TOWARD A HISTORY OF EPISTEMIC THINGS

Synthesizing Proteins in the Test Tube

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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]

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.

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"[9]—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 of a particular research activity. They are to be taken as a profound reorientation 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

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 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

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 differences 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 unexpected 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 manufactory 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 7

Spaces of Representation

> Thus everything depended on the representation we formed of an invisible process and on the manner of its translation into visible effects.
>
> —François Jacob, *The Statue Within*

Given its formal dynamics of differential reproduction, how does an experimental system deploy its epistemic power? If we follow Jacob, everything depends "on the representation we [form] of an invisible process and on the manner of its translation into visible effects." This formulation is ambivalent. Is representation just a manner of rendering the invisible visible, something hidden but ready to be disclosed? Is it a hide-and-seek game? Or is it a manner of translation, which literally means converting signs into other signs, traces into other traces, concatenating transformations? Is it a tracing game?

Whatever escape we may seek, when it comes to the heart of what the sciences are about, we touch on representation. The sciences, so the story goes, aim at a specific, in-the-limit, "true" representation of the world. This was the grand project leading to enlightenment: science's duty is to represent the world as it is in order to make its domination possible. Two accompanying metanarratives of how this project might be realized developed in the seventeenth and eighteenth centuries: empiricism and rationalism. Empiricism claimed that true representation comes through the undisturbed, if aided, senses and saw itself as based on observation. But then, how to dominate? Rationalism claimed that representation comes through actualized concepts and thus saw itself as based on intervention. But then, how to represent? Since Kant's critical attempt to avoid the conventionalist pitfalls of empiricism as well as the constructivist pitfalls of rationalism by grounding the possibility of experience on some transcendental conditions of reasoning, philosophy has become uneasy with both solutions. This uneasiness has persisted until today, as has the distinction between empiricism and rationalism, observation and experiment, and induction and deduction, under various labels.

The Meanings of Representation

"So what are we talking about when we speak of representation?"[1] Intuitively, we connect "representation" to the existence of something that is represented. "In sum, representation of an object involves producing another object which is intentionally related to the first by a certain coding convention which determines what counts as similar in the right way."[2] Upon closer inspection, however, the term reveals itself to be polysemic. If we speak about the representation of something given, the common sense of the notion is plain: we speak about a representation "of." If, however, we claim that we have seen the actor Bruno Ganz yesterday evening representing Hamlet, we speak of a representation "as." In this case, representation takes on a double meaning: that of vicarship and that of embodiment. Every play is governed by this tension, this "paradoxical trick of consciousness, an ability to see something as 'there' and 'not there' at the same time."[3] If, finally, a chemist tells us he or she has produced or represented a particular substance in his or her laboratory, the meaning of "representation of" is gone, and instantiation in the sense of the production of a particular substance has taken over. In this latter case, we deal with the realization of a thing. There is a continuum from vicarship to embodiment to realization.

These three connotations of commonplace *representation* have their equivalent in scientific representation. Roughly speaking and without unnecessarily stretching the parallel, we are, in the first case, accustomed to speaking of analogies, of hypothetical, more or less arbitrary constructs (symbols in C. S. Peirce's sense). In the second case, we may speak of models or simulations (Peirce's icons). In the third, we deal with an experimental realization (comparable to an index in Peirce's semiotic system, i.e., a trace).[4] Jacob argues in a similar fashion, although casting the triplet in a questionable hierarchical order, when he claims that experimental biology proceeds from "analogies" to "models" to "concrete models."[5] It appears historically contingent and a matter of case-by-case evaluation as to which of them figures prominently within a given scientific context. Note, in addition, that I am speaking here of the function of representation on the level of scientific practice itself, as it gets enacted in the materialities of the laboratory. The transition to the presumably pure semiological realm and hence to the problem of how the results of this work are graphically and linguistically recorded is certainly not an abrupt one. It proceeds continually and through any number of intermediates. We might easily recognize here again the three modes of representation, and we could follow their trajectories. I am not concerned here in the first place with the relation between theory and reality, between concept and object as such. With that, we would locate ourselves on another level—a level that a long tradition of analytical philosophy has judged to be the *Kampfplatz* of philosophy of science. I am concerned with describing the process of making science as a process in which traces are generated, displaced, and superposed, in the sense of the connotations of representation just mentioned. I contend that if the perspective of a clear-cut dichotomy between theory and reality, between concept and object, is being adopted precociously, this process tends to disappear from sight.

An Inextricable Interconnection

In recent years, the issue of representation in scientific practice has received increasing attention, not only from philosophers, but also from historians and sociologists of science and culture.[6] Let me stress that representation, as viewed in the previous section, not only tends to be "overrated," but should be put "*sous rature*."[7] First to be put under erasure must be the traditional meaning of *representation* as referentiality, its association with what has been called the copy theory of representation. I claim with Nelson Goodman that "the copy theory of representation [is] stopped at the start by inability to specify what is to be copied."[8] There is no such thing as a simple representation of a scientific object in the sense of an adequation or approximation of something out there, either conceptually or materially.[9] Upon closer inspection, any representation "of" turns out to be always already a representation "as." As Michael Lynch and Steve Woolgar note, "representations and objects are inextricably interconnected; [objects] can only be 'known' through representation."[10] Basically, my argument is that anything represented, any referent, as soon as we try to get hold of it, and, concomitantly, as soon as we try to shift it from subsidiary to focal awareness, is itself turned into a representation. As a result, the term loses its referential meaning. Engaging in the production of epistemic things means engaging in the potentially endless production of traces, where the place of the referent is always already occupied by another trace. To use a terminology familiar from linguistics, there is a permanent gliding replacement of any presumed "signified" by another "signifier."[11] Science, viewed from a semiotic perspective, does not escape

the constitutive texture of the inner workings of any symbol system: metaphoricity and metonymicity. Its activity consists in producing, in a space of representation, material metaphors and metonymies. From semiotics, we have learned that symbols take their meaning, not from the things symbolized, but from their relation to other symbols. At this point, it is more important to stress that the sciences share this structure with other symbolic/material worlds than to follow the worn tradition of specifying what makes them distinct.

Second, there is no representation without a chain of representations, with the immediate caveat that this is, as Claude Bernard already put it (although he did not make available the statement to the public), "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."[12] In the final analysis, then, the activity of scientific representation is to be conceived as a process without "referent" and without assignable "origins."[13] We deal with a "strange, transversal object, an operator of alignment whose power of veridiction resides in that it permits us to pass from a preceding transformation to that which follows."[14] As paradoxical as it may sound, this is precisely the condition of the often touted objectivity of science and of its peculiar historicity as well. If we accept this statement, any possibility of a deterministic referential account of science, be it based on nature or on society, is excluded.

Graphemes in Spaces of Representation

A scientific object investigated in the framework of an experimental system such as the in vitro system of protein synthesis is, as we have seen, articulated from material traces, or graphemes, within particular spaces of representation. Spaces of representation are coordinates of signification. They are opened as well as limited through the technicalities of the system. They disrupt the immediacy of presence of a phenomenon by rendering it as a mark; they are forms of iteration in a differential typology whose most obvious and prolific form, according to Derrida, is "writing."[15] Graphemes and spaces of representation do not exist independently. They mutually engender each other. There is no space of representation prior to an articulation of graphemes. And outside such a space, a particular graphematic trace remains without assignable meaning. Graphemes are what logicians abhor: elements without simplicity, that from which the simple arises through a process of degeneration. "Even before being determined as human (with all the distinctive characteristics that have always been attributed to man and the entire system of significations that they imply) or nonhuman, the *grammè*—or the *grapheme*—would thus name the element. An element without simplicity. An element, whether it is understood as the medium or as the irreducible atom, of the arche-synthesis in general."[16] We recognize, in this passage from Derrida's *Of Grammatology*, the fundamental form of Latour's conception of networks of human/nonhuman actants, although Latour certainly would not like to see the empire of his "hybrids" qualified as Derridean.[17]

Articulations of graphemes, or systems of signification, within the limits of an experimental setting, constitute the objects of a science. They channel the noise produced by the research arrangement and translate it into further traces, graphemes, "inscriptions," or "marks."[18] Graphemes, in the first place, are material articulations of significant units. Of course, they can be, and usually are, transformed into elements of more formalized spaces of representation. But this is not what makes them work as such. What makes them work is their oscillation between "density" and "articulation," a distinction that Nelson Goodman introduced in order to be able to conceive of hybrids of all sorts between continuous and discontinuous marks, from pictures to texts, and of all sorts of diagrammatic transitions on the basis of a "grammar of difference." Together with the transverse continuum between analogies, models and realized traces, this grammar of representative differences forms a system of coordinates in which resemblance has disappeared as a criterion of evaluation and which leaves room for all sorts of hybrids, these warrantors of "innovation, choice and unprecedented shifts."[19] Latour, too, takes representation as a particular kind of activity, as a process of inscription that results in a particular category of things, called "immutable mobiles."[20] They are characterized, not by what they depict, but by how they work. Immutable mobiles fix transient events (make them durable), and in doing so, allow them to be moved in space and time (make them available in many places). This is their power. What is significant about representation qua inscription is that things can be re-presented outside their original and local context and inserted into other contexts. It is this kind of representation that matters. Inscriptions are thus not mere abstractions. They are durable and mobile purifications, which in turn are able to retroact on other graphematic articulations—and, what is most important, not only on those from which they have originated.

Graphematic articulations are the material forms of the epistemic

things under investigation. Their materiality renders them recalcitrant to the theoretical forms of coherence we would like to impose on them. Their recalcitrance perpetuates the game of the "machine for making the future." Whether the traces that are produced in an experiment will prove "significant,"[21] depends on their capacity to become reinserted into the experimental context and to produce further traces. No experimental work can escape this recursive action, this iterative process of detaching an inscription from its transient referent and turning the referent itself into an inscription. The significance of an experimental event is its further signification, which, necessarily, comes ex post. If there is anything special about *scientific* inscriptions, it is their specific form of differential iteration, the complexity of which resists any shortcut description and which, therefore, I have tried to assess through the case study in Chapters 3, 4, 6, 8, 10, 12, and 13.

Experimental representation, then, may be taken to be equivalent to bringing epistemic things into existence. In their transiently stable forms, they may act as embodiments of concepts, as "reified theorems," to use Bachelard's expression.[22] But once reified, they are no longer interesting per se for those who do research. They remain interesting only as tools, as technical objects for constructing novel research arrangements. Jacob again: "Already, the results we had obtained no longer interested me. The only thing that mattered was what we were going to do with this tool."[23]

Uncertainty, fuzziness, and fugacity are at the heart of the experimental process of producing graphemes. The "epigraphy of matter" proceeds through groping.[24] "The abortive trials, the failed experiments, the false starts, the misguided attempts" prevail.[25] In the end, it is only the "*Verfahrungsart*" that counts. Goethe's essay on "The Experiment" closes with the following words: "The method [*die Verfahrungsart*] itself will fix the bounds to which they [imagination and wit] must return."[26] In this return, stratagems are at work that Derrida has characterized as "wandering in the tracing of *différance*."[27] Bernard, somewhat more prosaically, has described this process as "*tâtonnement*."[28] In it, there is no overarching telos, no ultimate perspective, no vanishing point at which the movement of research could come to a rest. This state of affairs is intimately related to the nature of the means by which the experimental game of producing graphemes is realized. The epistemic techniques through which it engenders its inscriptions lead, again and again, to unprecedented excesses that cannot be anticipated but appear only in the making. Through engagement in this game, the purpose of representation is continually subverted. As Goodman has put it, to represent is not to "mirror," but to "take and make."[29] All representation is production/reproduction, and the "eventuation" of epistemic things is distinguished by lack of reference to prototypes. Representation *is* "eventuation" (it is about intervention, invention, and the creation of events). Yet the ruse of this dialectic of fact and artifact consists precisely in that it functions by permanently deconstructing its constructivist aspect: the New does not enter through the obvious door but through some fissure in the walls.

Models

Let me try to approach the quandary of representation from a slightly different angle and to relate it to the story of this book. I have posited that, with an experimental system, a space of representation is established for engendering things that otherwise cannot be grasped as objects of epistemic action. Biochemical representations, in particular, create an extracellular space for reactions assumed to take place within cells. Traditional wisdom has it that such a representation constitutes a model of what is going on "out there in nature." Thus, biochemical "in vitro systems" would be models for "in vivo processes." But what goes on within the cells? There is no other way to learn about this than to provide a model for it. In other words, nature itself only becomes real, in a scientific and technical sense, as a model. Of course, there are *in vivo* experiments. But insofar as they are parts of a research arrangement, they are model systems too. The very necessity of representation in terms of modeling implies that unmediated evidence is excluded. The process of modeling is one of shuttling back and forth between different spaces of representation. Scientific objects come into existence by comparing, displacing, marginalizing, hybridizing, and grafting different representations with, from, against, and upon each other. It might be argued that this view amounts to standard conventionalism, if not relativism or even anarchism. I disagree. I would like to strongly assert the claim that it is not conventions by which scientists agree, for whatever reasons, that something is such and such. It is *convenience* in the etymological sense of the word: being drawn together in space and time, in a concurrent and recurrent gesture of involvement and involution.

The biochemist speaks of "model substances," of "model reactions," of "model systems," even of "model organisms." The concept of model, as it is used here, differs from its use in mathematical logic, where "models" are taken to be semantic interpretations of syntactic chains of signs.[30] The laboratory use of the term *model* is more instructive. The bench work language of the scientific practitioner translates with much more appropriateness what his work is actually about than what a particular philosophy of science declares him to be doing. The language games of scientific practice suggest that model things are substances, reactions, systems, or organisms particularly well suited for the production of inscriptions. The power of these "material generalities" resides in their ability to be disseminated through the capillaries of an experimental culture. More precisely, they create anastomoses through their spreading. Models are, to use laboratory language again, "ideal" objects of research in two respects: First, they are particularly well suited for experimental manipulation. This is the practical meaning of ideal. Second, they are idealized objects in the sense that they are, to a certain extent and in some respects, standardized, reduced, purified, isolated, contracted, and monofunctionalized entities that can be transported and subjected to local modifications. Models embody questions "accessible to laboratory experimentation."[31] But through this very embodiment, they also tend to change their "nature." Whether a bacterium, for instance, is considered as a model of genetic replication, of the production or function of antibiotics, or of the cause of certain diseases will materially change its structure with respect to its life as an epistemic thing.[32]

Nature as such is not a referent for the experiment; it is rather a danger. It counteracts the scientific enterprise. It is a constant threat of intrusion. If cells are fractionated, unfractionated cells have to be excluded from the space of representation. If one works with an in vitro system, every whole cell in it tends to behave as an artifact, a "whole cell artifact," as Zamecnik once aptly called it.[33] One must not contaminate an in vitro experiment with "nature." We end up with the uncomfortable situation that something we are used to calling natural, by remaining so, turns into something artificial. Consequently, we have to conclude that the reference point of an experimentally controlled system turns out to be another experimentally controlled system. The reference point of a model is another model. The basic rule of the scientific tracing-game reads: what engenders the traces can only be assessed by means of further traces. It is impossible to go behind a signifying chain, or "battery of signifiers." The quoted term is Lacan's, who uses it to indicate that the production of significance does not take place between the essence and the phenomenon but has a collateral constitution.[34] It is not the measuring devices that simply produce inscriptions *of* an object. The scientific object itself is cast into a traceable configuration. Temporally and spatially, epistemic things *are* and behave *as* inscriptions. The very movement of signification, the very making of scientific sense and non-sense, as the result of a structural necessity, turns presumptive referents, time and again, into signifiers.

As a result, the concept of model itself, much like that of representation, comes under erasure. The practice of modeling subverts the privileged position we grant to the model with respect to what is going to be modeled. Within the continuum between epistemic things and technical things, what we usually call a model occupies a middle position. As a rule, qua epistemic things, models are already sufficiently established to be regarded as promising areas of research and therefore to function as research attractors. On the other hand, they are not yet sufficiently standardized to serve as unproblematic subroutines in the differential reproduction of other experimental systems. Thus, an experimental model system has always something of the character of a supplement in the sense Derrida confers on the notion.[35] It stands for an entity that draws its effectiveness from its own absence. The supplement presents itself as a simple addition, but it has the potential to redirect the differential movement of the whole system. The subversion of an experimental system by a supplement shows both aspects characteristic of that movement: it tends to change the identity of the system's components, and it fails to do so by its very presence as a supplement. This is because a supplement, by definition, tends to be supplanted by the next. Likewise, a model is a model only in the perspective and by virtue of an imaginary reality at which it fails to arrive.

Traces

If epistemic things can be viewed as inscriptions, traces, and graphematic articulations, they can duly be addressed as grammatograms and, hence, generalized forms of "writing." The French notion of *écriture* is only inadequately grasped by its translation into writing. *Ecriture* is the "writing" and the "written," and it is the "how to be written" as well. It covers the graphematic space *and* the things from which it is built. According to Derrida, "to write is to produce a mark that will constitute a sort of machine which is productive in turn, and which my future disappearance will not, in principle, hinder in its functioning."[36] If epistemic things do not intrinsically display recordable marks that transform them into machines that become themselves productive, tracers are introduced into them: radioactive markers, fluorescent dyes, pigments, anything that gives inscriptions.

It is thus unnecessary to distinguish between machines that "transform matter between one state and another" and apparatuses or "inscription devices" that "transform pieces of matter into written documents."[37] What is a centrifugal fractionation? It transforms matter—it separates molecules—and it produces an inscription, say, a pattern of bands in the centrifugation tube. The argument has to be radicalized. The whole experimental arrangement—including both types of machines—has to be taken as a graphematic articulation. Written tables, printed curves, and diagrams are further transformations of a graphematic disposition of pieces of matter, a disposition that is embodied in the design of the experiment itself. Its elements—"elements without simplicity," "archisynthetic elements"[38]—are "big spots," "heavy peaks," "little shoulders," "yellow soups." Fractions, centrifugal pellets, and supernatants *are* a partition of the cytoplasm. They are handled as inscriptions. It is not simply the measuring devices that produce the inscriptions. The scientific object itself is shaped and manipulated "as" a traceable conformation. Temporally and spatially, the object *is* a bundle of inscriptions. It displays only what can be handled in this way.

What does the production of a chromatogram amount to? of a centrifugal partition? of an array of tubes to which pieces of filter paper are aligned, to which in turn counts per minute of radioactive decay are correlated? These are all material signs, entities of signification. The arrangement of these graphemes composes the experimental writing. What are graphemes made of? A polyacrylamide gel in a biochemical laboratory is, on the one hand, an analytical device for the separation of macromolecules; at the same time, it is a graphematic display of compounds visualized as stained, fluorescent, absorbent, or radioactive spots. It is a graphematic articulation. You cannot compare such an inscription, for example, a microsomal pellet, to microsomes tout court. You can only compare it to other representations to determine whether they match, whether they reinforce, displace, or marginalize each other. You can only compare it to other graphematic traces from other spaces of representation. Their matching gives us that sense of "reality" we ascribe to the scientific object under investigation. The "scientific real" is a world of traces. Both the concept of model as well as the concept of representation thus face the limits of their conventional meaning of imaging and delegation, of vicarship and correspondence. Upon close inspection, they assume the sense of a production of traces. There is a German word for the chemical process of production, characterization, isolation, and purification of a substance: *Darstellung*. This is what representation is about.

Scientific activity notoriously consists in undercutting the opposition between representation and referent, between "model" and "nature." Representation is not the condition of the possibility of getting knowledge about things. It is the condition of the possibility for things to become epistemic things. The scientific object is re-*presented* in being produced; and it is *re*-presented in the sense of a repetition, of an iterative act. Any production of representations is also, always already, a reproduction. I have developed this argument in Chapter 5. There is no external referent for this activity. Nature can be subjected to experimentation only insofar as it is already representation, insofar as it is an element, however marginal, of the game. Instead of conceiving of the epistemic activity of representing or modeling as an asymmetric relation, we should consider it as being symmetric: both terms of the relation are representations or models of each other. Instead of upholding the classical, biased relation between the signifier and the signified, we should displace it by an interconversion of signifiers: the very movement of signification turns the signified itself into a signifier, incessantly. The question of how the energy gets funneled into the process of protein synthesis, for example, successively took the form of its "oxygen dependence," of "inhibition by DNP," of "stimulation by a mitochondrial sediment," of a "mitochondrial factor," of an "ATP-regenerating system," just to mention a few of the translations we have encountered in the preceding chapters.

The production of inscriptions is not for that reason arbitrary. Although scientists are "always already within the theater of representation," they meet with "constraints."[39] For representations, in the long run, do count in scientific practice as far as they can be made coherent and resonant, as far as they take on consistency. Fortunately, on the other hand, this process is not deterministic. It is not just dictated by the technical conditions and the instruments involved in the endeavor. Producing traces is always a game of representation/depresentation. Every grapheme is the suppression of another one. Enhancing one trace inevitably means suppressing another one. In an ongoing research endeavor scientists usually do not know which of the possible traces should be depressed and which should be made more prominent. So, at least for shorter spans of time, they have to conduct the game of representation/depresentation in

a reversible manner. Epistemic things must be allowed to oscillate between different significations.

A Pragmatogony of the Real

In conclusion, things come to look differently from the viewpoint of a pragmatogony focusing on representation as rooted in and emerging from scientific practice. Ian Hacking, too, has sketched such a pragmatogony in the "Break" of his *Representing and Intervening*, which in turn certainly shares elements with Edmund Husserl's comments on the origin of geometry.[40] "*Human beings are representers*," says Hacking. "Not *homo faber*, I say, but *homo depictor*. People make representations."[41] But Hacking explicitly does not want to take representations as mental representations or as visual images in the first place. He wants to take them as physical objects that owe their character of "likeness" to the process of their own replication. As Jean Baudrillard once put it: "We are in a logic of simulation which has nothing to do with a logic of facts and an order of reasons. Simulation is characterised by a *precession of the model*. [Facts] no longer have any trajectory of their own, they arise at the intersection of the models." With that, Baudrillard concludes: "The very definition of the real becomes: *that of which it is possible to give an equivalent reproduction*. [At] the limit of this process of reproductibility, the real is not only what can be reproduced, but *that which is always already reproduced*. The hyperreal."[42] Thus, the concept of reality, as a second order concept, can only take shape against the background of such first order representations as a reflection on the status of the replica. "The real [is] an attribute of representations."[43] The concept of reality only makes sense within a context of replication, and it only becomes a problem when *alternative* systems of representation come into play. But what holds for the concept of reality holds also for the concept of representation. "The *problem* of representation arises as a function of analytic efforts to assign stable sense and value to structures of practical action that, in the interest of analysis, have first been dissociated from the particular occasions in which they are used."[44]

To bring alternative spaces of representation into existence is what scientific activity is about, and this is why the question of reality as an attribute of alternative representations, and the question of representation as an attribute of its alternative uses, will continue to stay at the center stage of the scientific enterprise.