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History, Philosophy, and the Central Metaphor

The Argument

Behind the dispute over the relative priority of theory and experiment lie conflicting philosophical images of the nature of scientific inquiry. One crucial image arose in the 1920s, when the logical positivists agitated for a "unity of science" that would ground all meaningful scientific activity on an observational foundation. Their goals and rhetoric dovetailed with the larger movements of architectural, literary, and philosophical modernism. Historians of science followed the positivists by tracking experimental science as the basis for scientific progress. After World War II, historians and philosophers of science created an antipositivist movement, inverting the positivist idea that observation had epistemic (and historical) priority. But this counter-movement retained the modernist aspiration to unity, now chaining observation to theory. Once again historians of science, following their philosophical colleagues, illustrated the new modernism with historical instances of observation dominated by theory.

Either reductionist scheme, by privileging one activity over the other, dictates an overly constrictive periodization. We need a wider class of periodization models ("central metaphors") that will allow instrumentation, experimentation, and theory a partial autonomy without granting any one the sole legitimate narrative standpoint. Such a heterogeneous representation of historical traditions may, surprisingly, make better sense of the perceived coherence of activity across theoretical transitions than the monolithic modernist representation of science it displaces.

1. Experiment and Theory

A factious dispute has arisen over the proper description of the relation of experiment to theory. One side views experiment and observation as coming first – epistemically and most often historically. On this view, the solid foundation of careful empirical work supports a shakier construct of theory designed to order and codify the hard facts of experience: theories come and go, observations are here to stay.

An opposing image of science[1] contends that theory comes first, telling experimenters what to do and, more importantly, fixing the terms of analysis so the results of laboratory work are conditioned in advance. Typically, authors pressing the "theory-first" side begin by denying that there is a coherent way to divide experiment from theory. They then argue that all scientific work is composed of beliefs and hypotheses being put to the test. Propositions, however vaguely formulated at the laboratory bench, are not fundamentally different from those sketched out in chalk on the office blackboard. Experiment is no more than theorizing in a different place and by different means. As Karl Popper put it, "Theory dominates the experimental work from its initial planning up to the finishing touches in the laboratory" (1968, 107).

Why should a working historian care about this debate? The dispute matters because the opposed philosophical assumptions underlying each position are in turn inextricably linked to a specific historiography. What I will call here the "central metaphor" of a *philosophy* of science has deep consequences for the structure of narrative offered by the *history* of science. In this paper I will focus on this link between these philosophical images of the experiment/theory relation and the allied assumptions that govern the writing of the history of science.

Toward this end, it is necessary to reflect on the original motivations for both the "observation-first" outlook and the "theory-first" rejoinder. In the brief remarks that follow, I will link this debate over the experiment/theory relation to an unstated but pervasive set of historiographical assumptions about periodization.[2] The argument is this: In the early decades of the twentieth century, the logical positivists argued for a unity of science; theirs was to be a unity achieved through a description of science that grounded all meaningful scientific activity on an observational foundation. Such a grand reductive unification participated in the broader cultural currents of modernism. Modernist architecture, for example, valorized simple elemental forms, and their universal applicability. In the antipositivist reaction that followed World War II, theory was given primacy. But the modernist struggle for scientific unity persisted, now as a unity grounded on theory, not on observation. Finally, only by challenging the underlying assumptions of this modernist search for a unifying scheme can the historian carve out the conceptual space needed to set the day-to-day culture of experimentation and instrumentation on an equal footing with high theory.

2. Positivism, Modernism, and the Unity of Science

It has become usual in recent history of science to rehearse the shortcomings of standard textbook presentations of scientific progress: observations not in accord

1 Yehuda Elkana has an interesting, though much more general, notion of "images of knowledge," which he uses as a shorthand for the criteria by which different social groups select "from the infinity of available problems in the body of knowledge those on which the research will concentrate" (see, e.g., Elkana 1981, esp. 15–19). My focus here is much more narrowly construed: the image of the relation of theory to practice expressed in twentieth-century philosophical and historical accounts of science.

2 The following discussion builds on Galison 1987, esp. chaps. 1, 5, and 6.

with previous conceptions of the world accumulate until they force a new set of theoretical views on the scientific community. Even now it is not hard to find physics textbooks that recount the origins of special relativity in terms of the inexorable march of optical ether-drift experiments.[3] According to these potted versions of history, Einstein "simply" generalized the clear observational fact that motion with respect to the ether could not be observed. In this way the strength of physical argumentation is rhetorically linked to its connection with observation (or experiment) and the historical sequence is described in such a way as to enhance the role of experience and denigrate the corresponding theoretical analysis.

The privileging of observation and experiment over theory is common both to a particular genre of *historical narrative* and to the tradition of *philosophical positivism*. Indeed, the emphasis on observation is one of the salient features that binds such otherwise diverse philosophers as Ernst Mach and Rudolf Carnap. In each case theoretical speculation is closely reined in by observation: Mach dismissed the ontological claims of atomism because of the long (and to his mind dubious) chain of inferences from observation to atoms.

Carnap's masterwork, *Der Logische Aufbau der Welt,* is usually translated as *The Logical Structure of the World,* but might better be rendered as *The Logical Construction of the World.* Its goal was stupendous: from elements of an individual's experience, the author would construct physics, then individual psychology, and ultimately all the meaningful concepts of all special social and natural sciences. The project rested on the assumption that each concept could be reduced, or transformed, into a set of propositions ("the protocol language") of immediate sense experience: "here now pointer at 5, simultaneously spark and explosion, then smell of ozone there" (Carnap 1981, 153). Later, Neurath argued at great length – and eventually persuaded Carnap – of the need for a universal, "physicalist" protocol language that would not be grounded on an individual's perception. Neurath's physicalist language would then serve as a common point of departure for all disciplines in all places at all times (e.g., Neurath 1983, 52–57).

In the early and heady days of the Vienna Circle, in the 1920s, such pronouncements about immediate experience were offered as the firm foundation of scientific thought. "We assumed," Carnap later recounted, "that there was a certain rock bottom of knowledge, the knowledge of the immediately given, which was indubitable. Every other kind of knowledge was supposed to be firmly supported by this basis and therefore likewise decidable with certainty. This was the picture which I had given in the *Logischer Aufbau* . . ." (1963, 57). It was a viewpoint, Carnap felt, that was supported by Mach, by Russell's "logical atomism," and by Wittgenstein's sweeping assertion that all propositions could be reduced to truth-functions of elementary propositions.

Young Carnap's protocol sentences were designed to serve as a litmus test for science: propositions that could be cashed out into these statements of immediate

[3] For criticism of this view see Holton 1973.

experience had meaning, those that could not were dismissed as meaningless. Though they differed on particular points, the Vienna Circle could be characterized by concern with meaning and language.[4]

The development of these protocol sentences allowed Carnap to build up what he referred to as an intersubjective "*universal* language." In the context of the mid-1920s, this search for universality had a peculiarly modern slant, echoing calls for universality in other fields. In his philosophical system, Carnap offered his protocol-based *lingua franca* to be available to every person and applicable to all states of affairs; it could therefore serve to unify all of the sciences. Indeed, in 1928, in one of the earliest Vienna Circle tracts, the authors proclaimed their program to be directed precisely towards such a

> unified Science: [whose] endeavors are to relate and harmonize achievements of individual researchers in the various branches of Science. From this choice of subjects arises the emphasis on collective work; hence also the prominence allotted to communicable knowledge; these aims inspire the search for a neutral system of symbols, free from the dross of historical languages.... (Black 1934, 10.)

The manifesto's commitment to internationalism and an international method and language of science drew from the Vienna Circle's various concerns – Carnap's long-standing humanistic and political commitment to international languages (he actively supported the study of Esperanto, Ido, Latino sine flexione, Occidental, and Interlingua); Neurath's Marxist commitment to internationalism; and a collective belief in modernist philosophy as a purging of the traditional basis of knowledge and philosophical arguments.

Carnap's almost palpable anger at the traditional questions of philosophy (especially metaphysics) has its own historical context. Within the Vienna Circle, few if any of the participants had a traditional philosophical training: Carnap had done his thesis in experimental physics, Neurath was a sociologist as much as a philosopher, and Schlick, like Carnap, had done graduate work in physics.[5] With their newly gained scientific philosophy all seemed possible, and Carnap recalls the excitement with which he read Russell's *Our Knowledge of the External World,* the last words of which called out for philosophy to follow the physicists into the uncharted waters of the new: "The one and only condition, I believe, which is necessary in order to secure for philosophy in the near future an achievement surpassing all that has hitherto been accomplished by philosophers, is the creation of a school of men with scientific training and philosophical interests, unhampered by

[4] Over the next few years the hard line that propositions had to have direct empirical correlates was partially relaxed, and by 1934 Carnap allowed that "verification in science is not . . . of single statements but of the entire system or a subsystem of such statements" (1934, 42).

[5] Schlick had done his dissertation with Max Planck in 1904, on the reflection of light in a non-homogeneous medium (Schlick 1974, xvii); Carnap had begun work on a doctorate in physics (on thermionic emission) in 1913, but stopped work when his advisor was killed in World War I (Carnap 1963, 6–7).

the traditions of the past, and not misled by the literary methods of those who copy the ancients in all except their merits."[6]

This modernist rhetoric of Russell, Neurath, Reichenbach, Schlick, and Carnap went beyond a dismissal of philosophical traditions. It was coupled with a highlighting of experience over theorizing, a deep-going commitment to the *unification* of the branches of the sciences under one unitary science, and the firm belief in progress – not only for science but for philosophy.

Philosophical modernism had close affinities with other brands of modernism; it paralleled the manifestoes of art, architecture, and politics of the 1920s. Indeed, the early tracts of the logical positivists resemble far more the daring pronouncements of the Italian Futurists, or the Russian Constructivists, than they do the more usual philosophical fare of the British Hegelians or the German and Austrian neo-Kantians. At the time Carnap was fighting for a reduction of all science to protocol sentences, the leading Bauhaus architects were advocating an abandonment of the superfluous ornamentation of traditional buildings and a reduction to the elementary forms of geometry. In this sense, Carnap's *Aufbau* ought to be seen as one of the great modernist endeavors, along with the Bauhaus; both fit to a tee Mies van der Rohe's dictum, "less is more." Just as the logical positivists wanted to unify all the sciences through their common basis in protocol statements, the Bauhaus architects sought to unify all the visual arts through a shared idiom of geometrical forms.

Think of the Swiss modern architect Le Corbusier who, in 1925, declared that "geometry is the basis. It is also the material foundation for symbols signifying perfection. . . . The machine develops out of geometry. Thus the whole of the modern age is made up above all of geometry; it directs its dreams towards the joys of geometry" (1964, 89). Or think of El Lissitzky: "Our age demands creations arising out of elemental forms (geometry)" (1964, 121). In both philosophy and architecture the defense of modernism is predicated on the search for a simple (elemental) "basis," a commitment to internationalist unity, and a sense that each enterprise was entering into a new epoch of progress associated with the age of science. Lissitzky argued that "an equals sign has been placed between engineer and architect" (1964, 122); Carnap put one between engineer and philosopher.

The primacy of observation thus counted for the positivists as a foundation of knowledge. Recasting the scientific enterprise into a form in which this basis was visible thus served the vital function of justifying theories that should be left standing, and exposing the frailty of structures without such empirical roots. Because such an empiricism was to be the gatekeeper of science, it is no surprise that historical accounts of scientific developments tended to adopt a rhetoric stressing observational foundations.

Philosopher and historian found common cause in the depiction of a science that was hierarchically built on experimental foundations. This went almost without

[6] Russell 1914, 242; Carnap cites this quotation as being particularly influential upon him when he read it in 1921 (Carnap 1963, 13).

saying for many historical projects. James Conant, for example, organized his justly famous *Case Histories in Experimental Science* around the premise that everyone needed to understand scientific reasoning and that scientific reasoning amounted to the reasoning contained in great experiments (Conant [1957] 1970, vii–viii). The two enterprises – philosophy of science and history of science – supported each other. For the positivist, because justified knowledge was ultimately grounded in observation, there was every expectation that a proper look into the historical record of classic scientific achievements would reveal that knowledge is built up from well-grounded observations.

The philosophical view that a theory-independent observation language exists, or might one day be constructed, also fits well with a strictly progressive image of the history of science. Observations accumulate, improving in scope and precision. Theories rise as they account for a greater fraction of this bedrock observation, and fall when they fail to account for it. Over the years the representation of progress changed somewhat, as Popperians substituted falsification for verification, or as Carnap introduced probabilistic confirmationism as an alternative to strict verificationism, but the existence of a secure, unproblematic empirical foundation on which to build remained essentially unchallenged.[7]

In a schematic but nonetheless revealing fashion, it is possible to represent what I will call the positivist's central metaphor as a graphical periodization of experiment and theory. In figure 1, time is taken to run from left to right. The lower "foundational" tier, labeled "observation, experiment," deliberately groups the two concepts together since the logical empiricists – their name notwithstanding – devoted practically no effort to the differentiation of types of empirical work.

. . .	theory_1	theory_2	theory_3	theory_4	. . .
. . .			observation, experiment		. . .

time ⟶

Figure 1. Positivist periodization. Observation (a highly schematized and so impoverished form of experimentation) forms a cumulative foundation and so is essentially without breaks. Theory, built on observation, does have breaks as new schemes are produced that reorganize past observations.

There is no break in the continuity of observational efforts, since the logical empiricists never questioned the cumulative ~~picture~~ of observation.[8] Coupled with the positivist's insistence on a cumulative, observational layer of activity is a fragmented

[7] Of course these doctrines did not have identical goals. Popper's falsificationism was to demarcate the limits of science; Carnap's verificationism and later confirmationism were to delimit the domain of the meaningful.

[8] Of course some empiricists might restrict themselves to naked-eye observation, others might welcome the use of amplifying instruments.

history of theory. The upper "superstructural" stratum depicts these laws and theories, which function ephemerally as organizers of the ever-growing body of observational facts. That there are breaks in this constructed level of activity comes as no surprise to the positivist. The logical empiricist grounds science on the observationally accessible, not the theoretical codifications of those hard-won facts.

3. Antipositivism and Modernism

Reacting against the radical division of primitive observation reports from pure theory and logic, historians and philosophers of science attacked the positivist picture during the 1950s and 1960s. In a sense, W. V. O. Quine is a transition figure between the two views. He accepted a kind of observation basis, one grounded on the behaviorist criterion of how an average person would respond under normal conditions to a specified stimulus. But Quine strongly opposed the total separation of observation from other forms of knowledge; for him, all were up for evaluation. As part of his analysis, Quine objected to the positivist's assumption that observation statements could be correlated one by one with theoretical propositions. Instead, he argued in his famous essay of 1951, "Two Dogmas of Empiricism," that, "I am . . . urging that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science." To replace the older metaphors, Quine offered his own famous image of a "man-made fabric which impinges on experience only along the edges." This drum head (if that is what it was) was flexible and the movement of one bit of theory would be accompanied by further movement elsewhere. "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system." Mere empirical statements are more easily reevaluated than highly theoretical ones. In particular, statements of physics, or logic, or ontology "may be thought of as relatively centrally located within the total network, meaning merely that little preferential connection with any particular sense data obtrudes itself" (Quine 1953, 44). Quine's view made all of science (observations, theory, and even logic) revisable, but placed high theory – the most abstract and general features of mathematical physics and logic – in a privileged position.

Others went further. Above all, the antipositivists sought to undermine radically the priority of observation and to bring theory to the fore. Total separation of observation from theory was impossible, they argued: Show us an experiment and we will expose the hidden theoretical assumptions behind it. The antipositivists insisted that no Carnapian protocol language could exist even in principle, a result variously labeled "theory contamination" or "theory-ladenness." In unison with the antipositivist philosophers, historians of science provided example after example in which theory changed first – and with theory change, more than experimental interpretation was reorganized.

Some antipositivist authors, for example Kuhn, went even further, drawing an analogy between theory change and the abrupt shifts of perception characteristic of the famous experiments in Gestalt psychology. Religious conversions offered another model for the adoption of a new scientific theory instead of the old, as did the difficulties of translating between languages. In support of this picture of theory-governed scientific thought, it became standard to invoke historical examples in which "more phlogiston" became "less oxygen"; electrons going up in a cloud chamber became positrons heading down. As the terms in our theories changed, the population of the world changed as well.

Paul Feyerabend put it bluntly:

> [My] thesis can be read as a philosophical thesis about the influence of theories on our observations. It then asserts that observations (observation terms) are not merely theory-*laden* (the position of Hanson, Hesse and others) but *fully theoretical* (observation statements have no "observational core"). But the thesis can also be read as a historical thesis concerning the use of theoretical terms by scientists. In this case it asserts that scientists often use theories to restructure abstract matters *as well as* phenomena, and that no part of the phenomena is exempt from the possibility of being restructured in this way. (1981, x.)

The corrective picture introduced by Thomas Kuhn, Paul Feyerabend, N. R. Hanson, Mary Hesse, and others directly confronted the positivist's empirical domain. Science no longer could be represented as advancing in a cumulative fashion; indeed, many authors began to emphasize the epistemic ruptures introduced by theoretical shifts. Across the fissures opened by theoretical change, little could cross. Some authors used the image of gestalt shifts to capture the radical reorganization of the empirical world. Others referred to sociological disruptions between generations, or to ontological rifts tearing like geological faults between theories on either side of the revolutionary divide.

Central to this image of science was the idea that the positivist's dream of correlating bits of experience with theoretical propositions would never succeed. Theory itself had to be accorded pride of place. Imre Lakatos chose to model the basic unit of scientific progress (the program) by a series of concentric rings – not too different in spirit from Quine's battened-down fabric. In the center (where else?) lay the "hard core" of a theory; for example, in the Newtonian program, the dynamical laws and inverse square law of gravitation lay in the hard core. Surrounding and insulating this theoretical core was the "protective belt" composed of auxiliary assumptions added to the program to rescue the hard-core beliefs against refutation. As long as the adding of auxiliary assumptions led to fruitful new discoveries and explanations, the program was progressive; when the auxiliary assumptions needed to protect the core began contributing only marginally to the advancement of learning, the program "degenerated" and was discarded.

Of interest in the Lakatosian model is, once again, the *primacy* of theory: theoretical assumptions lie in the hard core and survive all but the most sustained attack against the program as a whole. When the basic elements of theory do eventually crumble, the totality of the program collapses, hard core along with the myriad of lesser, "lower-level" assumptions linking high theory to the phenomenal world. Such a view was intended to break irrevocably with the positivists' position.

For positivists like Hempel, both philosophy and history showed the gradual build-up from experiment to phenomenal laws, and ultimately to theory. Thus, according to Hempel, the archetypical progression from fact to theory is well illustrated in atomic physics: Ångström mapped the spectral lines of hydrogen, J. J. Balmer codified those relations in an empirical formula, and Niels Bohr explained the formula on the basis of the old quantum theory (Hempel 1966, 37–39). By contrast, in Lakatos' scheme experiment and empirical laws play a negligible role. Of Balmer, Lakatos has this to say:

> The progress of science would hardly have been delayed had we lacked the laudable trials and errors of the ingenious Swiss school-teacher: the speculative mainline of science, carried forward by the bold speculation of Planck, Rutherford, Einstein and Bohr would have produced Balmer's results deductively, as test-statements of their theories, without Balmer's so-called "pioneering." (Lakatos 1970, 147.)

For Lakatos, as for many of his antipositivist contemporaries, theory was the engine of scientific change. Theory advanced of its own accord ("In the rational reconstruction of science there is little reward for the pains of the discoverers of 'naive conjectures'." [ibid.]) and the interesting history was the history of this domain of speculative exploration. Because theory was so central, when theory itself was fragmented, the whole cloth of scientific activity was effectively torn into unrelated bits. Instead of the image conveyed in figure 1, the antipositivists chose as their central metaphor a picture of scientific change that was grounded in theory.

To enforce the gestalt-switch character of the shift, it was necessary to insist that the moment of theory change was also the moment of empirical shift. I have tried to capture this image in figure 2, now with the breaks of periodization occurring simultaneously at the theoretical and experimental levels. Furthermore, the *direction* of epistemic primacy has shifted from the empirical to the theoretical. The statement that it is impossible to communicate across empirical gaps appears in this image as the totality of the rupture through all layers of scientific practice. Or, said another way, the absence of a continuous substratum of common practice across the break underlies the image of "different worlds," in which there is no overarching notion of progress, that has generated so much controversy in the community of historians and philosophers of science.

The central metaphor of the antipositivists has much to recommend it. By its critique of the positivist fantasy of a simply progressive empirical domain, the antipositivists' metaphor drew attention to the dynamic role that theory plays in experimental practice. This created historiographical room to link theoretical concerns with the larger context of scientific work including philosophical commitments,

...	observation$_1$	observation$_2$	observation$_3$	observation$_4$ ...
...	theory$_1$	theory$_2$	theory$_3$	theory$_4$ ...

time ⟶

Figure 2. Antipositivist periodization. Reversing the positivist hierarchy, antipositivists placed theory as the "ground" displacing observation to a secondary role. When theory suffers a discontinuity, so does observation.

ideological assumptions, or national styles of reasoning. A myriad of interesting historical studies have revealed how theoretical notions have significantly altered the construction, interpretation, and evaluation of experimentally produced data. Moreover, there is no doubt – as the antipositivists persuasively argued – that there are breaks in the arena of observation. The systematic study of the attraction and repulsion of rubbed objects does not continuously meld into the later experimental investigations into electrostatics and then electrodynamics.

There is an elegance to both the positivist and antipositivist pictures. Both have a grand scope as they set out to find universal patterns of scientific evolution, and both follow language as the guide to that understanding. In Carnap's words, "there is a *unity of language* in science, viz., a common reduction basis for the terms of all branches of science, this basis consisting of a very narrow and homogeneous class of terms of the physical thing-language" (Carnap 1955, 61). Neurath put the same thought less technically when he insisted on the universality – the internationality – of the language of science which was to underlie the move towards unified science: "Unified science is therefore supported, in general, by the scientific attitude which is based on the internationality of the use of the language of everyday life and on the internationality of the use of scientific language" (Neurath 1955, 23). It is this sense of the linguistic nature of the science problem that underlay both the positivist credo of unification and the antipositivist departures from it. Thus, as Charles Morris put it, "The degree of unity or disunity of science reveals itself here in the degree to which the sciences have or can have a common linguistic structure" (Morris 1955, 69).

When the antipositivists lashed out at the positivists they accepted Charles Morris' parameters of the debate – and answered that there was no common linguistic structure. Certainly this is a consequence of Quine's work on the indeterminacy of translation. It is also what I take to be the import of what Ian Hacking calls Kuhn's "meaning incommensurability," the inability of one language and its referential structure to translate fully into another language system.[9] Allied with that split are the others mentioned earlier: for Kuhn there is no "protocol language" that would serve as a common referent for the two languages. For the young Carnap there was.

[9] Quine's indeterminacy of translation is, quite different from Kuhn's meaning incommensurability. For Quine, translation is determinate with respect to a given manual of translation; the difficulty is that there are many possible manuals. For Kuhn, there isn't even one fully adequate translation system between theories.

For Kuhn, theory came epistemically first by setting out the boundaries of the language; for Carnap the observation language came first providing just that set of primitives from which the conventions of the higher-order language would be built.

Despite their disagreements about the existence of an independent observation language, at the core of the views of both Kuhn and Carnap lies the assumption that the activity of science is principally to be understood as an unravelling of the difficulties of language and reference. Furthermore, both positivist and antipositivist insist that there is a structure to which the evolution of scientific propositions could be reduced. For the logical positivists there were various criteria that offered a rationality to the substitution of one theory for another. These included confirmationism and verificationism. For Kuhn too there was a universal structure for the supplanting of one theory with another in his timeless cycle of epochs: normal science, crisis science, revolutionary science, and the return of normal science.[10] Like Polybius' cycle of governments, the Kuhnian structure transcended time and place. In a curious way it fulfilled one dream of the Encyclopedia by providing a unification of the sciences at the level of method. It was thus highly appropriate – more so than usually realized – that *The Structure of Scientific Revolutions* formed chapter two in volume two of the *International Encyclopedia of Unified Science.*

Kuhnian antipositivism and logical positivism thus share the search for a universal procedure of scientific advancement and a view that language and reference form the chief difficulty in the analysis of the experiment/theory relation. But the ties between positivist and antipositivist go further. Both models have a well-established hierarchy that lends unity to the process of scientific work. True, they are flip-side versions of one another, but in their mirror reflections there is a good deal of similarity. The central metaphor of figure 2 is an inverted version of figure 1, with the special assumption – in Kuhn's case – that the important experimental and theoretical breaks occur contemporaneously. The unity of each account is, to a certain extent, enforced by the provision of a privileged vantage point, what the literary critics would call a "master narrative": in the case of the positivists it is from the "observational foundation" building up in the case of the antipositivists it is from the theoretical "paradigm," "conceptual scheme," or "hard core" looking down.[11]

4. A Critical "Postmodern" Model

It is time, however, to reexamine critically the antipositivists' central metaphor, for in some respects it – like its inverted positivist image – carries a broad historiographical burden that it may not be able to support. There are three aspects in need of revision. First, antipositivist history and philosophy of science has tended to focus almost exclusively on theory or, when experimental work does enter, attention fastens on

[10] For some antipositivists, such as Feyerabend, there is no such universal schema; the exchange of one theory for another is anarchic.

[11] On Lyotard's notion of the master narrative see, e.g., Lyotard 1984.

experimental results ("facts"), as they are used to confirm, refute, or generate theory. When experiments themselves become central, the guiding question is almost invariably, "how do theories affect the acceptance or rejection of experimental outcomes?" When communities or sociological groupings are considered, the techniques used to study them work well for theoretical physicists: exchange of preprints, shared textbooks, shared theoretical assumptions, etc.

But physicists are not a homogeneous community. At least since the early twentieth century, physicists have themselves recognized the split between theorists and experimentalists.[12] The two groups are frequently considered separately as they apply to graduate school; in graduate school they take different courses; subsequently they enter upon markedly different apprenticeships as they develop their research. Techniques and practices shared among experimentalists form their own, partially autonomous, traditions not necessarily coincident with theoretical traditions. Thus any model that we adopt, even a provisional one, ought to leave room not only for theoretical traditions, with breaks and continuities, but for experimental traditions with their own, equally complex set of internal dynamics.

Just as the antipositivists accorded autonomy to the theoretical domain and so opened our understanding of theories' links with the broader history of ideas, an acknowledgment of experimental traditions could create the historical space to link laboratory work with the history of instruments, industry, and the wider domain of the history of material culture. For example, the cloud chamber, the workhorse of early twentieth-century atomic physics, has its roots in early photography and meteorological instruments (Galison and Assmus, forthcoming [1988]).

The critical model we need must also avoid the unwarranted assumption – shared by both positivist and antipositivist – that there is a universally fixed, hierarchical relation between experiment and theory. There certainly is no a priori reason to assume (with the positivists) that experiment "comes first" epistemically or historically. Nor should one assume (with the antipositivists) that theory is privileged over experience. In individual instances, it is frequently the case that one may impose structure on the other. But this is exactly the task at hand: to discover and articulate the mediative processes by which experiments and theories each constrain the other's activity.

There is a third, seemingly prosaic feature of the everyday practice of physics that the new critical model must incorporate. Just as we descended from the heights of pure theory to grant experiment a "life of its own" (to use Ian Hacking's felicitous phrase [1983]), the level of experimentation itself is not identical to a partially autonomous level of material culture. Here again one discovers continuities and

[12] See Galison 1987, esp. 137ff. I take physics as an example, first, because I know it best, and second, because the split between experimentalists and theorists is instantiated quite evidently in the social organization of the discipline and in the day-to-day practice of the science. In other fields, such as microbiology, the split is more subtle. Few microbiologists consider themselves "experimentalists" as *opposed* to theorists. I think, nonetheless, that there are identifiable traditions within the instrumentation, theory, and experimentation of fields like microbiology, but these distinctions must be drawn within the complex of activities conducted by the same people.

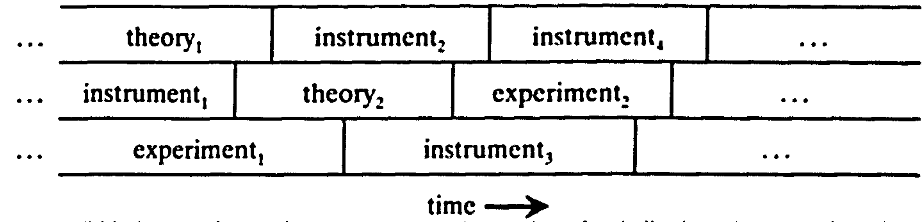

Figure 3. Critical postmodern periodization. Here we have a class of periodizations; there is no single fixed fashion in which the patterns of continuity and discontinuity are aligned, and there is no reductive hierarchy. Observation as a category is discarded, and experimentation, theory, and instrumentation are accorded levels of partial autonomy. Most importantly, the breaks can occur at any level *and* the levels are intercalated so it is quite possible for continuity to persist at one level while being broken at another.

breaks that do not necessarily coincide. The design and refinement of instruments often cuts across experimental programs. Instrument builders have their own journals, their own publications, their own exchange of machine components, designs, and, in more recent times, computer programs. Instruments often link together experiments that may appear unrelated when specified only at the level of results and theoretical questions. Instruments embody continuities of practice, expertise, and objects. Experimentalists recognize these links; this partially autonomous instrumental level needs a representation in our picture.

As a first sketch of a new central metaphor, or rather a more open field of infinitely many central metaphors – consider figure 3. By breaking up the experimental level into *intercalated* patches of continuity and discontinuity we incorporate the insight of the antipositivists: experiment and experience do not give unmediated access to universal, basic propositions. At the same time, by allowing experiment to continue across theoretical breaks, we (partially) resurrect the positivists' contention that theories often do change while leaving unbroken a chain – or at least a surviving section of links – of nontheoretical activity. (The qualifier "partially" is inserted as a reminder that (1) the levels other than high theory are not without theory altogether, and (2) the positivists' conception of observation was altogether too impoverished to embrace the full gamut of experimental and instrument-making activities.)

By granting a measure of autonomy to the practices of experimentation and instrumentation, we recognize what has become obvious to laboratory workers: the daily activities of instrument builders and experimentalists and their perspective on their discipline often differ strikingly from each other, and from the view of their theoretical colleagues in the upstairs office. Material culture and the accompanying practices of the laboratory are not vulgarized versions of high theory, nor its primitive building blocks. Thus in addition to being intercalated, there are three strata in figure 3 – experiments, instruments, and theory – not the older categorization of theory and observation.

By dropping the requirement of a universal, hierarchical relationship between theory and practice, we allow the subcultures of physics to follow whatever dynamical process exists in different historical epochs. For this reason, in addition to allowing the strata to be intercalated in their periodization, they are also *interspersed* to avoid any implication that I simply want to substitute a technological determinism for the earlier observation-foundationalism or theoro-centrism. This expanded metaphor (or class of metaphors) allows for a wide variety of possible relations between the categories of instrumentation, experimentation, and theorizing.

It may well be that for periods of time laboratories themselves constitute the central, driving power of the discipline. High-level theory may drop out of the picture altogether. Certainly the first years of high-temperature superconductors fit the model: theory so militated against their possibility that some workers in the field had to proceed without their bosses knowing what they were up to. Even after the initial discovery of these startling ceramics, the experimenters were struggling to make the material behave consistently, mold properly, and so on – their last worry was how precisely the Bardeen-Cooper-Schriefer electron-pair theory would ultimately be modified or supplanted. Condensed-matter theorists had virtually nothing to say about the startling new phenomena. At other times it is equally apparent that theory dominates; the field of theoretical high-energy physics turned in the 1980s to the study of strings – their links with algebraic topologists prospered while there was not even a clear prospect of linking their efforts to the activities of their experimental colleagues.

Finally, in an interesting way, the brick model may suggest how the practice of physics can involve discontinuities at so many levels and yet not disintegrate into isolated blocks of noncommunicating clay. When masons build real walls they know better than to stack bricks up one directly on top of another: the fault line of the masonry would leave the wall all too vulnerable to shock along the alignment. Only in an intercalated arrangement do the bricks give the edifice strength.

Disorder increases strength. This property at first seems surprising but it is a phenomenon repeated in many contexts. Condensed matter physicists know that amorphous solids are frequently preferable to crystals because crystalline order can catastrophically break down along axes of symmetry, ruining the possibility of predictable electronic behavior. Amorphous solids – precisely because of their long-range disorder – are utterly reliable; and, the mechanical strength of crystals is weakest along lines of symmetry – as any diamond cutter knows.

As with any metaphor, this one ultimately fails. Surely one could easily add more layers, or articulate more detail within each layer. More seriously, the metaphor only incompletely represents the interpenetration of the different layers and is hardly adequate to express the details of the interaction between strata. And of course it does not begin to do justice to the dynamical process by which new bits are added to it. But the point of this exploration is to carve out new space for research, to allow for an investigation of scientific practice in which experimentation is not depicted as a mere prerequisite to the construction of theory, nor as a primitive kind of theorizing.

We need a historiography with room for a multiplicity of cultures within the much larger rubric of scientific practice: a culture of theory, surely, but also a culture of experimentation, and a culture of instrument building. We need a history of the material culture of science, but one that is not the dead collection of discarded instruments. In its place we need a history of the way that scientists deploy objects to meet experimental goals whether or not these were set by high theory; a history of instrument-construction linked to the history of technology; a history that encompasses the relation of instruments to forms of demonstration; a history of the laboratory that tracks the development of the organization of scientific work; and a history of the embodiment of theory in hardware.

In short, we need a history of the laboratory without idolatry and without iconoclasm. We need a history of experimentation that accords that activity the same depth of structure, quirks, breaks, continuities, and traditions that we have come to expect from theory.

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References

Black, M., 1934. Introduction to Carnap 1934.

Carnap, Rudolph, 1934. *The Unity of Science,* trans. and ed. M. Black. London: Kegan Paul, Trench, Trubner and Co.

—, 1955. "Logical Foundations of the Unity of Science," in Neurath, Morris, and Carnap 1955.

—, 1963. "Intellectual Autobiography," in Schilpp 1963.

—, 1981. "Protocol Statements and the Formal Mode of Speech," in *Essential Readings in Logical Positivism,* ed. O. Hanfling. Oxford: Blackwell.

Cohen Robert S. and Marie Neurath, eds., 1983. *Otto Neurath Philosophical Papers, 1913–1946.* Dordrecht: Reidel.

Conant, James, general ed., Leonard K. Nash, associate ed., [1957] 1970. *Harvard Case Histories in Experimental Science.* Cambridge, Mass.: Harvard University Press.

Conrads, U., ed., 1964. *Programs and Manifestoes on Twentieth-Century Architecture,* Cambridge, Mass.: MIT Press.

Elkana, Yehuda, 1981. "A Programmatic Attempt at an Anthropology of Knowledge," in *Sciences and Cultures,* ed. E. Mendelsohn and Y. Elkana, 1–76. Dordrecht: Reidel.

Feyerabend, Paul, 1981. *Realism, Rationalism, and Scientific Method.* Philosophical Papers, vol. I. Cambridge: Cambridge University Press.

Galison, Peter, 1987. *How Experiments End.* Chicago: The University of Chicago Press.

Galison, Peter and Alexi Assmus, forthcoming (1988). "Artificial Clouds, Real Particles," in Gooding, Pinch and Schaffer 1988.

Gooding, David, Trevor Pinch, and Simon Schaffer, eds., forthcoming (1988). *The Uses of Experiment.* Cambridge: Cambridge University Press.

Hacking, Ian, 1983. *Representing and Intervening.* Cambridge: Cambridge University Press.

Hempel, Carl G., 1966. *Philosophy of Natural Science.* Englewood Cliffs, N.J: Prentice Hall.

Holton, Gerald R., 1973. *Thematic Origins of Scientific Thought.* Harvard: Harvard University Press.

Lakatos, Imre, 1970. "Methodology of Scientific Research Programs," in Lakatos and Musgrave 1970.

Lakatos, Imre and A. Musgrave, eds., 1970. *Criticism and the Growth of Knowledge.* Cambridge: Cambridge University Press.

Le Corbusier, 1925. "Collection de l'Esprit Nouveau," reprinted in Conrads 1964.

Lissitsky, Eleazar Markovich, 1929. Reprinted in Conrads 1964.

Lyotard, Jean-François, 1984. *The Postmodern Condition: A Report on Knowledge.* Minneapolis: University of Minnesota Press.

Morris, Charles, 1955. "Scientific Empiricism," in Neurath, Morris and Carnap 1955.

Neurath, Otto, 1955. "Unified Science as Encyclopedic Integration," in Neurath, Morris, and Carnap 1955.

—, 1983. "Physicalism," in Cohen and Neurath 1983.

Neurath O., C. Morris, and R. Carnap, eds., 1955. *International Encyclopedia of Unified Science,* 2 vols. Chicago: The University of Chicago Press.

Popper, Karl, 1968. *Logic of Scientific Discovery.* New York: Harper and Row.

Quine, W. V. O., 1953. "Two Dogmas of Empiricism," in *From A Logical Point of View.* Cambridge, Mass.: Harvard University Press.

Russell, Bertrand, 1914. *Our Knowledge of the External World.* Chicago and London: Open Court.

Schilpp, Paul A., ed., 1963. *The Philosophy of Rudolf Carnap.* La Salle, Ill.: Open Court.

Schlick, Moritz, 1974. *General Theory of Knowledge.* New York and Vienna: Springer.

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