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EDITED BY
MARIO BIAGIOLI

IN CONSULTATION WITH
PETER GALISON · DONNA J. HARAWAY
EMILY MARTIN · EVERETT MENDELSOHN
SHARON TRAWEEK

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25. Ibid., 39.
26. Ibid., 48 (my emphasis).
27. Ibid., 50.
28. Thomas Nagel, "Sexual Perversion," *Journal of Philosophy* (16 January 1969): 15.
29. Ibid., 16.
30. Nagel, *Mortal Questions*, 50–51.
31. Ian Hacking, "Proof and Eternal Truths: Descartes and Leibniz," in S. Geukroger, ed., *Descartes: Philosophy, Mathematics and Physics*, (New York: Barnes and Noble, 1980), 169.
32. See n. 12.
33. A. C. Crombie, "Philosophical Presuppositions and Shifting Interpretations of Galileo," in J. Hinfikka, D. Gruedner, and E. Agazzi, eds., *Theory Change* (Dorohecht: Reidel, 1981), 283.
34. Michel Foucault, *Herculine Barbin, Being the Recently Discovered Memoirs of a Nineteenth-Century French Hermaphrodite* (New York: Pantheon, 1980), vii–viii.
35. Ibid., viii.
36. Ibid., 127–28 (my emphasis).
37. Ibid., 135–36.
38. Ibid., 123. Tardieu's book was published in 1874. Parts of it had previously appeared in the *Annales d'hygiène publique* in 1872. A fuller discussion of questions of sexual identity would have to consider this document in detail.
39. Havelock Ellis, "Sexo-Aesthetic Inversion," *Alienist and Neurologist* 34 (1913): 156–67.
40. Ibid., 156.
41. Ibid., 159.
42. Ibid., 158, n. 7.
43. American Psychiatric Association, *Diagnostic and Statistical Manual of Mental Disorders*, 3d ed. (Washington, D.C.: American Psychiatric Association, 1980), 261.
44. D. M. Rozier, *Des habitudes secrètes ou des maladies produites par l'onanisme chez les femmes* (Paris: Audin, 1830). For a discussion of the changing iconography of the insane, see Sander L. Gilman, *Seeing the Insane* (New York: John Wiley, 1982).
45. Stanley Cavell, *The Claim of Reason: Wittgenstein, Skepticism, Morality, and Tragedy* (Oxford: Clarendon Press, 1979), 121.

10

Trading Zone

Coordinating Action and Belief

PETER GALISON

PART I: INTERCALATION

INTRODUCTION: THE MANY CULTURES OF PHYSICS

I will argue this: science is disunified, and—against our first intuitions—it is precisely the *dis*unification of science that underpins its strength and stability. This argument stands in opposition to the tenets of two well-established philosophical movements: the logical positivists of the 1920s and 1930s who argued that unification underlies the coherence and stability of the sciences, and the antipositivists of the 1950s and 1960s who contended that disunification implies instability. In *Image and Logic,* I have tried to bring out just how partial a theory-centered, single culture view of physics must be. Forms of work, modes of demonstration, ontological commitment—all differ among the many traditions that compose physics at any given time in the twentieth century. In this chapter, drawing on related work in the history and philosophy of science, I will argue that even specialties within physics cannot be considered as homogeneous communities. Returning to the idea of intuition I have sketched elsewhere, I want to reflect at greater length on a description of physics that would neither be unified nor splintered into isolated fragments. I will call this multicultural history of the development of physics *intercalated,* because the many traditions coordinate with one another without homogenization. Different finite traditions of theorizing, experimenting, instrument making, and engineering meet—even transform one another—but for all that they do not lose their separate identities and practices.

To oversimplify one might say the following: the logical positivists took the unification project to involve the identification of a "basis" language of observation that would be foundational across all theory. Antipositivists conclusively (in my view) demolished the possibility of such a hard and fast line between experiment and theory, and concluded (rightly) that no such "protocol language" could exist. But their argument went further, to a vision of science in which not only were theory and experiment inextricable from one another but also they lost their separate dynamics to the point where it did not make sense to think about breaks in one sphere of activity without concomitant shifts in the other. There is another (logical/historiographical/philosophical) alternative: invert the quantifiers. Agree that there is no observation language valid across every theory change, but at least leave open the possibility that for each change of theory (or experiment or instrumentation) there is a sphere of practice that continues unbroken. The burden of this chapter is to explore both historiographically and philosophically what it would mean to have such an intercalated history.

My original hope (which I sketch in part I of this chapter) was that such a laminated description of the larger community (composed of several subcultures) would do two things at once: it would underline the heterogeneity of practice within the wider physics community, while allowing continuities on one level to bolster discontinuities on another. Physicists' own experience of physics as maintaining a certain continuity even across conceptual breaks might, on this account, be ascribed to the local existence of continuity in the not purely conceptual arenas of practice.

But the more I pressed the laminated picture of intercalated practices (part II of this chapter), the more it seemed to delaminate. The criteria that divided the practitioners of theory, experiment, and instrumentation—different meetings, different preprint exchange, different journals—were the classic sociological dividers Kuhn (and many others since) productively invoked to identify distinct communities. Moreover, the experimenters and theorists often disagreed as to what entities there were, how they were classified, and how one demonstrated their existence—just the criteria Kuhn used to identify incommensurable systems of belief. With distinct communities and incommensurable beliefs, the layers seem to fall apart like decaying plywood; if they are significantly disconnected—if there are distinct communities using terms like *mass* and *energy* in significantly different ways—then the continuity of one level would hardly bolster discontinuity at another.

These considerations so exacerbated the problem that it seemed as if any two cultures (groups with very different systems of symbols, and procedures for their manipulation) would seem utterly condemned to passing one another without any possibility of significant interaction. But here we can learn from the anthropologists who regularly study unlike cultures that do interact, most notably by trade. Two groups can agree on rules of exchange even if they ascribe utterly different significance to the objects being exchanged; they may even disagree on the meaning of the exchange process itself. Nonetheless, the trading partners can hammer out a *local* coordination despite vast *global* differences. In an even more sophisticated way, cultures in interaction frequently establish contact languages, systems of discourse that can vary from the most function-specific jargons through semispecific pidgins, to full-fledged creoles rich enough to support activities as complex as poetry and metalinguistic reflection. The anthropological picture is relevant here. For in focusing on local coordination, not global meaning, I think one can understand the way engineers, experimenters, and theorists interact. At last I come to the connection between place, exchange, and knowledge production. But instead of looking at laboratories simply as the place where experimental information and strategies are generated, my concern is with the site—partly symbolic and partly spatial—where the local coordination between beliefs and action takes place. It is a domain I will call the trading zone.

LOGICAL POSITIVISM: REDUCTION TO EXPERIENCE

Early in this century, the logical positivists sought to ground knowledge on the solid bedrock of experience. Rudolf Carnap's masterwork, *Der logische Aufbau der Welt* is usually translated as *The Logical Structure of the World,* but might better be construed as *The Logical Construction of the World.* For it is a construction, a building-up from the elementary bits of individual experi-

ence to physics, then to individual psychology, and eventually to the totality of all social and natural sciences. To secure the foundations of this construction, both Carnap and Otto Neurath argued at length that some form of "protocol statements" and their manipulation through logic would form a language that would guarantee the validity of complex inferences constructed with them. "We assumed," Carnap recalled later,

> 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.*[1]

Carnap had a picture of knowledge being built up like a building, from a firm foundation of observation through the upper stories of physical theory, and up from there to the autopsychological, the heteropsychological and the cultural.

Figure 10-1 might be helpful, encapsulating what I will call the positivists' "central metaphor"

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

FIGURE 10-1
Positivist Periodization

Historians begin any investigation, implicitly or explicitly, with a periodization—a methodological commitment that prescribes the breaks and continuities appropriate to the domain under study. By fastening on reports of experience as the basis and the unifier of all science, the positivists committed themselves to an unbroken, cumulative language of observation. For Carnap, theories carried no such guarantee—as long as they could account in a shorthand way for the results of experience, they would stay. But theories come and go, protocol statements would remain.

Historians of science participated in the positivist movement of the philosophers and scientists. It is no accident that the justly famous Harvard Case Histories in Experimental Science[2] chronicled *experimental* triumphs: Robert Boyle's uncovering of the gas law, Pasteur's inquiry into fermentation, and Lavoisier's overthrow of the idea of phlogiston. As the laboratory workers marched onward, it came as no surprise to the positivists, or to their historian-counterparts, that theory fractured. If the equation $PV = nRT$ better accommodated observation, let it stand; if oxygen organized the facts in the laboratory better than phlogiston, then leave phlogiston by the way. The unification of science occurred at the level of observation/experiment (no sharp distinction being made between them); and the stability of the scientific enterprise rested upon the belief that this continuous, unified "physicalist" language provided a continuous, progressive narrative through the history of science.

ANTIPOSITIVISM: REDUCTION TO THEORY

The 1950s and 1960s saw a sharp reaction in both history and philosophy of science against the positivist picture. Quine denied the unrevisability of the Carnap/Neurath protocol statements, stressing that everything—even the general features of mathematics and logic—were up for revision; but if anything were to be privileged it would be high theory. Others went further. Most importantly the antipositivists insisted that no Carnapian protocol language could exist even in principle, a result sometimes referred to as *theory contamination* or *theory ladenness.*

Following the philosophers' lead—more than they might care to admit—historians of biology, chemistry, and physics adduced example after example in which theory changed first—and experiments then conformed to fit the mold.

Some of the leading antipositivists—including Thomas Kuhn and Russell Hanson—continued the positivists' fascination with early-twentieth-century Gestalt psychology and put it to new use. They now argued that theoretical changes shifted with the abruptness and totality of a Gestalt switch.[3] Just as the duck became a rabbit, experiments showing the absence of phlogiston now became experiments displaying the presence of oxygen. Theory changes *forced* changes all the way through experience, leaving no bit unaffected. Paul Feyerabend spelled out his antipathy for the positivists' central metaphor in no uncertain terms:

> [My] thesis can be read as a philosophical thesis about the influence of theories on our observations. It then asserts that observations . . . are not merely theory-*laden* . . . 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.[4]

For Feyerabend, the distinction between theoretical and observational terms was "purely psychological," (as opposed to the privileged role that observation held for the Vienna Circle). Through his own historical examples from the time of Galileo and classical antiquity, and allusions to the wider historical and sociological literature, he contended, "We may even say that what is regarded as 'nature' at a particular time is our own product in the sense that all the features ascribed to it have first been invented by us and then used for bringing order into our surroundings." In a doctrine he linked to Kant, Feyerabend insisted on the "all-pervasive character of basic theory."[5] And while Feyerabend allows that in certain particular cases, there may be facts held in common for different competing theories, in general that is not so: "Experimental evidence does not consist of facts pure and simple, but of facts analysed, modelled, and manufactured according to some theory."[6] Sometimes theories shape the scientific community's treatment of error, sometimes theory fashions the criteria of data selection, and even more pervasively theory is used to express the data. As an epigraph for his views Feyerabend chose a morsel of Goethe: "Das Hoechste zu begreifen waere, dass alles Faktische schon Theorie ist."[7]

Kuhn's view similarly was grounded in a thoroughgoing attack on the possibility of a sense-data language:

> The point-by-point comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change. . . . Ideally the primitive vocabulary of such a language would consist of pure sense-datum terms plus syntactic connectives. Philosophers have now abandoned hope of achieving any such ideal, but many of them continue to assume that theories can be compared by recourse to a basic vocabulary consisting entirely of words which are attached to nature in ways that are unproblematic and, to the extent necessary, independent of theory.[8]

This was the enemy: a neutral, unproblematic Archimedean point outside of a theoretical structure.

The positivist central metaphor was upended: now theory had primacy over experiment/observation, phenomena were no longer exempt from breaks. When theory changed, the rupture tore through the whole fabric of physics—including experiment/observation. Over such fissures in the tectonic plates of science nothing could cross. A new central metaphor replaced the old

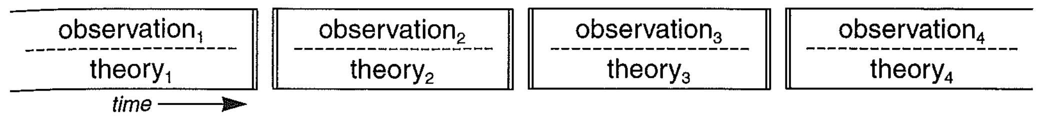

FIGURE 10-2
Antipositivist Periodization

The antipositivists' central metaphor has been extraordinarily fruitful. It has precipitated new philosophical debates on meaning and reference, and novel historical insight into the practice of science. No longer could science be described in the fantasy world in which observation was simply cumulative, and in which theory was isolated from philosophical commitments, reduced to a mere shorthand for logical strings of protocol statements.

Both the positivist and antipositivist periodization have a grandeur to them—they both sought and found a single narrative line that would sustain the whole of science, in observation for the positivists and in theory for the antipositivists. Both agreed that language was the linchpin of science—though the positivists looked for a language of experience, and the antipositivists located the key terms in theory. The positivists concluded that the common foundation of all specialties in basic observations guaranteed the unity of science. By denying the possibility of this foundation, antipositivists, preeminently Kuhn, split even the single discipline of physics into a myriad of noncommunicating parts separated by "microrevolutions." All was tied to the language and reference of theory, and theory was multiply torn.

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 10-2, 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 (Kuhn's way), it is the absence of a continuous substratum of common practice across the break that underlies the image of "different worlds," in which there is no overarching notion of progress. This is the thesis 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 their critique of the positivist vision of a simply progressive empirical domain, the antipositivists 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, ideological assumptions, or national styles of science. A myriad of interesting historical studies have revealed how theoretical notions significantly altered the construction, interpretation, and valuation 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.[9]

Kuhnian antipositivism and logical positivism 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 much 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 10-2 is an inverted version of Figure 10-1, 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"; in the case of the antipositivists it is from the theoretical "paradigm," "conceptual scheme," or "hard core" looking down and out.[10] This shared intuition that there are blocks of unified knowledge that, like tectonic plates, float past each other without linking has been expressed in many places and many ways.

As compelling as this antipositivist picture is, recent historical and philosophical work on experimentation suggests it needs revision. In the remainder of this paper, I would like to present an alternative sketch of the relation of experiment, theory and instruments reflecting this new work.

INTERCALATION AND ANTIREDUCTIONISM

Like Gaul, the practice of twentieth-century physics is divided into three parts. Indeed, precisely those criteria that Kuhn laid out some years ago as being the key to identifying separate scientific communities[11] apply to the groupings of experiment, theory, and instrumentation. There are separate journals, such as *Nuclear Instruments and Methods* and *Reviews of Scientific Instruments* for and by those physicists and physicist/engineers concerned with the design and implementation of particle detectors, accelerator technology, and computer data analysis systems. So too are there specifically theoretical publications, including *Theoretical and Mathematical Physics* or the *Journal of Theoretical Physics.* And there are specifically experimental serials, such as the eminent series *Methods of Experimental Physics.* There are separate conferences on theoretical, experimental, and instrumental subjects. Furthermore, the invisible colleges defined by pre- and reprint exchange frequently fall within (not between) these stratifications. Strikingly, in recent decades, graduate students at many institutions are accepted qua experimenter or qua theorist, and increasingly Ph.D.'s are awarded for contributions to instrumentation, considered as a distinct arena of research from experimentation.[12] There are prominent workshops, conferences, and summer schools that segregate these different subcultures. Think of the Johns Hopkins Workshop on Current Problems in Particle Theory, which in a given year might focus on lattice gauge theory, supersymmetry, grand unification, or other topics; the World Conference of the International Nuclear Target Development Society (its members make beryllium plates, not ICBMs); the Winter School of Theoretical Physics in Karpacz. Quite obviously there are national and international laboratories dedicated to experimental physics, some with significant and others with tiny theoretical groups. Less evident are laboratories in industry or in universities (and sometimes sections *within* larger laboratories), devoted solely to the development of instrumentation. Theoreticians have fewer places to themselves, but they are not insignificant: the Institute for Theoretical Physics in Santa Barbara, the Institute for Theoretical Physics in Leningrad, and the International Center for Theoretical Physics in Trieste, to name but a few. Nor are such assemblies restricted to high energy or nuclear physics. Condensed matter theorists often convene without their experimental colleagues in order to discuss the theory of metals or many-body problems. Astronomers sometimes find it appropriate to meet about instrumental

techniques in the radio or optical domains, and when the quantum gravity theorists convene there are few experimentalists or instrumentalists. More recently, computation has arisen as a distinct arena from all of the above, and regular convocations of computer specialists assemble for workshops such as "Computing for High Luminosity and High Intensity Facilities."[13]

While defections from one arena to another are possible, they are rare and discouraged. (Particle physicists like to point to the brilliant exception, Enrico Fermi, who, in his youth, contributed both to theory and experiment; he is a physicist's hero precisely because he traversed a barrier that only a handful have crossed in the last fifty years.) For all these reasons, it has become increasingly awkward to treat physics and physicists as constituting a single, monolithic structure. As historians, we have become used to treating cultures as composed of subcultures with different dynamics. It is now a commonplace that the political dislocations of the French Revolution did not alter economics, social structure, politics, and cultural life in the same measure. Indeed, as Lynn Hunt has shown, even the political impact of the revolution was felt differently by workers concentrated in towns and textile workers dispersed over the countryside.[14] It is high time that we recognize that the physics community is no less complex. Experimentalists—and one could make a similar statement about theorists and instrumentalists—do not march in lockstep with theory. For example, the practice of experimental physics in the quantum mechanical revolution of 1926–27 was not violently dislocated *despite* the startling realignment of theory: spectroscopy continued unabated, as did measurements of specific heat and black-body radiation. And practitioners of these experimental arts continued, undaunted, to conduct a continuing dialogue with theorists across the great theoretical divide. Each subculture has its own rhythms of change, each has its own standards of demonstration, and each is embedded differently in the wider culture of institutions, practices, inventions, and ideas.[15]

Thus for historical reasons, instead of searching for a positivist central metaphor grounded in observation, or an antipositivist central metaphor grounded in theory, I suggest that we admit a wider class of periodization schemes, in which the three levels are *intercalated* (see Figure 10-3).

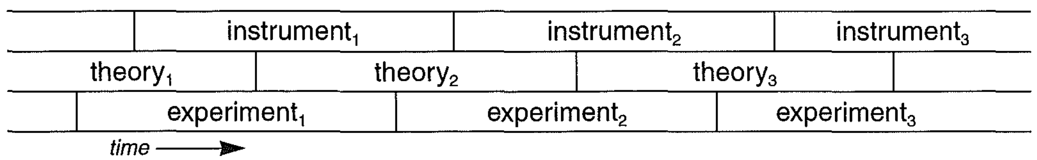

FIGURE 10-3
Intercalated Periodization

Different quasi-autonomous traditions carry their own periodizations. There are four facets of this open-ended model that merit attention. First, it is tripartite, granting (or at least offering the possibility of granting) a partial autonomy to instrumentation, experimentation, and theory. It is contingent, not preordained, that each subculture be represented separately as one can easily identify moments in the history of physics where the instrument makers and the experimentalists (to give one example) were not truly distinct. Nor is it *always* the case that break points occur separately. And there are many times when there were competing experimental subcultures each working in the same domain (bubble chamber users and spark chamber users, for example). Second, this *class* of central metaphors incorporates one of the key insights of the antipositivists: *there is no absolutely continuous basis in observation.* Both the level of experimentation and the level of instrumentation have their break points, just as theory does. Third, the local continuities are *intercalated*—we do not expect to see the abrupt changes of theory, experimentation, and instrumentation to occur simultaneously; in any case it is a matter of historical investigation to determine if they (contingently) do line up. Indeed, there are good reasons to expect that at the moment one stratum splits, workers in the others will do what they can to deploy accepted procedures that allow them to study the split before and after—when a radically new theory is introduced, we would expect experimenters to deploy their best-established instruments, not their unproven ones. Fourth, we expect a rough *parity* among the strata—no one level is privileged, no one subculture has the special position of narrating the right development of the field or serving as the reduction basis (the intercalated strands should really be drawn in three dimensions so no one is on top and each borders on the other two). Just as a bricklayer would not stack set the bricks for fear his whole building would collapse, each individual (or research group) does what it can to set breaks in one practice cluster against continuities in others. As a result of such local actions (not by global planning), the community as a whole does not stack periodize its subcultures.

Examples of the subsistence of experimental practices across theoretical breaks are now abundant in the new literature on experiment. For the first time there is a real interest in the dynamics of experiment outside the provision of data to induce, confirm, or refute specific theories. And among the philosophers, no one has done more than Hacking to separate the knowledge that emerges from the merely confirmatory role experiment usually plays in abstract accounts of scientific research.[16] Surely, then, Hacking would grant experimentation and the creation of phenomena just the sort of partial autonomy I have in mind with this class of periodization models. He would also agree that the experimental/phenomenal domain has its breaks.

Where I differ, perhaps, is in regard to parity among the subcultures. For while I am all for granting experimentation a life of its own, I do not think its life should come at the cost of poor theory's demise. More specifically, I read Hacking's work on the production of experimental entities this way: the possibility of intervening—making, moving, changing—is a way of imposing constraints on what can be the case. When it is possible to manipulate the objects, these restrictions are so severe that there is nothing for it, but to acknowledge the existence of electrons, positrons, or neutral currents.

Theory (or at least high theory), for Hacking, lacks the compulsive force of interventionist experimentation. For this reason he has defended an antirealism about theories and condemned those entities that theory alone demands—such as gravitational lenses or black holes.[17] But for many of the reasons Hacking originally defended the robustness of experiment, I want to defend the robustness of theory and of instrumentation: there are quasi-autonomous constraints on each level. When Duhem talks about the many theories that can each account for the data, he often has in mind positional astronomy as his example;[18] but most theoretical physics—such as particle physics or condensed matter theory—is as far from models of positional astronomy as the determination of Snell's law is from an experiment at SLAC. The theorist is *not* free to admit any particle or effect in order to come into harmony with the experimenter.

Experimenters come to believe in an effect for various reasons; one is the *stability* of the phenomenon—you change samples, you shift the temperature—and still the effect remains. Another road to the closure of an experiment involves the increasing *directness* of our probing of the phenomenon. By increasing the power of a microscope, the energy of a particle beam, the disposition of the apparatus, or the amplification of a signal, one probes further into the causal processes linking phenomena together.[19]

The theorist's experience is not so different. You try adding a minus sign to a term—but can't do it because the theory then violates parity; you try adding a term with more particles in it—forbidden because the theory now is nonrenormalizable and so demands an infinite number of parameters; you try leaving a particle out of the theory—now the law has uninterpretable probabilities; you subtract a different term and all your particles vanish into the vacuum; you split a term in two—now charge isn't conserved; and you still have to satisfy conservation laws of angular momentum, linear momentum, energy, lepton number, baryon number. Such constraints do not all issue axiomatically from a single, governing theory. Rather, they are the sum total of a myriad of interpenetrating commitments of theoretical, instrumental, and experimental practice: some, like the conservation of energy, centuries old. Others, like the demand for naturalness—that all free parameters arise in ratios on the order of unity—have their origin in recent memory. But taken together, the superposition of such constraints make some phenomena virtually impossible to posit, and others (such as the black hole) almost impossible to avoid.

Indeed, the astonishing thing about black holes is that they form (theoretically) in the face of enormous variations in the basic structure of our theory of matter. They don't depend on the details of this or that theory of the strong, the weak, or the electromagnetic force; and to remain consistent with other observations there is practically nothing one can do with the theory of gravity that would get in the way of the formation of black holes. The situation is similar with antiparticles. If one accepts special relativity and locality (the notion that cause and effect should be by near action, not action-at-a-distance) then changes in the charges of particles, the number of particles, the nature of forces, the existence or nonexistence of unification schemes all leave the basic symmetry intact: for every particle there is an antiparticle. This stubbornness against variation is the theoretical analogue of stability, and it is the experience of this stability that eventually brings theorists to accept such objects come what may (almost) from their experimentalist colleagues.

My sense of the heavily constrained nature of theoretical, experimental, and instrumental practice is what underlies my discontent with the heavy emphasis on the "plasticity" of physics. Constraints at the different levels allow theorists to come to beliefs about particles, interactions, electronic effects, stellar phenomena, black holes, and so on even when their experimental colleagues disagree or remain silent. The strength of the enterprise as a whole, on this view, emerges not because the domains of action are so plastic, but because they are so robust—and yet, despite that, fit together. The process by which this fitting occurs is emphatically not that either of a reduction to a protocol language or of a mutual translation of the two finite traditions. This is the intuition that motivates the historical material in *Image and Logic*, and the metahistorical reflections on it; the focus is on finite traditions with their own dynamics that are linked not by homogenization, but by *local coordination.*

PART II. THE TRADING ZONE

THE LOCALITY OF EXCHANGE

In an effort to capture both the differences between the subcultures and the felt possibility of communication, consider again the picture of intercalated periodizations discussed earlier but now focus on the boundaries between the strata. To characterize the interaction between the subcultures of instrumentation, experiment, and theory, I want to pursue the idea that these really are subcultures of the larger culture of physics. Like two cultures, distinct but living near enough to trade, they can share some activities while diverging on many others. In particular, the two cultures may bring to what I will call the *trading zone* objects that carry radically different significance for the donor and recipient. What is crucial is that in the highly local context of the trading zone, *despite* the differences in classification, significance, and standards of demonstration, the two groups can collaborate. They can come to a consensus about the procedure of exchange, about the mechanisms to determine when the goods are "equal" to one another. They can even both understand that the continuation of exchange is a prerequisite to the survival of the larger culture of which they are part.

I intend the term trading zone to be taken seriously, as a social and intellectual mortar binding together the disunified traditions of experimenting, theorizing, and instrument building. Anthropologists are familiar with different cultures encountering one another through trade, even when the significance of the objects traded—and of the trade itself—may be utterly different for the two sides. For example, in the southern Cauco Valley, in Colombia, the mostly black peasants, descended from slaves, maintain a rich culture permeated with magical cycles, sorcery, and curing. They are also in constant contact with the powerful forces of the landowning classes: some of the peasants run shops, others work on the vast sugarcane farms. Daily life includes many levels of exchange between the two sides, in the purchase of goods, the payment of rent, and the disbursement of wages. And within this trading zone both sides are perfectly capable of working within established behavioral patterns. But the *understanding* each side has of the exchange of money is utterly different. For the white landowners, money is "neutral" and has a variety of natural properties; for example, it can accumulate into capital—money begets money. For the black peasants, funds obtained in certain ways have animistic, moral properties, though perhaps none more striking than the practice of the secret baptism of money. In this ritual, a godparent-to-be hides a peso note in his or her hand, while the Catholic priest baptizes the infant. According to local belief, the peso bill—rather than the child—is consequently baptized, the bill acquires the child's name, and the godparent-to-be becomes the godparent of the bill. While putting the bill into circulation, the owner quietly calls it by its name three times and the faithful pesos will return to the owner, accompanied by their kin, usually from the pocket of the recipient. So, when we narrow our gaze to the peasant buying eggs in a landowner's shop we may see two people, perfectly harmoniously exchanging items. In fact, they depend on the exchange for survival. Out of our narrow view, however, are two vastly different symbolic and cultural systems, embedding two perfectly incompatible valuations and understandings of the objects exchanged.[20]

In our case, theorists trade experimental predictions for experimentalists' results. Two things are noteworthy about the exchange. First, the two subcultures may altogether disagree about the implications of the information exchanged or its epistemic status. For example, as we have seen, theorists may predict the existence of an entity with profound conviction because it is inextricably tied to central tenets of their practice—for example, group symmetry, naturalness, renormalizability, covariance, or unitarity. The experimentalist may receive the prediction as something quite different, perhaps as no more than another curious hypothesis to try out on the next run of the data-analysis program. But despite these sharp differences, it is striking that there is a context *within* which there is a great deal of consensus. In this trading zone, phenomena are discussed by both sides. It is here that we find the classic encounters of experiment with theory: particle decays, fission, fusion, pulsars, magnetostriction, the creep effect, second sound, lasing, magnetic deflection, and so on. It is the existence of such trading zones, and the highly constrained negotiations that proceed within them, that bind the otherwise disparate subcultures together.

TRADING BETWEEN THEORY AND EXPERIMENT

The example of relativistic mass is an appropriate place to start because over the last thirty years it has become the *locus classicus* for discussions of meaning incommensurability. For Kuhn, the advent of Einsteinian dynamics was a prototype of revolutionary change and, he argued, only at low velocities could the two concepts of mass be measured in the same way.[21] On this view, one would expect there to be no experimental mode of comparison of Einstein's concept of mass and the concepts of mass his theory displaced—those of H. A. Lorentz, Max Abraham, and Henri Poincaré, none of whom shared Einstein's view of an operationally-defined space and time. Feyerabend simply says there is no single experiment: where it appears there is one measurement of mass there actually are several—one experiment for the classical mechanic and one for the relativist. Any scientist who thinks differently, according to Feyerabend, is an instrumentalist not interested in interpretation at all, or is "mistaken," or is simply such a remarkable translator that they "change back and forth between these theories with such speed that they seem to remain within a single domain of discourse."[22] None of these alternatives seem to capture what goes on between theorists and experimentalists.

There is no doubt that the term *mass* was used differently by the different participants in what was referred to as the physics of the electron. Max Abraham and Lorentz both believed that electrons' mass originated purely as the result of their interaction with their own electromagnetic fields. Since they also took electrons to be the basic building block of matter, the *electromagnetic mass* of the electron was the basis of a world view in which mechanical mass was a derivative concept, and electricity the primary substance of nature. But while Abraham took the electron to be a rigid sphere with a uniform surface charge, Lorentz postulated, in addition, that electrons were flattened as they moved through the ether, and he used this hypothesis to explain the Michelson-Morley experiment. Soon afterwards, Poincaré introduced a modified version of Lorentz's theory, adding a nonelectromagnetic force to keep the deformable electron from blowing apart under the stresses of its deformation.[23]

These theories differ significantly from one another about the meaning of mass. And as radical as these theories might have seemed at the time, Einstein's was surely as shocking. Einstein abandoned the attempt to embed his notion of mass in the grand scheme of the electromagnetic world picture, and founded his theory on a positivist critique of the metaphysical categories of space and time, replacing them with clocks and rulers.

Kuhn's claim is that prerelativistic and relativistic uses of the term *mass* make comparison impossible: "Only at low relative velocities may the [Newtonian and Einsteinian masses] be measured in the same way and even then they must not be conceived to be the same."[24] In fact, there was a rich experimental subculture preoccupied precisely with comparing these different theories—and not at low velocities. With Max Kaufmann and Alfred Bucherer leading the way, these experimenters produced experiment after experiment using magnetic and electric fields to measure the mass of the high-velocity electron perpendicularly to its velocity. Moreover, their efforts were clearly understood by all four of the relevant theorists (Poincaré, Lorentz, Abraham, and Einstein) to arbitrate among theories. Lorentz recognized the relevance of one such set to his work and immediately conceded defeat: "Unfortunately my hypothesis [explaining mass by] the flattening of electrons is in contradiction with Kaufmann's results, and I must abandon it. I am, therefore, at the end of my Latin." These are not the words of someone for whom the experiment was irrelevant or incomprehensible. Only slightly less despairingly, Poincaré conceded that at "this moment the entire theory may well be threatened" by Kaufmann's data.[25] Einstein himself was more confident of his theory, and doubted the execution of Kaufmann's work; he did not challenge the relevance *in principle* of the results. Quite the contrary: Einstein went to considerable pains to produce predictions for the transverse mass of the electron so that Kaufmann and Bucherer could use their experimental methods to study the theory; he constructed a detailed analysis of Kaufmann's data; and he even designed his own modification of the electron-deflection experiments which he hoped someone would execute.[26] For the participants in the fast-electron experiments, there does not seem to be a problem in talking about the experiment or its proximate significance.

Feyerabend suggests that should scientists not acknowledge the existence of two (or presumably more) experiments lurking behind the apparent existence of just one, there were three possibilities. They could be instrumentalists. At least in the present case that would seem to be a hard position to defend. Einstein is famous for his insistence that his goal was to discover how much choice God had in his design of the universe. And while acknowledging that the axiomatic basis of theoretical physics could not be inferred from experience, he maintained throughout his life a deep-seated optimism about theoretical representations. "Can we hope to be guided safely by experience at all when there exist theories (such as classical mechanics) which to a large extent do justice to experience, without getting to the root of the matter? I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it." He goes on to say that experience may suggest theoretical ideas in the formal structure of a theory, and experience surely must ultimately be the standard against which physical theories are certified. "But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed."[27] These are not the words of an instrumentalist.

Could it be that Einstein, Lorentz, Poincaré, and Abraham were superfast translators and so could remain in "a single domain of discourse"? Presumably one would look for instances where Einstein switched into the language and calculational practices of the adherents of the electromagnetic world view. Such evidence might be reflections on the details of the charge distribution within or on the surface of the electron, or dynamical explorations of the means by which the electron might resist electrostatic self-destruction, or methodological statements advocating electromagnetism as the starting point of physical theory. As far as I know there are no such examples of this kind of work in the published or unpublished record. On the side of Lorentz (or Poincaré or Abraham) one would look for the opposite: indications, perhaps in private, that these theorists alternated their calculations with ones beginning with Einstein's heuristic starting point. Even if direct methodological statements were not forthcoming, we would expect at least some calculations that began with simple mechanical reflections and set aside the structure of matter. Again, even among the unpublished papers, I know of no such indications. The third and last alternative that Feyerabend put forward was that a scientist who denied the "two experiments in one" interpretation was just plain "mistaken." Lorentz might simply not recognize that Einstein had a different programmatic commitment. But Lorentz once remarked that Einstein "simply postulates what we have deduced." Conversely Einstein explicitly argued that he did not believe that mechanics could be reduced to electromagnetism. Each side recognized the

gap that existed between their orientations, and that this gap was central to the present and future development of physical theory.

The lesson I want to draw from this example is this: despite the "global" differences in the way "mass" classifies phenomena in the Lorentzian, Abrahamian, and Einsteinian theories, there remains a localized zone of activity in which a restricted set of actions and beliefs are deployed. In Kaufmann's and Bucherer's laboratories, in the arena of photographic plates, copper tubes, electric fields, and in the capacity of hot wires to emit electrons, experimentalists and theorists worked out an effective but limited coordination between beliefs and actions. What they worked out is, emphatically, *not* a protocol language—there is far too much theory woven into the joint experimental/theoretical action for that. Second, there is nothing *universal* in the establishment of jointly accepted procedures and arguments. And third, the laboratory coordination does not fully *define* the term mass, since beyond this localized context the theories diverge in a myriad of ways. Theorists and experimentalists are not miraculous instantaneous translators and they are not "mere" instrumentalists uninterested in interpretation. They are traders, coordinating parts of interpreted systems against parts of others. The holism that Quine advocated in the years after World War II is enormously compelling. It is hard to imagine ever trying to resurrect a demarcation criterion that would sever the observable from the theoretical. Yet perhaps we could say this: in the trading zone, where two Quinean webs meet, there are knots, local and dense sets of connections that can be identified with partially autonomous clusters of actions and beliefs.

THE PLACE OF THE TRADING ZONE

Trading between theorists and experimentalists in the heyday of electron theories was done by mail; given the separation of theoretical and experimental institutes on the Continent this is hardly surprising. In the United States and Britain, this geographical isolation was not as marked. When American universities began to acquire theorists in the 1930s they were housed under the same roof as their experimental colleagues. But it would be a distortion to talk about these communities as if they were coequal: only in the Oppenheimer group at Berkeley was there a strong prewar contingent of theorists. Elsewhere a Wendell Furry, a John Van Vleck, or a John Slater was a distinctly minority presence.

For many reasons World War II changed this relation. Quite obviously, Robert Oppenheimer's performance as director of Los Alamos put theory into prominence. But more importantly, theorists, experimentalists, and engineers were forced to work with one another in the large wartime projects. They emerged with nearly five years' experience of each other's way of approaching problems and an enduring faith that postwar science had to exploit the collaborative efforts that they credited for the atomic bomb and radar. In large part the collaboration consisted of establishing a place where ideas, data, and equipment could be passed back and forth between groups.

The Rad Lab, as it came to be known, was established in late 1940, around the British invention of a device that could produce microwaves of the right frequency for an effective radar. Lee DuBridge agreed to head the project on October 16; by late October a core group had established themselves in room 4-133 at MIT. At first, the divisional structure of the laboratory was designed to replicate the five-part electronic structure of radar, as if the laboratory were a small business: the modulator delivered pulses of power to the magnetron, the magnetron delivered microwave signals, the antenna emitted and collected these signals, the receiver sorted signals from noise, and the indicator displayed an image via a cathode ray tube. Each function had a room: the physical architecture closely matched the electronic architecture.

These three architectures—physical, electronic, and administrative—did not respect distinctions between engineers and physicists. William Tuller, for example, was an electrical engineer with a desk adjacent to that of Henry Neher, a physicist trained in experimental cosmic-ray investigations. William Hall, who had been an electrical engineer working for Metro-Goldwyn-Mayer doing sound recording, now shared the indicator corner of 4-133 with A. J. Allen, a physicist/electrical engineer, and Ernest C. Pollard, a physicist who had taken his B.A. and Ph.D. at Cambridge and in 1940 was an assistant professor at Yale. At first, theoretical physicists had no physical location in the laboratory—they were consultants, appearing from time to time very much the way they would visit a prewar cosmic ray, spectrographic, or magnetic laboratory. Face-to-face contact—literally so, as is evident from surviving seating plans—counted for much. As one experimental physicist put it at the time: "It is not enough that the discoveries and experiences of one group be occasionally presented in seminars or regular written reports. The former seldom go into sufficient detail to mean much, while the latter are either too detailed or simply unread." Instead, he suggested, the physicists needed to work physically in the same group. It was "[a] far swifter and more painless method of spreading new circuits and general Radar philosophy."[28]

At first glance, the war would seem to have made no contribution whatsoever to such an abstruse and abstract subject as quantum electrodynamics. The usual story about QED runs roughly as follows: during the 1920s and 1930s physicists interested in the subject, including Victor Weisskopf, H. A. Kramers, J. Robert Oppenheimer, Niels Bohr, Julian Schwinger, and others made halting progress in understanding how the quantum theory of the electron could be combined with special relativity. They made only intermittent progress, limited essentially to first-order calculations. For reasons of war work, all those living in the United States supposedly broke off their efforts during World War II to do their required (but "irrelevant" to pure physics) work on engineering, and then returned, triumphantly, to QED in the second half of the 1940s. The story is false on at least two levels. First, as Silvan Schweber has pointed out, the developments in QED were catalyzed in part by the results of wartime microwave technology that made possible the precision measurements of Willis Lamb, R. C. Retherford, Henry Foley, J. M. B. Kellogg, P. Kusch et al. in Rabi's laboratory and the work of Dicke at Princeton.[29] These were extraordinary experiments, but the impact of the war went even deeper. Radar work reconfigured the strategy by which Schwinger approached physical problems. Schwinger himself has alluded briefly to his judgment that his radar work had a strong impact on his postwar thinking; in what follows I will expand on his later remarks, making use of his actual work in radar to complete the picture.

Let us attend to practice—not results. During the war, Schwinger worked in the theoretical section of the MIT Rad Lab; his group had the task of developing a usable, general account of microwave networks. Ordinary network theory—the theory of radio waves in resistors and capacitors—utterly failed because microwaves have a wavelength of the same size as ordinary electrical components. In ordinary components such as resistors, copper wires, or cylindrical capacitors, the microwave energy would radiate away. This meant that the full set of calculational tools available for electronic circuits became useless. With the help of his coworkers, Schwinger began with Maxwell's equations and derived a set of rules by which engineers and physicists could make practical network calculations.[30]

As the war progressed and Schwinger assimilated more of the engineering culture of the Rad Lab, he began to abandon the physicists' abstract scattering theory of electromagnetism, and to search for the microwave analogue of the electrical engineers' more practical representations: simple "equivalent circuits" that imitated just the relevant aspects of the components. It was an old technique among electrical engineers, who were used to treating certain systems, such as loudspeakers, not by their real electrical, mechanical, or electromechanical properties, but as if the loudspeaker were a circuit of purely electrical components. In other words they (symbolically) put the complicated physics of the loudspeaker's electromechanically generated noise into a "black box," and replaced it in their calculations with "equivalent" electrical components. Similarly the conducting hollow pipes and cavities of microwave circuits could be replaced (symbolically) by ordinary electrical components, and so make the cavities amenable to algebraic manipulation—without entering each time into the details of complex boundary-value problems for Maxwell's equations. As the postwar Rad Lab "Waveguide Handbook" put it, the adoption of equivalent circuits "serves the purpose of casting the results of field calculations in a conventional engineering mold from which information can be derived [*sic*] by standard engineering calculations."[31] It is just this process of appropriation—this "casting" into an "engineering mold" that intrigues me. In this detachment of field calculations from their original context, the full meaning of the terms is cut short. Nor is the meaning suddenly and of a piece brought into engineering lore: microwave frequencies did not allow any simpleminded identification of electrical properties with the well-known categories of voltages, currents, and resistances. The product of this labor was a kind of simplified jargon binding elements of field theory talk with elements of engineering equivalent-circuit talk.

In short, the war forced theoretical physicists—such as Schwinger—to spend day after day calculating things about devices and, through these material objects, linking their own prior language of field theory to the language and algebra of electrical engineering. Modifying the theory, creating equivalent circuits for microwave radiation, solving new kinds of problems was not—and this is the crucial point—a form of translation. Even Schwinger's "glossary" identified newly calculated theoretical elements with recently fabricated fragments of microwave circuitry; neither was part of the prior practice of either the theorists or the radio engineers. Boundaries are substantial, translation is absent, and Gestalt shifts are nowhere in sight.

Schwinger himself has alluded to the link between the two seemingly unrelated domains of waveguides and renormalization. "[T]hose years of distraction" during the war were more than that: "[t]he waveguide investigations showed the utility of organizing a theory to isolate those inner structural aspects that are not probed under the given experimental circumstances. . . . And it is this viewpoint that [led me] to the quantum electrodynamics concept of self-consistent subtraction or renormalization."[32] With an understanding of Schwinger's work in waveguide physics, we are now in a position to unpack this connection between the calculations of radar and renormalization.

In the microwave case, it was impossible to calculate fully the field and currents in the region of the discontinuity; in the quantum electrodynamics case, it was hopeless to try to pursue the details of arbitrarily high-energy processes. To attack the microwave problem, Schwinger (wearing his engineering hat) isolated those features of the discontinuity region's physics that were important for "the given experimental circumstances"—for example, the voltages and currents emerging far from the discontinuity. In order to isolate the interesting features, he dumped the unneeded details of the electrodynamics of the discontinuity region into the parameters of an equivalent circuit. Faced with the fundamental problem of quantum electrodynamics, Schwinger concluded in 1947 that he should proceed by analogy: one had to isolate those features of the physics of quantum electrodynamics that were important for the given experimental circumstances—for example, magnetic moments or scattering amplitudes. To separate these quantities from the dross, he dumped the unneeded details of high-energy interactions into the renormalization parameters.

One lesson that theoretical physicists learned from their engineer colleagues during the war was, therefore, simple yet deep: concentrate on what you actually measure, and design your theory so that it does not say more than you must to account for these observable quantities. The adoption of this positivist attitude toward theorizing was such a sufficiently sharp break with earlier traditions of theory, that some of Schwinger's contemporaries never accepted it. Even Dirac, one of the greatest of twentieth-century theorists, resisted the idea of renormalization until his death in the 1980s. But the idea rapidly took hold, altering for at least several decades the theorists' attitude toward the limits of their description of nature.

CONCLUSION: THE COORDINATION OF ACTION AND BELIEF

In this trading back and forth between traditions at the Rad Lab, one can see an interesting analogue to Foucault's gloss of Jeremy Bentham's "Panopticon." The Panopticon was a central tower in an "ideal" prison that could control all its occupant could survey. The heterogeneous, self-consciously democratic structure of laboratories like room 4-133 at the Rad Lab offers both an analogue and *dis*analogue to Foucault's analysis of power and surveillance.[33] For at MIT each of the different subcultures was forced to set aside its longer-term and more general symbolic systems, in order to construct the hybrid of practices that all recognized as "Radar philosophy." Under the gun, the various subcultures coordinated their actions and representations in ways that had seemed impossible in peacetime; thrown together they began to get on with the job of building radar.

As the architecture emerged in parallel with the expanding Radiation Laboratory, one can see the visible manifestations of the new modes of exchange. Rooms are established with movable walls, the interchange with industry began to shape the physicists' self-conceptions. The laboratory not only resembled a factory, its integration was thoroughgoing: by the end of the war almost $3 billion had been spent on radar, the Rad Lab had 3,900 persons in its employment, and the laboratory with its "model shop" had delivered $25 million worth of equipment to the armed forces.[34] These developments had a profound effect on the physics community's plan for a huge centralized laboratory on the East Coast, one modeled explicitly on the Rad Lab: "The laboratory," one leading physicist wrote near the end of the war, "should be essentially of factory-type construction, capable of expansion and alteration. Partitions should be nonstructural." And to emphasize the ideological democracy of this new institution, he added: "[p]anelled offices for the director or any one else should be avoided."[35] But tearing down the paneling should not be confused with the homogenization of the community; there is no question of eliminating the categories of theorist, experimentalist, and engineer.

In planning the establishment of the National Accelerator Laboratory (later Fermilab) the founders recognized the enduring gap between the subcultures. Theorists, while necessary for the laboratory, would need contact with colleagues at neighboring universities and the creation of a weekly NAL "theory day"[36] and more ambitiously a "Theoretical Physics Center" at the laboratory. Even for a group of theorists working in the very midst of experimental activity, it was recognized from the start that the subject domains of theory and experiment were not perfectly coincident. While the laboratory attended to particle physics, its instrumentation and experiments, the theorists would work not only in strong interaction dynamics field theory, symmetries and groups, axiomatics, and phenomenological studies—they would also work in gravitation, general relativity, nuclear structure, astrophysics, quantum liquids, and statistical mechanics. "[I]t is understood, of course, that any individual theorist may move from one field to another within particle physics and from particle physics to one of the 'peripheral' fields with complete freedom of choice."[37] Precisely because of this recognized difference in the conceptual organization of experiment as distinct from theory, one would see breaks and continuities in theory that would be distinct from that of experiment.

"Sophistication in mathematical reasoning and technology that has accompanied progress in particle physics no longer allows an ordinary mortal to pursue the science both in an experimental laboratory and in the quiet of a study, as in the good old days of Faraday, Cavendish and Rayleigh, or even in the more recent time of Enrico Fermi."[38] I take it to be no accident that the separation of culture is signaled by a separation of place: the "experimental laboratory" is no longer coincident with the "quiet of a study." The contrast between the *vita activa* and the *vita contemplativa* has now been recreated inside the subdiscipline itself, and in the minds of the Fermilab directorate the division demanded a spatial solution: "All members of the group engage in exchange of ideas and knowledge with users [experimentalists from outside NAL who used the facilities] and experimentalists on the staff at Fermilab. These meetings of minds take place more formally in "[the] joint Experimental-Theoretical Seminar which takes place every Friday [and] is an innovative approach to communication among theorists and experimentalists at Fermilab." More frequent are informal meetings "in offices on the third floor of the Central Laboratory and at the Cafeteria, Lounge and airports";[39] these sites become trading zones. Throughout such exchanges there is no attempt to make experimentalists into theorists or vice versa. On the contrary, the concept of collaboration embraced by the physicists during the war involved a reinforcement of these subcultures and an emphasis on exchange.

These various examples of trading between subcultures suggest a model of scientific practice as much at odds with the picture of pure plasticity invoked by some interest theorists as with the rigidly segregated observation language of the early logical positivists. Or perhaps I should say it has links to both. *Within* traditions, I want to emphasize the relatively constrained nature of scientific practice—hardly anything goes. But when radical changes do occur—and no subculture is immune to such alterations—it does not necessarily follow that the other subcultures break as well. Moreover, the relative rigidity and foreignness of one subculture from another does not make crosstalk between the strata impossible; rather, it insures that as the trading domains become established, the structure of the enterprise as a whole has a strength that the antipositivists denied.

Moving away from the stack periodization schemes typical of the Gestalt psychological and sociological paradigm shifts comes at a price: we lose the vivid metaphorical imagery of totalistic transformations. In its place we need some guidance in thinking about the local configurations that are produced when two complex sociological and symbolic systems confront one another. Anthropologists are familiar with such exchanges, and one of the most interesting domains of such investigations has been in the field of anthropological linguistics surrounding the problems of *pidginization* and *creolization.* Both refer to languages at the boundary between groups. A pidgin usually designates a contact language constructed with the elements of at least two active languages; pidginization is the process of simplification and restriction by which a pidgin is produced. By convention, a pidgin is not used to describe a language that is used even by a small group of people as their native tongue. A creole, by contrast, is by definition a pidgin extended and complexified to the point where it can serve as a reasonably stable native language.[40]

Typically, pidgins arise as contact languages when two or more groups need to establish trade or exchange. One way that such languages arise is when a dominant but smaller group withholds its full language either to guard it to preserve their cultural identity, or because they believe that their social inferiors could not learn such a complex structure. To communicate, the dominant group then produces a "foreigner talk" which is then elaborated as it is used in day-to-day trading. This seems to have been the case, for example, in the production of "Police Motu." Originally the Motu (of what is now Papua New Guinea) created a simplified version of their language (a foreigner talk) to ply their extensive trading network, for example trading pots and sea products in exchange for game and bush products. William Foley, an anthropological linguist, speculates that at this stage the simplified Motu was not a distinct language from Motu itself. Beginning in the 1870s Europeans and later Chinese, Pacific Islanders, and Malay Indonesians arrived; they too acquired the foreigner talk version of Motu. When the British established colonial rule, they enforced their dominance with police, often not native speakers of Motu; the police slipped rather easily into the only lingua franca available, the simplified Motu, but now elaborated the language to make it serve its more complex function of colonial rule. As a more intricate and (forcibly) widespread language, the "Police Motu" gained in significance. Since, in addition, the "criminals" arrested by the police were often men of high social status in their villages (e.g., headhunters), when the incarcerated returned home they carried with them the "Police Motu," according it yet greater status.[41]

The simplification of a native language to a pidgin occurs on many axes.[42] Simplification in linguistic structure can occur *lexically,* through restriction in vocabulary or through monomorphemic words; it can occur *syntactically,* through the elimination of subordinate clauses, hardening of word order; *morphologically,* through the reduction in inflection or allomorphy; or *phonologically,* through the elimination of consonant clusters and polysyllabic words. At first such pidgins may be unstable, varying according to the prior linguistic practices of each learner. But gradually the pidgin in some cases will stabilize; sometimes this will occur when learners of different linguistic backgrounds need to communicate among themselves. As the pidgin expands to cover a wider variety of events and objects, it comes to play a larger linguistic role than merely facilitating trade. Eventually, as children begin to grow up "in" the expanded pidgin, the language is no longer acquired to solve specific functions but now must serve the full set of human demands. Linguists dub such a newly created "natural" language a creole and the process leading up to it, creolization.

I bring up the dynamics of contact languages and their stabilization, structure, and expansion because they offer at least a set of questions relevant to the confrontation of theorists with experimentalists. For example, the process by which experimentalists, theorists and instrumentalists simplify their practices for presentation to the other subcultures needs examination. Can we articulate the process along lines similar to the axes of lexical, morphological, grammatical, and syntactical axes presented by Ferguson? Consider the following example. In the early 1960s, Sidney Drell and James Bjorken set out to write a book on quantum field theory. They soon came to see that they in fact had written two distinct volumes: a first tome, directed at an audience outside the subculture of theorists, that began with the calculational rules of the

theory and a second containing theoretical justifications and proofs of the Feynman techniques. The first book covered Feynman diagrams and the classical applications they made simple—Bremsstrahlung (the emission of a photon by a charged particle), Compton scattering (the deflection of a photon by an electron), and pair annihilation (in which an electron and anti-electron fuse and emerge as a pair of photons). In order to study higher order corrections to processes including these, the authors introduced the renormalization procedure without a systematic exposition. It is a book of *techniques* that begin with rules (such as "For each internal meson line of spin zero with momentum q a factor: $i/(q^2 - \mu^2 + i\epsilon)$," where ($\mu$) is the meson mass and (ϵ) is a small positive number.[43]

Such a development [of the theory], more direct and less formal—if less compelling—than a deductive field theoretic approach, should bring quantitative calculation, analysis, and understanding of Feynman graphs into the bag of tricks of a much larger community of physicists than the specialized narrow one of second quantized theorists. In particular, we have in mind our experimental colleagues and students interested in particle physics.[44]

Left out of the experimentalists' volume is the framework in which the rules find their justificatory place. Also removed are the more general proofs, such as the demonstration that a calculation within quantum electrodynamics, to any order of accuracy, will remain finite.[45] As in Police Motu, the creation of a "foreigner" version of the symbolic system occurs on many fronts. There is an emphasis on plausible, heuristic argumentation rather than a more systematic demonstration, there is an increased focus on the calculation of measurable quantities over formal properties of the theory at some remove from experiment (such as symmetries and invariances). Perhaps more subtly, the theorists' version often links phenomena that are left merely associated for the experimentalists. For example, in the "experimentalists'" volume it is simply postulated that particles with half-integer spins (such as electrons) obey the Pauli exclusion principle, whereas in the "theorists'" volume, this contention is demonstrated for any local quantum field theory obeying Lorentz covariance and having a unique ground state.[46] These and other results are linked ultimately to a different structure in which the basic entities are embedded. In particular, in the experimentalists' volume the basic object—the field Ψ—stands for a wave function of a single particle. The experimentalists learn to manipulate this function in various ways following the rule of what is called "first" quantization: the position x and momentum p of classical physics are replaced by operators x and the spatial derivative d/dx. The differential equations that result are solved and the dynamics of the particle's wave function therefore determined. For the theorists, Ψ stands in not for the wave function of a single particle; rather Ψ itself is considered to be an operator at each point in space and time. Instead of standing in for a single particle, it represents a field of operators capable of creating and annihilating particles at each space-time point.

Despite this radical difference in the ontology—the set of what there is—a meeting ground exists around the description of the phenomenology of particle physics: How do photons recoil from electrons? How do electrons scatter from positrons? How do photons create pairs of electrons and positrons in the near presence of a proton? For these and similar questions, the experimentalists and theorists come to agreement about rules of representation, calculation, and local interpretation. In a strong sense, Bjorken and Drell Volume I is an example of an attempt to create a stable pidgin language, designed to mediate between experimentalist and theorist. Reduction of mathematical structure, suppression of exceptional cases, minimization of internal linkages between theoretical structures, removal from a more elaborate explanatory structure—these are all ways that the theorists prepare their subject for the exchange with their experimental colleagues. I take these moves toward regularization to be the formal-language analogues of phonetic, morphological, syntactical, and lexical reduction of natural languages.

By invoking pidgins and creoles, I do not mean to "reduce" the handling of machines to discourse. Quite the contrary. My intention is to *expand* the notion of contact languages to include structured symbolic systems that would *not* normally be included within the domain of "natural" language. On one side, this expansive attitude can be grounded by criticizing attempts to isolate natural languages; after all, even languages like English have been conditioned in part by very intentional intervention. Constructed language games such as backslang and rhyming slang have left grammatical traces within the "purely natural" languages. Even rules such as the use of "he" as a nongender specific pronoun have historical origins.[47] On the other side, "unnatural" languages such as signing, FORTRAN, and even electronic circuits can be used in such broadly expressive modes that any demarcation criterion seems bound to fail.

And indeed there is, not surprisingly, a corresponding "foreigner talk" that experimentalists develop on their side. Just as theorists reduce the complexity by suppressing the "endogenous" structure linking theory to theory, so experimentalists, when addressing theorists, skip the connecting details by which experimental procedures bind to one another.

These "separable" bits of procedure can come as isolable fragments of craft or engineering knowledge, as when the Alvarez group introduced indium as the binding material by which to bind bubble chamber glass to the steel chassis. Between such localized wisdom and material lay computer programs such as the PANG or KICK. Their exchange not only regularized practices in the "image" tradition, the track analysis programs carried over as well into the "logic" tradition, serving in the long run to facilitate the coalescence of the two previously competing cultures. Finally, in many cases, such as the postwar distribution of Ilford emulsions, radar oscillators, or multichannel analyzers, the medium of exchange can be physical. This suggests that the process of "black boxing" can be seen as the precise material analogue of the more linguistic forms of pidginization; just as terms like *electron* can acquire a decontextualized meaning, so *items* like a local oscillator can function as a binding element between subcultures when stripped from its original context and coordinated with a new one. After all, it was the military censors' abiding confidence that (in isolation) these instruments would *not* reveal their function in nuclear weapons or radar development that led them to declassify virtually all electronic instrumentation.

Beginning in Rad Lab seminars during the war itself, the techniques of circuit assembly, component coordination, testing, and general lore were codified into courses, and in the electronic boom after the war into a universe of practices sufficiently self-contained for students to grow up "in" microwave electronics, attached neither to field theory nor to traditional radio engineering. The pidgin has become a creole. Similarly, the development of *particle phenomenology* as a subfield of theoretical physics is an expansion outward of a trading zone: pidgin particle physics is pressed outward embracing an ever-widening domain of practices, some borrowed from the experimentalists and some from the quantum field theorists. As befits their boundary identity such physicists sometimes find themselves both in theoretical and in experimental groups.

What stabilizes a pidgin? What takes an aleatory alliance of linguistic practices assembled for a specific purpose and allows it to endure and expand? One interesting conjecture is that the alignment of three or more languages (*tertiary hybridization*) serves to prevent any single group from reabsorbing the pidgin back into one of the source languages.[48] Perhaps, and this is frankly speculative, one of the effective features of the huge war laboratories was precisely the imposed orchestration of the practices of theorists, experimentalists, instrument makers, along with electronic and mechanical engineers. It was the felt *difference* of this coordinated activity from the physicists' prior experience that led White, among others, to speak of a *Radar Philosophy*.

Tracing the handing of charts and copper tubes back and forth across the cultural divide, we could say, with the antipositivists, that the worlds of theory, experiment, and engineering cross without meeting. It would, however, be a description that does violence to the expressed experience of the participants. They are not without resources to communicate, but the communication takes place piecemeal, not in a global translation of cultures, and not through the establishment of a universal protocol language. Here is a summary slogan: laboratories are about the coordination between action and belief, not about translation.

This view of science as an intercalated set of subcultures bound together through a complex of pidgins and creole fits poorly in the debate over relativism and realism. In one sense, the view might be labeled "anti-anti-realist" since it directly opposes attempts to disintegrate science into blocks of knowledge isolated both from each other. But it is not for being anti-anti-realist, a defense of metaphysical realism as traditionally conceived. Nothing in the local coordination of the finite subcultures of physics guarantees anything like an asymptotic approach to truth.

Let me conclude with a metaphor. For years physicists and engineers harbored a profound mistrust of disorder. They searched for reliability in crystals rather than disordered materials, and strength in pure substances rather than laminated ones. Suddenly, in the last few years, in a quiet upheaval, they discovered that the classical vision had it backward: the electronic properties of crystals were fine until—*because* of their order—they failed catastrophically. It was amorphous semiconductors, with their *disordered* atoms, that gave the consistent responses needed for the modern era of electronics. Structural engineers were slow to learn the same lesson. The strongest materials were not pure—they were laminated; when they failed microscopically, they held in bulk. To a different end, in 1868 Charles Sanders Peirce invoked the image of a cable. I find his use evocative in just the right way: "Philosophy ought to imitate the successful sciences in its methods. . . . [T]o trust . . . rather to the multitude and variety of its arguments than to the conclusiveness of any one. Its reasoning should not form a chain which is no stronger than its weakest link, but a cable whose fibres may be ever so slender, provided they are sufficiently numerous and intimately connected."[49] With its intertwined strands, the cable gains its strength not by having a single, golden thread that winds its way through the whole. No one strand defines the whole. Rather, the great steel cables gripping the massive bridges of Peirce's time were made strong by the interleaving of many limited strands, no one of which held all the weight. Decades later, Wittgenstein used the same metaphor now cast in the image of thread, as he reflected on what it meant to have a concept. "We extend our concept of number as in spinning a thread we twist fibre on fibre. And the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres."[50] Concepts, practices, and arguments will not halt at the door of a conceptual scheme or its historical instantiation: they continue, piecewise.

These analogies cut deep. It is the *disorder* of the scientific community—the laminated, finite, partially independent strata supporting one another; it is the *dis*unification of science—the intercalation of *different* patterns of argument—that is responsible for its strength and coherence. It is an intercalation that extends even further down—even within the stratum of instruments we have seen mimetic and analytic traditions as separate and then combining, image and logic competing then merging. So too could we see divisions within theory—confrontational views about symmetries, field theory, *S*-matrix theory, for example—as one incompletely overlapped the other.

But ultimately the cable metaphor too takes itself apart, for Peirce insists that the strands not only be "sufficiently numerous" but also "intimately connected." In the cable, that connection is mere physical adjacency, a relation unhelpful in explicating the ties that bind concepts, arguments, instruments, and scientific subcultures. No mechanical analogy will ever be sufficient to do that because it is by coordinating different symbolic and material actions that people create the binding culture of science. All metaphors come to an end.

NOTES

Excerpted from chapter 9 in P. Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

1. Rudolf Carnap, "Intellectual Autobiography," in *The Philosophy of Carnap*, ed. P. A. Schilpp, Library of Living Philosophers, vol. 11 (La Salle, Ill.: Open Court, 1963), 57.

2. James B. Conant and Leonard K. Nash, eds., *Harvard Case Histories in Experimental Science*, 8 vols. (Cambridge, Mass.: Harvard University Press, 1950–54).

3. T. S. Kuhn, *The Structure of Scientific Revolutions*, 2d ed., International Encyclopedia of Unified Science (Chicago: University of Chicago Press, 1970), ch. 10; and Norwood Russell Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridge University Press, 1958), ch. 1–4.

4. Paul K. Feyerabend, *Realism, Rationalism and Scientific Method: Philosophical Papers*, vol. 1 (Cambridge: Cambridge University Press, 1981), x.

5. Ibid., 45, 118. The link to Kant occurs on p. 45, "As is well known, it was Kant who most forcefully stated and investigated this all-pervasive character of theoretical assumptions."

6. Ibid., 61.

7. Ibid., x.

8. T. S. Kuhn, "Reflections on my Critics," in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and S. Musgrave. Proceedings of the International Colloquium in the Philosophy of Science, vol. 4 (Cambridge: Cambridge University Press, 1970), 266.

9. J. L. Heilbron, *Elements of Early Modern Physics* (Berkeley and Los Angeles: University of California Press, 1982).

10. On the notion of the master narrative, see, e.g., Jean-Francois Lyotard, *The Postmodern Condition: A Report on Knowledge*. Theory and History of Literature, vol. 10, trans. Geoff Bennington and Brian Massumi (Minneapolis: University of Minnesota Press, 1984), 27–41.

11. T. S. Kuhn, "Second Thoughts on Paradigms," in *The Structure of Scientific Theories*, ed. F. Suppe (Urbana, Ill.: University of Illinois Press, 1974), 462.

12. The problem of the awarding of Ph.D.'s in physics for *purely* instrumental research has been the subject of much contention within the physics community. See, e.g., U.S. Department of Energy, Office of Energy Research, "Report of the HEPAP Subpanel on Future Modes of Experimental Research in High Energy Physics," DOE/ER-0380 (Washington, D.C.: U.S. Government Printing Office, 1988), 33, 53.

13. A tiny sample of such meetings are represented in the following volumes: G. Domokos and S. Kovesi-Domokos, eds., *Proceedings of the Johns Hopkins Workshop on Current Problems in Particle Theory, 7, Bonn, 1983* (Singapore: World Scientific, 1983); and Jozef Jaklovsky, ed., *Preparation of Nuclear Targets for Particle Accelerators* (New York: Plenum, 1981).

14. Lynn Avery Hunt, *Revolution and Urban Politics in Provincial France: Troyes and Reims, 1786–1790* (Stanford: Stanford University Press, 1978).

15. As we search to locate scientific activities in their context, it is extremely important to recognize that for many activities there is no single context. By viewing the collaboration among people in a laboratory not as a melding of identities but as a coordination among subcultures we can see the actors as separately embedded in their respective, wider worlds. E.g., in the huge bubble chamber laboratory at Berkeley one has to see the assembled workers as coming together from the AEC's secret world of nuclear weapons and from an arcane theoretical culture of university physics. The culture they partially construct at the junction is what I have in mind by the "trading zone."

16. Allan D. Franklin, *The Neglect of Experiment* (Cambridge: Cambridge University Press, 1986); Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987); David Gooding, Trevor Pinch, and Simon Schaffer, eds., *The Uses of Experiment: Studies in the Natural Sciences* (Cambridge: Cambridge University Press, 1989); Peter Achinstein and Owen Hannaway, eds., *Observation, Experiment and Hypothesis in Modern Physical Science* (Cambridge, Mass.: MIT Press, 1985); Jeffrey Sturchio, ed., Special Issue on Artifact and Experiment, *Isis* 79 (1988): 369–476; S. Shapin and S. Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985); and Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983).

17. Hacking, *Representing*, 274–75; and Hacking, "Extragalactic Reality: The Case of Gravitational Lensing," *Philosophy of Science* 56 (1989): 555–81.

18. P. Duhem, *The Aim and Structure of Physical Theory*, trans. Philip Wiener (Princeton: Princeton University Press, 1954), 168–73, and 190–95.

19. See Galison, *How Experiments End* (1987), ch. 5.6, "Directness." On the role of causal explanations and realism about phenomena see Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon, 1983).

20. The example of the secret baptism of money is from M. Taussig, *The Devil and Commodity Fetishism in South America* (Chapel Hill: University of North Carolina Press, 1980), ch. 7.

21. "This need to change the meaning of established and familiar concepts is central to the revolutionary impact of Einstein's theory. . . . We may even come to see it as a prototype for revolutionary reorientations in the sciences." Kuhn, *Structure*, 102.

22. Paul K. Feyerabend, *Problems of Empiricism: Philosophical Papers*, vol. 2 (Cambridge: Cambridge University Press, 1981), 159: "It is no good insisting that scientists act as if the situation were much less complicated. If they act that way, then they are either instrumentalists . . . or mistaken: many scientists are nowadays interested in *formulae* while we are discussing *interpretations*."

23. In all the massive literature on Einstein's special theory of relativity, the best historical book is Arthur I. Miller, *Albert Einstein's Special Theory of Relativity: Emergence (1905) and Early Interpretations (1905–1911)* (Reading, Mass.: Addison-Wesley, 1981). I have drawn liberally on this source for the preceding discussion of early experimental evidence on transverse electron mass experiments.

24. Kuhn, *Structure*, 102.

25. See Miller, *Special Theory*, 334–35.

26. Ibid., 341–45.

27. Albert Einstein, *Ideas and Opinions*, trans. S. Bargmann (New York: Crown, 1954), 274.

28. White, "A Proposal for Laboratory Organization," file "Reorganization," box 59, Radiation Laboratory papers (MIT), Office of Scientific and Research Development, National Archives, New England Region, Waltham, Massachusetts.

29. S. S. Schweber, "Some Chapters for a History of Quantum Field Theory: 1938–1952," in *Relativity, Groups, and Topology II*, Les Houches, Session 40, ed. B. DeWitt and R. Stora (New York: North-Holland, 1984), 163.

30. Schwinger and others at the MIT Radiation Laboratory struggled with the problem of determining the input and output characteristics of radiation to and from waveguide junctions. Working in parallel (or perhaps antiparallel) the theorist Sin-itiro Tomonaga was at work on radar for the Japanese. During the first part of the war, each of the two physicists approached the difficulty using classical "physicists' techniques"—exploiting relations among the amplitudes of waves arriving at and leaving the junction. When put into a scattering "*S*"-matrix these quantities obeyed certain symmetries and, when the process was lossless, unitarity. Because of these symmetries, the *S*-matrix formalism facilitated the solution of problems (such as the split of one waveguide into two) by side-stepping the vast amount of information contained in the full electromagnetic field description and representing only the relation of the *measurable* quantities coming in and out of the junction. Perhaps surprisingly, then, two of the founders of postwar particle physics built their theoretical scattering theory on their wartime radar work. Julian Schwinger, *Tomonaga Sin-itiro: A Memorial. Two Shakers of Physics* ([Japan]: Nishina Memorial Foundation, 1980), 14–16.

31. Nathan Marcuvitz, *Waveguide Handbook*, IEEE Electromagnetic Waves Series (London: Peregrinus, 1986).

32. Schwinger, *Shakers*, 16.

33. Michel Foucault, *Discipline and Punish: The Birth of the Prison*, trans. A. Sheridan (New York: Vintage, 1979), 195–228.

34. Henry E. Guerlac, *Radar in World War II*, Tomash Series in the History of Modern Physics 1800–1950, vol. 8 (Los Angeles: Tomash, 1987), 4. On the military and factory models for postwar laboratories, see Peter Galison, "Physics Between War and Peace," in *Science, Technology and the Military*, ed. E. Mendelsohn, M. R. Smith, and P. Weingart, Sociology of the Sciences, vol. 1 (Dordrecht: Kluwer, 1988), 47–86.

35. Smyth, "Proposal," 25 July 1944, revised 7 February 1945, 8, Princeton University Archives, published with permission of Princeton University Libraries, Princeton, New Jersey.

36. Goldwasser, circular letter, 14 January 1969, uncatalogued files, Fermilab Archives, Fermilab, Batavia, Illinois.

37. "Proposal for Theoretical Physics Center of the National Accelerator Laboratory," August 19, 1969, 2, uncatalogued files, Fermilab Archives.

38. B. W. Lee, "Theoretical Physics at Fermilab," *NALREP: Monthly Report of the Fermi National Accelerator Laboratory* (March 1978): 1.

39. Ibid., 7–8.

40. Much of the following is based closely on the excellent review of the literature on pidginization and creolization given by William A. Foley, "Language Birth: The Processes of Pidginization and Creolization," in *Language: The Sociocultural Context*, ed. F. J. Newmeyer. Linguistics: The Cambridge Survey, vol. 4 (Cambridge: Cambridge University Press, 1988), 162–83.

41. Ibid., 173–74.

42. See Charles Ferguson, "Simplified Registers and Linguistic Theory," in *Exceptional Language and Linguistics*, ed. L. K. Obler and L. Menn (New York: Academic Press, 1982), 60.
43. J. Bjorken and S. Drell, *Relativistic Quantum Mechanics* (New York: McGraw-Hill, 1964), 286.
44. Ibid., viii.
45. J. Bjorken and S. Drell, *Relativistic Quantum Fields* (New York: McGraw-Hill, 1965), 330–44.
46. Ibid., 170–72.
47. See, e.g., Peter Mühlhäusler, *Pidgin and Creole Linguistics*, Language in Society, vol. 11 (Oxford: Blackwell, 1986), 60.
48. Ibid.
49. Charles Sanders Peirce, "Some Consequences of Four Incapacities," in *Writings of Charles Sanders Peirce, A Chronological Edition, Vol. 2, 1867–1871* (Bloomington: Indiana University Press, 1984), 213.
50. Ludwig Wittgenstein, *Philosophical Investigations*, 2d ed., trans. G. E. M. Anscombe (Oxford: Blackwell, 1958), par. 67.

11

Making Up People

IAN HACKING

Were there any perverts before the latter part of the nineteenth century? According to Arnold Davidson, "The answer is NO. . . . Perversion was not a disease that lurked about in nature, waiting for a psychiatrist with especially acute powers of observation to discover it hiding everywhere. It was a disease created by a new (functional) understanding of disease."[1] Davidson is not denying that there have been odd people at all times. He is asserting that perversion, as a disease, and the pervert, as a diseased person, were created in the late nineteenth century. Davidson's claim, one of many now in circulation, illustrates what I call making up people.

I have three aims: I want a better understanding of claims as curious as Davidson's; I would like to know if there could be a general theory of making up people, or whether each example is so peculiar that it demands its own nongeneralizable story; and I want to know how this idea "making up people" affects our very idea of what it is to be an individual. I should warn that my concern is philosophical and abstract; I look more at what people might be than at what we are. I imagine a philosophical notion I call dynamic nominalism, and reflect too little on the ordinary dynamics of human interaction.

First we need more examples. I study the dullest of subjects, the official statistics of the nineteenth century. They range, of course, over agriculture, education, trade, births, and military might, but there is one especially striking feature of the avalanche of numbers that begins around 1820. It is obsessed with *analyse morale*, namely, the statistics of deviance. It is the numerical analysis of suicide, prostitution, drunkenness, vagrancy, madness, crime, *les misérables*. Counting generated its own subdivisions and rearrangements. We find classifications of over 4,000 different crisscrossing motives for murder and requests that the police classify each individual suicide in twenty-one different ways. I do not believe that motives of these sorts or suicides of these kinds existed until the practice of counting them came into being.[2]

New slots were created in which to fit and enumerate people. Even national and provincial censuses amazingly show that the categories into which people fall change every ten years. Social change creates new categories of people, but the counting is no mere report of developments. It elaborately, often philanthropically, creates new ways for people to be.

People spontaneously come to fit their categories. When factory inspectors in England and Wales went to the mills, they found various kinds of people there, loosely sorted according to tasks and wages. But when they had finished their reports, mill hands had precise ways in which