and clichés about sailors and social hierarchies (Corbin 1995, 187). A similar problem arises in history of medicine when the world of patients is reconstructed through the world of doctors' records and hospital files.

In the case of the scientist, the historiographical situation is easier, since in most cases scientists do not report about other scientists; they report about themselves. Yet even here one has to be careful, because the documents in question – be they textbooks or public speeches, letters or diaries – may very well represent the rhetoric of received and canonized views instead of undistortedly documenting the uses and abuses of the mind-body. Historians are always to some extent imprisoned within the confines of written documents and the languages these documents speak. However, the point here is less to display a purified and undistorted self than to examine the interplay between the given material culture and the mental equipment. That includes a close look at the apparatuses, instruments, and locales which were part of the experimental activities. This is not to say that experiments in physiology, in particular, would have necessarily to be reworked as has effectively been shown to be possible in physics (Sibum 1995). For many of them, this is not feasible for ethical reasons, but at least in certain areas of self-experimentation such an approach could help to gain a closer understanding of the mastering of instruments in a confined experimental situation, as well as the training of the muscles, the senses, or other cognitive and/or bodily functions. Besides the senses, an analysis of the art of practice will have to encompass the ideas, conceptual tools, virtues, passions, mental dispositions, and idiosyncrasies of the researcher. All that belongs to their mental equipment which is characterized by an assembly of heterogeneous elements. Their configurations and reconfigurations range from the prominence of sensibility in late eighteenth-century life sciences and the role of heroism in romantic self-experimentation to attempts to either include or exclude the participating observer in different versions of twentieth-century anthropology.

One striking example is the history of vertigo and attention as an integral part of self-experimentation. Scientists' understanding of attention underwent fundamental alterations in the course of the eighteenth and nineteenth centuries. These transformations were linked to the role of the scientist as subject and as object in the self-experiment, as well as to the establishment of attention as a fundamental virtue of observation. The ambiguous and reflexive character of attention lay in the dual role which it came to assume as an experimental tool and object of investigation. Until the eighteenth century vertigo was regarded as a merely pathological phenomenon and thus a subject fit only for physicians. In the late eighteenth century, however, empirical psychologists (*Erfahrungsseelenkundler*) came to see vertigo as a disturbance of the regular activity of the mind and started to investigate it more closely. From this time on, the phenomena of vertigo and attention became polarized. While attention was defined as a state of orientation, vertigo was understood as its opposite – disorientation, distraction. As a consequence, the history of these two mental qualities became inseparably intertwined. Moreover, this story shows that the formation and the transformation of categories such as attention and vertigo played a crucial role in establishing the criteria of scientific experience (Hagner 1998, 2001).



## CONCLUSION

Looking back at the intellectual projects and projections of the twentieth century that have been put forward as ways of understanding the development of the sciences, it is apparent that dichotomies played a central role in most of them. These dichotomies include the difference between theory and practice, internal and external factors of scientific development, context of discovery and context of justification, basic and applied research, experiment and observation, representation and intervention, engineering and bricolage. We do not deny the historical importance of these categories, but we propose that they be treated deliberately as an *explanandum* and not as an *explanans*. Solutions for overcoming these dichotomies like the universalization of representation in the 1980s lead into a dead end, because ultimately an overly generalized notion of representation explains everything and nothing (Hagner 1997b; Lynch 1994). Bourdieu's solution, a constructivist structuralism, risks leading to a rather ahistorical Hegelianism that does not seem very promising for an understanding of the dynamics of historical processes. Bourdieu tackles the problem of dichotomies without seriously asking how they came into being and why they became so powerful. When we take up the oxymoronic motif of a constructivist structuralism, we do not attempt a neutralizing synthesis of approaches to the history of scientific cultures which have passed their climax and need not be rescued from oblivion. Instead, the conception of artful practice relying on difference, not contradiction, in combination with an encompassing notion of material culture relying on repetition, not identity (Deleuze 1994), can contribute to an understanding of the relation between scientific activity with its heterogeneous arrangement of spaces, objects, and mentalities, and more general lifeworldly and social ideas, practices, and values. Notions such as experimental systems, epistemic objects, spaces of knowledge, and mental equipment are tools to develop a framework for replacing the traditional dichotomies mentioned above. Such a pragmatogonic view aims to salvage what Jacques Derrida once declared to be the very point of the 'structuralist invasion':

> an adventure of vision, a conversion of the way of putting questions before us (Derrida 1978, 3).

And it aims to retain from the constructivist legacy that sense of an irreducible historicity which must accompany all our reflection about the processes of gaining knowledge in their multiplicity of expressions. There is certainly a danger that talking about historicity might become as well a truism as talking about representation, power networks, disciplines, etc. Not a single epistemological leitmotif in science studies is immune against fading away after a while. The traces that it might leave behind are certain unspectacular standards, beyond which no one might want to return. But at the same time, standards are not the driving forces for scientific activity. Science studies and history of science have pointed out again and again that the notions of progress and linear knowledge accumulation in the sciences are problematic. Why should we be surprised if that problem would turn out to be also one of our own field?

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## NOTES

1 The three books by Chartier, Evans and Wehler are subject to an illuminating interdisciplinary debate. See Kiesow and Simon (2000).

2 Translations are our own, if not mentioned otherwise.

3 A detailed and stimulating attempt to chart twentieth-century biological kinship categories is undertaken by Haraway (1997, 219–29).

4 See also a similar note on p. 128.

5 On the German precisionists see Holmes and Olesko (1994).

6 This term is taken from George Canguilhem, who has argued that monsters were used as an instrument in the debates between preformationists and epigeneticists.

7 See also the insightful collection by Smith and Agar (1998). In contrast to our perspective, however, Smith and Agar are mainly interested in the "relations of knowledge to authority, power and control." Emphasizing local cultural contexts, they are interested in "issues of scientific and technological authority, embodied spatially in sites" (Smith and Agar 1998, 3–4, 9). Without underestimating the importance of discursive power for an understanding of modern science, we do not think that it is possible to explain the specificity of the dynamics of science and the production of new knowledge from a historiographical perspective that mainly focuses on authority, power and control.

8 This strategy of display was developed in the museums of ethnology and anthropology in, for example, Berlin or Dresden. See Zimmerman (2001).

9 The state of the art and possible future of displaying the sciences in the museum is discussed in Lindquist (2000). On the historicity of cases in natural history cabinets see (te Heesen 2001).

10 The term 'mental equipment' plays an important role in the tradition of Annales-historians. It attempts to incorporate ideas and conceptual tools into historical analysis. See e.g. Febvre (1985), Le Goff (1988, 100).

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# SOCIAL STUDIES OF SCIENCE AND TECHNOLOGY: LOOKING BACK, AHEAD

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