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Experimental Systems

Historiality, Narration, and Deconstruction

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INTRODUCTION

The title avoids the notion of history, or historicity. Instead, it speaks of "historiality." Why this apparent game with words? Moreover, how to speak about an issue that needed a neologism to be approached and that, provisionally and some twenty-five years ago, accompanied a text that since then has become the *locus classicus* of "deconstruction"? In other words, is it possible to think history without "origins" and without "grounds"?[1] The historians of science, like the historians of any cultural texture, are confronted with an unsurmountable obstacle. What is it they are dealing with? Are they looking at a past that is the transformation of another, foregoing past; or are they looking at a past that is the product of a past deferred, if not of a presence?

Early in 1991 a group of historians of biology assembled at the Natural History Museum in what had been East Berlin less than two years before. A paleontologist who had spent his whole professional life ordering and reconstructing fossil material pointed at the famous Solnhofen Archaeopterix of the Berlin collection and summarized the experience of forty years of work with the following words: "At the point of the emergence of the new, the new is not the new. It becomes a novelty only by a transformation that makes it a trace of something to which it has given rise."[2] The new is nothing but an irritation at the point where it first appears: It can be approached only in the mode of a future perfect. Of course, we may try to indicate the conditions of its emergence. But they, and so the new, seem accessible only through and by way of a kind of recurrence[3] that requires the product in order to assess the conditions of its production. This movement, sometimes denounced as whiggish but more often misunderstood as a form of teleological projection, calls for a bending of thought that cannot be linearized. Attempts at such linearization constitute the classical illusion of the task of a historical narrative: to tell—whatever methodological refinements are added to the core of the argument—the story of what, *then*, really happened. This presupposes the existence of an undistorted past "out there" that, from a detached present "in here," can in principle be grasped by means of an analysis whose means are

supposed not to have been altered by what is going to be synthesized.[4] Moreover, it perpetuates the illusion that the task of the historian is to relate "real history" as opposed to just telling stories.[5]

In contrast, what might be called *historial* thinking not only has to accept and even postulate a kind of recurrence inherent in any hindsight—hence interpretation, or hermeneutic action—but also it has to assume that recurrence works in the differential activity of the *system* that is *itself* at stake, and in its time structure. What is called its history is "deferred" in a rather constitutive sense: The recent, so to speak, is the result of something that did not happen. And the past is the trace of something that will not have occurred. Such is the temporal structure of the production of a trace.[6]

In what follows I first offer some comments on the historial character of the production of scientific knowledge. I then discuss a specific episode in the history of virus research in order to provide an empirical background to the more abstract considerations of the first part.

THE HISTORIAL STRUCTURE OF SCIENTIFIC ACTION

Within the tradition of a general history of science, the view of science as a continuous, accumulative process has seriously and lastingly been challenged by the model of a series of more or less radical breaks.[7] However, both the revolutionary and the gradual conception of scientific change assume a global epistemic structure, called "science," that as a whole either continuously grows—toward truth—or is periodically reconstructed according to a new paradigm. Although there is a heavy dose of relativism in the second view, a paradigm, at a given time, is assumed to have enough power to coordinate and make coherent the activity of a whole—and potentially *the* whole—scientific community. But even in the denial of a continuum of rationality there remains an element of "totalization." Science remains a normative process encompassing the ensemble of participants and their practices in a common endeavor. And there remains the general view of an overarching chronological coherence to the process of gaining scientific knowledge.

This view becomes all the more problematic[8] the closer one looks at the microdynamics of scientific activity.[9] At the level of the basic, functional units of scientific activity—we may call them, as practitioners of science do, *experimental systems*[10]—the possibility arises of an assessment of the scientific research process that radically subverts its monolithic, macroscopic appearance. This is not to advocate the introduction of a distinction here that has pervaded modern twentieth-century physics: the distinction between the microscopic indeterminacy and macroscopic determinism of what we take to be natural phenomena. Nevertheless, I do not want to exclude a certain resonance with a conception of uncertainty and indeterminacy that has undermined the scientific self-perception of a whole epoch.

At issue is, in a certain sense, the *fragmentation* of science into systems and their corresponding times. There is a resonance here, too, with recent developments in the field of the thermodynamics of irreversible processes. New possibilities are arising for structuring what on a very abstract level might be called the locality of time. Ilya Prigogine has suggested defining time not simply as a parameter (the t of Newtonian-to-Einsteinian physics) but to introduce an "operational" time into the theory of irreversible processes—that is, to determine time as an operator

(T) (Prigogine and Stengers 1988). Viewed formally, an operator is a prescription to manipulate, i.e., to reproduce a function so that the function itself survives the operation but at the same time is changed by some factor or factors. What is of relevance for the present discussion is that, with respect to the movement of material systems, systems of things, or systems of actions, time as an operator is not simply an axis of extension but a structural, local characteristic of any system maintaining itself far from equilibrium.

Thus every system of material entities, and therefore every system of actions concerning such entities, that can be said to possess reproductive traits may also be said to possess its own intrinsic, or *internal time.* It is not simply a dimension of its existence in space and time. It characterizes a sequence of states of the system insofar as they can be considered to undergo a continuing cycle of nonidentical reproduction. Research systems, with which I am concerned here, are characterized by a kind of differential reproduction by which the generation of the unknown becomes the reproductive driving force of the whole machinery. As long as this works, the system so to speak remains "young." "Being young," then, is not here a result of being near zero on the time scale; it is a function (if you will) of the functioning of the system. The age of such a system is measured by its capacity to produce differences that count as unprecedented events and keep the machinery going.[11]

Now we can look at the activities in a particular research field as an assembly, or an ecological network, of experimental systems. Some of them are close enough so that their reproductive cycles are able to become operationally coupled by exchange, and some of them are far enough from one another to perform their operational transformations independently (which in itself is a matter of the actual transformations going on within the different systems). Thus we end up with a *field* of systems that has a very complex time structure, or form of time. The systems, or reproductive series, retain their own internal times as long as they replicate as such, and the epistemic field can no longer be seen as dominated by a general theme, or paradigm.[12] There is no global frame of theory or political power, or social context strong enough to pervade and coordinate this universe of merging or bifurcating systems. And where the systems do get linked, this is not by stable connections but rather by possibilities of contacts generated by the differential reproduction of the systems and the constellation of their ages. There is no common ground, source, or principle of development. The constellation of differently aged systems constitutes a particular field of the possible. In this field, attractors constantly shift; there is no longer a fixed center.

The multiplicity of internal times in an open horizon creates what can be called *historiality:* It escapes the classical notions of linear causation, retroaction, influence, and dominance, as well as that of a purely stochastic process, to both of which the term "historicity" has been connected, by law or by singularity. It is only the trace that will remain which creates, through its action, the origin of its nonorigin. Therefore there can be no global foresight; François Jacob has brilliantly expressed this in the chapter entitled "Time and the Invention of the Future" at the end of his *The Actual and the Possible:* "What we can guess today will not be realized. Change is bound to occur anyway, but the future will be different from what we believe. This is especially true in science. The search for knowledge is an endless process and one can never tell how it is going to turn out. Unpredictability is in the nature of the scientific enterprise. If what is to be found is really new, then it is by definition unknown in advance. There is no way of telling where a particular line of research will lead" (Jacob [1981] 1982, 67).

So we are further than ever from the romantic illusion of history as an all-prevading "totality" dominated by mimetic, metamorphic, or "expressive" relations of the parts within the ensemble.[13]

The figure of differential reproduction of serial lines nestling in a landscape of research however creates another perhaps no less encompassing but much more fragile coherence—one no longer based on the simultaneity of all possible metamorphoses but on the coexistence of replicating systems carrying their own age along with them, thus escaping any unifying time of history. The global structure of this coherence is based not on expression, reflection, or mirroring but on the tension of an "ecological" reticulum, a patchwork of precocious and deferred actions with its extinctions and reinforcements, interferences and intercalations.

If we take Jacob's statement seriously, then we have to cope with the principal impossibility of any algorithm, of any logic of development that is ontologically or methodologically grounded. Then the difficulty for the historian arises that the linearizations of what he calls history are altogether fictions created for the sake of satisfying the desire for a logos-driven process. All that can be said about the "machine for making the future" (Jacob 1988, 9) called modern science is that it is creating the future. The present as the future of the past is not a "result"—whatever that means—of the past; the past is the result of a future—its presence as a surrogate.

Nevertheless, a future-generating device does not produce anything whatsoever. Although unpredictability is in the nature of scientific undertakings, their movement and performance can be characterized in a formal—i.e., structural—way. This may not be obvious after what has been said so far. The following indication has to be taken with every possible precaution. The problem—not its particular form—can be seen as analogous to the epistemological difficulties in dealing with the phenomena of biological evolution. If one follows the scheme that has been established since Darwin, one may say: The difficulty in understanding the process we call evolution lies precisely in its resting on contingencies which produce a scattering field that begins to filter itself because of its own finite possibilities of extension. Now a research process too has to produce something different from the present state of the art in order to remain a research process. And its character as a research endeavor is tied to the production of what only *post festum,* by becoming filtered, acquires the character of a novelty in the sense of a new attractor. Paradoxically, one could state: the goal of the research process is to produce results that by definition cannot be produced in a goal-directed way. The unknown is something that cannot be approached straightforwardly precisely because one does not know what is to be approached.

Given such conditions, a research device has to fulfill two basic requirements. First, it has to be stable enough so that the knowledge which is implemented in its functioning does not simply deteriorate in the course of continuing cycles of realization. This is a necessary but not sufficient condition for its becoming a device that is endowed with internal time and is thus able to act as a historial arrangement.[14] Second, it has to be sufficiently loosely woven so that in principle something unpredictable can happen and over many rounds of performance *must* happen. In everyday life and in most of our social contexts this is a situation that one tries to avoid as an inconvenience. Within the research context it is a situation that has to be actively promoted. In order to balance these two requirements, "experiencedness"[15]—to pick up a somewhat old-fashioned expression of Fleck's ([1935] 1979, 96)—is needed as the principal ability of those involved in the endeavor.

Both prerequisites, sufficient reproductive stability and sufficient sloppiness for the intrusion of the unknown—Max Delbrück characterized this as the "principle of measured sloppiness" (Fischer 1988, 152–53)[16]—make it something fundamentally different from a system closed upon itself. We could rather say—in a way metaphorically, but the character of metaphor depends on the determination we would like to convey to the diacritic boundaries of the signifier—that it follows the

movement of what Derrida has termed the "différance": an "economic concept designating the production of differing/deferring" (Derrida [1967] 1976, 23). In a paper elaborating on this notion Derrida writes: "Everything in the design of the *différance* is strategic and bold. Strategic, because no transcendental truth present outside the field of writing is able to dominate theologically the totality of the field. Bold, because this strategy is not a simple strategy in the sense in which one says that the strategy guides the tactics to a certain end, toward a telos or the motif of a domination, a dominance and a definite reappropriation of the movement of the field. A strategy, finally, without finality; one could call this *blind tactics, empirical roaming around*" (Derrida [1968] 1972, 7, my translation).[17]

I would like to emphasize the expression "blind tactics"—empirical *"tâtonnement"* (as Claude Bernard once called it (Bernard [1878] 1966, 19).[18] It is intimately linked to the nature of the *means* by which a text, and an experimental text as well, gets written. It is the nature of these means—material, graphic entities—that they contain the possibility of an *excess.* They contain more and other possibilities than those to which they are actually held to be bound. The excess embodies the historial movement of the trace: It is something that transgresses the boundaries within which the game appears to be confined. As an excess, it escapes any definition. On the other hand it brings the boundary into existence by cutting a breach into it. It defines what it escapes. The movement of the trace is recurrent. The present is the future of a past that never happened.

In describing an experimental system as pervaded by *différance,* this point is crucial. It stresses that the system undergoes a play of differences and oppositions governed by its own operator-time, *and at the same time* that it decalates or displaces what at any given moment appear to be its borders. This *décalage,* or displacement, implies that other experimental systems are already there against which the displacement can be operated. What goes on, then, could be characterized by the concept of "grafting" as invoked by Derrida[19]—once more, as a model of working on and with textual structure. Interestingly, and probably not by chance, it is derived from a biological background. Grafting keeps alive, as a support, the system on which one grafts. At the same time it induces the supporting system to produce not only its own seeds but also those of the supported graft. On the one hand the relation of graft and support is that of a tight insertion; on the other it is the continuation of a manifest separation. The graft is a special kind of excess: an *intrusion.* It brings the boundary into existence by transgressing it in the reverse direction. But its very functioning as a graft also shows the feasibility of the support to be intruded. Thus there is a fundamental complicity. What is inside and what is outside here ceases to be a question that can be answered in any meaningful way for the process at issue.

So the recombination and reshuffling of and within experimental systems is a prerequisite for producing stories from other stories, something that does not and cannot happen if the "lines" have become too "pure." The historial movement of the *différance* is always impure; it is a hybrid creation, it works by transplantation.

A CASE FROM VIROLOGY

By way of illustration I present here a very brief and condensed account of a case from the history of virus research.[20] Since the movement of experimentation gains its plausibility from the details of its trajectories, the story that follows inevitably entails some distortion and suppression.[21]

During the period 1910–11 Peyton Rous, who had obtained an appointment in the Pathology

Department of the Rockefeller Institute in 1909, succeeded in transferring a Plymouth Rock chicken tumor from one animal to another, healthy one, by injecting a cell-free extract of the former (Rous 1910, 1911). Because of its physical characteristics (e.g., it passed through a porcelain filter without losing its efficacy) Rous thought of the transferring agent as an ultramicroscopic structure, probably something similar to what since the turn of the century had begun to be thought of as "viral" agents,[22] as distinct from ordinary microbes. Viruses were by then beginning to be phenomenologically, and negatively, characterized as nonbacterial infectious entities. They were not retained by bacterial filters, were not visible in the light microscope, and did not grow on sterile bacterial media. Oncologists received the news from the Rockefeller labs with "downright disbelief" (Rous 1967, 26). It was an unprecedented finding. Rous himself was not able to find similar filterable agents in mammalian tumors during the years that followed, and so, disappointed, he left the field. The chicken sarcoma agent had made its appearance as a *cancer* agent, but it did not become connected to the field of the prevailing cancer research: human oncology. And given the means of pathological analysis then current, other than comparative questions could not be raised within such "viral systems."

However, the controversy over the chicken tumor agent—i.e., Rous's "filterable agent"—slowly intruded, became part of, and so was resumed about a dozen years later within *another* controversy: that over the nature of a virus. The behavior of viruses could be interpreted as stemming from a kind of parasitic ultramicroorganism; but their mode of action could just as well be that of a soluble biochemical substance produced by the living cell under certain conditions. Possibly by assuming that viruses were, so to speak, a minute pocket edition of bacteria, one went in the wrong direction. And maybe the chicken sarcoma agent was the right object of research just *because* one had hesitated in the beginning to accept it as a virus. Anyway, such questions were agitating James B. Murphy, director of the Cancer Laboratory at the Rockefeller Institute, who had earlier been a coworker of Rous, and they finally brought him back to the chicken tumor agent in 1928. The system had been put aside for a time, but it was not difficult to reactivate it at the place it had been in use before.[23] Some biochemical and biophysical characteristics of the agent, especially its behavior in electrodialysis, induced Murphy to believe that it might be an endogenous, "enzyme-like" structure rather than a parasitic organism (Murphy et al. 1928).

At this point in time—1929—Albert Claude came from Belgium to join Murphy's laboratory. He was supposed to set out to demonstrate the so-called nonliving character of the substance causing the tumor. Since the cancer-inducing activity survived rather harsh purification procedures, thus losing some antigenicity but without its activity being diminished, its purely "chemical" constitution appeared to become more and more plausible. Claude and Murphy began to think about the tumor agent as a kind of "transmissible mutagen," chemical in nature and endogenous in origin, that could induce a permanent alteration in the metabolic behavior of the cell (see Murphy 1931). Murphy saw connections to the "transforming agent" of *Pneumococci* (Griffith 1928), on which Oswald Avery's laboratory was then already working, also at the Rockefeller Institute. This was in 1931.

The biochemical characterization and especially the purification procedures available at the beginning of the 1930s did not however allow of any appreciable enrichment and thus of a more detailed analysis of the tumor-inducing component's composition. In 1928 an enzyme-like substance had been favored. Around 1931–32 a nonprotein component seemed to show up. By 1935 proteins and lipoids had taken over (Claude 1935). At that time, however, the experimental system

again began to become sluggish. On the other hand, Wendell Stanley, also of the Rockefeller Institute, had succeeded in crystallizing a virus for the first time—the tobacco mosaic virus (TMV) (Stanley 1935; see also Kay 1986). This reinforced the suspicion that viruses might be nonliving entities.

Also in 1935 two successful attempts at sedimenting the filterable chicken tumor agent by ultracentrifugation were reported (Ledingham and Gye 1935; McIntosh 1935). To keep his research machinery productive, Claude immediately began to implant into his system the method of ultracentrifugation. Within two years he had managed to concentrate the agent by a factor of about 2,800 (Claude 1938a). Upon chemical analysis, nucleoproteins became prominent (Claude 1939). Were they to be regarded as the active material? Anyhow, since the agent could be sedimented, the abandoned option of its being a *particle* came into play again. At the same time, however, matters took a further turn as the result of another astonishing observation. By comparing subcellular fractions of chicken tumor cells and healthy embryonic, actively dividing cells as a control Claude surprisingly found that the composition of the tumor particle could not be distinguished from what appeared to be its normal cellular counterpart (Claude 1938b). Two interpretations were possible. Either the bulk of the tumor fraction simply represented inert elements existing also in normal cells, or the particles of the normal cells might be precursors of the chicken tumor principle "which could assume, under certain conditions, the self-perpetuating properties of the tumor agent" (ibid., 402).

With that, an experimental process was instigated which soon gained its own momentum and quickly led away from the tumor agent that had kept Claude busy for almost ten years. A new option came into play. Ultracentrifugation had been introduced in order to isolate a submicroscopic *cancer* principle. Now it promised to become a tool for fractionating the cytoplasm of *normal* cells. What had been cancer research, now turned into cytomorphology.

For a short time Claude identified his particles with mitochondria or fragments thereof (Claude 1941). Between 1941 and 1943, however, he managed to refine his sedimentation and resuspension conditions and finally came to the conclusion that his "small particles" represented something different from and definitely smaller than mitochondria. Accordingly, he called them "microsomes" (Claude 1943). For quite a time he pursued their purification, starting with normal cells, assuming that he might work on the normal cellular counterparts of what under certain conditions caused uncontrolled malignant growth. Finally he realized that the evidence he could obtain did not support this view and that with microsomes he had something quite different in his hands. The graft had produced, so to speak, its own seeds.

A refined biochemical analysis of microsomes meanwhile had revealed that they contained considerable amounts of ribose nucleic acids in addition to their protein and lipid components. Nucleic acids were just coming into the focus of attention as possible candidates of the genetically active material. Oswald Avery, Colin MacLeod, and Maclyn McCarty were on the point of ascribing to them the transforming activity they had been following up for many years in their *Pneumococcus* experiments (Avery et al. 1944). Claude was inclined to assume that his microsomes might represent some kind of self-reproducing entities within the cell. However, like most researchers at the time he thought of the nucleic acid as a necessary cofactor of reproduction, rather than as of the reproducing entity itself. The latter was generally held to be a protein entity. As a variation on the theme, Jean Brachet (Brachet 1941) and Torbjörn Caspersson (Caspersson 1941) suggested that ribonucleic acid–containing cytoplasmic structures might be involved in the cellular making of protein. Claude, however, remained skeptical to the suggestion that microsomes might be associated with protein synthesis (Claude 1950).

From the early 1940s Claude began to work with still another technique of ultrastructural research: the electron microscope. After some pioneering work on the rather bulky mitochondria (Claude and Fullam 1945; see also Rasmussen 1993), he tried, together with Keith Porter, to visualize the submicroscopic microsomes. Taking advantage of Porter's skill in growing monolayered cell cultures, he succeeded in the electron microscopic representation of what later came to be referred to as the endoplasmatic reticulum (Claude et al. 1947). Much to his disappointment microsomes did not show up as particulate structures of the cytoplasm in these pictures. However, this combined technique of cell-culture preparation and electron microscopy enabled the long-neglected tumor agent to celebrate its ultrastructural comeback. Cells derived from chicken tumors appeared to be crammed with small, electron-dense particles that had no counterpart whatsoever in normal healthy cells. Around 1940 the tumor agent had lost its identity in favor of a regular, constitutive cytoplasmic particle. Now the tumor agent again acquired prominence, this time against a background of invisible microsomes. What remained, with these viral particles, was the motif of self-duplication. Claude, as he reported, even "saw" them duplicate in his ultrastructural pictures (ibid.). But these new particles appeared in malignant cells only, and so the end result of this long and labyrinthian pathway was an exogenous infectious entity—just the opposite of the assumed endogenous biochemical substance that had guided Claude's initial work twenty years earlier.

A summary could read like this: At the beginning, Murphy and Claude had looked for a chemical substance responsible for the induction of chicken sarcoma, which they wanted to establish as an endogenous cellular component, distinct from Rous's initially assumed viral parasitic "ultramicroscopic organism," as the cause of a fowl cancer (Rous 1911, 409). The result of Claude's endeavor however was that he was led by the vagaries of his research trajectory to pioneer the ultrastructural composition of the cytoplasm of normal cells via differential centrifugation. It soon became clear that what he had at first identified as mitochondria, and what he had subsequently begun to detach from them under the "noncommittal" term of microsomes (Claude 1943, 119–20), had little to do with a viral or, more generally, a cancer-inducing agent. Instead he found himself following the trace of something that slowly would become an organelle of protein synthesis rather than a self-replicator. From this organelle electron microscopic structures could finally be distinguished that again came to represent an entity quite different from the original assumption: the cancer-producing agent was not a soluble, single biochemical substance but rather a specific, exogenous pathogen. The microsomes were thus the outcome of research into something that in turn revealed itself as an entity that did not exist in the form in which it had been searched for: namely an endogenous, biochemical cancer-inducing agent.[24] Quite the contrary: the cancer-inducing agent gained identity only as a structure that could be *detached* from an endogenous component of the cell not known before, the microsome, which in turn had gained identity only in the search for a cancer agent. And yet in the labyrinth of all these transformations we are still in the realm of the experimental setup with which Peyton Rous had started in 1910, in which for the riddle of malignant growth he substituted another one, the riddle of the viruses: the chicken sarcoma system.

One thus gets the impression of a highly volatile research process, where at every step what is about to take shape creates unforeseen alternative directions for the next step to be taken. The significance or, better, the significant units of the experimental system concatenate into a constantly changing signifying context. There is no direct progress toward a definite "meaning"—whatever "meaning" might mean here. The so-called "tumor agent" successively signified a virus

(a turn-of-the-century-type virus), an enzyme-like endogenous component of the cell, a "transmissible mutagen," a factor regulating normal cell growth but having escaped control, a microscopic cellular organelle such as the mitochondrion, a submicroscopic "microsome," and finally an extraneous structure probably able to duplicate within the cell. For forty years the experimental system was in a sense oscillating around an "epistemic thing" that constantly escaped fixation; and by transplanting new methods into the setup—ultracentrifugation, electron microscopy—the system itself constantly shifted its borders. There was not, and could not be, any single perspective that could have brought the research movement into line, any definite direction to its "blind tactics," its "empirical roaming around." Claude was looking for something whose likeness he did not and could not know. What was a virus? If *we* want to "know" what a virus represented between 1910 and 1945, the material signifiers of the experimental game will have turned into something that they, at the time, could not (yet) have been. The signified organizing the recurrence draws them in a light corresponding to another conjunction. "Within the truth"[25] of a particular ongoing research there exist always only the minimal conditions for the coherence of a significant chain to be endowed with the dignity of a scientific object. At a given moment and in a given research process, what, say, a microsome or a virus "represents"—in the sense of how it is "produced," how it is "brought forth"—is an articulation of graphemes traced and confined by the procedures of the research process. Thus what André Lwoff of the Pasteur Institute in Paris had to say about viruses in 1957 is not to be read as a tautological joke but precisely points at the argument I am trying to make: "Viruses should be considered as viruses because viruses are viruses" (Lwoff 1957). Here the signified has been crossed out and the reference itself has become a signifier—which is the essence of narration.

CONCLUSION

What, then, about my introductory remarks concerning recurrence in the history of science? Claude, after his odyssey and, I should add, after having received his Nobel Prize,[26] accounted for his "discovery" (Claude 1975) in the frame of what one might call the "spontaneous history of the scientist."[27] Ilana Löwy summarizes the process quite precisely: "A scientist takes a biological entity, at first poorly defined and thus 'controversial in its nature,' purifies and characterizes it, and, as a consequence, he recognizes its true nature as a '*bona fide* virus'" (Löwy 1990, 100). In the spontaneous recurrence of the scientist the new becomes something already present, albeit hidden, as *the* research goal from the beginning: a vanishing point, a teleological focus. Without the avian sarcoma virus of 1950, Rous's sarcoma agent would have remained something different. But: The virus of 1950 must be seen as the condition of possibility for looking at Rous's agent as that which it had *not* been: the *future virus*. The new is not the new at the beginning of its emergence.

If we would not like to look at it as a mere idealization, or even a malevolent distortion, the retrospective view of the scientist as a spontaneous historian reminds us of the following: An experimental system has *more stories* to tell than the experimenter at a given moment is trying to tell with it. It not only contains submerged narratives, the story of its repressions and displacements; as long as it remains a research system, it also has not played out its excess. Experimental systems contain remnants of older narratives as well as fragments of narratives that have not yet been told. Grasping at the unknown is a process of tinkering; it proceeds not so much by completely doing away with

old elements or introducing new ones but rather by *re-moving* them, by an unprecedented concatenation of the possible(s). It differs/defers. If in the spontaneous history of the scientist the latest story appears always as the one which has already been told, or that at least has been tried to be told, this is not a deliberate dissimulation; it reflects a process of *marginalization* that is born into the ongoing research movement itself. But it reflects the rebuilding, the replacement, the patching, the brushing aside—in short, the *deconstruction* of the research meandering, as a *construction;* and it thus remains within the demiurgic illusion inherent in this notion. In the spontaneous history of the scientist, the present appears as the straightforward result of the past pregnant with what is going to be. Strangely enough, in a kind of double reversion, it inevitably also presents the new as the result of something that never happened. The historical, without realizing it, obeys and discloses the figure and the signature of the historial.

In a recent interview Jacques Derrida stated: "[The term] 'deconstructions,' as I prefer to say in the plural, has certainly never meant a project, a method, or a system. Above all not a philosophical system. Within always very limited contexts, it is a possible way to designate, metonymically in the end, what arrives or doesn't arrive to arrive, i.e., a certain dislocation that repeats itself regularly—and everywhere where there is something and not nothing: in the texts of classical philosophy certainly and exemplarily, but also in every "text," in the general sense I would like to attach to that term—that is, in experience as such, in social, historical, economic, technical, military 'reality'" (Derrida 1991, 26–27, my translation). This applies even more so, it appears, to an experience that is called scientific experimentation and for which the French language has the same and only this expression: *expérience.*

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NOTES

* A modified German version of this paper has been included in Rheinberger (1992b). For a full account of experimental systems, see Rheinberger, H. J., *Toward a History of Epistemic Things,* (Stanford: Stanford University Press, 1997).

1. "An unemphatic and difficult thought that, through much unperceived mediation, must carry the entire burden of our question, a question that I shall provisionally call *historial* [*historiale*]" (Derrida [1967] 1976, 24).

2. There is a deep irony at work in this coincidental complicity of political history and paleontology—unintended, but inescapable.

3. This holds for the monuments of natural history as well as for those of the history of science: "The same may be said of all the new varieties of scientific thought, which, after the event, come to project a recurrent light on the obscurities of uncompleted knowledge [qui viennent après coup projeter une lumière *récurrente* sur les obscurités des connaissances incomplètes]" (Bachelard [1934] 1984, 8, my emphasis; I have changed the English translation because Goldhammer's left nothing of the sense of the original sentence). According to Georges Canguilhem it is exactly at this point that the roads of the historian in the traditional sense and the epistemologist in the sense of Bachelard part: "The historian proceeds from the origins toward the present in such a way that the science of today is always to a certain degree founded in the past. The epistemologist proceeds from the actual toward its beginnings in such a way that only part of what yesterday took itself to be science finds itself within the present. So, in founding—never of course forever but over and over again—the science of today also destroys—forever" (Canguilhem [1963] 1975, 178–179, my translation).

4. Within the framework of art history as a history of formal sequences of things, George Kubler has insistently pointed to what André Malraux called the "Eliot effect": "T. S. Eliot was perhaps the first to note this relationship"—which is here referred to as recurrence—"when he observed that every major work of art forces upon us a reassessment of all previous works" (Kubler 1962, 35). German scholars might add that Goethe addresses this relationship on several occasions with respect to the sciences. He speaks of a "provisional rearrangement" that may become necessary "from time to time" (Goethe [1833] 1982, 424) and of a "rewriting" (Goethe [1810] 1957, 149) of the course of science.

5. For a critical assessment of the constitution of historical narrativity, see White 1980.

6. "The trace is not only the disappearance of origin—within the discourse that we sustain and according to the path that we follow it means that the origin did not even disappear, that it was never constituted except reciprocally by a nonorigin, the trace, which thus becomes the origin of the origin" (Derrida [1967] 1976, 61).

7. No historian and philosopher of science after Karl Popper has, both outside and *within the communities of scientists themselves,* had an influence comparable to that of Thomas S. Kuhn ([1962] 1970; 1979).

8. More recently Thomas Kuhn himself has come to stress not only the diachronic "incommensurability" of paradigms but also the synchronic incommensurability of bits and pieces of the enterprise called science. In characterizing it as a process "driven from behind" (Kuhn 1992, 14), he seems at first glance to contradict the notion of recurrence. Upon closer inspection, however, we note this to be his way of speaking about history without "grounds."

9. For an exploration of such microdynamics in the history of protein synthesis and transfer RNA, see Rheinberger 1992a; 1992b.

10. The notion of "experimental system" as it is used here derives from the everyday language of laboratory science—especially biomedicine, biochemistry, biology, and molecular biology. Cf., for example, François Jacob: "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" (Jacob [1987] 1988, 234). With the remarkable exception of Ludwik Fleck (Fleck [1935] 1979, 84–98), historians of science seem only lately to have become aware of its analytical potential in the sense of a "proto-idea" (ibid., 23–25) for shaping the material of their genuine field: the movement of scientific activity. Robert Kohler, in dealing with *Drosophila, Neurospora,* and the rise of biochemical genetics, speaks of "systems of production" (Kohler 1991). David Turnbull and Terry Stokes use the notion of "manipulable systems" in their analysis of *malaria* research at the Walter and Eliza Hall Institute of Medical Research in Melbourne (Turnbull and Stokes 1990).

11. In a quite similar way Kubler describes artistic activity "as a linked progression of experiments composing a formal sequence" whose "characteristic spans and periods" cannot be grasped by "calendaric time" (Kubler 1962, 83, 85).

12. This distinguishes an ensemble of experimental systems from a field of discursive practice in the sense of Foucault, although any experimental system as such can be seen as a discursive unit. See Foucault 1972.

13. The expression is that of Louis Althusser. See the introduction to Althusser and Balibar 1968.

14. In practice, a contemporary research or experimental system consists of a whole bundle of "actants": crafted persons such as experienced technicians, pre- and postdoctoral fellows who are continually coming in and leaving after a couple of years, senior scientists, a variety of measuring and manipulating machines and special equipment, calculation facilities, a system for purchasing sufficiently graded materials, as well as an adequate laboratory architecture. For the notion of "actant" see Latour 1987, 84; he refuses to distinguish between human and nonhuman "actants" in what he calls units of "translation" or of "machination" (ibid., 103–44).

15. Here *Erfahrenheit* is translated as the "state of being experienced." It is the ability to make judgments, an attribute that has to be distinguished from "experience."

16. Fischer is quoting from a letter of Max Delbrück to Salvador Luria dated from autumn 1948.

17. "One never goes farther than when one does not know where one is going" (Goethe [1833] 1982, 547, my translation).

18. Here again we have a remarkable parallel between the work of the experimenter and the work of the artist as described by Kubler: "Each artist works on in the dark, guided only by the tunnels and shafts of earlier work, following the vein and hoping for a bonanza, and fearing that the lode may play out tomorrow" (1962, 125).

19. "One ought to explore systematically not only what appears to be a simple etymological coincidence uniting the graft and the graph (both from the Greek *graphion:* writing instrument, stylus), but also the analogy between the forms of textual grafting and so-called vegetal grafting, or even, more and more commonly today, animal grafting" (Derrida [1972] 1982, 202).

20. For an extended account, see Rheinberger (1995). Cf. Ilana Löwy's instructive paper on variances in meaning in "discoverers'" accounts of their "discoveries" (Löwy 1990).

21. This is basically because the focus is on a single system, and points of encounter are only indicated. Nevertheless, the trace of the argument should still be discernible.

22. Up to the time of Claude Bernard and Louis Pasteur, the term *virus* had been a synonym for an infectious entity in general.

23. The tumor had been preserved in the laboratory by successive transplantations.

24. The post-Claudian irony of cancer research is that with its "oncogenes" it has come back to the concept of cancerous agencies as regular, albeit altered cellular components, mostly growth factors. Cf., for example, Weinberg 1987.

25. Being "within the truth" of a science means something radically different from saying "the truth in the space of a savage out there," as Michel Foucault has formulated by reference to Georges Canguilhem (Foucault 1972).

26. Claude was awarded the Nobel prize in medicine or physiology in 1974. See Porter 1974.

27. Louis Althusser developed the notion of a "spontaneous philosophy of the scientist," on which the above expression is analogously based. See Althusser [1967] 1974.

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