

TOWARD A HISTORY OF EPISTEMIC THINGS

*Synthesizing Proteins
in the Test Tube*

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CHAPTER 1

After All: An Epistemology of the Beginning



[It is] the vague, the unknown that moves the world.
—Claude Bernard, *Philosophie: Manuscrit inédit*

Forewords are afterthoughts. They are exemplars of the paradox of writing, hence of creating history. By definition, they precede the text, but, as a rule, they do so by deferment and insinuation. They try to impose a closure on what until then could be considered work in progress. Yet by this very gesture they keep the game going. The sciences are characterized by a permanent process of reorientation and reshuffling of the boundary between what is thought to be known and what is beyond imagination. Such a reorientation has at times been characterized as an approximation, an adjustment to “truth” or “reality” as a presupposed *terminus ad quem*. In contrast, there are reasons to pretend that the sciences are much better understood as continuously *engendering* what Gaston Bachelard has called the “scientific real,” thereby reshaping their agenda through their own action.¹ This recurrent action applies to the historian’s practice as well: he or she continuously reorients the historical understanding of the process of reorientation we see at work in the sciences. There is no end to this endeavor, just as there is no end to the empirical endeavor on which it bears.

To Begin With

To read Freud as an epistemologist might seem surprising. I will not enter into a discussion of the discourse of psychoanalysis, although I will touch upon it here and there in the following chapters, nor am I guided by the conceptual apparatus of that science. Rather, I am interested in finding out what Freud, both from a general perspective and from within his own

analytical practice, considers to be “the true beginning of scientific activity”—scientific activity tout court, as he seems to suggest in passing. I will have to question this assertion of universality as well as the hope for a “true beginning.” But it serves me as a preliminary starting point. The passage to which I refer reads as follows:

We have often heard it maintained that sciences should be built up on clear and sharply defined basic concepts. In actual fact no science, not even the most exact, begins with such definitions. The true beginning of scientific activity consists rather in describing phenomena and then in proceeding to group, classify and correlate them. Even at the stage of description it is not possible to avoid applying certain abstract ideas to the material in hand, ideas derived from somewhere or other but certainly not from the new observations alone. Such ideas—which will later become the basic concepts of the science—are still more indispensable as the material is further worked over. They must at first necessarily possess some degree of indefiniteness; there can be no question of any clear delimitation of their content. So long as they remain in this condition, we come to an understanding about their meaning by making repeated references to the material of observation from which they appear to have been derived, but upon which, in fact, they have been imposed. Thus, strictly speaking, they are in the nature of conventions—although everything depends on their not being arbitrarily chosen but determined by their having significant relations to the empirical material, relations that we seem to sense before we can clearly recognize and demonstrate them. It is only after more thorough investigation of the field of observation that we are able to formulate its basic scientific concepts with increased precision, and progressively so to modify them that they become serviceable and consistent over a wide area. Then, indeed, the time may have come to confine them in definitions. The advance of knowledge, however, does not tolerate any rigidity even in definitions. Physics furnishes an excellent illustration of the way in which even “basic concepts” that have been established in the form of definitions are constantly being altered in their content.²

This text is both clear and bewildering, if not contradictory. In “scientific activity,” the “grouping,” “classification,” and “correlation” of phenomena, cannot proceed from “clear and sharply defined basic concepts.” Nonetheless, scientific activity presupposes “certain abstract ideas [from] somewhere or other,” which, however, cannot be derived from what, as a new experience, is still wanting. If they are to allow “new observations” at all, they must “at first necessarily possess some degree of indefiniteness.” But they dare not be so indeterminate that they are unable to settle into the “material of observation.” As soon as one starts to apply these ideas to the material of experience, understanding them further will de-

pend on how the material is handled. This in turn makes it “appear” that the ideas had been “derived” from the material, although, “in fact,” just the opposite has taken place: they have been “imposed” upon it. Thus, on the one hand, such notions are “conventions,” and, on the other hand, they must not be “arbitrarily chosen” if they are to allow “new” observations on the material of experience. But how can we guarantee their nonarbitrariness? In the end, we can only do so because they bear “significant relations to the empirical material,” relations we “seem to sense before we can clearly recognize and demonstrate them.” This draws us inevitably into a circle where moving on is only possible by further convolution.

What Freud invites us to ponder here is the ineffable trace of scientific action or, as I would like to call it, the *experimental situation*. It appears as if the relationship of “deriving” ideas from the material of observation and of “imposing” ideas upon that material represented the focal point of the argument. In this game of deriving from/imposing upon, the contours of what will, perhaps one day, constitute the basic concepts of a science emerge in a gradual fashion. The solidus is meant to indicate that both processes are inextricably interconnected, in fact, that they become effective only in one and the same movement. The deriving from/imposing upon construction is a precarious one whose ongoing reorientation can be stopped only by destroying it. The tension in this movement demonstrates the ambivalence of what we like to call a “concept”: it is the product of a scientific activity, and it is a compelling instrument of performing that same activity. It causes some irritation to observe Freud’s attempts to cope—using the traditional opposition of concept and reality, theory and empirical foundation, idea and matter—with a movement that resists this scheme to the extent that it continually forces him to invent new formulations. What is more disquieting is that there is another level of reasoning beneath the deriving from/imposing upon movement of concept and reality. This schematism does not aim—despite its outward appearance—at the “basic concept” as the point at which it would come to rest; on the contrary, it aims at “new observations.” In working through the material, it gives hope of rendering accessible what until now was not accessible, of making explicit what up to now could only be “sensed.” Freud writes,

[I] am of opinion that that is just the difference between a speculative theory and a science erected on empirical interpretation. The latter will not envy speculation

its privilege of having a smooth, logically unassailable foundation, but will gladly content itself with nebulous, scarcely imaginable basic concepts, which it hopes to apprehend more clearly in the course of its development, or which it is even prepared to replace by others. For these ideas are not the foundation of science, upon which everything rests: that foundation is observation alone. They are not the bottom but the top of the whole structure, and they can be replaced and discarded without damaging it.³

Somewhat later, in Ludwik Fleck's writings, we encounter the expression of "pre-ideas" or "proto-ideas" as guidelines for the development of knowledge in the sense of a heuristics of "somewhat hazy" concepts.⁴ Yehuda Elkana has found the notion of "concepts in flux" to be an appropriate description for the early development of thermodynamics.⁵ In a different, immunological context, Ilana Löwy has used the notion of "boundary concept" and emphasized its function in organizing research fields and in creating "federative experimental strategies."⁶ I deliberately slip over the nuances of these attempts at what might be called, with Hans Blumenberg, an epistemology of the "non-conceptual" in science.⁷ Suffice to say here that what Freud calls the "nebulous, scarcely imaginable basic concepts," are not, in his or in all these related instances, the gemmules of an anticipated future theory. They are more like auxiliary organs of touch in an otherwise impalpable space of experience that, on occasion, one will be ready to "replace by others." As Freud sees it, in the edifice of science the concepts, whatever their degree of elaboration, form "the top of the whole structure, and they can be replaced and discarded without damaging it."

This does not imply considering them as mere ephemeral intermediates in a thoroughgoing empirical process of establishing facts. Freud is not, as the vocabulary might suggest, speaking in favor of a shallow empiricism; what he is attempting is to emphasize the primacy of scientific experience-in-the-making, for which conceptual indeterminacy is essential, over its conceptually defined and consolidated results. He is trying to characterize that state of affairs that Michel Serres has claimed nearly defies any possibility of a description—that "free-wheeling, fluctuating, non-determinate time in which those who do research are not yet quite sure what they are looking for, and yet blindly do know what they are after." "No," Serres continues, "he who does research, does not know. He gropes about, he tinkers, lingers, keeps his options open. No, he does not construct, thirty years before its realization, the calculator of the day after tomorrow. He does not foresee it, as we, who know and use it might

conclude." And Serres compares the history of science, which does not unfold in time but on which time acts as an operator, with a "meandering river, its many streamlets and tributaries, whose fluxes adventurously clash against obstacles, barriers, impediments, and icings, and force themselves into narrow passes, defiles and crevasses. Not to mention the turbulences and the more stable currents, the countercurrents that reverse the river's flow, its blindly ending ramifications, its dead backlashes."⁸

Serres, of course, is not the first to compare the scientific endeavor in its unruliness with a meandering river. Fleck used the metaphor but also pointed to its limits: yes, rivers meander, they may change trajectories, but, "provided enough water flows in the rivers and a field of gravity exists, all rivers must finally end up at the sea." Fleck has gone one step further and contended that for science in genesis and development "there is no such thing as the *sea as such*," where its movement may finally come to rest.⁹ The process of investigation does not and cannot come to an end, for the very reason that there is no possibility of anticipating the future objectal constellations that accrue from it. Instead of following the entropic track toward a stable equilibrium, the material activity of pattern formation we call research obeys the rules of a never ending ramification. It is in this latter sense only that the comparison of science with a meandering river seems appropriate.

Coordinates of Description

The passages cited in the previous section point to the hinge that holds the following chapters together. Alternately more reflective and more narrative, they revolve and evolve around the experimental groundwork of the empirical sciences—molecular biology in the given case. In the past decade and a half, philosophers and historians of science have begun to focus on experimentation as a field of investigation in its own right. The role of experiments in the sciences has begun to be recognized and reconceptualized as extending far beyond their seemingly unproblematic acceptance as instances of verification, of corroboration, of refutation, or of the modification of theories—in short, beyond their function as mere empirical instances in the evaluation of theoretical propositions. Viewed in a restricted sense and still largely within the framework of classic epistemological thinking, this means basically—to use Hans Reichenbach's terms—that experimentation has been removed from the "context of justification" and inserted into the "context of discovery."¹⁰ But such a

displacement does not remain without consequences for the logical functioning of the opposition between justification and discovery itself from which it proceeded.

This dislocation also affects the accompanying opposition between the realm of logic and that of a psychology of mind. By going a step further and by transforming the psychological space of discovery into a space of experimental manipulation, we also transcend the ideal of a creative genius, of a free play of individual mental faculties, bent and domesticated only by the stringency of their own performance. By plunging into the spaces of experimental manipulation, we find ourselves confronted with a rhizomic network of recurrent epistemic practices, a filigree of "investigative operations."¹¹ Correspondingly, we need a "pragmatogony" of scientific action.¹²

*post-Kuhn
Philosophy
of
Experiment
as
History*

After the Kuhnian move from continuity and verity to discontinuity and relativity of scientific knowledge, a move decisively radicalized by Paul Feyerabend, we are witnessing another turn—from Kuhn's predilection for viewing science as theory dominated to a post-Kuhnian engagement with the experimental aspects of the sciences.¹³ A philosophical landmark in this move has been Ian Hacking's *Representing and Intervening*. Hacking reminded philosophers of science that "experimentation has a life of its own."¹⁴ Historian of science Peter Galison has taken up the challenge and argued in favor of "granting a measure of autonomy to the practices of experimentation and instrumentation" that renders them respectable subjects of inquiry. Galison calls for "a history of the laboratory without idolatry and without iconoclasm," a history of experimentation that "accords that activity the same depth of structure, quirks, breaks, continuities, and traditions that we have come to expect from theory."¹⁵ Accordingly, Galison has proposed a "brick model" of scientific development and change. In it, on the one hand, the three levels of theory construction, experimental traditions, and instrument building are allowed to unfold their own developmental potentials and to follow their own time requirements in relative autonomy. On the other hand, they are expected to generate new coherences by "intercalation" within the ever-changing framework of local, situated research programs.

Timothy Lenoir has suggested locating the relation between experiment and theory in the proper realm of experimental practice itself, as a process of "packaging" practices and concepts: "The very construction of the concepts is intertwined with the practices which operationalize them, give them empirical reference, and make them function as tools for the

production of knowledge."¹⁶ The size and shape of these packages, however, depends on the qualification of what is to be taken as practices, and here the scope extends from the more restricted realm of epistemic practices, passing through all kinds of intermediates, to the all-encompassing conception of practice as a "form of life."¹⁷

Thus, once theory as the distinguishing and distinctive feature of the scientific enterprise is put back into the context of practice, the quest for the social context of practice commences. Today, a growing history and philosophy-of-science industry is carrying on that move and amalgamating it with what has come to be labeled "science as practice and culture."¹⁸ "Social construction of science" has become a shibboleth for those wishing to be members of the club. Actors, interests, politics, power, and authority have acquired the status of key terms in a "strong program" to treat science on a par with any other cultural activity.¹⁹ That Thomas Kuhn is "among those who have found the claims of the strong program absurd: an example of deconstruction gone mad"²⁰ might not surprise and might perhaps be put aside as a matter of taste. I will not go into detail here on the variants and certainly not into an extensive evaluation of social constructivist endeavors. They are of concern to my purpose insofar as they—like Steve Shapin and Simon Schaffer in their *Leviathan and the Air Pump*²¹—give voice to the relation of science and society as mutually coproductive, rather than burying the "generative entrenchment" of both under the cover sheet of sociologists' shoptalk about dominance and subordination, expression and influence.²² From a fundamental epistemological point of view it is perhaps Latour who most explicitly and most radically has called attention to an impasse in "science and society" studies from which there seems to be no easy escape. To put it sharply: what do we gain by substituting social conditions for what, long enough, have been taken to be the natural referents of scientific activity? If, in the perspective of social construction, we have lost the illusion of an ultimate referent called "nature," what do we gain by trying to compensate for this loss with the mirror image of "society" as a new and insurmountable foundation?²³ From where do we hope to derive the epistemic legitimization of this move? Andrew Pickering, in his most recent book, raises similar arguments in the performative idiom of the "mangle of practice."²⁴

With the tetragonic coordination of theory and practice, nature and society, we remain, despite all rotation of competences, within the confines of a conceptual framework that Jacques Derrida has qualified as the logocentric legacy of occidental metaphysics. "It could perhaps be said

that the whole of philosophical conceptualization, which is systematic with the nature/culture opposition, is designed to leave in the domain of the unthinkable the very thing that makes this conceptualization possible.”²⁵ In discussing Lévi-Strauss’s anthropological writings, Derrida sees him conserving “all these old concepts within the domain of empirical discovery while here and there denouncing their limits, treating them as tools which can still be used. No longer is any truth value attributed to them; there is a readiness to abandon them, if necessary, should other instruments appear more useful. In the meantime, their relative efficacy is exploited, and they are employed to destroy the old machinery to which they belong and of which they themselves are pieces.”²⁶ Derrida himself prefers to question the whole conceptual framework by working with/on its limits. His endeavor has come to be known as “deconstruction.”²⁷

Latour’s model of science in action presents itself as a “hybridogony” of networks that are “simultaneously real, like nature, narrated, like discourse, and collective, like society.”²⁸ To mention only one aspect, but in any event a very central one of his model, it tries to capture how, in the process of engendering scientific “things,” conditions are created for what I would like to call unprecedented events. Latour ponders,

No matter how artificial the setting, something new, independent of the setting, has to get out, or else the whole enterprise is wasted. It is because of this “dialectic” between fact and artefact, as Bachelard puts it, that although no philosopher defends a correspondence theory of truth it is absolutely impossible to be convinced by a constructivist argument for more than three minutes. Well, say an hour, to be fair.²⁹

Thus, no debunking of the sciences in the name of another authority, but instead of taking in their objectivity, their truth, their coldness, their exterritoriality—qualities they have never had, except after the arbitrary withdrawal of epistemology—we retain what has always been most interesting about them: their daring, their experimentation, their uncertainty, their warmth, their incongruous blend of hybrids, their crazy ability to reconstitute the social bond.³⁰

The following chapters try to convey a feeling for this quandary. The concepts that will accompany my narrative do not claim to posit themselves beyond the tetragon of the conceptual coordinates just mentioned—theory and practice, nature and society—at least not immediately and not from the beginning. Their purpose is humbler and more modest. I hope to help, through a series of displacements, crack open this age-worn framework that has become so deeply entrenched in our minds. I

will stick to the Derridean program of reworking these oppositions from within, of trying to perfuse and defer their limits. I will try to widen the gaps in which the very effort of tracing these distinctions has buried them, to the extent that we are getting a sense for what they have been hiding before us. Once again with Freud, philosophy and history of science, too, must derive their own “abstract ideas” from somewhere other than the “new,” still pending, “observations alone.” Instead of remaining categorial beyond question, these notions have to become involved in a process in which “the material is further worked over” so that perhaps, one day, they can be “replaced” and “discarded without damage.” Only the work of displacement itself will be able to tell us whence, when, and whether this will happen. I therefore start from the more narrowly conceived type of move from theory to experiment within the realm of scientific activity characterized by Hacking and Galison, and try to develop a framework in which experimentation takes meaning as a set of epistemic practices that constitute a specific kind of material culture. Nevertheless, this project is ambitious. It tries to characterize as incredibly prolific hybrids those structures that are recalcitrant to being classified as belonging to either realm, the natural or the social, the theoretical or the practical alone.

That “Scarcely Imaginable Basic Concept”—the exp system

The concept around which this book has taken shape initially was itself a kind of “nebulous, scarcely imaginable basic concept.” It is the experimental system as a point of orientation for the historian in the overly complex happenings of the modern empirical sciences. Its “somewhere” can be located in the everyday practice of the sciences at issue in this book: mid-twentieth-century biochemistry and molecular biology. To explore its historical emergence in biology and to trace its disciplinary range remains a task that is beyond the limits of this book. Be this as it may, the notion of experimental system is frequently used by scientists in biomedicine, biochemistry, biology, and molecular biology to characterize the space and scope of their research activity. Whoever asks a contemporary laboratory bioscientist what he or she is doing will be told about his or her “system” and the things that happen there. It is, in the first place, a practitioner’s notion, not an observer’s. Just to mention one example among scores that can be found in the research literature, Mahlon Hoagland speaks of the “selection of a good system” as a key to success on the “itinerary into the unknown.”³¹

Only very recently have historians of science started to become aware of the descriptive and historiographical potentials of this hazy and fuzzy concept.³² David Turnbull and Terry Stokes have used the notion of “manipulable systems” in their case study on malaria research at the Walter and Eliza Hall Institute of Medical Research in Melbourne.³³ Robert Kohler, in dealing with *Drosophila*, *Neurospora*, and the rise of biochemical genetics, has spoken of “systems of production.”³⁴ By now, the term *experimental system* is becoming fashionable.³⁵ The concept, as I use it, does not derive its justification from an a priori definition of how the empirical sciences ought to be shaped and of what they ought to be about. Neither must the concept of experimental system, despite the connotations commonly attributed to the notion of a system, be placed within the framework of systems theories. Nowhere do I argue on the basis of a systems theory. It will be easy to see, however, that notions such as “differential reproduction” (see Chapter 5) play an important role in sociological accounts of science on a systemic level.³⁶ For me, the notion of experimental system marks a point of departure for “further working over” that “material” of experience with which the historian of the empirical sciences is confronted. To try to do so is inevitably a deconstructive endeavor because one has to work with concepts whose appropriateness is the very point at issue.

How far this notion will take us, and above all what it is able to carry along with it, will be explored from different perspectives in the following chapters. In a very general sense, this book is concerned with experimental reasoning. But this expression has to be used cautiously because it can easily be misunderstood. For its grammatical structure presupposes reasoning as the *genus proximum*, whose specific difference it is to be guided by experimentation. What is at stake, however, is just the opposite. I try to circumscribe and identify a kind of movement oriented and reoriented by generating its own boundary conditions, within which reasoning displays itself as a dynamic interaction between material entities swept off by tracing. “In science, an idea can become substance only if it fits into a dynamic accumulating body of knowledge.”³⁷ The dynamic body of knowledge, the network of practices structured by laboratories, instruments, and experimental arrangements, is a reasoning machinery in its own right. Gaston Bachelard has spoken of the instruments of modern research as “theories materialized” and has concluded, “contemporary science thinks with/in its apparatuses.”³⁸ These instruments embody the heavy load of knowledge taken for granted at a particular time. Corre-

Bachelard

spondingly, by using a kind of mirror image, we might characterize theories, when come of age, as “machines idealized.” I agree with Bachelard that we need to know more about this peculiar kind of “scientific real,” whose “proper noumenal contexture it is to be able to orient the axes of the experimental movement.”³⁹ In analogy to Wittgenstein’s well-known expression, we could call this a “tracing-game.” Wittgenstein says: “I shall also call the whole, consisting of language and the actions into which it is woven, the ‘language-game.’ ”⁴⁰ And he continues: “Our mistake is to look for an explanation where we ought to look at what happens as a ‘proto-phenomenon.’ That is, where we ought to have said: *this language-game is played.*”⁴¹ Just as we are never able to locate ourselves behind this weaving as language users, we are not able to locate ourselves behind the conceptual tracing-game as scientific practitioners. Thus, I am not looking for a logic in the relationship between theory and experiment. I am not looking for a logic behind experiment. Rather, I am grappling with what must be seen, irreducibly, as the “experimental situation.” In this situation, which is irrevocably local and situated in space and time, there are scientific objects and the technical conditions of their coming into existence, there is differential reproduction of experimental systems, there are conjunctures of such systems, and graphematic representations. All these are notions related to the process of producing what I shall call epistemic things, and all will be explored further in the following chapters.

Within these complex, tinkered, and hybrid settings of emergence, change, and obsolescence, scientific objects continually make their appearance and eventually recede into technical, preparative subroutines of an ongoing experimental manipulation. As a result, there is again a continuous generation of new phenomena, which need not have anything to do either with the preceding assumptions or with the presupposed goals of the experimenter. They usually begin their lives as recalcitrant “noise,” as boundary phenomena, before they move on stage as “significant units.”

Concatenations

Throughout this exposition as well as in the chapters that follow, I frequently refer to a series of French-speaking philosophers, scientists, and historians of science, from Gaston Bachelard, Georges Canguilhem, Michel Foucault, Louis Althusser, Jacques Lacan, and Jacques Derrida to Michel Serres and Bruno Latour; from Claude Bernard to François Jacob, Isabelle Stengers, and Ilya Prigogine. To speak of a series here is not to

imply a genealogy. Such a genealogy does not exist. What exists is a finespun network of demarcation lines. The notion of network emphasizes the links by which the pieces of a system are held together.⁴² One might accuse me of producing a *contradiccio in adjecto* by introducing the term “*network of demarcations*.” I do it to stress that what makes each of these figures count in my account is just what makes them *different*. In contrast to looking first and foremost on what holds the threads of this web together, I am interested in what differentiates its texture. Therefore, I also avoid speaking of a tradition of structuralism, of poststructuralism, of deconstructionism, or postmodernism. What I see is an endless series of displacements, a concatenation of attempts to answer, from widely different starting points and domains of experience, the basic epistemological questions raised by the *sciences* of our century. If there is a glue that pastes them together, it is the transpositivistic challenge to objectivity.

Not without recourse to Nietzsche, Freud, and Martin Heidegger, a move reverberates throughout this web that touches the roots of the occidental *episteme*. What is at stake is the fissure between knowledge and truth, the fragmentation of the unity of knowledge through the sciences themselves, in space and time. What is at stake is the grand project of modernity, the instantiation of Kant’s rationalist credo that we understand only what we can make in terms of our conceptualizations. In his *Critique of Judgment*, at the end of the section on the analytics of teleological judgment, Kant states: “[When] we study nature in terms of its mechanism, we keep to what we can observe or experiment on in such a way that we could produce it as nature does, at least in terms of similar laws; for we have complete insight only into what we can ourselves make and accomplish according to concepts.”⁴³ Meanwhile, that credo has taken on a very non-Kantian appearance. What we can ourselves make and accomplish, we always only know in the form in which we locally do it, and not even this completely. “The grand narrative legitimization of the history of science as a history of rationality, progress or the search for truth must go, but so too must the debunking of science which too often motivates the repudiation of such modernist narratives. Take legitimations of scientific practices and beliefs always to be partial, and to take place in specific contexts, for particular purposes, to which large-scale (de-)legitimation has little relevance.”⁴⁴ No remedy has ever been raised against the weeds that always spring up eventually.⁴⁵ We would not have the incommensurable plurality of the sciences as we experience—and fear—their today if

their movement were not excessive, if they were not continuously producing a surplus that is beyond what we may have wanted, beyond what we might have been able to imagine. In this way, time and again they prevent the closure that a whole epoch of philosophical systems, from Descartes to Hegel, from Gottlob Frege to Rudolf Carnap, has tried to impose on them. Do we finally begin to understand Jacques Lacan’s “strange remark,” in his seminar on “Science and Truth,” that “our science’s prodigious fecundity is to be examined in relation to the fact, sustaining science, that science doesn’t want-to-know-anything about the truth as cause”?⁴⁶

Consequently, on an epistemological level, we need a “philosophy of epistemological detail.”⁴⁷ Traditional philosophy has seen in this situation a necessary and intrinsic limitation of empirical knowledge—whence that longing for an “integral philosophy” that would restore the Gordian knot cut through by the positive sciences. One can see it differently. One can conceive of this presumed limitation as being the prerequisite for the occurrence of events that cannot be anticipated. As a rule, the new is the result of spatiotemporal singularities. There is reason to assume that this is especially the case for matters of knowledge. Indeed, experimental systems are arrangements that allow us to create cognitive, spatiotemporal singularities. They allow us to produce, in a regular manner, unprecedented events. In this sense such systems are “more real” than reality. The “scientific real,” therefore, is not that ultimate referent to which all knowledge must finally accommodate itself. The reality of epistemic things lies in their resistance, their capacity to turn around the (im)precisions of our foresight and understanding. As Michael Polanyi says,

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 unthought of ways in the future.⁴⁸

CHAPTER 2

Experimental Systems and Epistemic Things



[The] meticulous care required to connect things in unbroken succession.

—Goethe, “The Experiment As Mediator between Object and Subject”

At a symposium on the structure of enzymes and proteins in 1955, Paul Zamecnik read a paper on the “Mechanism of Incorporation of Labeled Amino Acids into Protein.” When, in the ensuing discussion, Sol Spiegelman reported his own experiments on the induction of enzymes in yeast cultures, Zamecnik responded, “we would like to study induced enzyme formation, too; but that reminds me of a story Dr. Hotchkiss told me of a man who wanted to use a new boomerang but found himself unable to throw his old one away successfully.”¹

What Does It Mean to Do Experiments?

Better than any lengthy description, the opening anecdote illustrates an essential feature of experimental practice. It expresses an experience familiar to every working scientist: the more he or she learns to handle his or her own experimental system, the more it plays out its own intrinsic capacities. In a certain sense, it becomes independent of the researcher’s wishes just because he or she has shaped it with all possible skill. What Lacan states for the structuralist human sciences holds here, too: “The subject is, as it were, internally excluded from its object.”² It is this “intimate exteriority,” or “extimacy,”³ captured in the image of a boomerang, that we may call virtuosity.

Virtuosity creates pleasure. When Alan Garen once asked Alfred Hershey for his idea of scientific happiness, he answered: “To have one experiment that works, and keep doing it all the time.” As Seymour

Benzer wrote later, this became known as “Hershey Heaven” among the first generation of molecular biologists.⁴

Jacobs In his autobiography, François Jacob has formulated the same experience from the perspective of being engaged in an ongoing research process:

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.⁵

Thus, we have, at the basis of biological research, the choice of a system, and a range of maneuvers that it allows us to perform. Which is, if I see it correctly, a specific reformulation of Heidegger’s claim that to “open up a sphere,” and to “establish a procedure” is what modern research, considered as representing the “essence” of occidental science, is all about.

Heidegger In his “The Age of the World Picture,” Heidegger states with respect to the modern sciences:

The essence of what we today call science is research. [But] in what does the essence of research consist? In the fact that knowing establishes itself as a procedure within some realm of what is, in nature or in history. Procedure does not mean here merely method or methodology. For every procedure already requires an open sphere [*offener Bezirk*] in which it moves. And it is precisely the opening of such a sphere that is the fundamental event in research.⁶

To open a sphere and to establish a procedure: such ought to be the grounding feature of the modern sciences, viewed from a Heideggerian point of view.

With respect to intent and context, these quotations are utterly different. Zamecnik, Garen, Jacob, and Heidegger speak about experimentation in the light of acquaintance, satisfaction, constraint, and conquest. But in another respect they coincide: they all identify a research setting, or experimental system, as the core structure of scientific activity. Such a view, if taken seriously, entails epistemological as well as historiographical consequences. If we accept the thesis that *research* is the basic procedure of the modern sciences, we are invited to explore how research gets enacted at the frontiers between the known and the unknown. If we accept that biological research in particular begins with the choice of a system rather than with the choice of a theoretical framework, it will be in order to focus attention on the characterization of experimental systems, their

structure, and their dynamics. To speak of the “choice” of a system here does not mean that such arrangements are there from the beginning. To arrive at an experimental system is itself a laborious process, as my case study of the group at MGH will show. My emphasis is on the materialities of research. Therefore, as my point of departure I will not directly address the theory and practice issue and the relation between theory and practice, the theory-ladenness of observation, or the underdetermination of theory by experiment. My approach tries to escape this “theory first” type of philosophy of science perspective. For want of a better term, the approach I am pursuing might be called “pragmatogonic.” I would like to convey a sense of what it means for the participants in the endeavor to be engaged in epistemic practices, that is, in irrevocably experimental situations. Here I claim, with Frederick Holmes, “it is the investigations themselves which are at the heart of the life of an active experimental scientist. For him ideas go in and come out of investigations, but by themselves are mere literary exercises. [I]f we are to understand scientific activity at its core, we must immerse ourselves as fully as possible into those investigative operations.”⁷

In this chapter, I first turn to some structural characteristics of such investigative operations on the level of relatively *longue durée*. Let me recall an episode from the end of the eighteenth century. When Goethe was performing the optical experiments that led to his theory of colors, he wrote, in 1793, a remarkable essay entitled “The Experiment as Mediator between Object and Subject.”⁸ In this essay, Goethe addresses his problem in a similar, but still different, vein, neither with respect to virtuosity nor to pleasure, but—conforming to what Friedrich Kittler has called the “Aufschreibesystem 1800”—with respect to the duty of the scientist. The central sentence reads as follows: “To follow every single experiment through its variations is the real task of the scientific researcher.” Goethe compares what he calls “Versuch” with a point from which light is emitted in all possible directions. Through the step-by-step exploration of all of them, a research network is built up that eventually will come into contact with neighboring networks. Establishing such fields, according to Goethe, is the primary task of the experimentalist; disciplinary junctures may be the final outcome of his endeavor. “Thus when we have done an experiment of this type, found this or that piece of empirical evidence, we can never be careful enough in studying what lies next to it or derives directly from it. This investigation should concern us more than the discovery of what is related to it.”¹⁰ Five years later, Goethe asked Schiller

to comment.¹¹ In his reply, Schiller immediately pointed to the core of the argument: “It is quite obvious to me how dangerous it is to try to demonstrate a theoretical proposition directly by experiments.”¹²

Experimental Systems

According to a long-standing tradition in philosophy of science, experiments have been seen as singular, well-defined empirical instances embedded in the elaboration of a theory and performed in order to corroborate or to refute certain hypotheses. In the classical formulation of Karl Popper, “the theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions, and to no others. All other questions he tries hard to exclude.”¹³ Despite the radical shift in perspective in which social studies of science have attempted to deny the naked experiment its ability to decide scientific controversies, the familiar notion of the experiment as a test of a hypothesis is still virulent in them. Even Harry Collins’s argument from the “experimenter’s regress” embraces, in its very rejection, a view of the experiment as an ultimate arbiter.¹⁴

What does it mean to speak of experimental systems, in contrast to this clear-cut rationalist picture of experimentation as a theory-driven activity? Ludwik Fleck, Popper’s long neglected contemporary, has drawn our attention to the manufacture of scientific practices in twentieth-century biomedical sciences and has argued that—contrary to Popper’s claim—scientists usually do not deal with single experiments in the context of a properly delineated theory. “Every experimental scientist knows just how little a single experiment can prove or convince. To establish proof, an entire *system of experiments* and controls is needed, set up according to an assumption or style and performed by an expert.”¹⁵ A researcher thus does not, as a rule, deal with isolated experiments in relation to a theory, but rather with a whole experimental arrangement designed to produce knowledge that is not yet at his disposal. What is even more important, the experimental scientist deals with systems of experiments that usually are not well defined and do not provide clear answers. Fleck even goes so far as to claim that “if a research experiment were well defined, it would be altogether unnecessary to perform it. For the experimental arrangements to be well defined, the outcome must be known in advance; otherwise the procedure cannot be limited and purposeful.”¹⁶ These remarks are not to be taken as a trivial characterization of a de facto imperfection

Experiment as Test, Kuhn

Popper

Ludwig Fleck

systems

not well defined

of a particular research activity. They are to be taken as a profound re-orientation of our view of the inner workings of this process, a process “driven from behind,”¹⁷ a genuinely polysemic procedure defined by ambiguity, not one just limited by finite precision.

Experimental systems are to be seen as the smallest integral working units of research. As such, they are systems of manipulation designed to give unknown answers to questions that the experimenters themselves are not yet able clearly to ask. Such setups are, as Jacob once put it, “machines for making the future.”¹⁸ They are not simply experimental devices that generate answers; experimental systems are vehicles for materializing questions. They inextricably cogenerate the phenomena or material entities and the concepts they come to embody. Practices and concepts thus “come packaged together.”¹⁹ A single experiment as a crucial test of a properly delineated conception is not the simple, elementary, or basic situation of the experimental sciences. The inverse holds. Any simple case is the “degeneration” of an elementarily complex experimental situation. As Bachelard reminds us, “simple always means simplified. We cannot use simple concepts correctly until we understand the process of simplification from which they are derived.”²⁰ It is only in the process of making one’s way through a complex experimental landscape that scientifically meaningful simple things get delineated; in a non-Cartesian epistemology, they are not given from the beginning. They are the inescapably historical product of a purification procedure.²¹ This is, again and again, the experience we find when we look at autobiographical science narratives.²² But this is also what we find when we try to follow particular cases in the history of the modern life sciences. One of them will be expounded in Chapter 3 and traced throughout the rest of the book.

Epistemic Things, Technical Objects

In inspecting experimental systems more closely, two different yet inseparable elements can be discerned.²³ The first I call the research object, the scientific object, or the “epistemic thing.” They are material entities or processes—physical structures, chemical reactions, biological functions—that constitute the objects of inquiry. As epistemic objects, they present themselves in a characteristic, irreducible vagueness. This vagueness is inevitable because, paradoxically, epistemic things embody what one does not yet know. Scientific objects have the precarious status of being absent in their experimental presence; they are not simply hidden things to be brought to light through sophisticated manipulations. A

mixture of hard and soft, like Serres’s veils, they are “object, still, sign, already; sign, still, object, already.”²⁴ With Bruno Latour, we can claim it to be characteristic for the sciences in action that “the new object, at the time of its inception, is still undefined. [At] the time of its emergence, you cannot do better than explain what the new object is by repeating the list of its constitutive actions. [The] proof is that if you add an item to the list you *redefine the object*, that is, you give it a new shape.”²⁵

To enter such a process of operational redefinition, one needs an arrangement that I refer to as the experimental conditions, or “technical objects.” It is through them that the objects of investigation become entrenched and articulate themselves in a wider field of epistemic practices and material cultures, including instruments, inscription devices, model organisms, and the floating theorems or boundary concepts attached to them. It is through these technical conditions that the institutional context passes down to the bench work in terms of local measuring facilities, supply of materials, laboratory animals, research traditions, and accumulated skills carried on by long-term technical personnel. In contrast to epistemic objects, these experimental conditions tend to be characteristically determined within the given standards of purity and precision. The experimental conditions “contain” the scientific objects in the double sense of this expression: they embed them, and through that very embracement, they restrict and constrain them.²⁶ Superficially, this constellation looks simple and obvious. But the point to be made is that within a particular experimental system both types of elements are engaged in a nontrivial interplay, intercalation, and interconversion, both in time and in space. The technical conditions determine the realm of possible representations of an epistemic thing; and sufficiently stabilized epistemic things turn into the technical repertoire of the experimental arrangement.

Take the following example, to which I will return in detail in Chapter 13: When Heinrich Matthaei and Marshall Nirenberg, in their bacterial in vitro system of protein synthesis, introduced synthetic polyuridylic acid, among other ribonucleic acids, as a possible template for polypeptide formation, the genetic code assumed the quality of an experimental epistemic thing. When the genetic code was solved, the polyuridylic acid assay, within the same in vitro system, was turned into a subroutine for the functional elucidation of the protein synthesizing organelles, the ribosomes. To add one more example, less than twenty years ago, enzymatic sequencing of DNA was a scientific object par excellence. It was a new possible mode of primary structure determination among older

ones.²⁷ A few years later, it became a procedure that had been adopted by the leading DNA laboratories around the world. In the early 1980s, it was transformed into a technical object with all the characteristics of such a “translation.” Today, every biochemical laboratory may order a sequence kit, including buffers, nucleotides, and enzymes from a biochemical company, and perform the sequence reaction routinely in a semi-automatic machine. Latour has spoken of “black boxing” in this context.²⁸ Unfortunately, this expression mainly reflects one particular aspect of the process: its “routine” nature after the event. Perhaps at least as important, however, is its impact on a new generation of emerging epistemic things. Black boxing does not mean just setting aside.

Through this kind of recurrent determination, certain sets of experiments become clearer in some directions but at the same time less independent because they more and more rely on a hierarchy of established procedures. “Once a field has been sufficiently worked over so that the possible conclusions are more or less limited to existence or nonexistence, and perhaps to quantitative determination, the experiments will become increasingly better defined. But they will no longer be independent, because they are carried along by a system of earlier experiments and decisions.”²⁹

The difference between experimental conditions and epistemic things, therefore, is functional rather than structural. We cannot once and for all draw such a distinction between different components of a system. Whether an object functions as an epistemic or a technical entity depends on the place or “node” it occupies in the experimental context. Despite all possible degrees of gradation between the two extremes, which leave room for all possible degrees of hybrids between them, their distinctness is clearly perceived in scientific practice. It organizes the laboratory space with its messy benches and specialized local precision services as well as the standard scientific text with its specialized sections on “materials and methods” (technical things), “results” (halfway-hybrids) and “discussion” (epistemic things).³⁰

If both types of entities are engaged in a relation of exchange, of blending and mutual transformation, why then not cancel the distinction altogether? Does it not simply perpetuate the traditional, problematic distinction between basic research and applied science, between science and technology? If science in action should not be conceived in terms of an asymmetric relation from theory to practice, why then uphold a gradient between epistemic and technical objects? Why then construct a division whose only effect is that it permanently has to be undone? The answer is:

because it helps to assess the game of innovation, to understand the occurrence of unprecedented events and with that, the essence of research.

A Little Note on Technoscience

My remarks in the preceding section suggest that the notion of “technoscience” often used in science studies to characterize contemporary scientific large-scale enterprises needs to be handled with caution.³¹ The tendency to lump together what should be understood in its interaction is already virulent in the notion of “phenomeno-technology,” which, according to Bachelard, “takes its instruction from construction.”³² Technoscience suggests an identity of science and technology that, I argue, tends to disguise the essential tension of the research process—no matter whether we are concerned with big or little science, hard or soft. It subscribes to the domination of the “theme” (in my words, epistemic objects) by the “method” (in my words, technical objects) that Heidegger, twisting around a sentence of Nietzsche, has characterized by the following words:

In the sciences, not only is the theme drafted, called up [*gestellt*] by the method, it is also set up [*hereingestellt*] within the method and remains within the framework of the method, subordinated to it [*untergestellt*]. The furious pace at which the sciences are swept along today—they themselves don’t know whither—comes from the speed-up drive of method with all its potentialities, a speed-up that is more and more left to the mercy of technology. Method holds all the coercive power of knowledge. The theme is a part of the method.³³

In this passage, Heidegger sees the sciences as tending to become subordinated to and finally swallowed by technology. Heidegger claims that “from the point of view of the sciences, it is not just difficult but impossible to see this situation.”³⁴ Let me claim, in contrast, that it is exactly the viewpoint of opposing philosophy to technoscience and identifying scientific knowledge with “technowledge” that finally leads to the exile of the “theme” and to its surrender to Heideggerian “thinking.” I perceive thinking as remaining a constitutive part of experimental reasoning, conceived as an embodied disclosing activity that transcends its technical conditions and creates an open reading frame for the emergence of unprecedented events.

Mahlon Hoagland, like many of his fellow molecular biologists, sees scientific activity basically as a “generator of surprises” on the “itinerary into the unknown.”³⁵ Research produces futures, and it rests on differ-

ences of outcome. In contrast, technical construction aims at assuring presence, and it rests on identity of performance. How could it fulfill its purposes otherwise? If the momentum of science gets absorbed into technology, we end up with “extended present.”³⁶ A technical product, as everybody expects, has to fulfill the purpose implemented in its construction. It is first and foremost an answering machine. In contrast, an epistemic object is first and foremost a question-generating machine.³⁷ It is not technical in itself, although it grants the “goings-on of technics,” as Samuel Weber appropriately has translated *Wesen* in the context of Heidegger’s “Questing After Technics”: “The goings-on of technics are ongoing, not just in the sense of being long-standing, staying in play, lasting, but in the more dynamic one of moving away from the pure and simple self-identity of technology. What goes on, in and as technics, its *Wesen*, is not itself technical.”³⁸

Yes, technical tools define any system of investigation—“any study thus begins with the choice of a ‘system.’” They circumscribe the boundaries of an experimental system. Proper fluctuation and oscillation of epistemic things within an experimental system require appropriate technical and instrumental conditions. Without a system of sufficiently stable identity conditions, the differential character of scientific objects would remain meaningless; they would not exhibit the characteristics of epistemic things. We are confronted with a seeming paradox: the realm of the technical is a prerequisite of scientific research. On the other hand and at any time, the technical conditions tend to annihilate the scientific objects in the sense attributed to this notion. The solution to the paradox is that the interaction between scientific object and technical conditions is eminently nontechnical in its character. Scientists are, first and foremost, *bricoleurs* (tinkerers), not engineers. In its nontechnicality, the experimental ensemble of technical objects transcends the identity condition of its parts. According to the same pattern, established tools can acquire new functions in the process of their reproduction. Their insertion into a productive or consumptive process beyond their intended use may reveal characters other than the original functions they were designed to perform.³⁹

A Word on Historiography

Research systems are tinkered arrangements that are not set up for the purpose of repetitive operation but for the continuous reemergence of un-

expected events. Experimentation, as a machine for making the future, has to engender unexpected events. However, it also channels them, for their significance ultimately derives from their potential to become, sooner or later, integral parts of future technical conditions. This movement implies that, in the last resort, it is the future integration into the realm of the technical that grants the scientific objects their “legitimate position” within the history of knowledge. No historiography of science can escape this movement of recurrence implanted into its very subject matter: the epistemic things. For every new scientific object sheds a “recurrent light” on those by which it was preceded.⁴⁰ A historiography that blindly streamlines this movement *post festum* has been criticized as “whiggish.”⁴¹ This is not the place for tracing the arguments against whiggish history and the subsequent critique of this notion.⁴² But it is the place to emphasize that no historiography of science—including my own—can escape what might be called a position of “reflected anachronicity.”

The Case Study and Its Context

The present investigation, in its case study chapters, concentrates on the history of molecular life science in the formative years between 1947 and 1962. More precisely, it focuses on a particular experimental system, an *in vitro* system for the biosynthesis of proteins. Even more narrowly, it looks at a particular research group based at the Collis P. Huntington Memorial Hospital of Harvard University at the Massachusetts General Hospital (MGH) in Boston. The work of Paul Zamecnik, Mahlon Hoagland, and their colleagues originated from a cancer research program and, over a period of fifteen years, was transmuted into one of the core systems of the new biology.

Molecular biology, as I hope to show, must be regarded as the result of an extraordinarily complex development that can by no means be described in an adequate fashion through, for example, the fusion of already existing biological disciplines, such as microbiology, genetics, or biochemistry. Nor is it simply another biological discipline supplementing the historically established canon of biological disciplines.⁴³ Above all, what could be called, with Foucault, the epistemic and technical “formation” of the discourse of molecular biology, is not the straightforward product of the efforts of a few research schools led by a few prominent figures, such as the phage group of the California Institute of Technology in Pasadena and Cold Spring Harbor, the Cavendish crew in Cambridge,

and the Pasteur *équipe* in Paris. This is a myth created by some *Festschriften* dedicated to the “members of the club.”⁴⁴ Neither is it the result of an all-encompassing, paradigmatic theory based on the notion of information. Richard Burian even goes so far as to deny that there exists a unifying theory of molecular biology at all. To assert this, however, is not equivalent to claiming that it was constituted by a mere “battery of techniques.”⁴⁵ Generally speaking, what we today call molecular biology emerged from and was supported by and constructed of a multiplicity of widely scattered, differently embedded, and loosely (if at all) connected biochemical, genetical, and structural research systems. But all of them, in one way or the other, sought to characterize living beings down to the level of biologically relevant macromolecules. By implementing different modes and models of technical analysis, these systems helped to create a new epistemotechnical space of representation in which the concepts of molecular biology, increasingly revolving around the metaphor of information, gradually became articulated. In terms of what could be called its historical “eventuation,” this process is still poorly understood. It appears that we still have to find an appropriate level of analysis through which the key features of the all pervading dynamics of the new biology might become obvious.

In the following chapters, I propose that we turn away from the perspective of a more or less well-defined disciplinary matrix of twentieth-century biology and move toward what scientists are inclined to call their experimental systems. Such systems, I repeat, are hybrid constructions: they are at once local, social, technical, institutional, instrumental, and epistemic settings. As a rule, and insofar as they are research systems, they do not respect macrolevel disciplinary, academic, or national boundaries of science policy and research programs. Insofar as they orient research activity, they may also prove helpful for the orientation of the historian. If experimental systems have a life of their own, precisely what kind of life they have remains to be determined.

In following the development of epistemic things rather than that of concepts, topics, problems, disciplines, or institutions, boundaries have to be crossed, boundaries of representational techniques, of experimental systems, of established academic disciplines, and of institutionalized programs and projects. In following the path of epistemic things, classifications have to be abandoned. Does this study belong to the history of cancer research? of cytomorphology? of biochemistry? of molecular biology? Is it a prehistory of protein synthesis? All of these—and none. My

path takes its starting point from protein synthesis research as part of a cancer research program. By way of differential reproduction, by way of the implementation of skills, tracing techniques, and instruments, such as laboratory rats, radioactive amino acids, biochemical model reactions, centrifuges, and technical expertise, it gained a momentum of its own. In the rapidly changing landscape of the new biology, it became disconnected from cancer research where it had been rooted. Instead, through several unprecedented shifts, it ended up with transfer RNA, which provided one of the experimental handles for solving the central puzzle of molecular biology: the genetic code.

Most of the material analyzed in this book has so far received little attention from historians of biology or medicine.⁴⁶ There are reasons for this. The material cannot easily be categorized as belonging to either fundamental science or technology, to biology or to medicine: it is situated at their intersection. And it cannot easily be reconstructed in terms of paradigmatic conceptual shifts, which renders it resistant to a historiography oriented toward theoretical breakthroughs. Instead, the breakthroughs I am describing lie in the disseminating power of epistemic things that eventually became transformed into technical things. They lie in the structure of a particular experimental culture of representation, of rendering biological processes manipulable *in vitro*, which is so characteristic of the life sciences of our century.

Historians of physical specialties are confronted with new technologies in the first place when analyzing such shifting experimental cultures. And they tend to think of instruments in terms of devices that become more and more sophisticated, eventually ending up as large-scale machinery. In biochemistry and in molecular biology, this is not necessarily the case. No ultracentrifuge was instrumental in establishing the *in vitro* protein synthesis system to be described in the next chapter—although this instrument became crucial for its subsequent development. In fact, the most efficient biochemical and molecular biological instruments are those that accommodate themselves to the level of the analysis—that is, ultimately to the level of molecules. In the establishment of the *in vitro* protein synthesis system, radioactive amino acids happened to play this role of molecular tools, or instruments. Of course, it goes without saying that there is no routine purchase of isotopes without big machinery such as cyclotrons.⁴⁷ But the organic synthesis of amino acids from these isotopes can be, and was, at the beginning, performed with the moderate equipment of an organic chemistry laboratory. On the one hand, such

tracer molecules are technical devices for following particular metabolic pathways. But insofar as they are integral parts of the scientific object under scrutiny, it is not easily possible, in the given case, to draw a clear-cut distinction between the scientific object and the technical conditions of its evaluation. It depends largely on the experimental context whether radioactive tracing is to be considered a technical means of analysis or whether it constitutes the epistemic things that are the objects of research.

In addition, instruments by themselves are not the moving forces of the experimental “goings-on.” It is their embedment in experimental systems that counts. Instruments display their power only as integral parts of what I call spaces of representation.⁴⁸ Without a space of tracing, things cannot be treated as part of the “scientific real.”⁴⁹ Representations are epistemic things in the first place, they are traces deriving from things like “tracers” rather than concepts. The fractional partition of a cell homogenate and the corresponding radioactivity pattern constitute a representation of the cytoplasm upon which it is possible to act: a material space of signification.

My case study here of the group at MGH shows that it is not the overall orientation of an institutional setting or the initial formulation of a research program, or the sheer introduction of new technology that ultimately determines that program’s subsequent direction and scientific productivity. Thus, there is no possibility of a deterministic account, be it socially, theoretically, or technically motivated. Experimental systems grow slowly into a kind of scientific hardware within which the more fragile software of epistemic things—this amalgam of halfway-concepts, no-longer-techniques, and not-yet-values-and-standards—is articulated, connected, disconnected, placed, and displaced. Certainly they delineate the realm of the possible. But as a rule, they do not create rigid orientations. On the contrary, it is the hallmark of productive experimental systems that their differential reproduction leads to events that may induce major shifts in perspective within or even beyond their confines. In a way, they proceed by continually deconstructing their own perspective. Experimental systems, in fact, do not and cannot tell their story in advance.

Let me conclude this chapter with a quotation from Brian Rotman. It is a note on the xenogenesis of texts, and I find it very appropriate for describing an experimental system: “What [a xenotext] signifies is its capacity to further signify. Its value is determined by its ability to bring readings of itself into being. A xenotext thus has no ultimate ‘meaning,’

no single, canonical, definitive, or final ‘interpretation’: it has a signified only to the extent that it can be made to engage in the process of creating an interpretive future for itself.”⁵⁰

Experimental systems give laboratories their special character as particular cultural settings: as places where strategies of material signification are generated.⁵¹ It is not, in the end, the scientific or the broader culture that determines “from outside” what it means to be a laboratory, a manufacture of epistemic things becoming transformed, sooner or later, into technical things, and vice versa. It is “inside” the laboratory that those master signifiers are generated and regenerated that ultimately gain the power of determining what it means to be a scientific—or a broader—culture.

CHAPTER 5

Reproduction and Difference



We may say that when we learn [a] probe, or a tool, and thus make ourselves aware of these things as we are of our body, we *interiorize* these things and *make ourselves dwell in them*.

—Michael Polanyi, *Knowing and Being*

Experimental systems, together with the scientific objects wrapped up in them, are inherently open, if bottlenecked, arrangements. Their movement is such that it cannot be predicted if they are to retain their character as research devices. Epistemic things, let alone their eventual transformation into technical objects and vice versa, usually cannot be anticipated when an experimental arrangement is taking shape. But once a surprising result has emerged, has proved to be more than of an ephemeral character, and has been sufficiently stabilized, it becomes more and more difficult, even for the participants, to avoid the illusion that it is the inevitable product of a logical inquiry or of a teleology of the experimental process.

Labyrinths

“How does one re-create a thought centered on a tiny fragment of the universe, on a ‘system’ one turns over and over to view from every angle? How, above all, does one recapture the sense of a maze with no way out, the incessant quest for a solution, without referring to what later proved to be *the solution* in all its dazzling obviousness?”¹ An experimental system can readily be compared to a labyrinth, whose walls, in the course of being erected, in one and the same movement, blind and guide the experimenter. In the step-by-step construction of a labyrinth, the existing walls limit and orient the direction of the walls to be added. A labyrinth that deserves the name is not planned and thus cannot be conquered by following a plan. It forces us to move around by means and by virtue of checking out, of groping, of *tâtonnement*.² He who enters a labyrinth and

does not forget to carry a thread along with him, can always get back. But there has not yet been found the thread that would indicate the direction in which to proceed through a labyrinth. There is a striking parallel in this respect between the work of the experimentalist and the way George Kubler sees the work of the artist: “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.”³ The metaphor of the labyrinth matches that of the mine.

Reproduction

Thus, the temporal coherence of an experimental system is granted by recurrence, by repetition, not by anticipation and forestalling. Its future development, on the other hand, if it is not to end in idling, depends upon groping and grasping for differences. Together, this adds up to what can be called *differential reproduction*. The term *reproduction* has many meanings. I would like to briefly indicate in what sense I am *not* using it and which of its connotations are central to my argument. I do not use the term to stress the continuity of an experimental program as against sudden breaks and frequent changes. I do not use it to designate a copying procedure, a process of making replicas from an original. Nor do I use it to indicate that a good experiment should be able to be replicated at will, that is, that its results should be reproducible as that term is commonly understood. Rather, I use the term *reproduction* in a sense somewhat akin to that used in evolutionary contexts. It serves to indicate that experimentation has to be seen as an ongoing and uninterrupted chain of events through which the material conditions for continuing this very experimental process are maintained. Reproducing an experimental system means keeping alive the conditions—objects of inquiry, instrumentation, crafts and skills—through which it remains “productive.”

All innovation, in the end and in a very basic sense, is the result of such reproduction. Reproduction, far from being simply a matter of securing appropriate and reproducible boundary conditions for the experiment, characterizes scientific activity as a material process of generating, transmitting, accumulating, and changing information. The generation of new phenomena is always and necessarily coupled to the coproduction of already existing ones. Without this coproduction, there would be no basis for comparison; in fact, there would be a sudden dissipation of all knowl-

edge implemented in the experimental process. For this very reason, experimental systems are necessarily localized and situated generators of knowledge. Their reproductive situatedness, not their logicality, marks their cohesion over time, and thus their historicity. To establish a scientific object means that it will have emerged from differential reproduction and that it will be able to be inserted in the reproductive cycle of an experimental system. Epistemic things, therefore, are recursively constituted and thus intrinsically historical things. They derive their significance from their future, which is unpredictable at the real time of their emergence. They are constituted by recurrence. We can state with Heidegger,

The methodology through which individual object-spheres are conquered does not simply amass results. Rather, with the help of its results it adapts itself for a new procedure. Within the complex of machinery that is necessary to physics in order to carry out the smashing of the atom lies hidden the whole of physics up to now. [In] the course of these processes, the methodology of the science becomes circumscribed by means of its results. More and more the methodology adapts itself to the possibilities of procedure opened up through itself. This having-to-adapt-itself to its own results as the ways and means of an advancing methodology is the essence of research's character as ongoing activity.⁴

But let us be cautious. Yes, there is a pressure for connectivity within an experimental setup. First, however, it does not take the form of the usual claims of theoretical consistency and commensurability, and, second, it remains local, that is, restrained to particular setups within a multitude of others that constitute the fractal boundaries of a research field. I will pursue to this problem in Chapter 9.

From another angle, the construction of experimental systems can be described as a “*jeu des possibles*.⁵ The title of François Jacob's essay alludes to the tinkering of evolution as well as to the process of scientific change. As far as scientific research is concerned, we have to conceive of the “possible” in the double sense of the word: it is something that is in the realm of an experimental system, and it is something that is beyond proper control. The possible has a strange and fragile presence. On the one hand, it does not exist in any strong sense of the word, and, on the other hand, “one must already have decided—*il faut déjà avoir décidé*—what is possible.”⁶ With Jacques Derrida, we could speak of a “game of the difference.”⁷ I will come back to the implications of this comparison. Here I would only like to draw attention to the point that it is the character of “fall[ing] prey to its own work” that brings the scientific enterprise into

propinquity with what Derrida has called “the enterprise of deconstruction.”⁸ Let me recall, in this context, a remark by nineteenth-century physiologist Claude Bernard in his philosophical notebook: in physiology, he says, “there is a succession of evolving facts which follow each other in time, but which do not necessarily engender each other. It is a chain whose links do not have a relation of cause and effect, neither to the one that follows, nor to the one that precedes.”⁹ In differential reproduction, as perceived by Bernard and outlined in this chapter, there is no relation of cause and effect, no necessary development, no predetermined direction, but a chance to generate unprecedented events and a possibility to have them retroact on the system and so become concatenated.

Tacit Knowledge

The intricacy of this game of the possible is reflected in the personal experience of those who do research. As Bernard, again, has put it, “one must have felt one's way for a long time—*il faut avoir tâtonné longtemps*—, have been mistaken thousands and thousands of times, in short, have grown old in the practice of experimentation.”¹⁰ To feel one's way requires *Erfahrenheit* on the part of the experimenter. “Being experienced,” as Fleck uses the expression, is not simply “experience.”¹¹ Experience enables us to judge a particular piece of work or a particular situation. Being experienced enables us to literally embody the judgment in the process of making new experiences, that is, to think with our body. Experience is an intellectual quality. *Erfahrenheit*, that is, acquired intuition, is a form of life. There is a countersense to this expression that is essential to the meaning it is intended to convey. *Erfahrenheit* has to be acquired, and it transcends what *can* be learned. It amounts to what Michael Polanyi has called the “tacit component,” the “tacit dimension” of knowledge, or “tacit knowing.”¹² It lines up with other attempts to do justice to the “intimacy” of scientific work, to the exuberance of science in action, to what is beyond methodological axiomatization, to “cunning reason,” to the fact that plans of intelligent actors do not control action in any strictly definable sense of the word, so that when it comes to in situ action, “you rely not on the plan” but on embodied skills.¹³ In Polanyi's vision of personal knowledge, “tacit knowledge” and “explicit knowledge” do not simply coexist in a happy relation of complementarity. He claims that knowing in general, from everyday practical performance to

knowledge involved in productive scientific research, is either tacit or rooted in tacit knowledge, thus suggesting that wholly explicit knowledge is simply unthinkable, an illusion produced by analytical philosophy. “Subsidiary awareness” is an integral part of the gestural package of any researcher.¹⁴

Embodied reasoning does not for these reasons proceed without rules. But “the aim of a skillful performance is achieved by the observance of a set of rules which are not known as such to the person following them.”¹⁵ With respect to the type of biochemical reasoning explored in this book, several of such implicit rules can be invoked. Although they can be made explicit, their efficiency rests on being subsidiarily present in the design of an experiment. The first may be called a “symmetry principle.” Symmetry considerations govern the arrangement of epistemic and procedural controls within an experimental setup. Usually they take the form of testing all possible combinations of different components within a multi-componential assay. We have seen an instance of this procedure when looking at Siekevitz’s *in vitro* protein synthesis protocol in Chapter 4. A second rule may be called the “homogeneity principle.” It refers to the precautions to be taken in the use of preparations of cellular compounds when data sets obtained from different preparations are compared. Never change the batch of material within a single set of experiments, and always repeat the last assay when you try a new batch. The third rule is an “exhaustion principle.” It says that the series of similar compounds to be tested within one and the same experimental context should always be complete. Never leave out one of the compounds because you think that it does not work anyhow.

All these rules are learned through action, and their implementation can take widely different forms in different experimental contexts. They constitute, in a certain sense, a materialized and exteriorized system of imagination. They constitute a kind of experimental spider’s web: the web must be meshed in such a way that unknown and unexpected prey is likely to be caught. The web must “see” what the spider actually is unable to foresee with its unaided senses. But the web must not become too rigid. In deliberating upon the manner in which a system is to be handled so as to let the unknown intrude and invade it, Max Delbrück has spoken of a “principle of measured sloppiness.”¹⁶ “If you are too sloppy, then you never get reproducible results, and then you never can draw any conclusions; but if you are just a little sloppy, then when you see something startling you [nail] it down.”¹⁷ Connivance rather than stubborn rigor is at

the center of the experimental enterprise. If there is a principle at all that guides the experimental roadmanship, it consists in “being attentive to the answers arising on the margins or even outside the expected discourse.”¹⁸

Difference

We can restate this dynamic, soft, and subtle pattern of embodied and situated knowledge production in more philosophical and general terms and say with Gilles Deleuze, the thinker of the difference: “Difference and repetition have taken the place of the identical and the negative, of identity and contradiction. For difference implies the negative, and allows itself to lead to contradiction, only to the extent that its subordination to the identical is maintained. The primacy of identity, however conceived, defines the world of representation. But modern thought is born of the failure of representation, of the loss of identities, and of the discovery of all the forces that act under the representation of the identical. The modern world is one of simulacra.”¹⁹ I will come back to Deleuze’s promulgation of the foundering of representation in contemporary thought in Chapter 7. In the present context, my concern is with identity and contradiction. Identity and contradiction have been the key terms of the grand philosophical systems of the eighteenth and nineteenth centuries, from Kant to Frege, from Hegel to Marx. In contrast, difference and repetition are the key terms of a “philosophy of epistemological detail,” of a “differential scientific philosophy” in the Bachelardian sense.²⁰ This shift of attention allows us “to make all these repetitions coexist in a space in which difference is distributed.”²¹ Where identity is mute, repetition confers tacit knowledge. Where contradictions have to be resolved, differences can coexist and thus enter the game of becoming prominent or marginal, displaced or articulated. Research systems in particular must allow for the emergence and distribution of differences, as we have seen in Chapters 3 and 4. Experimenters are not interested in identities; they proceed in the search for “specific differences.”²² It is not astonishing that observers of laboratory shoptalk have come to recognize that there is a “preference for disagreement” at the bench as an implicit strategy for producing novel, “not previously obvious” features, and that “in practice, it seems, participants prefer a principle of variation over replication.”²³ Replication aims at identity, repetition at variation.

Several remarks are in order with respect to the differential reproduction of an experimental system. First, one never knows precisely how the

setup differentiates. As soon as one knows exactly what it produces, it is no longer a research system. This makes me hesitate to speak about experimental systems in terms of sheer “systems of production,”²⁴ an expression borrowed from economic life and laden with connotations such as directivity, efficiency, quantitation of output, and automation. “No method has ever led to an invention.”²⁵ An experimental system that gradually acquires contours, creates resonance between different representations, and conveys manageable meanings to stabilized signals, must create at the same time a space for the emergence of things unheard of. Inasmuch as it is stabilized in a certain respect, it can be destabilized in another. To arrive at new results, the system *must* be destabilized—but without a previously stabilized system there will be no “results.” Stabilization and destabilization imply each other. In the language of the laboratory, the result is that unit in which the dynamics of an experimental system is captured. It is not a final statement, it is a piece that fits or does not fit in an ongoing puzzle. To remain productive in an epistemic sense, an experimental arrangement must be sufficiently open to generating unprecedented events by incorporating new techniques, instruments, model compounds, and semiotic devices. At the same time it must be sufficiently closed to prevent a breakdown of its reproductive coherence. It has to be kept at the borderline of its breakdown. In this respect, too, experimental systems in science obey a “rule of order” similar to that of formal sequences in the production of art.²⁶

The minds of inventors and scientists, much like those of artists, are not oriented toward recognizing what exists; they “turn more upon future possibilities, whose speculations and combinations obey an altogether different rule of order, described here as a linked progression of experiments composing a formal sequence.”²⁷ The classification into sequences, according to Kubler, “stresses the internal coherence of events, all while it shows the sporadic, unpredictable, and irregular nature of their occurrence.”²⁸

If research systems become too rigid, they turn into devices for testing, into standardized kits, into procedures for making replicas. They lose their function as machines for making the future. Such transformations, however, happen regularly and are not necessarily dead-end streets. Epistemic things turned into technical objects become integrated as stable subroutines into other, still growing experimental systems and may help to produce unprecedented events in different contexts. The transformation of research objects into stable subroutines of other research arrange-

ments is what confers that special kind of material information storage, or mass action, to the process of experimentation. But by the same token, this process generates a historical burden. With Norton Wise, we could speak of the “resistance” or “resilience” of such a network, whose “structure as a whole puts severe constraints on what it means to know or to explain, or indeed for a thing to exist.”²⁹ Most new epistemic things, therefore, take their first shape from old tools. Yet in the long run and as a rule, technical procedures are completely replaced by subroutines that embody the actual, stabilized knowledge in a subtler way. The historian of science usually looks at a museum of abandoned systems. The liver slice system that provided the starting point for characterizing malignant growth in terms of protein synthesis, described in Chapter 3, was completely replaced within a few years by a fractionated homogenate. There is a life cycle to experimental systems. They are brought into being as research devices, become transformed into kits, and finally are replaced. But there is a symmetrical counterpart to this cycle. Kits can become destabilized and turned into research devices, either by transplantation or by the introduction of new representational techniques.

Différence

To remain a research system, an experimental arrangement must be managed in such a way that it keeps being governed by difference. I use the term *difference* to characterize the specific, displacing dynamics that distinguishes the research process. An experimental system that is organized in a way such that the production of differences becomes the orienting principle of its own reproduction is governed by and at the same time creates that kind of subversive movement Derrida has called *différance*. Pointing to an “irreducible absence of intention” that goes along with all invention, the term conveys a sense for the “event-riddleness” that is at the heart of research experiments.³⁰ Perhaps the notion discloses its meaning best by way of a language game. It is a word that exhibits its difference to “difference” only in written form and that, when spoken, is made silent by the very act of its pronunciation. With it, Derrida intends to capture a movement he calls “the production of differing/deferring,” which is bound to the radical exteriority of inscription, of writing as well as of producing other material traces.³¹ *Différance* has become the shibboleth of a literary fashion—deconstruction. But in the eyes of its inventor, it is not just a strategy of literary criticism, as opposed to hermeneutics. If a

strategy at all, “one might call this blind tactics, or empirical wandering.”³² Nor is deconstruction a mere intellectual attitude. It describes a movement that pervades all sorts of spaces of experience, their material constitution as well as their discursive framing. “Deconstructions,” as Derrida prefers to say in the plural, are specified by “a certain [metonymic] dislocation which repeats itself regularly [in] every ‘text,’ in the general sense I would like to attach to that name, that is, in experience as such, in social, historical, economic, technical, military ‘reality.’”³³ And let me add that these repeated dislocations occur particularly in the experience of scientific experimentation. “Differential” reproduction in the sense of a permanent dislocation of epistemic entities is precisely what endows a research system with its generative power, and what renders the process genuinely *historial*.³⁴ This second Derridean neographism hints at the special temporal character of the *differance*. “Historial *differance*” is an operator, not a parameter, of a system.

The attentive examination of experimental systems and the networks they constitute will help us to gather indispensable information for a future “differential typology of forms of iteration.”³⁵ The epistemology and history of biology can contribute to such a typology precisely because the life sciences are so prolific in creating these specific forms of iteration we call experimental systems. We have to pay much more attention to the synchronic as well as diachronic dimensions of these forms of transfer, their spacing, and their power of dissemination. The characterization of such transfer patterns has been initiated by Isabelle Stengers under the notions of “operations of propagation” and “operations of passage.”³⁶

This book is about the iterative enforcement of a local research system and its subsequent dissemination. I have chosen to narrate the history of the rat liver *in vitro* system of protein biosynthesis in sufficient detail so that the experimental moves at the microlevel become visible, the fortunate choices as well as the abortive trials. As I mentioned in Chapter 3, the Huntington Memorial Hospital, under the directorship of Joseph Aub, had embarked upon a program of cancer research based on the study of growth phenomena. Zamecnik’s group at MGH had spent the first years of work looking at growth deregulation in malignant tissue within the framework of a biomedically oriented research environment. Protein synthesis was assumed to be a possible target of the neoplastic behavior of cancer cells. But between 1947 and 1952, through a series of differential, dislocating experimental events, the emphasis shifted. The perspective on cancer became deferred, and although it was not completely and abruptly

abandoned, it glided into, was subverted by, and became reoriented as an inquiry into the conditions of cell-free incorporation of amino acids into proteins of normal cellular tissue. A first *in vitro* system was in place in 1952. I will now outline its differential reproduction between 1952 and 1955.

Chapter 1

1. Misleadingly translated as “scientific reality” (Bachelard 1984, p. 6). French and German texts are quoted from translations where they exist and when I had access to them. Occasional alterations have been made, however, where I have judged them to be necessary. All translations from the French or German original are my own.
2. Freud 1957b, p. 117.
3. Freud 1957a, p. 77.
4. Fleck 1979, pp. 23–27.
5. Elkana 1970.
6. Löwy 1992. For the notion of “boundary object,” see Star and Griesemer 1988.
7. Blumenberg 1986.
8. Serres 1989, pp. 4, 15.
9. Fleck 1979, p. 78.
10. Reichenbach 1938, pp. 6–7.
11. Holmes 1985, p. xvi.
12. See Latour 1990a, pp. 160–64, with reference to Serres 1987.
13. Kuhn 1962; Feyerabend 1975. For the post-Kuhnian engagement, see, among others, Latour and Woolgar 1979; Knorr Cetina 1981; Hacking 1983; Lynch 1985; Collins 1985; Shapin and Schaffer 1985; Franklin 1986, 1990; Galison 1987; Latour 1987; Gooding, Pinch, and Schaffer 1989; Gooding 1990; Le Grand 1990; Lynch and Woolgar 1990b; Pickering 1992, 1995; Rheinberger 1992a; Buchwald 1995; Rouse 1996.
14. Hacking 1983, p. 150.
15. Galison 1988, pp. 209, 211.
16. Lenoir 1988, pp. 11–12.
17. Lenoir 1992; see also Shapin and Schaffer 1985.
18. Pickering 1992.
19. For key texts of the strong program of sociology of science, see Barnes 1974; Bloor 1976; Barnes 1977; Collins 1985.
20. Kuhn 1992, p. 9.
21. Shapin and Schaffer 1985.
22. The notion is borrowed from William Wimsatt’s account of biological models of developmental constraint. See Wimsatt 1986.
23. Latour 1987, pp. 132–44.
24. Pickering 1995. Pickering’s book came to my attention only after completion of this manuscript.
25. Derrida 1978, pp. 283–84.
26. Ibid., p. 284. Note the proximity of the formulations in this passage to Freud’s text on *Narcissism*; see note 3 in this chapter.
27. Derrida 1976, p. 24.
28. Latour 1993a, p. 6.
29. Latour 1990b, p. 64.
30. Latour 1993a, p. 142.
31. Hoagland 1990, p. xvi.
32. Rheinberger 1992a and 1992b.
33. Turnbull and Stokes 1990.
34. Kohler 1991b, and 1994.

35. Cf., e.g., the recent special issue on “Immunology as a Historical Object” of the *Journal of the History of Biology* 27:3 (1994); see also Rabinow 1996.
36. See Luhmann 1990, especially chapter 5, “Science as System,” pp. 271–361.
37. Hoagland 1990, p. xx.
38. Bachelard 1984, p. 13; Bachelard 1951, p. 84.
39. Bachelard 1984, p. 6; I have altered the translation slightly.
40. Wittgenstein 1953, paragraph 7.
41. Ibid., paragraph 654.
42. See, e.g., Latour 1987; Wise 1992; Hentschel 1993.
43. Kant 1987, p. 264.
44. Rouse 1991, p. 161.
45. Guggenberger 1991.
46. Lacan 1989, p. 22.
47. Bachelard 1968, p. 12.
48. Polanyi 1965, fourth lecture on “The Emergence of Man,” pp. 4–5. Quoted in Grene 1984, p. 219.

Chapter 2

1. Zamecnik, Keller, Littlefield, Hoagland, and Loftfield 1956. The symposium was held at the Research Conference for Biology and Medicine of the Atomic Energy Commission, Oak Ridge National Laboratory, Gatlinburg, Tennessee, April 4–6, 1955.
2. Lacan 1989, p. 10.
3. Lacan 1986, p. 122.
4. Judson 1979, p. 275. Jacob renders the dictum in the following form: “Al Hershey, one of the most brilliant American specialists on bacteriophage, said that, for a biologist, happiness consists in working up a very complex experiment and then repeating it every day, modifying only one detail” (Jacob 1988, p. 236).
5. Jacob 1988, p. 234.
6. Heidegger 1977b, p. 118. “Die Zeit des Weltbildes” might be more appropriately translated as “The Epoch of Planetary Configuration.”
7. Holmes 1985, p. xvi.
8. Goethe 1988. A more accurate translation of “Der Versuch als Vermittler von Objekt und Subjekt” would be “The Assay as Mediator of Object and Subject” (emphasis added).
9. Kittler 1990.
10. Goethe 1988, p. 16.
11. Staiger 1966, letter of Goethe to Schiller, January 10, 1798.
12. Ibid., letter of Schiller to Goethe, January 12, 1798, pp. 539–42.
13. Popper 1968, p. 107.
14. Collins 1985.
15. Fleck 1979, p. 96, emphasis added. Ludwik Fleck, mainly because of his

notion of "Denkstil," has often been misconstrued as a forerunner of the Kuhnian way of thinking in "paradigms."

16. Fleck 1979, p. 86.

17. Kuhn 1992, p. 14.

18. Jacob 1988, p. 9.

19. Lenoir 1992. See also Lenoir 1988, where a number of related positions are discussed.

20. Bachelard 1984, p. 139.

21. Ibid., chapter 6. See also Bachelard 1968.

22. As far as the history of molecular biology is concerned, one of the most brilliant examples is Jacob 1988. In recent years, we have witnessed a rapidly accumulating body of autobiographies from molecular biologists of the first generation. See, among others, Watson 1968, Luria 1984, McCarty 1985, Crick 1988; Kornberg 1989; Hoagland 1990. For a review of some of these works, see Abir-Am 1991.

23. For a more fine-grained analysis, see Hentschel 1995.

24. Serres 1987, p. 191.

25. Latour 1987, pp. 87–88.

26. For the notion of "constraint," see Galison 1995.

27. Sanger, Nicklen, and Coulson 1977.

28. Latour 1987, p. 131 and elsewhere.

29. Fleck 1979, p. 86, emphasis in first sentence omitted.

30. For more on scientific texts in biology, see Myers 1990; see also Bazerman 1988.

31. E.g., Latour 1987, p. 174.

32. Bachelard 1984, p. 13.

33. Heidegger 1971, p. 74.

34. Ibid., p. 75.

35. Hoagland 1990, p. xvi.

36. "Erstreckte Gegenwart," as Helga Nowotny has called it (Nowotny 1994, p. 52).

37. See Jardine 1991.

38. Weber 1989, p. 982.

39. See Damerow and Lefèvre 1981, pp. 223–33; Rohbeck 1993, especially chapter 6.

40. Bachelard 1984, p. 8; translated here misleadingly as "revealing light." For the notion of recurrence, see also Bachelard 1951, chap. 1, "Les récurrences historiques: Epistémologie et histoire des sciences."

41. Butterfield 1957.

42. See, e.g., Mayr 1990. On the problem of "whig history," see also Clark 1995.

43. It is therefore not simply a tautological joke when Francis Crick proposed, on "doubtful grounds," as he admitted with an ironically self-deprecating gesture, that molecular biology "can be defined as anything that interests molecular biologists." See Crick 1970, p. 613.

44. See, e.g., Cairns, Stent, and Watson, 2d ed. 1992; Rich and Davidson 1968; Monod and Borek 1971; Lwoff and Ullmann 1979.

45. Burian 1996.

46. Scholarly historical work on the subject is still greatly lacking. For some information, see Portugal and Cohen 1977; Judson 1979; Bartels 1983; Rheinberger 1992b, 1993, 1995, 1996; Burian 1993a; Morange 1994, especially chapters 12 and 13.

47. Cf. Kohler 1991a.

48. For a detailed discussion, see Chapter 7 in this volume.

49. Bachelard 1984, p. 6.

50. Rotman 1987, p. 102.

51. Knorr Cetina, Amann, Hirschauer, and Schmidt 1988.

Chapter 3

1. Zamecnik and Stephenson 1978; Stephenson and Zamecnik 1978; Agrawal, Ikeuchi, Sun, Sarin, Konopka, Maizel, and Zamecnik 1989; Zamecnik and Agrawal 1991; see also Lunardini 1993.

2. Zamecnik, letter to Rheinberger, November 5, 1990.

3. Ibid.

4. Faxon 1959, p. 229; Aub, Brues, Dubos, Kety, Nathanson, Pope, and Zamecnik 1944; see also a series of six consecutive papers in the *Journal of Clinical Investigation* 24 (1945), starting with Nathanson, Nutt, Pope, Zamecnik, Aub, Brues, and Kety 1945.

5. The history of the Massachusetts General Hospital and its research facilities is well documented. Cf. Faxon 1959; Garland 1961; Castleman, Crockett, and Sutton 1983.

6. Zamecnik 1960, 1962a, 1969, 1976, 1979, 1984; Siekevitz and Zamecnik 1981; Hoagland 1990, 1996. See also Rheinberger 1993.

7. See Faxon 1959, pp. 204–7, 231–40; Castleman, Crockett, and Sutton 1983, pp. 343–50. See also Zamecnik 1974, 1983; Bucher 1987; Hoagland 1990, pp. 37–39.

8. Zamecnik, research notes (ZRN), draft "for International Cancer Research Foundation/application, 3/8/45."

9. Loftfield, correspondence on a draft of this manuscript, May 17, 1993 (abbreviated as Loftfield, correspondence).

10. Zamecnik 1950, p. 659.

11. Ibid., p. 660.

12. On the neglected history of the "multi-enzyme programme of protein synthesis," see Bartels 1983.

13. See Zamecnik and Lipmann 1947; Zamecnik, Brewster, and Lipmann 1947.

14. Faxon 1959, p. 48.

58. See MGHR, Committee on Research Minutes, books 1–4, 1947–59. For a history of the Office of Naval Research, see Sapolsky 1990.
59. MGHR, Committee on Research, book 2, February 1951–December 1953, p. 325.
60. Loftfield, correspondence.
61. Loftfield, interview, 1993.
62. See Kay 1993.
63. Interview with Paul C. Zamecnik, March 16, 1990.
64. Siekevitz, letter to Rheinberger, July 1, 1994.

Chapter 5

1. Jacob 1988, p. 274.
2. Ibid., p. 255.
3. Kubler 1962, p. 125.
4. Heidegger 1977b, p. 124.
5. Jacob 1982.
6. Ibid., p. 11; I have altered the translation.
7. Derrida 1976, pp. 23–24 and elsewhere.
8. Ibid., p. 24.
9. Bernard 1954, p. 14.
10. Bernard 1974, p. 15.
11. Fleck 1979, p. 96.
12. Polanyi 1958, 1967, 1969, especially part 3.
13. For the quotations here, see Keller 1983, p. 198, MacColl 1989, p. 90; Elkana 1981, pp. 42–48; and Suchman 1990, p. 310, respectively.
14. Polanyi 1969, pp. 138–58.
15. Polanyi 1958, p. 49, emphasis in original omitted.
16. “It is the old story of the principle of measured sloppiness that leads to discovery” (Fischer and Lipson 1988, p. 184). Fischer quotes from a letter of Max Delbrück to his friend Salvador Luria dating from autumn 1948.
17. Here, Delbrück is quoted from a meeting in Oak Ridge in 1949 (Fischer and Lipson 1988, p. 184).
18. The quotation is taken from Dagognet’s characterization of Claude Bernard (Dagognet 1984, p. 18).
19. Deleuze 1994, p. xix.
20. Bachelard 1968, p. 12.
21. Deleuze 1994, p. xix.
22. Loftfield, interview, 1993.
23. Amann and Knorr Cetina 1990, pp. 104, 111.
24. Kohler 1991b.
25. Serres 1980, p. 126.
26. Kubler 1962, p. 33 and the following.
27. Ibid., p. 85.
28. Ibid., p. 36.
29. Wise 1992, p. 34.
30. Derrida 1988, pp. 18–19.
31. Derrida 1976, p. 23.
32. Derrida 1982b, p. 7.
33. Derrida 1991, pp. 26–27.
34. Derrida 1976, p. 24. I will come back to this notion in Chapter 11.

35. Derrida 1988, p. 18.
36. Stengers 1987.

Chapter 6

1. Bachelard 1968, p. 94.
2. Zamecnik 1979, p. 296.
3. Stephenson, interview, 1991.
4. I will come back to the *E. coli* system in Chapter 12.
5. MGHR, Committee on Research, Executive Committee Minutes, book 2, January 1951–December 1952.
6. Gale and Folkes 1953c.
7. Gale and Folkes 1953b, p. 728. For a more extended account of Gale’s work, see Rheinberger 1996.
8. St. Aubin and Bucher 1951.
9. Frantz, letter to Rheinberger, July 7, 1994.
10. A buffer system highly fortified with cofactors and metabolic substrates.
11. Bucher, interview, 1993.
12. Bucher 1953; Frantz and Bucher 1954.
13. Originally, it had been used to stabilize suspensions of mitochondria. See Chapter 4.
14. Siekevitz 1952.
15. MGHR, Committee on Research, Executive Committee Minutes, book 3, January 1953–December 1954.
16. Zamecnik and Keller 1954, p. 338.
17. For a detailed account, see Rheinberger 1995.
18. They were not alone in observing such stimulation. Winnick (1950) had reported a stimulatory effect of ATP on amino acid incorporation in fetal liver homogenates and Greenberg on particles sedimented at low speed (Peterson and Greenberg 1952; Kit and Greenberg 1952).
19. Lipmann 1941, 1949.
20. It amounted to \$10,406.88 for the period July 1, 1954 to June 30, 1955. MGHR, Committee on Research, book 2, February 1951–December 1953.
21. Letter of Zamecnik to Dean A. Clark, MGHR, Committee on Research, Executive Committee Minutes, book 3, January 1953–December 1954.
22. ATP, phosphocreatine, and the enzyme creatine phosphokinase were included.
23. Keller and Zamecnik 1954, p. 240.
24. Zamecnik and Keller 1954, p. 337.
25. Ibid., p. 351.
26. Stephenson, interview, 1991.
27. See, e.g., Peterson and Greenberg 1952.