Difference Machines: Time in Experimental Systems

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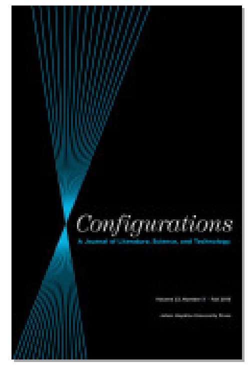

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Difference Machines: Time in Experimental Systems

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ABSTRACT: For a long time, *identity* and *contradiction* were the categories in which historical trajectories were conceptualized. Following Gilles Deleuze, this essay uses the categories of *reproduction* and *difference* instead to convey an idea of how the sciences develop on the basis of experimentation—a development that does not rest on anticipation, as is usually thought, but reveals itself as a process "driven from behind," as Thomas Kuhn once put it. The essay exposes the temporal structure characteristic of experimental systems on the basis of an example from the recent history of the life sciences.

In his collection of *Essays on the Anthropology of Reason*, anthropologist of science Paul Rabinow takes up the concept of *assemblage* used by Gilles Deleuze in order to describe those special constellations in which novelty arises. A Deleuzean assemblage—*agencement* is the original French term[1]—is to be seen as a dynamic conglomeration of heterogeneous things: a style or culture that affects them, a territory that assembles them, and the potential for *de*-centration according to which eventually something unfolds that, following Rabinow, "emerges out of a lot of small decisions; decisions that, for sure, are all conditioned, but not completely predetermined."[2] Elsewhere, he goes on to say that "from time to time, new forms unfold that have

1. Gilles Deleuze and Félix Guattari, *Mille plateaux* [*A Thousand Plateaus*] (Paris: Editions de Minuit, 1980).

2. Paul Rabinow, *Anthropologie der Vernunft—Studien zu Wissenschaft und Lebensführung* [*Essays on the Anthropology of Reason*] (Frankfurt am Main: Suhrkamp, 2004), p. 63. Since the papers in this volume have been revised and the interviews not previously published, I quote from the German edition.

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something peculiar around them; something that elevates existing actors, things, and institutions into a new mode of existence, engages them in a new assemblage; an assemblage that lets things [not only appear in a different light, but] happen in a different way."[3]

Placed in the context of my own investigations, a translation could read as follows: *experimental systems* (a kind of assemblage specifically geared toward the production of new scientific knowledge) are at the core of the dynamics of our contemporary empirical sciences. Usually due to the concurrence of incremental decisions and not of major revisions to begin with, conjunctures happen from time to time (within the components of, as well as between such systems) that subsequently let things appear in a different light and happen in a different way in the future. Experimental systems did not always exist, certainly not in early modern science; they are the products of historically more recent developments, structures resulting from the experimental turn of the modern sciences and that have obviously proven useful for practicing the art of empirically exploring the unknown.

Deleuze, on whom Rabinow draws here, also provided the two decisive theoretical keywords in this context: *difference* and *repetition*. "Difference and repetition," we read in Deleuze's book from 1968 of the same title,

> have taken the place of the identical and the negative, of identity and contradiction. For difference implies the negative, and therefore leads to contradiction, to the extent only that its subordination under the identical is maintained. The primacy of identity, however conceived, defines the world of representation. But modern thought is born out of the failure of representation; out of the loss of identities and of the discovery of all those forces that act beneath the representation of the identical.[4]

Difference and repetition, difference and iteration—to put it from a Derridean perspective—are the very driving forces behind experimental systems. Seen in the light of such systems, the history of the sciences is no longer to be thought and conceived of as a history of controversies (contradiction) and their eventual solution (identity). What we are concerned with is rather a dynamic coexistence of experimental trajectories persisting over a shorter or longer period of time. Experimental systems have to be constantly reproduced, but in a differential and iterative manner, if they are to remain arrangements in which new knowledge is generated—knowledge that

3. Ibid., p. 115.

4. Gilles Deleuze, *Différence et répétition* [*Difference and Repetition*] (Paris: Presses Universitaires de France, 1968), p. 11.

lies beyond what one has been able to imagine and anticipate at a particular point in time. In this sense, they function, to use the words of molecular biologist Mahlon Hoagland, as "generators of surprise."[5] Difference and reproduction, difference and repetition, or difference and iteration, respectively, are the inseparable sides of the same coin. Their game governs the impediments, as well as the breakthroughs, in the course of a research process. In order to remain productive, experimental systems must be designed and conducted in such a way that the creation of differences becomes the reproductive driving force of the whole machinery: thus they are to be addressed as veritable *difference machines*.

Differential reproduction confers on experimental systems a particular kind of historicity, or temporality. Their inner workings can only be grasped if one considers the temporal dimension; they unfold, to use Ian Hacking's words, "a life of their own."[6] And with that, they also have a *time* of their own. They come into being, they iterate themselves, but they can also disappear again. Their time trajectory, however, does not point toward something, but rather away from the current state of the art. Historian of science Thomas Kuhn addressed this precise point when he said that research is "a process driven from behind."[7] It is not a teleological enterprise, as is often suggested; certainly, you can have a goal in mind, and as a rule one must if one carries out research, but the end result defies again and again our capacity to anticipate. Any outcome is thus never something one would have been able to approach straightforwardly.

It has become commonplace that the emergence of novelty in the modern sciences is inextricably intertwined with the experiment. But how can we capture what is actually going on there while, as art historian George Kubler once put it in his magisterial volume *The Shape of Time*, one stands before the "tunnels and shafts of earlier work,"[8] knows them alone, and questions what direction to dig further? There is a dictum of François Jacob's, the French molecular biologist of the Pasteur Institute, who, around 1960, through a particular experimental conjunction with his colleague Jacques Monod, opened the way toward understanding the basics of gene regulation,

5. Mahlon B. Hoagland, *Toward the Habit of Truth: A Life in Science* (New York: W. W. Norton, 1990), p. xvii.

6. Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983), p. 150.

7. Thomas S. Kuhn, *The Trouble with the Historical Philosophy of Science* (Cambridge, MA: Harvard University, 1992), p. 14.

8. George Kubler, *The Shape of Time: Remarks on the History of Things* (New Haven, CT: Yale University Press, 1962), p. 125.

and with the operon model laid the groundwork for today's molecular developmental biology. The dictum can be found in Jacob's autobiography *The Statue Within*; it is about choosing a particular "shaft" as it were: "In analyzing a problem, the biologist is constrained to focus on a fragment of reality, on a piece of the universe which he arbitrarily isolates to define certain of its parameters. In biology, any study thus begins with the choice of a 'system.' On this choice depend the experimenter's freedom to maneuver, the nature of the questions he is free to ask, and even, often, the type of answer he can obtain."[9]

Jacob wrote these lines with the life sciences in mind, but they apply to all the empirically founded sciences. In this passage, the emphasis lies on the restriction of the action range by the choice of a shaft; that is, on the necessity to concentrate on a segment of a process that in its totality is always much more complex. Experimentation requires such restriction, and it has been the decisive and irreplaceable motor of modern research. In the same vein, French epistemologist Gaston Bachelard, possibly the most important philosopher of science of the twentieth century, always emphasized that the fragmentation of the sciences (below the level of our traditional academic disciplines, both in the laboratory and the field) is not to be misunderstood as a fatal specialization and a threat to *Bildung*, but instead is to be viewed as the prerequisite for the extraordinary mobility of modern research. Everything thus depends on understanding not only the closing, but even more so the opening character of such restrictions. Novelty happens less in the heads of the scientists and more in the experimental systems they create, at the bench. Instead of minds, we have to pay due attention to the sophisticated material culture responsible for the directions and pathways that scientific thinking can take. Experimental systems are extremely tricky and thick arrangements, as it were. One can see them as spaces of emergence—cultures of "access to an emergence," in the words of Bachelard[10]—as structures created by research in order to let things materialize that are not otherwise able to manifest themselves (to become "thing-able") and therefore thinkable. They are like spider webs; something will be caught in them, but one does not exactly know what it will be nor when it will come. They are devices for the creation of unprecedented events. Jacob has uniquely spoken in this respect of "machines for making

9. François Jacob, *The Statue Within: An Autobiography* (New York: Basic Books, 1988), p. 234.

10. Gaston Bachelard, *Le rationalisme appliqué* (Paris: Presses Universitaires de France, 1949), p. 55.

the future" (*machines à fabriquer de l'avenir*),[11] and thus of difference machines.

With German philosopher Hans Blumenberg, we could also say that research represents the incarnation of what it means to act "at a spatial and temporal distance." In research, this means that one preferentially acts on "things that one does not perceive," as Blumenberg puts it in his theory of unconceptuality, insofar as the concepts involved in such action need to "possess enough indeterminacy in order to be able to capture experiences that are still to be made." The concept is thus "in need of a margin for all the concrete that is to be subjected to it."[12] Scientific concepts, at least insofar as they are relevant for research and thus for "making the future" (here, the concept of *gene* comes to mind for the life sciences of the twentieth century), are therefore usually not overly precise, and they must not be so. If, according to Blumenberg's philosophical anthropology, the animal trap as "reified expectation" is the "first triumph of the concept" in the history of mankind,[13] we could consequently say that experimental systems, as "knowledge traps," are one of the late triumphs of the scientific spirit in action.

Throughout the disciplines—the sciences, philosophy, sociology, anthropology, art history, and history of science—as we can judge from the previous and following quotes, research is thus seen as an iterative process of groping—literally, *re*-search—that operates on the border between the known and the unknown.[14] The basic problem lies in the fact that one does not precisely know what one *does not know*. What is at stake is the creation of new knowledge, and what is really new, is, by definition, unforeseeable. With the experiment, the researcher establishes an empirical structure, an environment that allows her or him to become capable of acting in this state of ignorance about knowing, and about one's awareness of not knowing. In a late paper, US sociologist of science Robert Merton (whose work is unduly neglected these days) regarded "specified ignorance" as a mark of science and pointed to the productive function, the positivity of ignorance in the research process.[15]

11. Jacob, *The Statue Within* (above, n. 9), p. 12.

12. Hans Blumenberg, *Theorie der Unbegrifflichkeit* [*The Theory of Unconceptuality*] (Frankfurt am Main: Suhrkamp, 2007), pp. 10–12.

13. Ibid., p. 14.

14. Hans-Jörg Rheinberger, "Nichtverstehen und Forschen," in *Kultur Nicht Verstehen* [*Culture Not Understanding*], ed. Juerg Albrecht, Jörg Huber, Kornelia Imesch, Karl Jost, and Philipp Stoellger (Zürich: Edition Voldemeer, 2005), pp. 75–81.

15. Robert K. Merton, "Three Fragments from a Sociologist's Notebooks: Establishing the Phenomenon, Specified Ignorance, and Strategic Research Materials," *Annual Review of Sociology* 13 (1987): 1–28.

We need to go a step further: what ultimately drives science is "*unspecified* ignorance"—ignorance at one remove. As nineteenth-century French physiologist Claude Bernard put it succinctly, "[i]t is the vague, the unknown that moves the world."[16]

We need to not forget, however, that in a developed experimental arrangement, a considerable quantum of knowledge is embodied that, at a particular point in time, counts as *established*. As a reproduction "conserve," we could say, it takes the shape of instruments, devices, and apparatus—all "reified theorems," in the words of Bachelard.[17] This stock of knowledge requires its own care: calibrating and testing apparatus may even take the better part of the working time of a scientific experimenter. That the machines perform their work as noiselessly as possible is a prerequisite for being able to focus on the epistemically "vague"; as a whole, the exploratory experiment has to be set up in such a way that things are made to happen that escape a prediction. Bernard once said that "one has claimed that I would find what I do not search for, whereas Helmholtz [note the sideswipe about his German colleague] only finds what he is looking for."[18] And then, the experimental spirit is constituted in a complementary fashion to the experimental structure. What comes to my mind here again and again is a bon mot that Boston's protein-synthesis researcher Paul Zamecnik—I will come back to his work in a moment—circulated at a symposium in the 1950s. "We would also like to investigate induced enzyme formation," he answered to a question posed by his colleague Sol Spiegelman, "but this reminds me of a story that Dr. Hotchkiss told me. There was a man who wanted to get a new boomerang. But he was unable to throw away his old one."[19] *Researcher* and *research object*, this means, enter into an intimate relationship with each other in the experiment. The better one knows one's object, the subtler it resists one's wishes; one has it in one's hands, but at the same time it escapes one's command.

The experiment is a search engine, as it were, but with a very curious temporal structure: it produces things about which one can only

16. Claude Bernard, *Philosophie* (Paris: Hatier, 1954), p. 26.

17. Gaston Bachelard, *Les intuitions atomistiques (Essai de classification)* [*The Atomistic Intuitions (an Attempt at Classification)*] (Paris: Boivin, 1933), p. 140.

18. Claude Bernard, *Cahier de notes, 1850–1860* [*Notebooks, 1850–1860*], ed. Mirko Drazen Grmek (Paris: Gallimard, 1965), p. 145. Feeling that he had gone too far, however, he immediately added: "This is true. But exclusiveness in either direction is not good."

19. Paul C. Zamecnik, E. B. Keller, J. W. Littlefield, M. B. Hoagland, and R. B. Loftfield, "Mechanism of Incorporation of Labeled Amino Acids into Protein," *Journal of Cellular and Comparative Physiology* 47, supp. 1 (1956): 81–101.

say afterwards that one should have been searching for them. In this respect, Bernard was completely right when he once categorically stated: "Knowledge is always something a posteriori."[20] In a very similar vein we read in Bachelard, "[r]eality is never 'what we might believe it to be': it is always what we ought to have thought. Empirical thought is clear only in retrospect, when the apparatus of reason has been developed."[21] *Recurrence* is the notion with which Bachelard addressed this peculiar form of historicity of scientific knowledge acquisition. While knowledge can become reified in an act of recurrence, there exists no set of rules capable of ensuring its production. And sooner or later the current state of knowledge, including the methods of its production, will be replaced. Science must permanently "risk itself in new acquisition"[22] so that "the history of science appears as the most irreversible of all histories."[23] Knowledge production always occurs in untidy times and places, in times of confusion, as well as stubborn, ready-made opinions, and in places where things are tried out. That is why there will always be a need for, to quote Bachelard once again, "epistemological acts . . . that generate unexpected impulses in the course of scientific development."[24] There is, however, no algorithm that would grant the occurrence of such acts. Unlike many of his contemporaries, Bachelard did not want to separate this space of untidiness from epistemology; rather, he proclaimed it epistemology's essence. It is here that the examination of experimental systems inserts itself. Such systems are the focus of an epistemology of the concrete; they indicate the points at which these "epistemological acts" with their "unexpected impulses" can happen.

I would like to briefly present, in order to be a bit more specific at one point, such an experimental trajectory in an exemplary fashion. It concerns the historical path of investigating the problem of how proteins are fabricated in the cell (fig. 1). However, I will not be able to go into the details of this image here.[25] If the image con-

20. Bernard, *Philosophie* (above, n. 16), p. 21.

21. Gaston Bachelard, *La formation de l'esprit scientifique* [*The Formation of the Scientific Spirit*] (Paris: Vrin, 1938), p. 13.

22. Gaston Bachelard, "Le problème philosophique des méthodes scientifiques" (1951), in *L'engagement rationaliste* [*The Rationalist Engagement*] (Paris: Presses Universitaires de France, 1972), pp. 35–44, quote on p. 39.

23. Gaston Bachelard, *L'activité rationaliste de la physique contemporaine* [*Rationalist Activity of Contemporary Physics*] (Paris: Universitaires de Paris, 1951), p. 25.

24. Ibid.

25. See Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Palo Alto, CA: Stanford University Press, 1997).

veys the impression of a maze, it also provides an intuitive idea of the options and the decisions that go along with the coming into being of, and the work with, such a system, but also the impasses that it can reach at any time. We see here how instruments, questions, and findings from within the experimental system entangle themselves with entries and grafts from outside. It began in 1945 (upper left) with a new combination of cancer research and in vitro cytology, which was conducted by the group of the aforementioned Zamecnik at Massachusetts General Hospital in Boston. In the early 1950s, it was transformed into a system in which a new form of biochemistry took shape, one that in essential points differed from classical enzyme chemistry as established during the first decades of the twentieth century. This new form of biochemistry was based on differential fractionation and the use of radioactive tracers; then, in the late 1950s, with the identification of transfer RNA, it became an integral component of the research core of molecular genetics. In the early 1960s (lower right), it led to the elucidation of the genetic code, one of the key events of the golden age of molecular biology.

Between these poles extends the trajectory of a single experimental system. The transition from viewing protein synthesis as an anabolic process (among others) to conceiving it as the translation of a genetic message via ordered peptide-bond formation guided by nucleic-acid sequences took shape in several steps within this system. Along with that, the general theoretical assumptions also changed greatly. If, in 1945, one had seen the synthesis of proteins as an inversion of their enzymatic cleavage—a logical consequence of enzyme chemistry—in 1960, it was described as a process of the transmission of genetic information: that is, within a completely different conceptual horizon. Space does not permit going into further details here, and it is also not necessary for making the argument. But at least it may be summarized that what we are concerned with here is a specific constellation of factors, an assemblage in the sense of Rabinow, in which a particular model organism (first the rat, then the bacterium *Escherichia coli*), a certain style—or better perhaps, a culture of experimentation (biological work in the test tube) and two new research instruments (the ultracentrifuge and the procedure of radioactive tracing) came together to make a complete series of unprecedented displacements possible. Such constellations, of course, can be of widely different natures and thus accordingly have to be analyzed in their proper details, but I think that the message of this case study is exemplary.

The succinct analysis of such experimental trajectories can teach us in particular that decisive displacements of knowledge assemblages

—as a rule, although mistakenly, called "discoveries"—never happen in the way in which they become represented in the public arena, be it in research publications or in retrospective accounts of the actors. The historian of science who is lucky enough to have recourse to preserved laboratory notes can have the repeated experience that the order of the so-called discovery and the order of representation in science play in two different registers. Karl Marx, as an aside, was one of the first to point to this discrepancy in his critique of political economy.[26] Consequently, one must not let oneself be deceived by the order of representation, with its presumed certainties and logical deployment of arguments.

To return to the introductory quote of Deleuze, what we have to pay attention to, then, are all those "forces that act beneath the representation of the identical." Jacob once spoke in this context of a difference between what he called "the day science" and "the night science":

> When you look more closely at "what scientists do," you might be surprised to find that research actually comprises both the so-called day science and night science. Day science calls into play arguments that mesh like gears, results that have the force of certainty. . . . Conscious of its progress, proud of its past and sure of its future, day science advances in light and glory. By contrast night science wanders blind. . . . Night science is a sort of workshop of the possible where what will become the building material of science is worked out.[27]

The twists that the difference machines of research can take are of a multiple, and often surprisingly mundane, nature. Technical accidents can bring phenomena to the fore that hitherto had not attracted attention. A mishap becomes a productive factor; control experiments can be turned into research experiments. In this case, an unquestioned assumption becomes a problem; it lies in the essence of a control to embody what one knows in compact form. Techniques being applied can have results other than those intended, which means that such techniques create an excess that goes beyond the anticipated effect. For example, components of a system regarded as contaminants can show themselves resistant against removal and thus become transformed from a disturbance into an object of investigation, as with the so-called soluble RNA

26. Karl Marx, *Das Kapital*: *Kritik der Politischen Ökonomie* [*Capital: Critique of Political Economy*] (Berlin: Dietz Verlag, 1972), p. 27.

27. François Jacob, *Of Flies, Mice, and Men* (Cambridge, MA: Harvard University Press, 1998), p. 126.

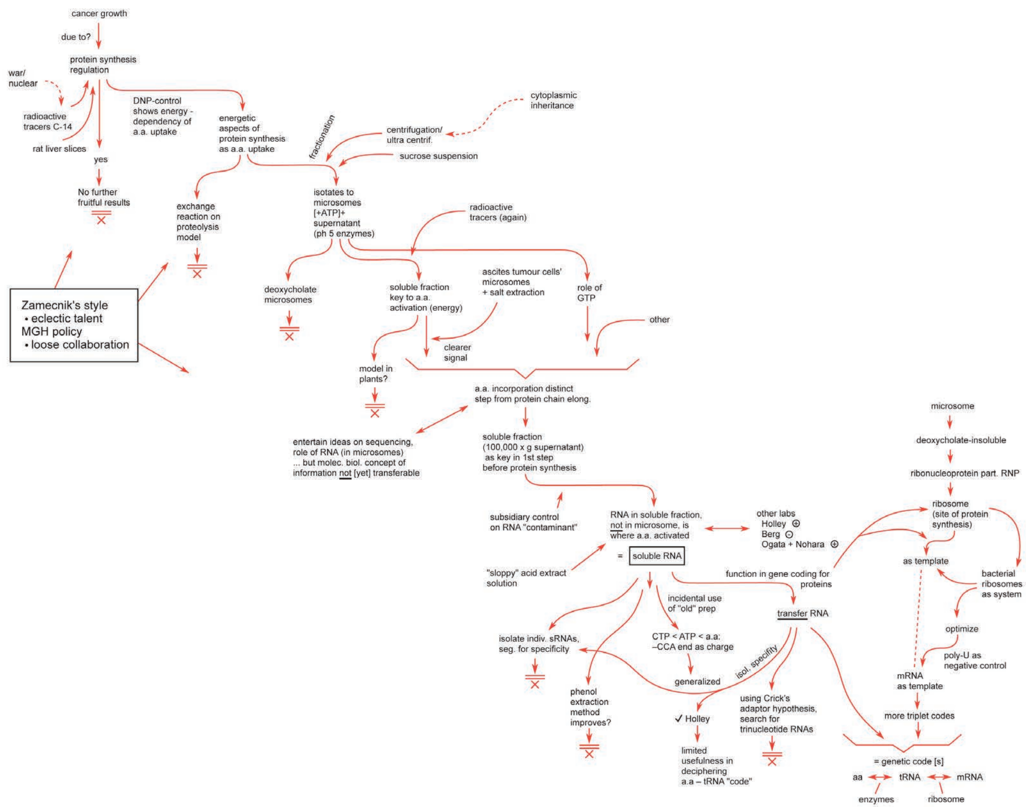

Figure 1. Protein synthesis research, 1945–1960. (Image courtesy of Douglas Allchin, reprinted with permission.)

in the protein-synthesis system described above. It mutated into transfer RNA—the adaptor molecule that subsequently proved to be the catalyst for deciphering the genetic code. And then there are the instances of the surprising incidental findings where everything depends on simply not overlooking them. As far as I know, a typology of unprecedented turns in the experiment has not yet been written, but along with Robert Root-Bernstein, one can safely say that in the case of science, "without experiments with serendipitous results soon all theorizing would come to a halt."[28] He summarizes that "science *is* change," in the sense of "actual, effective surprise,"[29] and refers to the conviction of philosopher of science Stephen Toulmin that *novelty* in science is often as unexpected and brought about as unintentionally as we know it from nature—that is, from evolution.[30]

One need not subscribe to this analogy to recognize that experimental systems can be regarded as the structures that make it possible for such turns in the knowledge-gaining process to happen; they are the structures that make it possible to handle hazards and chance events in a productive manner—perhaps even better, to make it possible that those kinds of chance events can happen that lend themselves to being handled in an epistemically productive manner. All science in the making—that is, at the border of the unknown—is dependent on them. Where one does no longer know, says Bernard in his *Cahier de notes*, there "one must find."[31] And at another point, he contends that "[o]ne certainly can claim that nobody has ever made a discovery by seeking it directly."[32] Ludwik Fleck once called this the "Columbus effect": one looks for India, and one finds America.[33] Experimental systems are the form in which the modern sciences have cast this indirect, tentative approach in shapes that are themselves historically changing. The turns that are relevant here cannot be brought about by force; in the end something imponderable remains, having less to do with the often-touted "flashes of illumination" than with the multiplicity of the elements that enter into

28. Robert Scott Root-Bernstein, *Discovering: Inventing and Solving Problems at the Frontiers of Scientific Knowledge* (Cambridge, MA: Harvard University Press, 1989), p. 365.

29. Ibid., p. 376.

30. Stephen Toulmin, *Foresight and Understanding: An Inquiry into the Aims of Science* (New York: Harper & Row, 1961).

31. Bernard, *Cahier de notes* (above, n. 18), p. 135.

32. Ibid., p. 149.

33. Ludwik Fleck, *The Genesis and Development of a Scientific Fact*. (1935; reprint, Chicago: University of Chicago Press, 1979), p. 69.

an experimental constellation and the spaces of displacement they create. They are of a material, as well as social nature, of a cultural as well as epistemic kind. No ideal type and no ideal mixture can be distinguished here. "Everybody follows his own path," to quote Bernard for a last time from one of his notes, "and I have rid myself from the rules by rendering myself between the disciplines, what others maybe might not have dared."[34] Particular disciplines find themselves historically in different stages of deployment, and certain research strategies will therefore prove more or less successful. Each and every experimental system is concrete in the last instance; in an elementary sense, bound to its time and place.

As a consequence, the forms that the relationship between difference and repetition—the "shapes of time" in Kubler's words—can take are as concrete as they are protean and therefore must be investigated in their characteristic multiplicity. However, to stay with an idea that Kubler has offered to us, these structures interact. With Kubler, we can see the vertical axis of the history of things not as a massive and homogeneous swelling or dwindling stream, but rather as composed of fibers, each endowed with its own temporality. "We can imagine the flow of time as assuming the shapes of fibrous bundles,"[35] he pointedly summarizes at the end of his book. Kubler uses the comparison as an image for the interaction of what he calls "series [of] prime objects" and their "mutants" in the history of art and architecture.[36] To take seriously his admonition for bringing the history of art and the history of science together under the heading of a "history of things," we can regard ensembles of experimental systems as instances of such "fibrous bundles" and see how they are held together. This would amount to, then, a history of material experimental cultures—and to another essay.[37]

Acknowledgement

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34. Bernard, *Cahier de notes* (above, n. 18), pp. 128–129.

35. Kubler, *The Shape of Time* (above, n. 8), p. 122.

36. Ibid., pp. 39–40.

37. Hans-Jörg Rheinberger, "Cultures of Experimentation," in *Culture Without Culturalism: Cultures and Styles of Scientific Practice*, ed. Karine Chemla and Evelyn Fox Keller, Durham and London: Duke University Press, in press.