

# The Self-Vindication of the Laboratory Sciences

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## 1 Theses

The unity of science was once a battle cry, but today it is the fashion to emphasize the disunities among the sciences. I am right up there on the bandwagon (Hacking 1991). Some suggest that there is nothing in general to be said about science unless it be the message of Latour (1987) that everything in the world and our knowledge of it is to be understood on the model of politics, or maybe, is politics. I am partial to Wittgenstein's word "motley"—as in "the motley of mathematics" (Wittgenstein 1956, 88). We all want to give an account of the motley of the sciences. But here I shall try to say something quite general about established laboratory sciences. In philosophy we must strive for both the particular and the general.

What follows is metaphysics and epistemology, a contribution to our radically changing vision of truth, being, logic, reason, meaning, knowledge and reality. Such a contribution from a disunifier such as me is necessarily more local than traditional metaphysics. I address just one pervasive aspect of the laboratory sciences. Despite our recent enthusiasm for refutation and revolution, these sciences lead to an extraordinary amount of rather permanent knowledge, devices, and practice. It has been too little noted of late how much of a science, once in place, stays with us, modified but not refuted, reworked but persistent, seldom acknowledged but taken for granted. In days gone by an easy explanation of the growth of knowledge satisfied almost everyone: science discovers the truth, and once you find out the truth, then, in a liberal society, it sticks. As Ernest Nagel put it in *The Structure of Science* (1961), more powerful theories subsume their predecessors as special cases. Today, after Kuhn's *Structure of Scientific Revolutions* (1962), we are more circumspect. It has become surprising that so much empirical knowledge has accumulated since the seventeenth century.

My explanation of this stability is that when the laboratory sciences are practicable at all, they tend to produce a sort of self-

vindicating structure that keeps them stable. This is not to suggest that they are mental or social constructs. I am not about to argue for idealism but rather for down-to-earth materialism. Mine is a thesis about the relationships between thoughts, acts, and manufactures. It can be thought of as an extension of Duhem's doctrine that a theory inconsistent with an observation can always be saved by modifying an auxiliary hypothesis, typically a hypothesis about the working of an instrument such as the telescope. His was a thesis about thoughts; like most philosophers of theory he did not reflect on how we change not only our ideas but also the world. His doctrine, especially for those who read Quine, is taken to imply the underdetermination of scientific knowledge. When properly extended, it has quite the opposite effect, of helping us to understand how the world and our knowledge of it are so remarkably determinate.

Duhem said that theory and auxiliary hypothesis can be adjusted to each other; he left out the whole teeming world of making instruments, remaking them, making them work, and rethinking how they work. It is my thesis that as a laboratory science matures, it develops a body of types of theory and types of apparatus and types of analysis that are mutually adjusted to each other. They become what Heisenberg (e.g., 1948) notoriously said Newtonian mechanics was, "a closed system" that is essentially irrefutable. They are self-vindicating in the sense that any test of theory is against apparatus that has evolved in conjunction with it—and in conjunction with modes of data analysis. Conversely, the criteria for the working of the apparatus and for the correctness of analyses is precisely the fit with theory.

The theories of the laboratory sciences are not directly compared to "the world"; they persist because they are true to phenomena produced or even created by apparatus in the laboratory and are measured by instruments that we have engineered. This "true to" is not a matter of direct comparison between theory and phenomenon but relies on further theories, namely, theories about how the apparatus works and on a number of quite different kinds of techniques for processing the data that we generate. High-level theories are not "true" at all. This is not some deep insight into truth but a mundane fact familiar since the work of Norman Campbell (1920, 122–58), who noted that fundamental laws of nature do not directly "hook on to" the discernible world at all. What meshes (Kuhn's word) is at most a network of theories, models, approximations, together with understandings of the workings of our instruments and apparatus.

My thesis is materialist, both in its attention to the material side

of what we do in science and in its opposition to the intellectualism of Duhem. The thesis has almost nothing to do with recent manifestations of scientific realism or antirealism, being compatible with almost all the significant assertions made by either party. There is only one way in which my thesis is contrary to a bundle of metaphysical doctrines loosely labeled "realist." Realists commonly suppose that the ultimate aim or ideal of science is "the one true theory about the universe." I have never believed that even makes sense. The present picture suggests that there are many different ways in which a laboratory science could have stabilized. The resultant stable theories would not be parts of the one great truth, not even if they were prompted by something like the same initial concerns, needs, or curiosity. Such imaginary stable sciences would not even be comparable, because they would be true to different and quite literally incommensurable classes of phenomena and instrumentation. I say incommensurable in the straightforward sense that there would be no body of instruments to make common measurements, because the instruments are peculiar to each stable science. It is just this literal incommensurability which also enables us to understand how a "closed system" can remain in use and also be superseded, perhaps in a revolutionary way, by a theory with a new range of phenomena.

The crude idea of my thesis, although at odds with most traditional metaphysics and epistemology, is hardly novel. Our preserved theories and the world fit together so snugly less because we have found out how the world is than because we have tailored each to the other. One can think of my detailed account below as a gloss on Heisenberg's "closed systems." Once we recovered from the impact of *The Structure of Scientific Revolutions*, the question of the stability of science was immediately raised. For example, the "finalization" of science has become a lively topic for people who have learned most from Habermas (Böhme et al. 1983). There are more striking agreements with contributors to the present volume. My emphases, and in the end my philosophy, differ from Pickering's, but for present purposes my materialism lives happily as a mere part of what he calls his "pragmatic realism," in which "facts, phenomena, material procedures, interpretations, theories, social relations etc. are, in Latour's words (borrowed from Marx) 'co-produced'" (Pickering 1990, 708). The list in this quotation begins with "forms of life," which I do not omit by inadvertence; on the other hand, the taxonomy of elements of laboratory experiment, given later in this paper, expands his "etc." in ways of which he can only approve.

Another author in this volume, David Gooding (chap. 3), has an-

other "etc." list: he speaks of an "experimental sequence" which appears as the "production of models, phenomena, bits of apparatus, and representations of these things." He points the way in which "the representations and the phenomena gradually converge (his emphasis) to a point where the resemblance between what can be observed and what is sought is [as Faraday put it] 'very satisfactory.'" We agree that the interplay of items in such a list brings about the stability of laboratory science. I think of the matériel of an experiment as more central to its stabilization than do writers in the tradition of social studies of science. By the matériel I mean the apparatus, the instruments, the substances or objects investigated. The matériel is flanked on the one side by ideas (theories, questions, hypotheses, intellectual models of apparatus) and on the other by marks and manipulations of marks (inscriptions, data, calculations, data reduction, interpretation). Thus where my colleagues in this book are content with lists and etc.'s, I venture a doubtless imperfect organization or "taxonomy" of elements of laboratory experiment. The agency that Gooding puts back into experiment is just that work that is done by people, which brings the elements in my "taxonomy" into consilience and thereby creates a world of things, ideas, and data that is stable.

## 2 Contents

First, in (3) I say what I mean by a laboratory science. In (4) I suggest one source in the history of twentieth-century science for the present conviction of philosophers (but not of scientists) that science is rather unstable. Then I argue for the contrary point of view. In (5) I point to some reasons we might think that science is stable, reasons that seem to me superficial or misleading, and which are not my concern.

In (6)–(9) I give my taxonomy of elements of experiment which, I claim, are mutually adjusted to produce the self-vindicating character of laboratory science. I am at pains to list these because it is so easy to slip back into the old ways and suppose there are just a few kinds of things, theory, data, or whatever. My taxonomy is among other things a demonstration of the "motley of experimental science," which at the same time strives for some breadth of vision and does not merely meander from fascinating case to fascinating case. And then in (10) I mention some items, assuredly relevant to laboratory science, which are omitted from my taxonomy because they are not items that experiments literally use. (For example, Millikan did not "use" an atomistic weltanschauung when he measured

the charge on the electron, although without a certain vision of how the world is, his research would have proceeded quite differently, and as we know from his rival Ehrenhaft, might have come to contrary conclusions.)

The remainder of the paper develops the theses of (1) in such a way that it is possible to jump there immediately, skipping the taxonomy in (4)–(9) and referring back to it only when need arises. In (11) I discuss my extension of Duhem's thesis. In (12) I discuss what happens to a laboratory science as it matures and stabilizes. In (13) I examine the relationship between self-vindication and our expectations that good theories should be true. The thesis of self-vindication seems to make the sciences all too internal to the laboratory; how then are they applied outside? In (14) I sketch two answers, one for a practical worry of this sort and one for a metaphysical one. Finally, in (15) I remark that the stability of the laboratory sciences has nothing to do with the problem of induction. But an experimentally oriented philosophy does paint that problem in slightly different but no less skeptical colors than Hume, Russell, or the logical empiricists would. The worry is that nothing would work any more.

### 3 Laboratory Science

I do not want to invite arguments about what a laboratory is, or whether such and such is a laboratory science. The laboratory is a cultural institution with a history (or rather histories) that I shall not discuss in this abstract presentation. "Laboratory" is a far more restricted idea than "experiment"; many experimental sciences are not what I call laboratory sciences. I have in mind laboratories that have "come of age" (chap. 4). Laboratory sciences are surely connected by a family of resemblances and by a central core of examples from which they more or less differ. "Laboratory science" is a radial category in the sense of Lakoff (1986); what he would call the "prototype" laboratory sciences are those whose claims to truth answer primarily to work done in the laboratory. They study phenomena that seldom or never occur in a pure state before people have brought them under surveillance. Exaggerating a little, I say that the phenomena under study are created in the laboratory. The laboratory sciences use apparatus in isolation to interfere with the course of that aspect of nature that is under study, the end in view being an increase in knowledge, understanding, and control of a general or generalizable sort. Botany is thus not what I call a laboratory science, but plant physiology is. Paleontology is not a laboratory

science, even though carbon dating has usually been done in a laboratory, where one also uses Italian iridium to test hypotheses about the extinction of dinosaurs. Likewise, although there is plenty of experimentation in sociology, psychology, and economics, not much of it is what I call laboratory science, not even when there is a university building called the psychology laboratory. There is too little of that “apparatus used in isolation to interfere.” In saying this I neither praise nor condemn, nor do I argue that only laboratory sciences are stable—Linnaean botany may hold the palm for stability, if not for growth. Boundaries matter little; I wish only to say from the start that the sciences that are chiefly observational, classificatory, or historical are not the subject of the following discussion.

According to my definition, astronomy, astrophysics, and cosmology cannot be laboratory sciences, for they cannot in general interfere with the nature that they study. They cannot create astrophysical phenomena. But I have found that a number of people with entirely different agendas protest that astronomy and astrophysics are or have become laboratory sciences. So let me to some extent agree. Cosmology does include much laboratory work, such as investigations of gravity or an alleged fifth force (we make a laboratory in Greenland, dropping objects through a hole bored in a kilometer of ice, enriched by myriad detectors). High energy physics projects that are intended to simulate some of the birth pangs of the universe bring some cosmology down to earth, trapping it in a very big Swiss or Texan laboratory. I thus agree with G. Munevar, who insisted on this point in discussion.

Nor is the use of laboratories for astronomy novel. Old and new instruments used in astronomy and astrophysics, from spectrometers to space-launched gyroscopes to neutrino detectors, include laboratory apparatus; indeed laboratories are now put in space. Simon Schaffer (forthcoming) implies in a recent paper that in the nineteenth century there was enough experimentation in astrospectroscopy to think of it as a laboratory science. Much of what I say below about stability applies to the very work of Huggins and Maxwell that Schaffer describes, so there may be little at issue here.

Knorr Cetina might push me one step further. She notes that imaging is being radically changed, so that data are now stored digitally. The stored data become the object of investigation rather than anything that is directly observed. “Once the transition is complete,” she writes, “astronomy will have been transformed from an observational field science to an image-processing laboratory science” (chap. 4). I am more cautious about this than about most other statements in her paper, partly because it has been a long time since

astronomy was an "observational field science." The caricature of the astronomer as the one who peers through the telescope is as absurd as the cartoon of the scientist in the white lab coat. The painting by Vermeer called *The Astronomer*, dated 1658, portrays a somewhat androgynous figure in an attractive closet, protractors in hand, with what I think is a chart partially unrolled on a table (Städelsches Kunstinstitut, Frankfurt am Main).

Although image-processing laboratory science is indeed a part of astronomical and cosmological research, there remains much more to astronomy and astrophysics than that. Image processing creates many phenomena of its own. It also provides transportable data that can be analyzed by anyone. Nevertheless, in my realist mode I would not say that it creates any astronomical phenomena in the same sense in which experimenters created the phenomenon of lasing. And I don't think it is true to say with Knorr Cetina that "the objects of investigation become 'detached' from their natural environment." The digitized data are no more and no less detached than the material confronting Vermeer's astronomer. (He is *working* his data, just like the lab that buys data from Mount Palomar.) Meanwhile the objects of investigation, Saturn, superconducting cosmic strings, or the strangely oscillating Beta-lactantae don't become detached, even if we study them by images that are detached and reconstituted electronically. I am too much of a literalist to say that "the processes of interest to astronomers become miniaturized," or that "planetary and stellar time scales are surrendered to social order time scales." (Once again, how is it different with Vermeer's astronomer?) But even if I did assent to Knorr Cetina's sentences, we would still discern a sense in which astronomy and astrophysics are not laboratory sciences in the sense explained above. And it is the stability of laboratory sciences, in my sense given there, that is my topic, and my account does bear on those parts of astronomy increasingly incorporated into the laboratory.

There are yet other definitions of the laboratory, hardly recognized as such by laboratory scientists, but which cannot escape the keen eye of the ethnographer. Thus Latour (1987, chap. 6) characterizes the laboratory as a center of calculation. This vision is to be expected from an author who regards the production and manipulation of inscriptions as the central scientific activity.<sup>1</sup> The laboratory

1. Collins and Yearley (chap. 13) also draw attention to Latour's fascination with inscriptions. Latour is a bracing reminder of that glorious Parisian world of long ago, the late sixties, when inscriptions were the reality and text was substance. In my opinion Collins and Yearley misunderstood this. They are so locked in to their Anglo-

arm of science will be that which calculates. The paleontologists and the astrophysicists have, then, their laboratories for sure. Vermeer on this view painted his calculating astronomer not in a closet but in the laboratory. Latour writes in a letter of 21 February 1990, referring to Latour (1990), that explorers too are creatures of the lab. Moreover, my list of laboratory sciences “could nicely be expanded to collections and museums and archives.”

That vision of science, verging on what I have labeled lingualism or linguistic idealism (Hacking 1975, 174, 182), is not mine. Mine is thoroughly materialist and interventionist, and my laboratory is a space for interfering under controllable and isolable conditions with matter and energy, often done in museums—my office is a hundred meters from a great museum whose basement is full of what I call laboratories—but seldom in archives. But let me make peace: one-third of my taxonomy—section (9)—is about marks and the manipulation of marks, so I claim to honor Latour’s insight without losing my materialist focus.

Latour has encouraged a new problematic. We must not lose sight of an old one, the relation between theory and experiment. The laboratory sciences are of necessity theoretical. Another third of my taxonomy—section (7)—is about several distinct kinds of theory. *By a laboratory science I don’t just mean that part of a science that is conducted in a laboratory; I include all the theoretical superstructure and intellectual achievements that in the end answer to what happens in the laboratory.* I hope my taxonomy will serve those who realize that there are quite different types of theory.

Latour has another criticism of my approach. He writes in the same letter that “curiously your materialist outlook—with which I agree—does not include ‘new phenomena’ as the main production of the laboratory. I am in this sense more realist than you.” Oddly, my very first essay on experiment was called “Speculation, Calcu-

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phone theory of language that they cannot conceive of inscriptions as being other than “representational.” They even cite Travis as showing that certain mass spectrometer inscriptions “were not universally accepted as representing reality,” as if that were germane. Parisian inscriptions don’t represent anything (let alone reality). They are autonomous objects, material beings that work without signifying. I doubt that actant theory particularly derives from inscriptionalism in the way that Collins and Yearley suggest. Unlike those two authors, I have no objection to Latour’s theory of actants. I object only to something quite different, namely, the metonymic definition of the laboratory in terms of merely one of its activities, namely, inscribing. Maybe I go further than Latour, for I might take inscriptions to be among the actants, right up there with fishers and molluscs, working and worked on, everywhere people go since the moment that our species came into being as *Homo depictor*.



lation, and **the Creation of Phenomena**," published in German in Duerr 1981, 2, and rewritten for Hacking 1983 (chaps. 10, 12, and esp. 13, "**The Creation of Phenomena**"). Latour continues, "You do not leave room for **the creation of** new entities in **the** lab through **the** lab (what I call a new object, that is, a list of actions in trials that will later coalesce in a thing and will later be thrown 'out there' as **the** ultimate cause of our certainty 'about it')." One difference between my 1981 self and Latour is that I did not think of electrons being created, but did think of **the** photoelectric effect being created, in a pure state. I asserted that **the** most cautious metaphysical realist should admit that nowhere on earth did **the** pure photoelectric effect exist on earth until we made it. Nowhere in **the** universe, so far as we know, did anything lase anywhere in **the** universe before 1945 (maybe there were a few masers around in outer space). But now there are tens of thousands of lasers within a few miles of me as I write. Lasing is a phenomenon created in **the** lab. This is not a constructionalist theme, and so Latour and I go different ways with it. I do not know that new phenomena are **the** "main production" of **the laboratory**, as Latour says, but they are one of its most important products. I am glad that Latour's criticism has enabled me to reiterate one of my favorite themes, **the creation of phenomena**, previously left out of this essay. And I also can avoid **the** misunderstanding of Latour when he writes of **the** items in my taxonomy below that they are "a fixed list of elements shaping phenomena." Nothing was further from my mind than **the** idea that experiments merely shape phenomena that already exist in **the** world ready to be shaped.

Finally I should make two disclaimers about stable **laboratory** science. First, I am not in general discussing research at **the** frontiers of inquiry. That can be as unstable as you please, even when it is what Kuhn called normal science. As a matter of fact such research is usually highly regimented. Results are more often expected than surprising. We well understand why: it is not that sort of short-term stability that is puzzling. I am concerned with **the** cumulative establishment of scientific knowledge. That has been proceeding apace since **the** scientific revolution. Secondly, I regard stability not as a virtue but as a fact. If values are to be mentioned, stability upon which one cannot build is a vice. **The** noblest stability, perhaps, is that of a science that has been surpassed by deeper enquiries and new types of instrumentation and yet which remains humbly in place as a loyal and reliable servant for our interventions in, our interactions with, and our predictions of **the** course of events: one thinks of geometrical optics or Galilean mechanics. I shall repeat

this, because I am regularly misunderstood: *This paper does not praise stability. It does not imply that stability is a good thing. It does not admire stability. It observes it and tries to explain it.*

#### 4 Origins of the Instability Myth

Talk of stability flies in the face of recent wisdom about revolutions, but an emphasis on the fallibility of science is in part the consequence of unusual circumstances in physics early in the twentieth century. The shake-up that resulted at a certain historical moment was splendid. The comfortable belief that science is cumulative had been held for all too long. Mistakes, so ran the official story, are often made. Gigantic muddles long persist. But in due course and after hard work some truths will out, become established, and serve as steps upon which to advance into the unknown. This complacency fell apart under the criticisms first of Popper and then of Kuhn. They were wonderfully liberating. They turned that dutiful inductive discipline, philosophy of science, weary with years, into something sparkling, even if sometimes tintured with fantasy. I am here using the names "Popper" and "Kuhn" to denote not only individuals but also successive generations. Now why did stability suddenly become unstuck? Popper, like so many of his peers, was deeply moved by Einstein's successive revolutions in space-time, special and then general relativity. They were matched by the old and new quantum mechanics of 1900 and 1926–27. Stirring times, but also anomalous ones. They stand out because so many of the eternal verities, in the form of a priori knowledge about space, time, continuity, causation, and determinism, were abandoned. Refutation and revolution were in vogue where stability and subsumption had been the norm.

To a quite extraordinary degree these transitions, especially Einstein's, were thought out and made convincing almost entirely independently of any experimental work. Pure thought, it seemed, could anticipate nature and then hire experimenters to check out which conjectures were sound. Although relativity was often presented in its day as a refutation of Kant's transcendental aesthetic, while quantum mechanics wrecked the transcendental analytic, this was an utterly Kantian moment in the philosophy of science. Any sense of the subtle interplay between theory and experiment—or between theoretician and experimenter—was lost. The conception of physical science as unstable, as a matter of refutation and revolution, went hand in hand with a total lack of interest in the role of experimental science. So it is not surprising that today we

should start to think about stability again; for the present decade has seen the revival, among historians, philosophers, and sociologists of science, of serious thinking about the laboratory.

Why do I speak so confidently of stability? For a number of reasons. One is quite familiar to students of the physical sciences, whose practitioners in moments of philosophizing speak of theories being valid in their domain. Thus Heisenberg wrote (1948, 332) that "some theories seem to be susceptible of no improvement . . . they signify a *closed system* of knowledge. I believe that Newtonian mechanics cannot be improved at all . . . with that degree of accuracy with which the phenomena can be described by the Newtonian concepts, the Newtonian laws are also valid" (for convenient references to the development of Heisenberg's idea, see Chevalley 1988). I would amend this slightly, for the phenomena are not described directly and without intermediary by Newtonian concepts. It is rather certain measurements of the phenomena, generated by a certain class of what might be called Newtonian instruments, that mesh with Newtonian concepts. The accuracy of the mechanics and the accuracy of the instruments are correlative, and that is one of the explanations of the stability of laboratory science.

Before even entering the research laboratory, the student, like it or not, finds that many mature sciences are pedagogically stable. We learn geometrical optics when young, the wave theory as teenagers, Maxwell's equations on entering college, some theory of the photon in senior classes, and quantum field theory in graduate school. Each of these stages is taught as if it were true, although of course many byways, such as Newton's corpuscular light rays, are omitted. Science teachers have to bear the brunt of a familiar criticism. They teach science as if it were dead. In a way that is right. Much science is dead. That does not excuse bad teaching; there is probably a greater proportion of lively classes in classical Greek than in thermodynamics. Unlike the conjugations, which are the luxury of a few, the Carnot cycle must be taught.

There are perennial debates on American college campuses: should every student have some acquaintance with the great books of the West? The issues are ideological and hinge on a conception of the nature of culture and civilization. There is nothing comparably ideological about learning how to use Planck's constant or the mechanical equivalent of heat. No physicist would dream of compelling students to read Planck or Dirac, let alone Boltzmann or Joule. But the students have to master the dead and digested science associated with those names, not because of their cultural or even pedagogical value, but because that is part of the stable knowledge with

which many of the students will change bits of the world, and on which a few of the research oriented among them will build new knowledge.

Our editor remarks that according to Feyerabend (1978) this is a bad way of teaching. There is a lot of bad teaching, but I doubt that it is wrong to teach stable science. The error is reverence for what is established, and a dulling of the critical spirit, but that is an entirely distinct point. The only science education on near-Feyerabendian lines with which I am acquainted is offered at the Ontario Science Centre in Toronto. Twenty-five high school seniors are told to find things out and are given remarkable experimental material—the castoffs from what five years earlier was frontline research—and quite strong theoretical resources. They spend a semester doing two of physics, chemistry, or biology, and to satisfy an English requirement, learn science writing. The morale is extraordinary; the quality of learning superb. The depression that results when the students proceed to a university classroom amounts to trauma and Feyerabendian disillusion with science. Nevertheless one of the things that the students are constantly forced to do is to acquire on their own the chunks of stable theoretical knowledge and experimental technique demanded by their own learning and research. The students don't revere it. They believe it when they need it and doubt it when it does not work. And that is the way in which old stable science is an essential part of science education.

Let it be granted that there is some stability. Is that not the road to boredom and stultification? It is tempting to suppose that although the making and solidifying of an established science might have been intensely creative, once the work is in place it is to be used only for pedestrian purposes. The action lies elsewhere, in the creating of new science. We use geometrical optics all the time, but it is hardly a topic for live research, or so it may be said. We may rely on Newtonian mechanics for launching the Hubble space telescope, but the mechanics is not itself a topic for investigation. Yet of course there are Newtonian problems that remain deeply challenging, the many-body problem being the classic example. Ergodic theorems, in which one shows how stochastic processes can arise from within a deterministic world, lead on to chaos theory, a domain that blends mathematics, experiment, and concept formation in ways that may in retrospect come to seem quite novel. Even at the level of plain laboratory science, established knowledge—which we had thought superseded except in application—can be combined with new facilities for instrumentation to yield profound innovations. S. S. Schweber (1989) has a telling example. In 1981, workers at the University

of Washington devised the Penning trap, which contains a single electron in a definite space. Everything they did was planned according to, and can be explained by, the prerelativistic (pre-Dirac) theory of the electron, which might have seemed to be a dead closed system of interest only to the so-called philosophers of quantum mechanics, who write as if quantum field theory does not exist. But not only was prerelativistic theory used by the Washington workers: it is also not clear that their work could have been conceived or made sense of otherwise. For their purposes, the crude old account of the electron is better than any other. One reason that a stable laboratory science may come to life is that advances in technique or technology developed for other purposes can sometimes be applied only in its old mature intellectual and experimental framework.

### 5 Seeming Stability

There are a number of reasons to expect established science to feel or look stable, which are independent of the more radical metaphysical theses advanced in this paper. I shall mention three. The first is our habit of splendid anachronism. We cheerfully speak of Maxwell's equations or the Zeeman effect, but what we understand by these things is very different from what was meant by those whom we honor. In the case of experimental techniques, a great many of them fade away, and only the most gifted experimenter can duplicate what the textbooks casually say was done. New instruments make obsolete the skills needed to build old instruments; replication requires perverse antiquarianism.

Thus old science is not preserved, the cynic will say: what is stable is that various events have been turned into facts that are no longer of immediate interest. We do other things and accept on faith most knowledge derived in the past. It can be well argued that the Zeeman effect and the anomalous Zeeman effect are not now what they were when they were discovered, and it is the practice of teaching and naming that makes things seem so constant.

Second, a secure sense of stability arises from the fact that scientific practice is like a rope with many strands. One strand may be cut, but others survive intact: the rope, it seems to the unreflective, holds unchanged. Peter Galison (1987, chap. 5) observes that in any laboratory science several traditions are at work at any one time. There are, for example, theoretical, experimental, and instrumental traditions. There may be a break in a theoretical tradition which has little effect on the instruments that are used or the ways they are used. The strong sense of continuity during such a theoretical mu-

tation results from the fact that instrumental and experimental practices may continue largely unaffected by changes in theory. Even when the explanations for the practices change, so that people understand what they are doing differently, very much the same skills and material apparatus may be used as before (e.g., Heinz Post's example of seeing anthracene ring molecules: Hacking 1983, 199ff.). Likewise an ongoing theoretical tradition can make us experience continuity in a time of radical instrumental innovation.

A third and much more common source of felt stability is our practice of turning various elements of science into what Latour (1987, 2) calls "black boxes." These include not only off-the-shelf apparatus but also all sorts of systems for operating on symbols, for example, statistical techniques for assessing probable error. Material black boxes include standard pieces of apparatus bought from an instrument company, borrowed from the lab next door, rented from the Bureau of Standards, or abandoned by a military research facility when it has moved on to fancier gadgets. The laboratory worker seldom has much idea how the box works and cannot fix it when it is broken. Yet it encodes in material form a great deal of preestablished knowledge which is implicit in the outcome of an experiment. Indeed theoretical assumptions may be "built into the apparatus itself" (Galison 1987, 251; emphasis his)—and that is true not only of Galison's high energy physics but of some of the most simple and direct observational devices (Hacking 1989, 268).

If we had to build every piece of equipment from scratch, not only would laboratory science be vastly more labor-intensive but it would also be a great deal less stable. Devices that worked last year for one purpose would—as anyone who has spent some time in a laboratory will know—not work this year for the next project. We are tempted to say that it is the commercial or semicommercial instrument makers and salespeople that have long kept science on an even keel. We do not just buy an instrument and switch it on. As long as there have been instruments there have been facilitators who show how an instrument or class of instruments can be put to all sorts of new purposes. Historians have hardly begun to tell us about the great instrument makers of London or Berlin in the eighteenth century, let alone those of Lisbon in the fifteenth. I doubt that they were so different, except in point of specializations, from what we find running through the *Proceedings* of an electron-microscopy conference that is being held as I write. We find sessions for the fairly new scanning tunneling (electron) microscope (STM), with talks on how to apply it to Planar membranes, Doped polypyrrole on ITO glass, vapor-deposited and electrodeposited metal films, etc.

The speakers are from Shell Development, Westinghouse Research and Development, Fuji, the Advanced Research Laboratory of Hitachi, Philips Analytical Electron Optics Laboratory, as well as academic institutions in Basel, Ithaca, Freiburg, and Moscow (Bailey, 1989). At such a conference we can get a bird's-eye glance at how a type of device barely out of the research stage becomes a black box that the next generation will use as a stable laboratory tool. The consumer won't have much idea how the tool works: unlike transmission electron microscopes, whose theory is in some weak sense understood by those who use it, the new microscopes are built according to principles of quantum tunneling that sorely vex even the most diligent student of macromolecules or metallurgy. And we do not yet know quite what the black box may do: a Berkeley undergraduate playing with an STM after hours found he could image DNA molecules, contrary to anyone's expectations based on extant theory of the apparatus.

### 6 Items Used in the Laboratory

Thanks to the many recent studies by philosophers, historians, ethnographers, and sociologists of experimental science, we have much richer sources of information about the laboratory than were available a decade ago. This welter of colorful examples makes it hard to produce any tidy formal characterization of experiment. Hence our powers of generalization are limited. I shall try to return some degree of abstraction to the philosophy of science by listing some familiar elements in laboratory experimentation. We must guard against too strict a set of distinctions. Descriptions of experimental procedures have long been regimented to make them look as if experiments have much in common. The format for writing up a laboratory report is inculcated in school and preserved, modified, or reinforced—in ways that vary from discipline to discipline—in preprints and journals. The modest uniformity is largely an artifact of how our scientific culture wants to conceive itself and has much to do with our construction of what we call objectivity. Admitting as I do that there is less in common among experiments than we imagine, I shall nevertheless list some elements that are often discernible. Their prominence and even their presence varies from case to case and from science to science.

The items are not of the same kind. When I develop the theme of the self-vindication of laboratory science, I shall hop from category to category, and so in the following section I present a taxonomic scheme of reference. My list of elements could be thought of as

dividing into three groups: ideas, things, and marks. These three monosyllabic labels should be inoffensive. There is nothing invidious in calling various kinds of questions and theories "ideas." They are among the intellectual components of an experiment. The material substance that we investigate or with which we investigate is not always best called a "thing"; instruments are things; are Norway rats or polarized electrons or bacteriophages things? But "things" serves; it is the briefest contrast with "idea." I speak of the outcomes of an experiment as marks, and subsequent manipulation of marks to produce more marks. This is reminiscent of Latour's insistence that a laboratory instrument is simply an "inscription-device" and that the immediate product of a laboratory is an inscription (1987, 68). For me, "mark" is not only the shorter word but also more suitably ambiguous, allowing it to cover a number of my items. According to my dictionary, marks are "visible impressions," "signs or symbols that distinguish something," "written or printed signs or symbols," "indications of some quality," and also "goals."

We shall never confuse theory with apparatus (an idea with a thing), and seldom shall we find it difficult to distinguish an instrument from the data that it generates or the statistical analysis that we make of them (although marks are things, we won't here confuse a thing with marks or the manipulation of marks). But within my three subgroups of ideas, things, and marks, some of the elements run into each other, and we may disagree about how to file items within my list. That is of no moment here, for stability arises from the interplay of these elements, and an account of it does not require a rigid taxonomy.

## 7 Ideas

1. *Questions.* There is a question or questions about some subject matter. The question answered at the end of the experiment may be different from the one with which the investigators began. Questions range from those rare ones emphasized by philosophers: "which of these competing theories is true or false?" to the commonplace, "what is the value of this quantity?" or "does treating X with Y make a difference, and if so what good differences and what bad ones?" When a question is about a theory, I shall speak of the theory in question. Crucial experiments have two theories in question.

2. *Background knowledge.* In what is so often called theory we should distinguish at least three distinct kinds of knowledge about the subject matter of the experiment. The divisions (2, 3, and 4) that I propose are sharp in some disciplines and vague or almost nonex-



istent in others. First is **the** background knowledge and expectations that are not systematized and which play little part in writing up an experiment, in part because they are taken for granted. These are surely inescapable. Science without background beliefs makes no sense.

3. *Systematic theory*: theory **of** a general and typically high level sort about **the** subject matter, which by itself may have no experimental consequences.

4. *Topical hypotheses*, as I shall call them, are part **of** what in physics is commonly named phenomenology. Because that term has another meaning in philosophy, and because it can also be used for (5), we want another name. We are concerned with what connects systematic theory to phenomena. Logical empiricism, with its strong emphasis on language, spoke **of** bridge principles (Hempel 1966, 72–5). **The** name is attractive, although “principles” suggests something that cannot readily be revised, whereas we are concerned with what is revised all **the** time in **laboratory** work. Indeed **the** core bridge principle idea was revealingly expressed by a writer not in **the** classic mold **of** logical empiricism, namely, N. R. Campbell (1920, 122–58), who spoke **of** a “dictionary” to connect purely theoretical concepts with observational terms. **The** connections I have in mind are too revisable for me to speak **of** principles or a dictionary. I call them topical hypotheses. *Hypothesis* is here used in **the** old-fashioned sense **of** something more readily revised than theory. It is overly propositional. I intend to cover whole sets **of** approximating and modeling procedures in **the** sense **of** Cartwright (1983), and more generally **the** activity that Kuhn (1962, 24–33) called **the** “articulation” **of** theory in order to create a potential mesh with experience. It is a virtue **of** recent philosophy **of** science that it has increasingly come to acknowledge that most **of the** intellectual work **of the** theoretical sciences is conducted at this level rather than in **the** rarefied gas **of** systematic theory. My word *topical* is meant to connote both **the** usual senses **of** “current affairs” or “local,” and also to recall **the** medical sense **of** a topical ointment as one applied to **the** surface **of the** skin, i.e., not deep.

5. *Modeling of the apparatus*. There are theories, or at least background lore, about **the** instruments and equipment listed below as (6–8). To avoid ambiguity I shall speak **of the** {theoretical} modeling **of the** apparatus, an account **of** how it works and what, in theory, it is like. We are concerned with phenomenological theory that enables us to design instruments and to calculate how they behave. Seldom is **the** modeling **of** a piece **of** apparatus or an instrument **the** same as **the** theory in question (1) or **the** systematic theory (3).

Sometimes it may just be vague background knowledge (2). It may overlap with the topical hypotheses (4). The apparatus of Atwood's machine (1784) for determining local gravitational acceleration is a turning fork with a brush on one prong that is dropped so that the brush sweeps out a curve on the detector, a plate of glass with white-wash on it. The theory (and practice) of the tuning fork is plainly part of the theoretical modeling of the apparatus, and it has almost nothing to do with the systematic theory of gravitational acceleration or Galilean mechanics. Note that in this case there is no topical hypothesis. To heighten the contrast between modeling of the apparatus and topical hypotheses, consider the plight of the grandest of unified theories, superstring theory. Constructed in at least nine dimensions, it has no experimental consequences at all. The task of one kind of phenomenology is to articulate the theory so that it does mesh with our three- or four-dimensional reality. That is a matter of devising topical hypotheses. A quite different task is the design of apparatus and understanding how it works, the job of theories about and modeling of the apparatus.

## 8 Things

6. *Target*. This together with elements (7)–(10) comprises the matériel of the experiment. These items—not all of which need to be present in an experiment—are often, in physics, described using a military analogy. First is a target, a substance or population to be studied. The preparation of the target—in old-fashioned microbiology by staining, use of microtomes, etc.—is best kept separate from the modification of the target, say by injecting a prepared cell with a foreign substance. Similar distinctions can be made in analytic chemistry.

7. *Source of modification*. There is usually apparatus that in some way alters or interferes with the target. In certain branches of physics, this is most commonly a source of energy. Traditional inorganic chemical analysis modifies a target by adding measured amounts of various substances, and by distillation, precipitation, centrifuging, etc. In the case of Atwood's machine we have neither target nor source of modification; it is a detector pure and simple. There is nothing ultimate about my classification: a classic description of apparatus due to James Clerk Maxwell, best adapted to physics, would divide this item into a source of energy and devices for transport of energy, the latter divided into eight functions (Galison 1987, 24). Note that although most energy sources are controlled by us, one of the most powerful, with one of the most distinguished

track records, comes from on high: the cosmic rays. And the next major neutrino project, called DUMAND, will use neutrinos as a source of energy vastly greater than any hitherto used in high energy physics.

8. *Detectors* determine or measure the result of the interference or modification of the target. I also count as a detector a modest cosmological laboratory device such as Atwood's machine, where no target is influenced (certainly not gravity). Commonly we include both detectors and sources of modification as apparatus. In many circumstances the detectors are called instruments, but they are not the only instruments. Many of the most imaginative detectors can become what I shall call tools: Michelson's interferometer, once the subtlest detector on earth, has, for example, become a tool for eliminating some of the instrumental error that plagues astronomical imaging (Cornwell 1989).

9. *Tools*. As we contemplate proton-antiproton colliders and scanning tunneling electron microscopes, let us not forget the more humble things upon which the experimenter must rely. In the preparation of the target, I mentioned microtomes to slice organic matter thin, stains that color it, chemicals that react, taken off the shelf, or altered a little for this or that purpose. They are hardly worthy to be ranked with sources of modification or detectors, but we cannot get along without them: we also use them at least in the light of background lore (how a stain or a slicer will alter a specimen, and how it will not), and often in the light of a good deal of topical and apparatus lore. This residual category of tools overlaps with preceding ones. Is litmus paper tool or detector? In the child's chemistry set, it is a detector of acidity, but in the high school lab, it is a tool like a screwdriver. Any off-the-shelf device, especially one developed in a discipline unrelated to the immediate experiment, could be classified as a tool, so that we would restrict (7) and (8) to instruments that were actually made or adapted in the course of the experiment. From this perspective many data generators (10), such as machines to photograph, count, or print out events of interest, would register as tools. And what shall we say of frog's eggs? They are available from suppliers by the kilo, eggs into which a designated genetic string is injected because they reproduce it by the eggful, some minuscule fraction of it serving as a target for an experiment. Are these eggs tools? Let us say they are. What of the Norway rat, loyal servant of anatomists, physiologists, and nutritionists in the nineteenth century, and after much inbreeding and induced mutation, at the forefront of immunogenetics and recombinant experiments at this very moment? (Gill et al. 1989). Are these Norway rats tools? What

about their pituitary glands, used in endocrinology assays in ways made familiar to philosophers by Latour and Woolgar (1979)?

10. *Data generators.* Atwood's machine needs a person or robot with a ruler to measure the distances between successive passages of the brush over the center line. People or teams who count may be data generators. In more sophisticated experiments, there are micrographs, automatic printouts, and the like. There is no need to insist on a sharp distinction in all cases between detector and data-generating device. In the early days a camera taking micrographs from an electron microscope was a data generator that photographed a visible image for study, analysis, or the record. Today the camera is more often the detector; the data generator may be a scanner working from the micrograph.

## 9 Marks and the Manipulation of Marks

11. *Data:* what a data generator produces. By data I mean uninterpreted inscriptions, graphs recording variation over time, photographs, tables, displays. These are covered by the first sense of my portmanteau word "mark." Some will pleonastically call such marks "raw data." Others will protest that all data are of their nature interpreted: to think that there are uninterpreted data, they will urge, is to indulge in "the myth of the given." I agree that in the laboratory nothing is just given. Measurements are taken, not given. Data are made, but as a good first approximation, the making and taking come before interpreting. It is true that we reject or discard putative data because they do not fit an interpretation, but that does not prove that all data are interpreted. For the fact that we discard what does not fit does not distinguish data from the other elements (1)–(14): in the process of adjustment we can sacrifice anything from a microtome to a cyclotron, not to mention the familiar Duhemian choice among the hypotheses in the spectrum (1)–(5) for the ones to be revised in the light of recalcitrant experimental results.

12. *Data assessment* is one of at least three distinct types of data processing. It may include a calculation of the probable error or more statistically sophisticated versions of this. Such procedures are supposed to be theory neutral, but in complex weighing of evidence they are sensibly applied only by people who understand a good many details of the experiment—a point always emphasized by the greatest of statistical innovators, R. A. Fisher, although too often ignored by those who use his techniques. Slang talk of statistical cookbooks—recipes for making computations of confidence intervals or whatever—has more wisdom in it than is commonly sus-

pected. Good cooks must know their foodstuffs, their fire, their pots; that is true by analogy of the person who tends the apparatus, but it is equally true that good statisticians have to know their experiment. Data assessment also includes a nonstatistical aspect, the estimation of systematic error, which requires explicit knowledge of the theory of the apparatus—and which has been too little studied by philosophers of science.

13. *Data reduction*: large or vast amounts of unintelligible numerical data may be transformed by supposedly theory-neutral statistical or computational techniques into manageable quantities or displays. Fisher used the word “statistic” to mean simply a number that encapsulated a large body of data and (independently of Shannon) developed a measure of the information lost by data reduction, thus determining the most efficient (least destructive) types of reduction.

14. *Data analysis*: an increasingly common form is well described by Galison (1987) in connection with high energy experiments. The events under study in an experiment are selected, analyzed, and presented by computer. This may seem like a kind of data reduction, but the programs for analyzing the data are not supposedly theory-neutral statistical techniques. They are chosen in the light of the questions or focus of the experiment (1) and of both topical hypotheses (4) and modeling of the apparatus (5). In this case, and to a lesser extent in the case of (11) and even (12), there is now commonly an echelon of workers or devices between the data and the principal investigators; Galison argues that this is one of the ways in which experimental science has recently been transformed. There are many other new kinds of data processing, such as the enhancement of images in both astronomy and microscopy. And (11)–(14) may get rolled into one for less than \$2,000. “With the new \$1,995 EC910 Densitometer, you can scan, integrate, and display electrophoresis results in your lab PC. Immediately! No cutting, no hand measuring. Programs accept intact gel slabs, columns, cellulose acetate, chromatography strips and other support media” (Software extra, \$995; from a typical 1989 ad on a back cover of *Science*).

15. *Interpretation of the data* demands theory at least at the level of background knowledge (2), and often at every other level, including systematic theory (3), topical theory (4), and apparatus modeling (5). Pulsars provide an easy example of data interpretation requiring theory: once a theory of pulsars was in place, it was possible to go back over the data of radio astronomers and find ample evidence of pulsars that could not have been interpreted as such until there was theory. The possibility of such interpretation also mandated new

data reduction (12) and analysis (13), and the systematic error part of the data assessment (11) had to be reassessed. More about interpretation below.

## 10 Qualifications

It is tempting to follow Galison (1988, 525) and take (2)–(5) as the “establishment of knowledge prior to experimentation.” That suggests something put in place before the experiment and enduring throughout it. My picture of experimentation is, in contrast, one of potential modification of any of the elements (1)–(15), including the prior “knowledge.” Many things are “established” before the experiment—not just knowledge but also tools and techniques of statistical analysis. But none of these is established in the sense of being immutable. As promised, far from rejecting Popperian orthodoxy, we build upon it, increasing our vision of things than can be “refuted.”

Second, I have omitted from my list something that is rather rigid during the time span of even the most extended experiment—what we indicate with words like *weltanschauung* or Holton’s (1978, 1987) “themata” and “thematic presuppositions,” or even A. C. Crombie’s “styles of scientific reasoning” (Hacking 1982, 1992). We have expectations about what the world is like and practices of reasoning about it. These govern our theories and our interpretation of data alike. Quite aside from our Humean habits, we think of Kelvin’s dictum so characteristic of positive science at the end of the nineteenth century: we do not understand a thing until we can measure it. That smacks more of metaphysics than methodology—the world comes as measurable. We think of Galileo’s doctrine that the author of nature wrote the book of the universe in the language of mathematics. We think of the twinned aspects of post-Baconian science to which Merchant (1980) and Keller (1986) have drawn attention: (a) the expectation that we find out about the world by interfering with it, ideally in military fashion with targets; (b) the expectation that nature “herself” works that way, with forces and triggering mechanisms and the like, and in general a master-slave mode of interaction among her parts. These conceptions, be they mathematical or magisterial, are visions of what the world is like.

I have omitted such things from (1)–(15) because experimenters do not literally use them. Some philosophers would say that experiments presuppose large-scale entities such as themata or styles or paradigms. Many a cynic would say that there are no such things. In the present essay I need not engage in that debate, because whatever

the status of such entities—be they analytical concepts or mind-framing schemata or sheer fiction—experimenters do not change their ideal conceptions of the universe in the course of, or at any rate because of, experimental work. Such notions are not molded to fit into {1}–{15}: they stand above them. It is true that systematic theory {2}, black-box tools {9}, and procedures of data assessment {12} or reduction {13} are seldom much affected by experimental work, but they can be, and they certainly are explicitly used in ways in which weltanschauungs or *Denkstile* aren't.<sup>2</sup>

Finally, I have said nothing about the most important ingredient of an experiment, namely, the experimenters, their negotiations, their communications, their milieu, the very building in which they work or the institution that foots the bills. I have said nothing of authors, authority, and audience. In short, nothing of what Latour indicates by his titles *Science in Action* and *Laboratory Life*. This is once again because I am concerned with elements that are used in the experiment. But that is weak, because experimenters use money, influence, charisma, and so forth. We can nevertheless to some extent hold on to the difference between what the experimenters use in the experiment and what is used in order to do the experiment or in order to further its results {Latour would protest that stable science arises only when the world of the laboratory is embedded in a far-larger social network}. Those tired words “internal” and “external” seem useful here; I have been offering a taxonomy of elements internal to an experiment.

Despite my restriction to the internal, my concern with stability

2. Andrew Pickering noted at this point that “the recent move to microanalyses of practice seems to have left these big, underlying, unifying aspects of culture hanging (if they exist),” and rightly urged more discussion {letter of 28 November 1989}. I agree; a talk given on 6 October began, “A philosophical task in our times is to connect {a} social and micro-social studies of knowledge, {b} metaphysics, and what we might call {c} the Braudelian aspects of knowledge” {Hacking 1992a}. By {c} I meant “relatively permanent, growing, self-modulating, self-revising features of what we call science,” exemplified by entities mentioned in the paragraph above. My own view is that there is no one story to tell about all the disparate Braudelian entities, but I have attempted to give an account of {a}–{c} for my notion of styles of reasoning. These are not matters for the present paper. But I show how the theory of self-vindication advanced here would be located within my theory of the self-authentication of styles of reasoning. Laboratory science forms one of my six designated styles of reasoning, but vindication is distinct from authentication. I use “self-authentication” to mean the way in which a style of reasoning generates the truth conditions for the very propositions which are reasoned to using that style, suggesting a curious type of circularity. Thus self-authentication is a logical concept. Self-vindication is a material concept, pertaining to the way in which ideas, things, and marks are mutually adjusted.

accords quite well, if in a conservative and conservationist fashion, with studies of the social construction of scientific facts. Unlike pedestrian antirealists of an instrumentalist or empiricist or positivist sort, constructionalists hold that facts and phenomena are made, not observed, and that criteria for truth are produced, not preordained. They hold that scientific facts are real enough once the making has been done, but that scientific reality is not "retroactive." My investigation of stability is precisely an investigation of that kind of product from a different vantage point. I am moved to the investigation by a curiosity about the death that follows laboratory life, about the cumulative inaction that follows science in action.

## 11 Extending Duhem's Thesis

Duhem (1906) observed that if an experiment or observation was persistently inconsistent with theory, one could modify theory in two ways: either revise the systematic theory (3) or revise the auxiliary hypotheses (in which we include both topical hypotheses [4] and modeling of the apparatus [5]). His classic example was astronomy, not a laboratory science, but the message was clear. Should a theory about the heavens be inconsistent with data, he said, we may revise astronomy, or modify either the theory of the transmission of light in space or the theory of telescope (5). But that is only the beginning of the malleability of my fifteen elements. For example, we can try to modify the telescope or build a different kind of telescope. That is, try to save the systematic hypothesis by adapting the detector (8).

Several recent contributions help to enlarge the Duhemian vision. Pickering (1989) regards the topical hypotheses (4), the modeling of the apparatus (5), and the matériel as three "plastic resources." He has an elegant example, retold with a different emphasis in Pickering 1990, of getting an experiment to work. The same example is also used, with purposes not unlike mine, by Gooding (chap. 3).<sup>3</sup> There were two competing theories in question (1): free charges come either in units of  $e$ , the charge on the electron, or  $1/3 e$ , the

3. The repetition of the example is now becoming embarrassing, and I welcome Gooding's providing two more examples that make additional points. I appropriated Pickering's example after reading an unpublished paper of his (1986), partly because I had been following the other side of the investigation, that of Fairbanks at Stanford, who established that there are free quarks (Hacking 1983, 23ff.). If the example is ever used again, Morpurgo and Fairbanks should be considered together. As it happens, many of the things Pickering said about Morpurgo are remarkably transferrable to Fairbanks's work on supercooled niobium balls.



charge on a quark (there was also the background assumption (2) that these alternatives exhaust the possibilities). The matériel was a highly modified version of Millikan's oil drop apparatus to determine the charge on the electron. This nicely divides into target, source of modification, and detector. The initial results of the experiment were consistent with there being a continuum of free charges. The investigator had to change both his source of modification (7) and his modeling of the apparatus (5). That is, he had to tinker with the equipment (it was a matter of moving condenser plates in a way counter to that predicted by the original theoretical model of the apparatus), and he had to revise the account of how the apparatus worked. The experiment ended by producing data that could be consistently interpreted by only one of the two competing systematic theories: no free quarks were there to be observed.

Pickering emphasizes apparatus, modeling, and topical hypotheses. Ackermann (1985) draws our attention to other groupings of my elements, well summed up in his title, *Data, Instruments, and Theory*. He is concerned with a dialectical relationship between data (11), interpretation (15), and systematic theory (2). Despite his title he has, like Duhem and unlike Pickering, a passive attitude to instruments, for he thinks of them pretty much as black boxes, as established devices that generate data which is literally given. He thinks of an instrument in the way in which an eighteenth-century navigator would regard a chronometer, or a cell biologist would think of a nuclear magnetic resonance spectrometer—as off-the-shelf reliable technology. According to Ackermann, the primary task of the scientist is to interpret data in the light of theory and to revise theory in the light of interpretation. Thus his story is like most traditional philosophy of science, except that his data are my (11). They are not theory laden but are material artifacts, photographs, or inscriptions, the productions of instruments—marks, in short.

The data themselves are something given by instruments, or by a set of instruments of a certain kind, which Ackermann calls an instrumentarium, and each instrumentarium has its own data domain. The instrumentarium of classical mechanics, he says, is different from that of quantum mechanics, and the old mechanics interprets data delivered by one kind of instrument, while the newer mechanics interprets data produced by another kind. Ackermann proposes that a laboratory science becomes stable when there is a class of instruments that yield data of a certain kind such that there is a body of theory that can interpret the data uniformly and consistently. A theory, as I understand him, is then true to the data generated by a certain class of instruments, and different theories can be

true to different classes of data delivered by different instrumentaria. This suggests a new and fundamental type of incommensurability. It used to be said that Newtonian and relativistic theory were incommensurable because the statements of one could not be expressed in the other—meanings changed. Instead I suggest that one is true to one body of measurements given by one class of instruments, while the other is true to another. I have already remarked that Ackermann's discussion of instruments is far too respectful and that his conception of disjoint instrumentaria is farfetched. The texture of instrumentation and its evolution is vastly more subtle than he makes it to be. Nevertheless his simplistic picture has the germ of an important truth.

Duhem, Pickering, and Ackermann point to interplay among several subsets of the elements (1)–(15). Pickering attends to the modeling of the apparatus and the working of the instruments: we acknowledge data as data only after we have gotten handmade apparatus to work in ways that we understand. Duhem emphasized the intellectual elements (1)–(5). Ackermann, observing that data can be understood in many ways or not at all, put the emphasis on a dialectic involving theories and interpretation, regarding instruments and the data that they produced as fixed points. We should learn from all these authors. Let us extend Duhem's thesis to the entire set of elements (1)–(15). Since these are different in kind, they are plastic resources in different ways. We can (1) change questions; more commonly we modify them in midexperiment. Data (11) can be abandoned or selected without fraud; we consider data secure when we can interpret them in the light of, among other things, systematic theory (3). But it is not just Ackermann's interpretation of data by theory that is in play. Data processing is embarrassingly plastic. That has long been familiar to students of statistical inference in the case of data assessment and reduction, (12) and (13). Because statistics is a metascience, statistical methodologies are seldom called into question inside a laboratory, but a consultant may well advise that they be. Data analysis is plastic in itself; in addition any change in topical hypotheses (4) or modeling of the apparatus (5) will lead to the introduction of new programs of data analysis.

We create apparatus that generates data that confirm theories; we judge apparatus by its ability to produce data that fit. There is little new in this seeming circularity except taking the material world into account. The most succinct statement of the idea, for purely intellectual operations, is Nelson Goodman's summary (1983, 64) of how we "justify" both deduction and induction: "A rule is amended if it yields an inference we are unwilling to accept; an inference is

rejected if it violates a rule that we are unwilling to amend." There is also more than a whiff of Hanson's (1965) maxim that all observation is theory loaded, and of the corresponding positivist doctrine that all theory is observation loaded. The truth is that there is a play between theory and observation, but that is a miserly quarter-truth. There is a play between many things: data, theory, experiment, phenomenology, equipment, data processing.

## 12 Maturing Science

Adjustment does not imply stability. All that is said in the preceding section is consistent with the "underdetermination of theory by data"—the usual lesson drawn from Duhem's reflections. Yet the common experience of the laboratory sciences is that there are all too few degrees of freedom. All of those items like (1)–(15) and more can be modified, but when each one is adjusted with the others so that our data, our machines, and our thoughts cohere, interfering with any one throws all the others out of whack. It is extraordinarily difficult to make one coherent account, and it is perhaps beyond our powers to make several. The philosophical task is less to understand an indeterminacy that we can imagine but almost never experience than to explain the sheer determinateness of mature laboratory science. On the one hand it is utterly contingent that our intellectual structure (1)–(5) is what it is, but given that it is the way it is, only rarely can it be changed, although it can be superseded.

How, then, does a laboratory science mature? Here is a very liberal adaptation of Ackermann's idea. A collection of kinds of instruments evolves—an instrumentarium—hand in hand with theories that interpret the data that they produce. As a matter of brute contingent fact, instrumentaria and systematic theories mature, and data uninterpretable by theories are not generated. There is no drive for revision of the theory because it has acquired a stable data domain. What we later see as limitations of a theory are not data for the theory.

For example, geometrical optics takes no cognizance of the fact that all shadows have blurred edges. The fine structure of shadows requires an instrumentarium quite different from that of lenses and mirrors, together with a new systematic theory and topical hypotheses. Geometrical optics is true only to the phenomena of rectilinear propagation of light. Better: it is true of certain models of rectilinear propagation. It is the optics and the models and approximations that comprise the topical hypotheses (4) that are jointly true to the phenomena. No matter how it is supplemented, geometrical optics is

not true to the phenomenon of blurred edges of shadows—a phenomenon that, unlike most, is there for the noticing. Theories and phenomenology true to the phenomena of shadows became established because they were true to the phenomena elicited by a new family of instruments that began to be developed in the nineteenth century. There is no requirement that theories that address one kind of data should address another.

Stable laboratory science arises when theories and laboratory equipment evolve in such a way that they match each other and are mutually self-vindicating. Such symbiosis is a contingent fact about people, our scientific organizations, and nature. In referring to nature I do not imply that nature causes or contributes to such symbiosis in some active way. I do not invoke nature as an explanation of the possibility of science, in the way in which those fantasists called scientific realists sometimes invoke nature or underlying reality to explain the “success” of science. I mean only that we might have lived in an environment where laboratory science was impracticable. Also, as I note in my final section on induction, we may live today in an environment in which all our apparatus ceases to work tomorrow.

Symbiosis and stability are one contingency; there is another more interesting one. Laboratory science might have been the sort of enterprise that either stagnates or else is revisable only by abandoning all that has gone before. The contingency that prevents stagnation without nullifying an existing order of theory and instrumentation is this: new types of data can be produced, thought of as resulting from instruments that probe more finely into microstructure, and which cannot be accommodated at the level of accuracy of which established theory is capable. A new theory with new types of precision is needed (recall Heisenberg on closed systems, mentioned above). Space is created for a mutual maturing of new theory and experiment without dislodging an established mature theory, which remains true of the data available in its domain.

Kuhn (1961) noticed almost all of this with characteristic precision. Fetishistic measurement sometimes hints at anomaly that can only be tackled by devising new categories of instruments that generate new data that can be interpreted only by a new sort of theory: not puzzle solving but revolution. This is the overriding theme of his study of black-body radiation (Kuhn 1978). He omitted only the fact that the old theory and its instruments remain pretty much in place, in their data domain. Hence new and old theory are incommensurable in an entirely straightforward sense. They have no common measure because the instruments providing the measurements

for **the** one are inapt for **the** other. This is a scientific fact that has nothing to do with “meaning change” and other semantic notions that have been associated with incommensurability.<sup>4</sup>

This iconoclastic (but practical) vision makes good sense of **the** disunity of science. We staunchly believe that science must in **the** end be unified, because it tries to tell **the** truth about **the** world, and there is surely only one world. (What a strange statement, as if we had tried counting worlds.) **The sciences** are disunified for all sorts of reasons as cataloged in Hacking (1990). One of these is **the** sheer proliferation of specializations so well recounted by Suppes (1984, chap. 5). But it is also disunified in a way that has not hitherto been much discussed. It is disunified in part because phenomena are produced by fundamentally different techniques, and different theories answer to different phenomena that are only loosely connected. Theories mature in conjuncture with a class of phenomena, and in **the** end our theory and our ways of producing, investigating, and measuring phenomena mutually define each other.

### 13 Truth

Could two theories with no common measure, in **the** above literal sense, both be true? Is not at most one theory true, **the** old mature one or an aspiring new one that takes account of a new data domain? Only if we suppose that there is in **the** end only one true ultimate theory that corresponds to **the** world. Some philosophers who are halfway along this road find solace in saying that different theories are true of different aspects of reality, but what work is “reality” doing here? We need say no more than this: **the** several systematic and topical theories that we retain, at different levels of application, are true to different phenomena and different data domains. Theories are not checked by comparison with a passive world with which we hope they correspond. We do not formulate conjectures and then just look to see if they are true. We invent devices that produce data and isolate or create phenomena, and a network of different levels of

4. A great many distinct ideas can be associated with **the** “no common measure” theme. I distinguished three of them in Hacking 1983, 67–74. In unpublished work, Kuhn expresses a preference for **the** more ordinary word “untranslatable,” to be explained less by a theory of meaning than by a theory of natural kinds and a lexicon of natural-kind terms. I try to develop consequences of this idea in Hacking 1992b. **The** literal version of “no common measure” above—called a “new kind of incommensurability” above—is one aspect of what Pickering (1984, 407–11) calls “global incommensurability,” which he illustrates with **the** contrast between **the** “new” and **the** “old” high energy physics of **the** 1970s and early 1980s.

theory is true to these phenomena. Conversely we may in the end count them as phenomena only when the data can be interpreted by theory. Thus there evolves a curious tailor-made fit between our ideas, our apparatus, and our observations. A coherence theory of truth? No, a coherence theory of thought, action, materials, and marks.

We don't want here a theory of truth at all. Not that I'm against truth, or the word "true" in its place. One of the uses of the word, as has often been remarked, is to enable us to agree with, approve of, or commit ourselves to a batch of assertions that we don't want to bother asserting—out of a desire for brevity or a quest for style, or because we lack the time to talk at length, or because we don't know in detail what the assertions actually assert. We dearly need this use of the word "true" in science, since few can remember what any theory, systematic or topical, is in all its complexities. Hence we refer to theories by their names and say that what we name is true. It is no metaphysics that makes the word "true" so handy, but wit, whose soul is brevity.

We modify, I have said, any or all of my fifteen elements in order to bring them into some kind of consilience. When we have done so we have not read the truth of the world. There usually were not some preexisting phenomena that experiment reported. It made them. There was not some previously organized correspondence between theory and reality that was confirmed. Our theories are at best true to the phenomena that were elicited by instrumentation in order to get a good mesh with theory. The process of modifying the workings of instruments—both materially (we fix them up) and intellectually (we redescribe what they do)—furnishes the glue that keeps our intellectual and material world together. It is what stabilizes science.

#### 14 Application

When defining the laboratory sciences, I said that the end in view was an increase in knowledge, understanding, and control of a general or generalizable sort. If mature laboratory sciences are self-vindicating, answering to phenomena purified or created in the laboratory, how then are they generalizable? For nothing is more notable than our success, from time to time, in transferring stable laboratory science to practical affairs. The aim of most "mission-oriented" science (to use the jargon of a decade ago) in industrial, medical, military, and ecological spheres is precisely to increase our

knowledge and our skills to solve a practical problem that existed before and remains outside **the laboratory**.

I don't think that there is a problem here. Sometimes techniques and devices developed in **the laboratory** move into our larger environment and indeed help us in some already-chosen mission. Sometimes they don't. When prototypes have been made industrial (be they machines or medicines), they will work reliably in controlled conditions. They may or may not be useful in **the** more luxuriant foliage **of** everyday life. In fact, few things that work in **the laboratory** work very well in a thoroughly unmodified world—in a world which has not been bent toward **the laboratory**. That **of** course is a contingent matter; it could have been different. But whatever was **the** case, success or failure in a mission does not vindicate or refute a theory which is true to phenomena generated in **the laboratory**. Vindication and refutation occur only on that site; value in a mission is something else. All **the** jokes about military gadgetry hinge on this banal fact. If people opposed to conventional medicine had a sense **of** humor, and if **the** rest **of** us didn't feel that jokes about disease were sick, then they could make exactly **the** same jokes about medical research that we peaceniks make about weapons research. **The** military like to advertise their gadgets as working with surgical precision. When was **the** last time they were in a surgery?

I must, however, acknowledge a metaphysical worry in **the** offing. I invite it even with my halfhearted use **of** **the** phrase "true to." Suppose I am right, that **the** mature **laboratory sciences** are true to phenomena created in **the laboratory**, thanks to mutual adjustment and ensuing **self-vindication**. If so, **the** applicability **of** **laboratory science** is no mere contingency but something **of** a miracle. There are two distinct responses to this, depending on what kind **of** miracle **the** protester has in mind. I think a metaphysical miracle is intended, but first a more modest one.

Taking as an example Pasteur's success with anthrax, a perfect instance **of** rapid movement **of** knowledge and technique from **laboratory** to **the** field, Latour writes that "if instead **of** gaping at this miracle we look at how a network is extended, sure enough we find a fascinating negotiation between Pasteur and **the** farmers' representatives on how to *transform **the farm into a laboratory***" (Latour 1987, 249; emphasis his). That indicates a special case **of** an enormously important observation. We remake little bits **of** our environment so that they reproduce phenomena first generated in a pure state in **the laboratory**. **The** reproduction is seldom perfect. We need more than **the** (4) topical hypotheses and **the** (5) modeling **of** **the**

laboratory apparatus; we need more thinking of the same kind as (4) and (5). But the application of laboratory science to a part of the world remade into a quasi-laboratory is not problematic, not miraculous, but rather a matter of hard work.

Latour's response nevertheless leads to the metaphysical miracle. For it invites the observation that anthrax has been eliminated from many regions. Smallpox no longer exists on the face of the earth, and the potential for making a person sick of smallpox now exists, we believe, only in a small number of securely locked refrigerators in a few national laboratories. Isn't that because we have found out something about our environment outside the laboratory and then applied our hard-won knowledge? And does that not mean that there are (and were) certain truths about anthrax, in addition to Pasteur's speculations being true to phenomena generated in the laboratory?

The source of this worry is the metaphysical mistake of thinking that truth or the world explains anything. "If the treatment works, then the world or the truth about the world makes it work, and that is what we found out in the laboratory and then applied to the world." Not so. I said that mature laboratory sciences are true to the phenomena of the laboratory. In so saying I was describing, not explaining anything. A science is true to the phenomena when it fits the analyzed data generated by instruments and apparatus, when modeled by topical hypotheses. Every one of those fifteen items of mine that is germane to a test has to be brought in for the vindication of the science, and when the science is mature, they are in such mutual adjustment that there is what I call self-vindication. Indeed, what we want to be the case in mission-oriented research is that the reproducible apparatus (or chemical or whatever) also has happy effects in the untamed world. But it is not the truth of anything that causes or explains the happy effects.

## 15 Induction

The doctrine of mature self-vindicating laboratory sciences has no more to do with the problem of induction than does Popper's methodology of conjectures and refutations or Kuhn's analysis of scientific revolutions. That is as it should be. The problem of induction was posed in connection with bread, postmen, and billiards. It has nothing special to do with science, although it has everything to do with civilization, for the question was posed for the wares of cooks and craftsmen (bread and billiard balls) and for institutionalized



people (postmen). The problem of induction must nevertheless take its own form within my conception of science, just as it must, or should, within every other.

The problem of induction must not be confused with our manifest fallibility. Quite aside from questions about the projection of the past onto the future, there is no guaranteed irrefragable eternal self-vindication of a laboratory science. Sometimes a theory may be true to a body of phenomena and have a closed data domain in the way that I have suggested and yet fail to survive. The transformation of the particle theory of light into the undulatory theory is of just this sort. In the beginning it was not a new kind of instrument that did in the old ideas: the phenomena that made the wave theory compelling were elicited by what one might call Newtonian instrumentation (much of it worked by the adamant corpuscularian David Brewster) even before Fresnel had provided the mathematics of the wave theory that was fully able to interpret the data. A longish period of stability within a data domain does not promise that things have come to an end.

A more interesting case is the caloric theory of sound. Laplace calculated the velocity of sound assuming a substance he called caloric, and it fit the experimental determinations of the day. Yet it looks as if they are out by 30 percent. The velocity of sound is indeed nontrivial (there are at least three distinguishable "velocities of sound"), but even so we can't understand what Laplacean experimenters were doing. We abandon their phenomena as gladly as we forget caloric. So much is familiar conjecture and refutation. It may invite cynicism about stability but not philosophical skepticism, to which I now turn.

I should like to reverse the emphasis of philosophical skepticism. In our time it has chiefly focused on propositions; those true of the past might not hold true of the future. Our expectations and beliefs might not rightly project onto the future. The philosopher of experiment must descend from semantics and think about things and actions instead of ideas and expectations.

A laboratory science could become genuinely unstable. Our technologies might cease to work. Phenomena might no longer oblige. What would change, in my skeptical fantasy, is that our apparatus would no longer be able to elicit phenomena. Nothing that I have said about stability should prevent that form of wonder we call the problem of induction. The question, "why expect the future to be like the past?" takes on a new form for the laboratory and for the phenomena that it produces. "Why should types of devices that we

have made, and have made to behave in certain ways in the past, continue to do so in the future?"<sup>5</sup>

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