# On the Narrative Order of Experimentation

*Hans-Jörg Rheinberger*

The claim to do nothing else than to let the things themselves tell their stories has a long tradition in the sciences. The venerable metaphor of the legibility of the world and of the letters in which the book of nature is written plainly corresponds with that demand of self-exposure. As Hans Blumenberg has shown, it has accompanied the sciences from the early modern times to the present, from the mathematical vision of Galileo Galilei to the letter-universe of the Human Genome Project.[1] The demarcation criterion for a discourse that can rightly claim to be scientific would thus be to allow things to express themselves according to their own grammar and their own lexicon. Succeeding in creating such a space of self-exposure would render scientific discourse transparent, and the congenial knowledge would be one that is essentially undistorted by the medium of its representation. To put it in another way: It would coincide with that representation. The question would thus not so much be whether scientific texts do narrate or not. Their scientificity would not consist in the fact that they would operate, in contrast to a descriptive narration, in the mode of an explanation, or according to different, but equivalent epistemological distinctions. Scientific texts would rather distinguish themselves from the many and multiple, invented or true stories that we tell ourselves about anything and everything, by the fact that they have *another author*. What I would like to do in this paper is to give this vision a particular twist: In trying to subvert it, I will take it up in a peculiar way.[2]

Posing the question of narration with respect to scientific knowledge thus means not only to pose the question of its content, or object, but in the last

1 Blumenberg, Hans, Die Lesbarkeit der Welt. Frankfurt am Main: Suhrkamp, 1981.

2 For an early and preliminary exposé of the following, see Rheinberger, Hans-Jörg, „Noch etwas über die experimentelle Ordnung der Dinge." In Wissenschaft und Welterzählung: Fakt & Fiktion. Die narrative Ordnung der Dinge, edited by M. Michel, Zürich: Chronos, 2013, 270-271.

instance, the question of the *subject* of the sciences. In trying to answer this question, one sooner or later faces the alternative that can be formulated as follows. In both there is narrative, albeit of a different author: Either one chooses the line sketched above and thus makes the scientist disappear behind the transcendence of the divine—or secular—order of things; then the subject of the sciences are the objects announcing themselves in their proper idiom. Or one opposes this kind of objectivism and aligns the scientific discourse about the order of things with the stream of all those stories that *we* tell us ourselves. As a consequence, according to temperament, the sciences present themselves as one grand or many small narrations. This is the classical dividing line.

In *Toward a History of Epistemic Things* I attempted to show that the *experimental* order of things is realized in a dynamic process condensed in experimental systems.[3] An attentive historical consideration of experimental systems opens a perspective pointing beyond the noted dichotomy. Experimental systems can be seen as the actual and actualizing technical setups for things of epistemic interest, for objects to become constituted as epistemic entities at all. A context of this kind is needed in order to endow an object with the very character of a thing of knowledge. For the experimenter, this context presents itself primarily as an instrumental one, as a necessary condition of manipulation. At a second look, however, one realizes that this context always consists already of what can be called "sedimented" knowledge, taking up an expression of Edmund Husserl.[4] Research technologies are materialized knowledge environments. One can consider them as collectively authorized vehicles, carriers that in their relation to an epistemic object constitute something like trans-subjective generators of events and of surprises. They are machines for producing what could be called epistemo-differences.[5] Sedimented knowledge brought into a research configuration thus has agency.

Now, we can reasonably argue that the essence of history is canalized contingency. History lives from and through events. Without boundary condi-

3 Rheinberger, Hans-Jörg, Toward a History of Epistemic Things. Synthesizing Proteins in the Test Tube. Stanford: Stanford University Press, 1997.

4 Husserl, Edmund, "The origin of geometry." In Edmund Husserl's "Origin of Geometry": An Introduction, edited by J. Derrida, translated by J. P. Leavey, Jr., New York: Harvester, 1978.

5 Wilhelm Johannsen talked about "genodifferences" as the epistemic objects of the genetic experimental systems of his time. Johannsen, Wilhelm, "The genotype conception of heredity." American Naturalist 45, 1911: 129-159, on p. 150.

tions, however, an incident cannot present itself as an event. And without events, there is no historical process. Even so, there is no good story without a certain amount of surprising turns in the framework of a plot. And in a really good story, it is the plot itself that provokes the turns. In his book *Grains et issues*, published in 1935, Tristan Tzara, the Romanian writer and one of the leading figures of French surrealism, characterized his view of a good literary story as follows. The passage is titled "The Experimental Dream": "Thus the story follows, and spreads across the frame of a logical development that reduces itself to an account of successive facts, but leaving an irrational and lyrical remnant open for discovery. This, in turn, overflows the vessel intended for it, and at times engulfs and floods the base, the foundation, the traditional scaffolding of the story. It is a lyrical superstructure whose elements are derived from the base structure and which, once it is realized, impacts back onto that structure from the heights of its new power. Occasionally, its force intensifies to such an extent that it undermines the meaning of the structure, corrupts it, abolishes it, annihilates it in its essence."[6] This is how Tzara sees, in the realm of literary production, a scaffold that allows, as he put it, to "bring forth new events not foreseen by the original plan."[7]

In the realm of science, we know of such structures precisely as experimental systems. They provide the space for knowledge provoking contingencies, for epistemic surprises that are more than the spurious sparks and accidents of a lucky intuition. They are arrangements that both produce history through their temporal order and stories through the permanent shift and displacement of meaning that characterizes them. The history and the stories that the sciences bring forth are written by experimental systems, which we have to address as difference machines. They do not have a once-for-all shape: Case studies are needed to set them in a proper light. They are to expose what could be called a poetology of research.

There is a line by Michael Polanyi — who started his career as a physical chemist and later became a philosopher of science — a stance that is telling in many respects in our context. Marjorie Grene quotes one of its versions in her book *The Knower and the Known*. It is a statement about what might be

6 Tzara, Tristan, Grains et issues, edited, introduced and annotated by H. Béhar, Paris: Garnier-Flammarion, 1981, 155-156.

7 Ibid., 155.

called the *research situation*[8] and reads as follows: "This capacity of a thing to reveal itself in unexpected ways in the future, I attribute to the fact that the thing observed is an aspect of reality, possessing a significance that is not exhausted by our conception of any single aspect of it. To trust that a thing we know is real is, in this sense, to feel that it has the independence and power for manifesting itself in yet un-thought of ways in the future."[9]

From the perspective of the researcher, we could say that what we are dealing with is an act of delegation. Setting up an experimental system revolving around an epistemic object and exploring some of the inexhaustible aspects of its thingness means to undercut the traditional subject-object relation in the sense of a face-to-face relation between an observer and something being observed. In an experiment, the act of observing is mediated by a technical arrangement of sorts that one brings into interaction with the epistemic object. According to Blumenberg, the action at a distance that this implies lies at the very basis of conceptualization *überhaupt*. Research, then, is second order conceptualization. It focuses on the process of conceptualization itself. An interaction of this sort has to be crafted in a way that the outcome — the traces that the interaction leaves behind — is not completely determined in advance. If it were, we would be dealing with a demonstration and not a research experiment. A research experiment lives from its aspect of "un-conceptuality," to use Blumenberg's notion for this peculiar tension.[10] It results from the effort to expand the realm of the conceptual which, in the very same movement, always also risks to reveal itself inappropriate.

Epistemic entities are thus things that by necessity leave something to be desired. They represent a knowledge-generating relation to the world: We can call it epistemicity. It is exploratory, driven by the desire to find, not to assert what is already given. When the great French experimental physiologist of the nineteenth century, Claude Bernard, once confided to his laboratory notebook

8 On this and the following, see also Rheinberger, Hans-Jörg, "On epistemic objects, and around." In WdW Review: Arts, Culture, and Journalism in Revolt, edited by D. Ayas and A. Kleinman, Rotterdam: Witte de With Publishers, 2017, 376-381.

9 Polanyi, Michael, Duke Lectures (1964), Microfilm, Berkeley: University of California Press, 1965, Library Photographic Service, 4th Lecture, 4–5. Quoted in Grene, Marjorie, The Knower and the Known, Washington DC: Center for Advanced Research in Phenomenology & University Press of America, 1984, 219.

10 Blumenberg, Hans, Theorie der Unbegrifflichkeit, Frankfurt am Main: Suhrkamp, 2007.

that "where one is no longer in the position to know, one must find,"[11] he expressed this situation in an exemplary and succinct manner.[12] Experimenters are specialists in creating situations in which such finding becomes possible. The movement of finding in science neither obeys the logic of mere chance nor that of pure necessity. It obeys a logic of its own, composed of elements of both, and in so doing, undercuts the stochastic rigor of the former *and* the deterministic rigor of the latter. It is a peculiar engagement with the material world that, on the one hand, requires intimacy with the matter at hand, and on the other, disentanglement, the capacity of *Verfremdung*. It has become common to address the event-provoking character of research under the label of serendipity, and it is probably not by chance that the term, which Robert Merton smuggled into the discourse on science, can be traced back to a fairy-tale from Persia.[13]

A reminiscence that dates back to the time when I was working on my case study on the history of protein biosynthesis research may illustrate the future-oriented power of finding, which at the same time acts as a recursive narrative force. Paul Zamecnik, whose laboratory at the Massachusetts General Hospital in Boston was the focus of this study, had been invited on several occasions, from the late 1950s to the middle of the 1980s, to lecture on the achievements of his lab. It is striking to see how he reported on the way the main steps of the research trajectory of his group changed over the distance of twenty years. In 1958, at a time his work was just beginning to enter the limelight of emerging molecular biology,[14] he presented a story that described the main findings strictly along his experimental trajectory. He used a vocabulary that remained largely operational and reflected the techniques his group was using to dissect the biosynthetic process under investigation: Amino acids were "incorporated" into protein, reflecting the fact that radioactivity added

11 Bernard, Claude, Cahier de notes 1850–1860, présenté et commenté par M. Grmek, Paris : Gallimard, 1965, 135.

12 It may be of linguistic interest here to note that the unit of research that usually leads to a publication is "the finding."

13 Merton, Robert K., and Elinor Barber, The Travels and Adventures of Serendipity: A Study in Sociological Semantics and the Sociology of Science, Princeton: Princeton University Press, 2006. As it were, the title of the book sees its subject matter itself as dominated by the principle of serendipity.

14 See, e.g., Zamecnik, Paul, "Historical and current aspects of the problem of protein synthesis." In The Harvey Lectures 1958-59, New York and London: Academic Press, 1960, 256-281.

in the form of radioactive amino acids was found to be associated with protein in the course of the experiment. "Microsomes" and later "ribonucleoprotein particles" were identified as the sites of protein synthesis, the former term referring to a cellular fraction that could be sedimented at high speed, the latter to the chemical constitution of a purified fraction. "Soluble RNA" was found to take up amino acids, referring to a ribonucleic acid fraction that remained soluble during high speed fractionation, and so on. The terminology carefully remained at the level of the technical set-up of the experimental system. In terms of theory, it was deliberately non-committal. In 1979, thus twenty years later,[15] the vocabulary hat completely changed, Now, the laboratory had engaged in "ribosome" studies from its beginning in the late 1940s, discovered "transfer RNA" around the middle of the 1950s, and with that, contributed a decisive link to understand the "language of the gene and that of the protein," that is, the process of "translation" as it was delineated at the end of the 1950s. Now, the story was told as one of molecular biology from the very outset, in a form that would have been unthinkable at the point where it started.

In the second part of this paper, I would like to explore the historiographical consequences of this deliberately epistemic view on the scientific research process. If experimental systems are to be seen as units of making scientific events happen, they can and should, of course, also become units of historiographical narration. The challenge of such case studies is to escape the illusion created by mapping the historiography directly onto the historical dynamics of the process. Georges Canguilhem has warned insistently against such a conflation. In his seminal paper on the object of the history of the sciences, he pleads for a clear distinction between objects of nature, objects of the sciences, and objects of the history of the sciences. In his address to the Canadian Society for the History and Philosophy of Science in 1966 in Montreal, he framed their respective differences with the following words: "The object in the history of the sciences has nothing to do with the object of a science. The scientific object, constituted by methodical discourse, is secondary, however not derived with regard to the natural, initial object that one might call pretext, in playing with the sense of that word. The history of the sciences occupies itself with these secondary, non-natural, cultural objects, but it is not derived from them, as little as they are derived from the first. The object of the historical discourse is, in effect, the *historicity* of the scientific discourse,

15 Zamecnik, Paul, "Historical aspects of protein synthesis." Annals of the New York Academy of Sciences 325, 1979: 269-301.

inasmuch as this historicity represents the effectuation of a project that is internally normalized, but traversed by accidents, retarded or diverted by obstacles, interrupted by crises, that is, moments of judgment and of truth."[16] In his paper, Canguilhem polemicizes against a historical narrative that would nothing but emulate the scientific object by making use of the vocabulary of the sciences themselves. What is thus needed is a vocabulary that tries to capture the *historical* nature of scientific development, the dynamics of a process that, according to Canguilhem, is "normalized" *and* "interrupted by crises" at the same time. It thus creates the conditions of its own regulation *and* the conditions of critical transcendence without which that kind of historicity would not exist. For the perspective from experimental systems, this means that we need to think about the conceptual historical tools of a particular kind of micro-history. It is a micro-history that stands in contrast to other forms of traditional micro-history that have their place in the overall agenda of the history of the sciences, such as biographical or institutional narratives.

The focus of this kind of micro-history is on the materiality of the research process, with particular attention to the aleatoric moments that emerge from it and that have the power to orient it toward unforeseen directions. What we observe here is a peculiar kind of relationship between material continuity and conceptual reorientation. Take the example of the early genetic work of Carl Correns that extended over half a decade between 1894 and 1900.[17] Correns started to cross varieties of corn as well as peas with the explicit idea in mind to produce a clear instance of xenia—the appearance of characters of the pollinating variety on the seed and fruit of the mother plant—and then eventually elucidating its physiological background. Four years into the process, his goal was subverted by the observation of a roughly 3:1 ratio between the characters of the original varieties in the second generation of self-pollinating pea hybrids—instead of any unambiguous instance of xenia. The material continuity of his crossing regime together with the careful recording of the results allowed him to re-read the entire experimental process in terms of a re-discovery of Mendel's laws and to focus his attention on their corroboration in the final round of crossings. What we observe here is the material

16 Canguilhem, Georges, « L'objet de l'histoire des sciences ». In Etudes d'histoire et de philosophie des sciences, Paris : Vrin, 1968, 9-23, on p. 17, my emphasis.

17 Compare Rheinberger, Hans-Jörg, An Epistemology of the Concrete, Durham and London: Duke University Press, 2010, chapter 4.

continuity of an ongoing experimental process and, at the same time, a complete replacement of the epistemic object followed through the vagaries of that process.

The examples could be multiplied by widely different variants that cannot be understood without peculiar attention to the intricacies of the respective experimental processes. Therefore, such micro-histories generally require that the historian have a laboratory record at his or her disposal which will allow her or him to zoom into the experimental turning points. In the particular case of Correns, the laboratory protocols did not only have the function of memorizing the results of his experiments, they became part and parcel of the experimental process as it went on. For the historian, they are the surrogate for the experimental process, that is, the paper form of the experimental narrative itself that must serve him or her as a foil for her or his own efforts to come to terms, in Canguilhem's sense, with the historicity of the scientific object at hand.

The vocabulary of a historical epistemology living up to this challenge is not to be found ready-made in the annals of traditional epistemology. It requires an ongoing effort for those who continue to be interested in such a micro-history of the "mangles of practice," to put it in the words of Andrew Pickering.[18] For a deeper understanding of the scientific practices in the different corners and niches of the scientific universe, this approach remains indispensable. And there is one other thing that needs to be considered here. If we are to arrive at an understanding of how scientific knowledge and other cultural forms of knowledge and knowledge production hang together, we need to approach them from below and not from the bird's view of the theoretical products of selected sciences. Case studies of this kind are the privileged places of narration of a very particular, metonymic character: The very telling of such microstories only makes sense if they point beyond themselves. Their way of generalization has the form of 'standing for.' This is, however, in the nature of *microstoria* and distinguishes it from just local stories. Microstories pretend to tell a lesson. In that sense, we could even compare their role for history with the role experiments play in the sciences. Each and every experiment is a concrete, singular event. But it is only accepted as an experiment worth of consideration if it can be looked at as an instantiation of a more

18 Pickering, Andrew, The Mangle of Practice, Chicago: The University of Chicago Press, 1995.

general state of affairs. Otherwise one would not take it to be more than just fancy.

Different time frames are in need of different objects of historiographical narration. The story of an experimental system usually does not exceed the career span of a particular scientist or group of scientists. It amounts to what George Kubler considers as a project or productivity cycle, or indiction period, typically more than ten and less than twenty years.[19] If we would like to assess the dynamics of scientific development over a longer period of time, we need to think about entities other than experimental systems to guide our narratives. In the context of the history of the empirical sciences, one way of approaching the meso-range of the next order of magnitude, that is, the order in the range of a century instead of a decade, is to look at what I like to call experimental cultures.[20] Using experimental cultures as an object of narration, on the one hand, preserves the focus on practice inherent in the micro-approach. On the other hand, it allows for an understanding of how particular spaces of scientific activity are being formed that transcend a specific laboratory with its more or less unique experimental setup. These areas have taken different shapes in the history of the modern sciences. Until early into the twentieth century, they tended to condense into disciplines usually subjected to more or less stringent social codices. Just to give one example: A latecomer in the family of biological disciplines at the end of the nineteenth century was genetics. It rested on an experimental regime that was taken over from the realm of breeding plants and animals and adapted to the specification of those units that were thought to be responsible for the expression of certain organismic characters: the genes. Its two practical prerequisites were the selection of pure lines, on the one hand, and the capacity of such lines to be crossed with different ones, so that the behavior of the different characters could be followed in the progeny. A unique experimental culture resulted. It combined the living with mathematics: It made experimental use of living organisms, and the outcome of the experiments lent itself to the mathematical precision instrument of statistics. The gene as a new epistemic object took shape in this experimental context. Its characteristics remained formal, however. The

19 Kubler, George, The Shape of Time. Remarks on the History of Things, New Haven: Yale University Press, 1962, 101-102.

20 Compare Rheinberger, Hans-Jörg, "Cultures of Experimentation." In Cultures without Culturalism, edited by K. Chemla and E. Fox Keller, Durham and London: Duke University Press, 2017, 278-295.

experimental regime of classical genetics presented no handle to follow them down to the material, molecular level. On the other hand, a new instrument arose that soon would proliferate into other disciplinary specialties of the life sciences: the creation of model organisms. They were to accompany the life sciences as a living instrument over the entire twentieth century.

If we look at the twentieth century, however, we witness the emergence of spaces that are much less stringently codified and organized than disciplines, whose boundaries are less rigid, and whose constituency is more ephemeral than what we associate with the concept of discipline. It is therefore not by chance that historical epistemologists have, from the early twentieth century onward, pointed to this phenomenon and tried to find a conceptual framework to describe it. Gaston Bachelard, for instance, used terms such as "canton" or "district" in the image of different quarters of a city.[21] A generation later, Pierre Bourdieu introduced the notion of "field" to characterize relatively coherent areas of social and cultural activity, including scientific practice.[22] In doing so, he simultaneously aimed to make scientific practice comparable to other forms of practice.

A good example for such an experimental culture is in vitro experimentation. Test tube biology had its origins in what came to be called biochemistry at the beginning of the twentieth century. Its aim was to create artificial environments for partial biological reactions. It turned the "inner milieu" of Claude Bernard into an outer milieu.[23] It also had its place, however, in cell biology and in microbiology. And in the middle of the twentieth century, it formed an essential part of emergent molecular biology. We thus see clearly that we are dealing with a space that is more fine-grained than that of discipline, and above all, one that has a different focus. With his notion of "cultures of emergence," Bachelard has pointed to the core of all such different, sub-disciplinary knowledge spaces: the eventuation of novelty.[24] Shifting one's narrative attention toward the specificities of these experimental environments and their potential of innovation means to keep practice in the center and, at the same time, finding a way to de-localize the story that is to be told. It

21 Bachelard, Gaston, Le rationalisme appliqué, Paris : Presses Universitaires de France, 1949.

22 Bourdieu, Pierre, Pascalian Meditations, Palo Alto : Stanford University Press, 2000, esp. Chapter 3.

23 Bernard, Claude, Leçons sur les phénomènes communs aux animaux et aux végétaux (1878-1879), Paris : Vrin, 1966, in particular the Second Lecture, section III.

24 Bachelard, Le rationalisme appliqué, 133.

entails another form of generalization than the one we encountered with experimental systems as micrological units of narration. Whereas their modus was metonymic, here the modus is analogical and parallactic.

Finally, what about macro-histories? Underneath experimental systems as well as cultures of experimentation, we witness a flux of time that obeys yet another order of duration, sometimes extending over several centuries. The question is what kinds of entities would be apt to serve as narrative guides over such extended periods of time? From the perspective of scientific practice, Foucauldian "discourse" comes to mind as a possible candidate to do the job. Following Michel Foucault, a discourse consists of an epochal, overarching set of practices and standards as well as the beliefs embodied in them that delimit what is conceivable and enunciable within that framework.[25] However, when the focus is on the dynamics of a particular realm of science in the making, one needs to be more specific. The sciences unfold in the context of discourses, but they are only spots of condensation within them. Thomas Kuhn has talked about a "disciplinary matrix" in this respect,[26] a structure into which paradigms are wired. But in either case, the focus is on closure, on what is excluded. What we are to be looking for here, however, is a narrative guide that would focus on the *openings* and displacements along a longer-term trajectory and that would not exclude the unprecedented at this temporal macro-level.

Again, Canguilhem can be helpful here. He suggested that such long-term histories might best be written as histories that follow the trajectory of scientific *concepts* from one realm of inquiry to another and to observe their vagaries and varying embodiments and instantiations.[27] The focus then lies on broader figures of change. Let us take the example of the concept of heredity and sketch its trajectory in extremely broad strokes.[28] A concept of heredity was absent from the space of natural history until the late eighteenth century. Theories of generation, be they preformationist or epigenetic, were not in need of such a concept. Around 1800, it entered the realm of the biological

25 Foucault, Michel, The Discourse on Language. Appendix to The Archeology of Knowledge, New York: Pantheon, 1972, 215-237.

26 Kuhn, Thomas, "Second thoughts on paradigms." In The Essential Tension, Chicago: The University of Chicago Press, 1977, 293-319.

27 Compare Canguilhem, Georges, La Formation du concept de réflexe aux XVIIe et XVIIIe siècles, Paris : Presses Universitaires de France, 1955.

28 Müller-Wille, Staffan, and Hans-Jörg Rheinberger, A Cultural History of Heredity, Chicago: The University of Chicago Press, 2012.

from the realm of the legal where it had its original place: the idea of material properties transmitted from one generation to the next. Concomitantly, the notion of generation completely changed its meaning. Conceptualizing generational change in terms of such properties—of which organisms acted as their carriers—took shape over the course of the nineteenth century in different practical and discursive areas such as medicine, agricultural breeding, anthropology, and evolution. To begin with, it created a scattered epistemic space, one that only became unified toward the end of the nineteenth century. Along with this condensation, the epistemic space of heredity became compacted as an epistemic object sui generis: the gene. The gene then permeated all of twentieth century life sciences, and it inhabited a plethora of experimental systems and cultures of experimentation, thereby creating a number of successive auras around itself. They can be addressed as so many "images of knowledge."[29] The first of these images was that of an 'atom of life.' But as such an atom, it remained elusive. The second was that of a material 'information carrier.' Now the atom had materialized as a molecule, but what it meant to carry information remained elusive. The third image was that of an element of a 'map.' Now it became the node of a network, but the nature of that network remained to be determined. And the story goes on.

I have restricted myself to this example to characterize the transgressive power of the epistemic object 'gene' in the space of research—the privileged space of my concern in this paper—and I have neglected the onto-theological and technological stories that accreted around heredity with all their social, political, cultural, and medical consequences—those second-order life-world materializations of powerful images of knowledge from which the sciences can, of course, not be detached. Narratives at this level would talk, for instance, about the mechanization of the worldview, the geneticization of society or, in general terms, the scientification of our world picture. But with that, we would have reached a level of generalization that borders on what is being called "grand narratives." A generation earlier, its label was "ideology." This is a level that does not make sense from a perspective that focuses on the process of research. It is even counter-indicated, as long as one is convinced—of which I remain—that scientific exploration, together with a few other cultural activities such as art, is endowed with an ongoing and irresistible subversive power. To be subversive means nothing else than to have the power of resisting totalization.

29 Elkana, Yehuda, Anthropologie der Vernunft, Frankfurt am Main: Suhrkamp, 1986.

Now, a narrative is a narrative only as long as one can imagine that it might have been otherwise. Narration therefore comes with an intrinsic quantum of potential plurality, and therefore with an unavoidable amount of concreteness and circumstantiality. An abstract story—no less than an abstract experiment—is a contradiction in terms. This elective affinity is the ground on which experiment as narration and narration as experiment can come together and on which their paths can cross and inform each other.