Trading Zones and Interactional Expertise

3 Trading with the Enemy

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One way to think through what a concept like the trading zone does is to press objections against it, for only then do sharpened boundaries pull foreground from background. Analyzing such confrontations tracks my ideas about these scientific subcultures and exchange languages. But because it is sometimes useful to start with the history of a concept, I want to begin there—and then follow the history into more analytical territory.

What grabbed me most in Marx's work—and the history of work more generally—was certainly not the labor theory of value and the interminable battles over its limits. Instead, what impressed me were the discussions of machines: the descriptions of looms and labor, the vivid depiction of how bosses drove down the number of cubic feet of air that weaving girls had in their quarters. Among the historians who were current when I was starting out, it was the work of the Annales School I liked best: the history of how medieval land was ploughed (Marc Bloch); how rice fields were easier to police than the hill towns of Tuscany (Fernand Braudel). I liked seeing how work worked—how cars were pounded together, mine faces stripped of coal, and secretarial work narrowed. Studies like those by Harry Braverman (*Labor and Monopoly Capital*, 1974) intrigued me; so too did the great historical studies by E. P. Thompson in *The Making of the English Working Class* (1966).

It was the actual scientific work that I wanted to get at in writing about the history of science—and such a history of science seemed impossible to achieve if one ignored the laboratory. I was utterly transfixed by these experimental spaces; I had spent a year in a plasma physics lab studying ion waves, and several months in an applied physics lab trying to figure out how to best spray water to keep a miniature, idealized house from burning to the ground. I had studied with a truly great experimental physicist, Robert Pound, and watched, riveted, as he, a true master, plucked electrical signals out of the noise.

In *How Experiments End* (1987), I wanted, above all, to capture the weight that experimental practice had as a distinct form of reasoning—a form of reasoning not reducible to inspiring theory or checking after the fact. On the contrary, the point was to show how experiments really did move to a rhythm distinct from that of theory, that experimentalists' decision that they'd seen something real (for example) was *not* grounded on the same standards and forms of argumentation that satisfied theorists that they had found a bona fide effect. It was this quasi-autonomy that led me away from the then overwhelmingly popular Kuhnian picture of mutually incomprehensible paradigms. I just didn't see the experimentalists finding incommensurability in their practices before and after theoretical breaks such as the 1905 advent of special relativity.

During that period—late 1970s and early 1980s—the laboratory and the experiment were discussed more often in science studies. But as much as I objected to the marginalization of experiment in favor of theory, I also bridled at the reanimated reductive form of positivism that dismissed theory and theorists, placing reality in experiment above all else. Theory, like experiment, had its own culture of demonstration, its own short-, middle-, and long-term constraint structure that characterized what it meant to be a theorist. By the time I published *How Experiments End*, I had a picture of three intercalated, quasi-autonomous subcultures of theory, experiment, and instrument making.

So far, so good. But then I got good and stuck. Here was the problem. On the one hand we had the Kuhnian picture of paradigmatic splits—revolutions—that thoroughly and unbridgeably cleaved science onto one side or the other of a great divide. This view was taken up with increasing frequency even among my allies in the new and burgeoning field of laboratory studies. On the other hand, I saw the weight given to experimental culture as pulling in another direction—toward the intercalated picture of three subcultures that I hoped would better capture the phenomenology of scientists' experience—scientists who seemed very rarely to have seen themselves as forever banished from the far shores created by a putative epistemic split.

In 1988, I reported on where I was with this train of thought about intercalation, rupture, and continuity in my essay "History, Philosophy, and the Central Metaphor." In writing this piece for *Science in Context*, I found that the intercalated periodization not only failed to resolve the incommensurability problem, but made it much worse. For Kuhn had used three criteria to pick out a paradigm-bearing community of scientists. First, the community shared a basic agreement about what there was in the world and how these things interacted (ontology); second, they held in common a set of acceptable means for learning about these entities (epistemology); and third, they shared an understanding of basic physical laws (nomology). Alongside this framework of knowledge was its sociological support: scientists within a community had shared routes for circulating knowledge, such as preprints, conferences, and journals. But if the experimentalists were to be autonomous enough to cut across the theorists' paradigms—as I saw they often were—it was precisely because they did *not* share the full-bore commitments of the theorists to the nature of objects, laws, and ways of acquiring knowledge; and they certainly had a large body of their own conferences, preprint exchange networks, and journals.

It followed that what I had on my hands was a picture of both a diachronic incommensurability of the Kuhnian sort *and* a synchronic incommensurability between experimentalists and theorists (for example) at any particular time. I had wanted a picture of layered strength, like a New England stone wall; instead, I got one made of fragile, delaminating plywood. I was stuck, banging my head against this rock wall. By 1989, I had written various pieces of a book (*Image and Logic*, 1997) about the third subculture (instrument making), and it was ever clearer that I had painted myself into a corner: the more I argued that the three subcultures had a kind of autonomy from one another, the more blatant it became that I had no idea how to address the way that cross-talk might actually link them.

If the incommensurability problem was harder to solve, it was at least increasingly clear to state. The most important lesson of the previous decade, it seemed to me, had been the *locality* of practice. I thought of theory as having its own form of local practices—for example, I had long been interested in how Hermann Minkowski's Goettingen-mathematical way of formulating relativity theory differed from Einstein's own. Conversely, I was fascinated with the characteristic ways that experimentalists handled their forms of argumentation and demonstration in the laboratory. But our ways of talking about language in the history of science were anything but local. Instead, they were, through and through, *global*. W. V. O. Quine and Thomas Kuhn, Rudolf Carnap and Otto Neurath—even (or especially) in the new (neo-Kuhnian) sociology of science of the 1980s, we were still talking about global translation. My specific problem was that I had boundaries this way and that (diachronic and synchronic, among three subcultures) and no good way of showing anything *local* about what happened at the various intersections.

Put differently: by the late 1980s, we had an increasingly adequate account of local practice and were joining it, unworkably, to a global account of language. That was the difficulty. And the way to address it was to figure out how to talk about language as practice—*local* language practices. So I began poking around in the literature of anthropological linguists, who I hoped would have something to say about languages in border zones. What I found was perfect for what I wanted: the anthropological linguists had indeed studied such situations. Soon I came across a book, Tom Dutton's *Police Motu: Iena Sivarai* (Police Motu: Its story, 1985), which I cottoned to immediately. Dutton not only showed the ways that a hybrid pidgin formed, but tracked it across time, showing how a "natural" exchange language could become more and more expansive until it could function in full, used in radio programs and everyday life.

I began thinking about more partial kinds of scientific languages, of the *work* needed to produce the first collective and coordinative moves that would join biology to chemistry, and slowly articulate a language that borrowed from both but was subservient to neither. Why not think of these hybrid arenas of practice as a form of language? After all, on what grounds would one dismiss as beyond the pale of "real" language the highly restricted call-out system of the type Wittgenstein identifies as a language game in the *Philosophical Investigations*? Are Fortran or C++ "just metaphors" for a language? And if algebra and geometry count as languages, why not count algebraic geometry? When a physicist says, "We can say that in the language of differential geometry or in the language of quantum field theory," is this utterance purely metaphorical? On what grounds would one base such a high-handed dismissal?

For analytic, functional, and political reasons, the anthropological linguists were committed to the idea that pidgins and creoles were languages, not inferior or debased forms of "real" or "natural" languages. Indeed, anthropological linguists were irritated for a reason. In the days before Chomsky, linguists used to treat pidgins and creoles as "simple," not "true" languages; in fact, it used to be a commonplace to rank languages from complex to simple. (I remember reading many years ago the early modern mathematician and engineer Simon Stevinus's confident assertion that Dutch was the best possible language for science. Finding French authors who judge French to be the clearest of possible voices for reason is not very hard—for that matter, we need no commission of inquiry to locate the extensive German literature on the superiority of German for poetry, music, or philosophy.) Happily, linguistics has given up the ranking of languages along with the systematic demotion of interlanguages like the various forms of pidgin English.

Why should science studies plant its flag in a nineteenth-century conception of language? No contemporary linguist takes Dutch, French, German, or English as "pure" these days—everyone knows that linguistic hybridity goes all the way back. Contemporary English, for example, contains a long series of borrowings, intrusions, and mixtures, inter alia from Anglo-Frisian dialects, Scandinavian German, Old Norman, Latin, and Greek—and the adoptions and adaptations continue unabated. Are there periods of relative stability? Of course. So it is in science, too. Classical physics is not pure in some originary sense. We know perfectly well that it was formed out of a complicated admixture of mathematical statics, craft practices, natural philosophy, printing technologies, and much else besides. So why look down on chemical physics or physical chemistry as the impure, lesser form of science or scientific language? To pursue this opening allows us to ask many more questions. For example, what pieces of physics and what pieces of chemistry are brought together? Where do the calculational procedures originate? Which laboratory procedures are brought into the combination and which are left behind?

Understanding the locality of interlanguages—and how they might be applied to science—cracked the final theoretical obstacle to linking practices of theorizing, instrument making, and experimenting. I could now see the way "out-talk" functioned when experimentalists addressed theorists. I could ask questions sharply: What exactly is left behind and what put forward when an instrument maker addresses an experimentalist? In other words, what forms of regularization occur in the *scientific* registers of jargons, pidgins, and creoles? What did theoretical physicists hold back and what did they put on the table when they talked to radio engineers when the two groups tried to build radar during World War II? It took another eight years for me to complete *Image and Logic* (long story, long book), but by the fall of 1989 the framework for treating local trade and trade languages—rather than global translation—was in place.

Over the years, I grappled with many objections as I came to terms with the trading zone and its associated exchange languages.

Objection 1 In order to talk about trading, exchange, and hybridity, there has to be some stable notion of the entities that are engaged in that trade. But could such cultures really be pure and completely stable? What differentiates a pure culture from a hybrid culture; what, in fact, is a scientific subculture?

We will never get anywhere with a too-rigid notion of stability or purity. Your body is constantly replacing cells, but enough of it remains for it to be possible to identify and reidentify yourself as the same person. Quasi-stability, not rigidity, is the relevant criterion: by "quasi-," I mean that the changes in a given period are small relative to that which stays roughly the same. We reidentify a university as the "same" even if it were to pass, as so many have, from being a seminary through periods of being a sheltered, private teaching institution to being an outwardly looking research and teaching facility. Scientific practices can and do form subcultures—and the question is, similarly: Do the commonalities across periods of change hold stable enough to merit reidentification? That said, all of science remains in flux, and every tired attempt to grab hold of the necessary and sufficient criteria for scientificity has failed in one way or another. Quantification? Much of morphological biology isn't quantitative. Prediction? Most of evolutionary theory would utterly fail that test. Experimental? String theory, despite its remarkable contributions to mathematics and to a theoretical elucidation of black holes and field theory certainly is not that. Explanatory? If you demanded persuasive explications of the action of many proven life-saving drugs before you took them, you would die in the waiting room. No, we are getting nowhere if we start with the idea that there is a pure, stable, transcendental "nature" of physics, chemistry, biomedicine, or mathematics. What we have are quasi-stable scientific subcultures (roughly shared ways of handling practices with their attendant values, symbols, and meanings). Above all, we need to know how these scientific subcultures connect to each other, to the surrounding world, and to change.

With *How Experiments End* and *Image and Logic* done, I had a pretty good idea how, following experiments and instruments, one could track incipient trading zones between the laboratory and wider technical cultures. For example, Luis Alvarez knew how to flip a hydrogen liquefier from producing hydrogen for Atomic Energy Commission H-bombs at the Eniwetok atoll to making hydrogen for AEC bubble chambers at Berkeley. In the material and work exchanges between the civilian and military sectors, one can see a great deal: movement of expertise, personnel, materiel, and funding. We can see, quite dramatically, how the culture of nuclear physics research took on new forms—a new scale of work more like a factory than a cottage industry, with semiworks, hierarchical administration, and a new division of labor between builders and users. Physicists developed new kinds of demonstrations using computer-aided analyses and simulations alongside a novel scientific-engineering identity for the practitioners.

But how did exchange work in the dominion of theory? To answer this question, I started with the very paradigm of theoretical science, Albert Einstein and his work on relativity theory. It never had made much sense to me that the young physicist was working in a patent office on new electromagnetic devices fifty or sixty hours a week *and* on the foundations of electromagnetism *and* that the two had nothing whatsoever to do with one another. One day, I was idly staring at a line of electric clocks in a European railway station. They seemed to be quite well aligned—but when I noticed that even their second hands were marching in lockstep, it was clear that these were not just good clocks; they were electrically synchronized. I wondered: could Einstein have been thinking of real, not just imaginary, synchronized railway clocks? The literature on patents at this time bore out my speculation: in 1904–1905, there was a spike in the already intense interest among Swiss clockmakers in taking out patents on synchronization mechanisms for railroad clocks. More than that, synchronized clocks had become a hallmark of urban modernity, useful for long-distance stock exchange trades, but also a shining testament to the pace and vigor of city life. Patents on electric and electrosynchronized clocks would have landed on Einstein's and his colleagues' desks—and, in mid-May 1905, he used train clock synchronization as the "metaphor" by which he defined simultaneity in his relativity paper. But to understand the nature of the binding ties between the literal-practical and the metaphorical-theoretical, I wanted to see this played out elsewhere. I wondered who else would have been worried about both the technology of time coordination and the physics of simultaneity?

Henri Poincaré was the obvious candidate, though I started in the wrong place, looking for ways in which his teaching at the École Professionnelle Supérieure des Postes et Télégraphes might have bound time signal exchange and his theoretical work on simultaneity. That was wrong. Instead, a much more fruitful line of inquiry opened up out of the seemingly "pure" metaphor he used to explain the procedural-material way in which simultaneity needed to be specified. Einstein had launched his critique of absolute simultaneity by reasoning about train clocks. Poincaré began his *The Measure of Time* (1898) with an allusion to two telegraphers sending signals back and forth to establish longitude differences. As it turned out, Poincaré was very much involved with the Paris Bureau of Longitude—corresponding with his British counterparts, struggling to sort out technical aspects of the telegraphic exchanges across the Channel—exchanges designed precisely to sort out simultaneity to a few thousandths of a second. More: Poincaré had been the spokesman for a dangerous, multiyear longitude expedition to the Andes; he had even served an important stint as president of the Paris Bureau of Longitude.

Here, in the procedures of simultaneity, was a trading zone with *theoretical* physics. The statement S: "Two clocks A and B are synchronized, and simultaneity defined, when a back-and-forth signal taking time 2T is exchanged from A to B and back; assuming the one-way signal takes just time T, when A sends a signal at her noon to B, B sets his clock to noon plus T when he gets it." Let's be specific. Say I send you a signal at noon, and suppose that it takes two millionths of a second to go back and forth. Then, when you get my noon signal, you set your clock to noon plus one millionth of a second. What is statement S? Is S "truly" a physics statement and only derivatively one from the engineering effort to map the world? (It is not hard to find versions of S featured prominently in many of Poincaré's physics publications.) Or is S really a statement from engineering and only derivatively one from physics? (It was actually a procedure used every day by the French military geographers.) Or is S in the first instance philosophical? (It certainly showed up in Poincaré's epistemological writings on the nature of time.)

My view is that attempts to make one subculture the basis, the unmoved prime mover, are doomed to failure. Instead, we would do much better to think about the arena of simultaneity and synchronization as one area illuminated, as if by intersecting searchlights, by all three sets of practices—physics, engineering, philosophy. Though deployed differently in each of these discourses, statement S—the procedural definition of simultaneity—was part of all three. This multiplicity of partially overlapping time talk is why such ideas shone so luminously. Once he had the clock coordination procedure, Poincaré jumped back and forth, month to month, addressing longitude finders, physicists, and philosophers—sometimes changing register within a few weeks.

S should not be thought of as bolted to a granite *base* above which floats a derivative and ephemeral superstructure. Instead, S stands in the intersection of three roads of practice. I choose the idea of an intersection quite deliberately—precisely to avoid assuming that there is a fixed starting point, a one true source that other domains of the sociotechnical world uniformly "appropriate," "reflect," or "translate." An intersection is awkwardly said to be "in" this or that of its defining paths: Times Square is not on Broadway any more than it is on Seventh Avenue or on Forty-second Street. No, the whole point of an intersection is that it lies in *all* the roads that cross. Trading zones are such intersections of discursive and material practice, partially—but not completely—shared.

The key concept here is *incomplete* coordination. I hand you a salt shaker and in exchange you pass to me a statuette. We may agree to the trade—we do not in any sense have to agree to the ultimate use, signification, or even further exchange value of the objects given. The *only* thing we have to come to accord about is their exchangeability. While for me the statuette may be a religious object, for you it could be a purely aesthetic or functional one—on this we do *not* have to agree. We strip away meaning and memory when we pass the object to a trading zone. As linguists have long known, this cutting down, this regularization of our symbolic systems is something at which humans seem to be quite good. And exactly that creation of regularized interactions and partially interpreted objects marks the trading zones of science.

Back in the 1960s, two outstanding particle theorists, James Bjorken and Sidney Drell, decided to write a textbook on quantum field theory, and proceeded in two parts. In the first, they addressed "our experimental colleagues and students interested in particle physics" (Bjorken and Drell 1964). The goal was to transform "quantitative calculation, analysis and understanding of Feynman graphs into a bag of tricks" useful to a larger group of theorists than those fully in command of quantum field theory. The second volume (Bjorken and Drell 1965) would then fill in the gaps, prove theorems, and explore the regions where ordinary Feynman diagrams could not go. This move toward regularized, rule-governed procedures and away from the surrounding or underlying theoretical structure marks a dramatic shift in register. Out-talk is marked by the connections of rules of calculation to patterns of observation; in-talk moves among the concepts, exploring relationships and demonstrating systematic properties within the theory itself.

Does that mean Bjorken and Drell's second volume for experimenters is simpler, derivative, less important than the first? Not at all. In fact, some of the detailed calculations of specific scattering processes are significantly more elaborate than the proofs that follow in the more theoretical volume. But the out-talk volume written for experimentalists was, without any doubt, more procedural. To find out how likely X is to happen, draw diagrams Y and assemble a mathematical expression from those diagrams using rules Z. Integrate and solve.

Linguists are well aware that there seems to be a cross-cultural capacity in language to be able to switch registers, to shift to more regularized uses of syntax, semantics, and phonetics. We can and do quite deliberately (in English) switch to subject/verb/object syntactic constructions, and drop embedded dependent clauses; we can restrict vocabulary (limit the lexical structure); and (in phonetics) move from complex vowel strings to a highly regular CVCV construction, in which consonants (C) and vowels (V) alternate. This flattening of exceptional constructions often characterizes "out-group" communication, for example to a new language learner. It is precisely this change in register (regularization) that characterizes the difference between the two volumes of the Bjorken and Drell textbook on quantum field theory. They wrote volume one (explicitly) for experimentalists, while they produced volume two (explicitly) for theorists. Experimentalists get a form of out-talk, a version filled with very elaborate uses of quite difficult applications of Feynman diagrams, but stripped of the talk about Feynman diagrams that explores exceptions, mathematical difficulties, internal structure, proofs, theoretical analogies. One final, fascinating bit: Bjorken and Drell suggested that the out-talk (the Feynman rules) might well outlive the in-talk (field theory)—that the diagrams may become the foundation, with the field theory nothing but a "superstructure."

The point of emphasizing the power of what goes on in the trading zone is that the trading zone is not "mere mortar" between the solidity of bricks. What is exchange work today may well become the disciplinary pillars of tomorrow: science is forever in flux, not just in its results but in the contours of its disciplines. Nanoscience began as an interdisciplinary initiative, but by the early twenty-first century it loomed as a major continent in the map of the sciences. In the labs of nanotechnology, the atomic physicist, surface chemist, electrical engineer, and molecular biologist make common cause. And as they seek to construct objects a billionth of a meter long, their refrain is: Leave the inessential behind; bring the necessary to work.

We regularize as well when deploying material means in action. If you are teaching a beginner to fly, you make every landing the same; then, only gradually, you introduce the myriad of particular exceptions to the rules for soft field, short field, and short/soft field landings, for example. We individuate and modularize concepts, separating them from their original multiple and interconnected functions. Like words, phrases, propositions, and arguments, objects also perform many functions simultaneously: a bicycle wheel rolls (like a log), stabilizes (like a gyroscope), and maintains rigidity (like a doubled arch). We are as capable of stripping down these multiplicities in material means and actions as we are in "strictly linguistic," symbolic, or diagrammatic ones. In trading zone science, the disciplines themselves are relentlessly, restlessly shifting shape.

Objection 2 Neoliberal Reductivism The very idea of trading or exchange presupposes an underlying notion of *money*, with all the economic assumptions that implies. Indeed, by its very nature, any model that includes trade reduces knowledge making to money making, and so is reductive, transhistorical, and transcultural. Trade imposes ideas of profit, universal valuation, and divisibility, and, worse, assumes a calculated rationality of self-interest. Isn't a trading zone at root a free-trade-zone view of science that, in the end, amounts to a misfired, neoliberal attempt to be universal—a neoclassical economic theory extended too far?

First, we know from a raft of work in anthropological economics that the Western mode of handling money within a market economy is by no means universal. So the idea that any form of exchange presupposes an underlying currency, or that money presupposes a single form of rationality, simply will not hold water. For example, Stephen Gudeman (2001) argues that there is always a tension between mutual or community exchange and market exchange. In his view (reaching back through Marx and Aristotle), the search for profit is by no means universal in every economic formation. Profit as a desirable outcome of exchange is, in fact, the result of quite particular forms of work and life.

More generally, cultures produce many ways to exchange goods. In some contexts, Gudeman reports, a twentieth-century peasant community in Panama found the idea of making a profit fully unrecognizable. When they did see profit making—through their contact with outside traders—they found this form of buying and selling almost incomprehensible. Even the presence of money itself in the form of coins and bills does not guarantee a particular stance toward it. Another anthropologist, Michael Taussig (1980), strikingly showed that there were culturally specific groups for whom the peso could be blessed—and in such a way that, after a purchase, it would eventually return. As the blessed peso shows, even if money were present in all forms of trading (which it is not), money alone would imply neither a universal rationality of how money is used nor an acceptance of the properties of money that would find a recognizable description in a standard Western economics textbook.

Second, from a long tradition of work within anthropology, going back at least as far as Marcel Mauss, we know that exchange relations can be of many types. There are gifts, as Mauss made clear, that can carry varying degrees of symbolic-personal baggage, incur obligations on the part of the recipient, and more generally function outside a simple model of neutral objects that pass from one person's possession to another. For example, there are general, or unreciprocated, gifts (e.g., from parents to children); there are direct forms of bilateral exchange that demand reciprocity (barter is one type of such an exchange but not the only type); there are circular exchanges (where X may give to Y, Y to Z, and so on … until somehow, and maybe much later, someone gives back to X). In her *Beamtimes and Lifetimes* (1988), Sharon Traweek used the anthropologist's contrasting notions of circular and bilateral exchange to analyze the movement of postdoctoral researchers in particle physics from lab to lab, and models their circulation on the nonreciprocal exchange of women among groups.

Does the circulation of postdoctoral researchers among laboratories presuppose a specifically monetary logic of exchange involving profit? Of course not. Trade and exchange form a broad genus of which Western, neutral, monetized, storable, divisible cash is but a single, very particular species. In fact, as Thorstein Veblen pointed out long ago (Veblen 1915, ch. 3), it is a form of "derangement" to extend our conception of a single form of monetarized exchange everywhere—to run away with metaphor, to allow financial capital, for example, to be confused with industrially productive capital.

If we are going to avoid such derangements of overgeneralization, we must stay focused on the specific kind of exchanges relevant to exchange languages in the scientific-technical trading zone. At root, the relevant aspect of exchange is this: what an object means to me when I give it to you may very well not be what you, as the recipient, understand that object to connote. What matters is coordination, *not* a full-fledged agreement about signification. I hand you a crystal, you hand me a flute. All we need to know in that moment is that we agree to exchange—not the structure of the crystal or flute, not their origin, meaning, uses, or provenance. *Nothing* in this swap requires a reference explicitly or, for that matter, implicitly to money as a commensurable entity, to a universal instrument of value, or to a universal logic. It is the possibility of this relative superficiality—the possibility of a *thin description* that interests me in the trading zone. It is thin insofar as we do not need to refer to some universal currency of rationality or value. And thin in a second sense: we can bypass the presupposition that there is any agreement among the people exchanging things about the full signification (or thick description) of the objects exchanged.

In the old battle between logical positivism and antipositivism, we have a fatal recapitulation of the struggles between Enlightenment ideas and romanticism. The logical positivists, who modeled themselves quite explicitly on the Encyclopedists of the eighteenth century, desperately wanted a universal common divisor, a language of science (Frege-Russell logic plus experiential protocol statements) that would cut across theories, places, peoples, and times. The antipositivists from Kuhn on down wanted worlds apart, more akin to Boasian cultures, the legacy of nations, each with its own incommensurable worldview. Frameworks, paradigms, programs—each aimed to capture a scientific world that stood on its own, that could be judged only according to its own terms, that denied absolutely the Enlightenment *characteristica universalis*.

The picture of scientific cultures and subcultures tied to each other and to technical and other cultures of the wider world fits neither the Enlightenment nor the romantic view. The structure and content of specific trading zones are by no means universal. The fragmentary, halting attempt to build up structure between biology and chemistry is not automatically a key to cracking the relation between biologists and physicists. There is no protocol sentence or one-size-fits-all logic of combination. But at the same time, while the trading zone picture cracks the perfect autonomy of the romantic paradigm, its splendid isolation is not so secure. Instead of languages that are purely "Newtonian" or "Einsteinian," we have a hybrid mix of local structures bridging the two: theories with an ether and no way to detect it, experiments that allowed local comparison of electron flight in electromagnetic fields, theories with ether and no particles, theories with particles and no ether—and much in heaven and earth besides.

Trade focuses on coordinated, local actions, enabled by the *thinness* of interpretation rather than the thickness of consensus. Thin description is precisely what makes it possible for the experimentalist and the theorist to communicate, albeit in a register that by no means captures the full world of either, let alone both. Thinness is what makes it possible for the surface chemist to work with the atomic physicist, the virologist with the electrical engineer, the computer scientist with the molecular geneticist. The theorist does not have to probe the myriad laboratory procedures that lie behind the experimentalist's confidence that liquid hydrogen has particular thermodynamic properties; and the experimentalist does not need to know the full mathematical-physical reasoning backing the theories that lie behind a calculation. What they need is consensus in a restricted zone, a zone where coordination is good enough.

Objection 3 Power and Diffusion The very nature of trading seems to presuppose a voluntary agreement between equals, as if power differences did not exist. Doesn't this skew the very nature of exchange between different scientific and technological actors? Doesn't a power asymmetry make an analysis of contact in a trading zone impossible when power imbalances are in play? What happens when power is maximally unbalanced, when there is a simple domination, restructuring the subordinate in the image of the powerful?

There was a time when imperial history aimed to show that the British or Americans or French were the affecting, unaffected masters of the world. Their languages and cultures and economies were supposed to supplant those of the locals with no residue. Imperialists were viewed as civilizing forces by some and as despoilers by others, but whether they were seen as sanctifiers or sackers of the castle, historians tended to agree on this: Delhi was reshaped by London, Dakar by Paris, Samoa by Washington—but not for a moment were the imperials themselves seen as being reshaped by their encounter with the conquered. The problem is that none of this story of one-way cultural imperialism held up much past the mid-1960s. No good history of the last half-century tells of French impressionism without including the encounter with Japanese prints and stamps or African masks; no analysis of the origins of American jazz or rock and roll can be composed without including the musical culture of West Africa or modern African-American music history.

Returning to science, I'd like to look at the heartland of the purest pure physics: quantum electrodynamics. The powerful and prestigious theorists—the young American Julian Schwinger and his opposite number in Japan, Sin-Itiro Tomanaga, who would each later win a Nobel Prize—surely *spread* their knowledge, *imposed* their views, "disseminated," "radiated," "multiplied," "diffused" their knowledge down one-way channels issuing from the center. It seems to be true, in this case, that the center transmits, while the periphery (more or less properly) receives.

But dig a bit. It turns out that Schwinger and Tomanaga reformulated the foundation of physics after World War II. During the war, they had fought on opposite sides, each working with their respective radio engineers, each side at first having cobbled together radar sets, later groping their way in the new domain of microwave engineering and industrial production. Before the war, radio engineers, who were low in prestige compared with theoretical physicists, wouldn't have shown up, at least in the United States, at the highest levels of the most prestigious universities. But the war shifted the relation of physicists and engineers during the years spent reformulating electrodynamics so that the radio engineers could use it to fight the war. The high-flying physicists began to see their own endeavor in the image of radio engineers: from 1946 to 1948, black-box input-output analysis and relations, "effective" circuit elements, and modular calculation strategies began to show up *inside* the heartland of the high-born theory. Both Schwinger and Tomanaga testified that their work on wartime radar had been important for quantum electrodynamics.

Maybe the purest of the pure is thus not quite so far as we imagine from the black-speckled microwave transmitter mounted in a B-29. For Schwinger and the radio engineers, and indeed for the vast majority of the American and British panoply of physical sciences, the propellant was war. With the Blitz pounding London, there was nothing abstract about the Nazi threat—there would be an effective radar system, or the war would be lost. External forces—war, economics, natural disaster—can drive participants into exchange. The radar engineers had no idea how to produce effective circuits without the help of the physicists. And the physicists were in no position to design the apparatus without the experience of the radio engineers.

Intriguingly, here we have an example where the trade—the coordinated exchange between electrodynamic theory (by theoretical physicists) and very pragmatic microwave circuit design (by radio engineers)—reshaped high theory. It did so not by importing microwave resonators or antennae directly into quantum electrodynamics. Instead, the *syntax*, so to speak, of a laboratory science—the characteristic rules of manipulation—got taken up by the physicists. As an example: radio engineers had a way of analyzing problems that required the reduction of each component to an equivalent circuit (a circuit that had the same input-output relations but was physically much simpler; the engineer ignored physical details that had no importance to the output). Immersed in such design problems when he was assigned the wartime task of producing equivalent circuits for microwave devices, Schwinger learned to calculate things by ignoring everything that was not essential to the task of relating input to output.

Schwinger learned, for example, to ignore those aspects of particle collisions that were not important to the final state of the system. This systematic stance of the radio engineer—to focus on the input-output relations and ignore physical complications that do not affect the final state—became the basis for Schwinger's take on the development of the physicist's most abstract achievement: renormalization theory, which showed how to get finite, precise predictions from the theory of quantum electrodynamics. Here we have a striking example of a trade conducted between communities of very different prestige and authority: physicists from the most powerful universities, working in a radar program that they controlled, trading with radio engineers.

From examples like the joint work of radio engineers and theoretical physicists, a new question arises: What characterizes the forms of technical exchange that take place under such conditions of inequality? This, as it turns out, is a question that arises in nonscientific interlanguages. Some anthropological linguists have argued that the subordinate group often donates syntax, while the superordinate group provides lexical or referential structure. I suspect that something similar went on between the physicists and the engineers: calculational strategies were from the engineers, terms from the physicists. While this pattern may not be universal, it is suggestive. At the very least such examples prompt a set of questions: In instances of *unequal* exchanges between scientific-technical subcultures, what precisely does make it to the interlanguage from each side? It is a question that cannot even arise if we stop our analysis with proclamations about "interdisciplinarity," "collaboration," or "symbiosis." Those terms point at the problem; all the interest, in my view, lies in unpacking what the nature of this coordination is and how it evolves over time.

We can then ask the reciprocal question: What happens at the other extreme—when the groups involved in trade are more or less equal, rather than utterly disparate in their prestige and authority? What drives exchange in the *absence* of command structure (of the kind a government exercises in total war)? Though I can give only the most schematic of sketches here, the following is an example from the early 1990s, when mathematicians (more specifically, algebraic geometers) confronted physicists (string theorists). Both groups were quite prestigious within their professions, both thoroughly abstract; neither could be said to have had the upper hand.

Since the late nineteenth century, mathematicians had struggled to count the number of independent curves of a given type that could be drawn on a surface. Over decades, with Herculean efforts, the "enumerative geometers" worked out the first of several such problems, coming up with 2,875 curves of type one (n1). Then, in 1986, came a triumph: Harvard's Joe Harris managed to find the second-degree curves (conics), and reckoned their number to be n2 = 609,250. Moving up to curves of the third degree promised to be painful beyond measure, but two Norwegian mathematicians set themselves the task with the help of a cheap computer. By 1990, they had a result. At the same time, using utterly different methods having to do with the scattering of one string by another, a group of physicists claimed to be able to resolve such problems as the calculation of n2. When the mathematicians tried to flick them away, the physicists offered to provide n3 … and n4, n5, and even n10 … in a matter of hours.

Each group found almost everything about the other's approach incomprehensible. They used different methods, they had different objects and a different vocabulary. But they knew one thing: they clashed—over a number. For now matter how you sliced it, the two results were incompatible:

Physicists: n3 = 317,206,375

Mathematicians: n3 = 2,682,549,425

Not much later, the Norwegian mathematicians discovered an error in their computer program, and they too got n3 = 317,206,375. From that moment in 1990, the two sides knew they had to sort out what the other side was doing. The mathematicians saw that the physicists had, by methods utterly unknown and indeed incomprehensible to them, found an easy road to results that the mathematicians themselves had struggled to achieve for decades. The physicists now understood that their hunt for an account of string theory collisions had taken them into mathematics—and they wanted to understand the strange spaces in which the strings would live. For a brief moment, the two groups shared what amounted to the world's smallest imaginable trading zone: a single number, n3.

Bit by bit, what began as a punctiform trading zone became much more. Jointly authored papers, conferences, and graduate programs began to emerge; and in concert, but not without tension, mathematicians and physicists composed a growing but still restricted vocabulary and set of procedures. Within a few years, they were debating the virtues of training a new generation of scientists who could move back and forth between mathematics and physics, exploiting not only the concepts and methods but also the intuitions of both. This was a trading zone propelled not by external demands of the state, but instead by the separate—and quite different—ambitions of the two sides.

If one is content to label work between scientific subcultures as "interdisciplinary," questions remain that are utterly obscured. Of course we know there is collaboration—that is what we want to understand. To tackle the joint workings of different groups by referring to a label is not much help. It reminds me of Molière's quack who explains the sleep-inducing power of opium as being its *virtus dormitiva*. What we need is a much more interesting and effective active ingredient than "virtus dormitiva"—instead a way of approaching joint work that parses what comes with what, and how ways of speaking, calculating, and building are coordinated.

Objection 4 Language and Materiality Within the trading zone, exchange languages—scientific jargons, pidgins, and creoles—are supposed to structure the nature of what is handed back and forth. Isn't this use of language *just a metaphor* from linguistics, an unrelated field? Worse, if science really is nothing but linguistic, where does that leave us with material objects? My point in writing *How Experiments End* and *Image and Logic* was to reintroduce the materiality of argumentation; yet doesn't exchange-language talk eviscerate the materiality we have worked so hard to reinstall in the study of science?

We have been trying to understand the linguistic face of science for a long time—certainly since the beginning of modern philosophy of science between the two world wars. Rudolf Carnap's (1937) "logical syntax" was directly and explicitly an attempt to get at the structure of argumentation without buying into what he considered the inevitable subjective metaphysics of trying to ground the language of science through the direct and subjective referentiality of statements like "I see red." Could I ever really know how someone else experienced blue? Of course not. Instead, relational structures—locating blue on the spectrum—could be shared (we agree blue is between violet and red), and this syntactic rather than semantic structure would undergird objective knowledge. In addition to Carnap's approach, Otto Neurath's "physical thing language" also made language essential, as did their joint insistence on protocol language as the sine qua non of meaningful talk in science. Could you or could you not take your scientific claims and express them through such utterances as "smell ozone 12 noon here"? If so, proceed; if not (as the Vienna Circle claimed), you may be making noise, but you are not speaking meaningfully. (For more on Carnap, Neurath, the Vienna Circle, and Quine, see Galison 1997, chs. 1 and 9; and more generally Giere and Richardson 1996.)

Quine too spoke of theories as languages—and he pointed out that there would always be more than one way to translate from one theory language to the other. Though he famously split from Carnap on some issues, Carnap also constantly emphasized the multiplicity of languages that could be invoked to express certain structures. In his most famous work, *Der logische Aufbau der Welt* (properly translated as "The Logical Construction of the World"), Carnap underscored the possibility of reexpressing the same structure in different ways: one could start with "my" experience, or one could start with a more social-collective base. Either way, the relations among propositions would remain the same.

During the 1960s' backlash against logical positivism, philosophers of science changed their account of science dramatically. No longer would protocol utterances remain the universal language base of science. But the idea that scientific accounts were, in the end, a form of language, did not perish. Instead, Thomas Kuhn took the big paradigmatic theories—of heliocentrism, phlogiston chemistry, oxygen chemistry, classical physics, and relativity (to name but a few)—and considered them as full-on languages, analogous in their own right to English, French, or German. Kuhn's thesis then put a twist on Carnap's principle of tolerance (or for that matter Quine's conventionalism). Where Carnap and Quine argued that there would always be *more* than one translation, Kuhn shocked readers by claiming that there wouldn't even be one. Speakers of "Newtonian," as it were, could never, without gaps and awkwardness, fully translate what they had to say about the world into "Einsteinian." Any such attempt would fail for the same reason that there are inevitable misfirings between texts in German and French: there was no adequate translation.

That Kuhnian picture of full-blown but incommensurable languages—languages intact in themselves but without a common divisor like a protocol language—smoothly carried over into much of classical 1980s-style science studies. (Indeed, incommensurable languages grounded the methodological and philosophical commitment to relativism, and 1980s science-and-technology-studies relativism cast itself as the polar and only alternative to the putatively referential claims of a naïve realism.) Even in the rather distant frame of philosophy, Michel Foucault took science and divided it into epistemes, each of which was marked by a particular logic by which statements (*énoncés*), rather than true/false propositions, could be ordered.

Throughout the decades from the 1920s through the 1970s, science was thus formulated and reformulated as a kind of language. Debates raged about the kind of language it might be, of course, but that it was a language, a form of structured communication, was not really debated. Crucially, the language of science was *always* a global one, invariably analogized to a snapshot, unchanging and global, of a contemporary French, English, or German.

My original problem—the problem that drove me to the idea of trading zones and scientific exchange languages in the first place—was my frustration in trying to join a local picture of practices with this fixed, global idea of language. The two clashed. By contrast, interlanguages are exactly characterized by their change over time and by their locality—exactly what one needs in order to talk about scientific language in the context of a shifting set of laboratory or blackboard practices.

Trading zone languages can be quite heterogeneous: they are sometimes nothing but a few terms held in common, a bare scientific jargon. As we saw earlier, in the clash between the string physicists and the mathematicians, they were, at the outset, at the very minimal limit—*all* they had in common was the (disputed) number of curves on a certain surface. Here was a trading zone with nothing in common but the number of curves of degree three. That particular point zone eventually did expand, hugely—into a much wider world of mathematical-physical discourse that transformed both "parent" fields (string theory and algebraic geometry). Other trading zones carried over from a scientific jargon to a scientific pidgin to a scientific creole—think of biochemistry, which is now quite capacious enough to "grow up in" scientifically. But there is absolutely no teleological guarantee. Not every jargon gets developed into a pidgin; not every scientific pidgin molts into a creole in full bloom. In the nonscientific world, examples abound of pidgins that froze and died: some of the Korean-English pidgins forged during the Korean War of the early 1950s simply vanished when the firing stopped. As another example, iatrogenics was a science its creators hoped would join Newtonian mechanics and physiology. It perished without a trace after the eighteenth century. So, more or less, did "neutronics," an interdisciplinary field that nuclear scientists, engineers, and health physicists hoped would flourish after 1945.

Sometimes the language of science does read, quite literally, as language: propositions, statements, observations, hypotheses, and conditionals are all recognizably linguistic even if technical in scope. But at other times practices do not necessarily form linguistic objects, in a strict sense. Diagrams and symbols, for example, have their own combinatorial logic. We are used to talking about the "language of mathematics," and for good reason. I am interested in language in an expanded sense that would embrace such symbol languages—whether computer codes, abstract algebra, formal logic, or the calculations of quantum physics. Each carries with it its own form of syntax, its own rules of simplification, generalization, and composition. Similar, though perhaps less familiar, are languages formulated in ways that make use directly of spatial or topological relationships—electronic schematics, group-theoretical Dynkin diagrams, Minkowski space-time diagrams, Feynman diagrams (on the latter, David Kaiser's work on the piecewise transport of Feynman diagrams is central).

Diagrams too have their rules of manipulation. Reasoning with them does not necessarily require constantly returning to words or even algebra. Indeed, that's why they are so useful: spatial arrangements suggest variations and allow manipulations *without* translating into another idiom such as words. (Write down a Feynman diagram for a particular scattering process and a physicist might say, "What about this diagram?," and modify the first by flipping a dotted line but preserving the same number of vertices, for example.) Moving from the manipulation of electrical diagrams to the manipulation of circuit elements themselves is not such a big jump—in fact, there are machines that take diagrammatic representations and *produce* the circuits. My view is that the regularized, rule-governed procedures that manipulate material or symbolic objects are also a form of language, and it becomes entirely appropriate to speak of wordless jargons, wordless pidgins, and wordless creoles.

In fact, we know perfectly well that such objects move back and forth—every day we pass musical scores, mathematical symbols, and electrical circuits back and forth between people who speak different natural languages. So it is within science—physicists of different theoretical persuasion can view a bubble chamber image and still find a thin description upon which both can agree: "that's an electron, hit by a neutrino, scattering and emitting a photon that becomes an electron-positron pair." Or think of the monumental efforts that have been made to produce anatomical, astronomical, or neurohistological atlases. Throughout the nineteenth century, these were produced in vast numbers precisely to work in a visual register free of detailed sectarian interpretation. Such atlases did not need to be simple or peripheral—but they did need to address an audience outside this or that tendency within the field. Like out-talk by speakers of a language, doctors or astronomers could produce images and objects open beyond the originating culture.

This set of thoughts returns us to the root idea of the whole scheme of trading zones: it is possible to share a local understanding of an entity *without* sharing the full apparatus of meanings, symbols, and values in which each of us might embed it. Images, symbol systems, calculational and diagrammatic schemes—even complex objects—could be part of a generalized notion of language that is far from "just words." Indeed, language, as I want to use it, is a regular yet flexible apparatus that may take many forms, from the recognized, everyday "natural world languages" to the myriad, systematic registers in which we communicate.

Objection 5 Applicability For a concept to be useful, it must have limits. But if everything is always hybrid, if every situation admits of a trading zone, then isn't this concept just a restatement of what we already know? What *isn't* hybrid; where does exchange *not* take place? In other words: What are the useful limits of the concept of the trading zone?

Exchange involves coordination between scientific-technical cultures. These cultures are specified by practices that pick out a certain quasi-stable configuration of practices—and the meanings, values, and symbols linked to the practices. But the necessary condition for a trading zone is that practices (and their interpretations) tend to travel in packs rather than along arbitrarily combined trajectories. These "packs" might be a set of affiliated experimental procedures in organic chemistry in the early twentieth century; or they might be the mathematical toolkit of the quantum physicist in the 1930s. Here they are tactics, there they are strategies—but also regulative values (what counts for mathematicians as well defined; what counts as a proof). Together, the assembly of practices, values, and meanings do more than simply pick out a problem-solving mechanism; they also set out the contours of scientific identity, defining what it means to say, "I am an experimental high-energy physicist," or "I am a theoretical biologist."

In the 1980s, many particle theorists viewed high-end mathematics (of the type string theorists practiced) with reserve, if not outright moral suspicion. They judged the ins and outs of Calabi-Yau spaces to be too fancy to have purchase on reality, not close enough to lab results. By contrast, nuclear physics and much of atomic physics seemed to those same 1980s theorists to be *insufficiently* theorized—too *close* to the measurable, too *liberal* in their acceptance of heuristic, phenomenological, and partial models, too *weakly* mathematized. Together, skills and stances offered the late-twentieth-century particle theory practitioners a way of looking at their corner of science and at what they stood for as scientists. Around certain practices came not only the meaning and symbols, but also the virtues and sins that gave a moral structure to this cut through scientific life. More generally, practices—along with the values associated with meanings and symbols—offer the defining attributes of scientific or technical subcultures.

Should we characterize *any* set of embodied practices as a subculture? The question is an empirical one. Is there enough regularity, enough covariance within a given set of practices, to merit our picking out that regularity for attention? We have to be prepared sometimes for the answer to be "no." If there is enough regularity to justify speaking of quasi-stable subcultures in contact with one another, then, and only then, is the trading zone idea useful, because it is then that the thinness of the exchange proves valuable—in contrast to the thickness of the established cultures.

For emphasis: the trading zone concept is *not* always applicable. Indeed, we know that many sciences—physics included—at some historical moments do *not* have this particular partition: experimenting, theorizing, and instrument making. Certainly such a division was not a commonplace in the time of Galileo. It would be equally distorting to split Gregor Mendel's work on segregation and assortment into a "theoretical" and "experimental" biology. In physics, William Thomson (later Lord Kelvin) cannot be thought of as *either* an experimenter *or* a theorist. But we can go farther. The right question to ask (about Thomson's work, for example) is this: Does Thomson, when theorizing, participate in a discourse (an ordered set of practices, whether linguistic, symbolic, or physical) that forms a roughly covarying set? Thomson struggled to relate the structure of atoms to the nature of ethereal vortices, the generation of smoke rings, and the theories of knots. In that case, he clearly was working in different practice sets—knot theory, for example, linked him to a group of mathematicians, whereas his broader theory of atoms connected him with other physicists. The relevant trading zone question is then empirical-historical: What was and wasn't shared between the broader atomic theory to which he was committed and his specific work on knots—what pieces of each were linked, and in what ways?

There is one final class of limits to address—the limits in which the trading zone concept comes into contact with other work in the understanding of science. (I have in mind a kind of mathematical limit rather than a limit of validity: $1/\sqrt{(1 - v^2/c^2)}$ goes to 1 when v/c is small but to infinity when v/c approaches unity.) First, consider cases where the exchanged object is not, in fact, part of an ordered trading language—where the object stands alone, so to speak, not subject, or at least not importantly subject, to rules of combination and association in the trading zone. In this instance, one has objects that sit on the boundary that can be compiled, collected, and used by different groups. This corresponds to Susan Leigh Star and James Griesemer's (1989) very useful notion of "boundary objects," which they developed to discuss the flora and fauna collected by California amateurs to document the forms of life in the Golden State. As they showed, professionals in different fields used these samples in different ways. Unlike the case, say, of algebra and geometry, out of which algebraic geometry was formed, or biology and chemistry combining to constitute biochemistry, the archival flora collections didn't, in the long run, become a field in their own right, for a collection is not a discipline. Boundary objects might be thought of as a kind of time slice of a trading language where the lexical lists exist, but our attention is not focused on the syntax.

A second limit of the trading zone occurs in the limit of an asymptotically large power difference between the groups trading. We have seen cases where the power differential was small (as it was with the mathematicians and string theorists); we've seen cases where the domination of one group by the other was fairly significant (physicists working on radar with radio engineers). But one can imagine instances where the discrepancy is so enormously huge that essentially no input comes at all from anywhere but the superordinate group. Given that even slaveholder cultures were reshaped dramatically by slave culture, it is not clear that such an absolute gap could exist, but in that limit, one could imagine a scientific laboratory that imposed itself like a kind of implantation, a colonial outpost that repelled all forms of locally produced materials, machinery, products, or personnel. In that forceful extension of the center, one indeed would find the kind of situation that Simon Schaffer (1991) captures so well with his notion of a "multiplication of contexts," by which a laboratory in London or Paris could move its experimental apparatus. Or think of Bruno Latour's

(1985) important work on the ways the world must be configured to allow the scientist to "travel very far without ever leaving home," the material analog of his "immutable mobiles" (movement without going anywhere). These are indeed the extremes of power imbalance, the annihilation of locality, the far-limit horizon of cultural contact in which the superordinate group hugely outweighs the subordinate one.

Finally, there is a third limit point of trading zones. Consider what happens when a group presents out-talk in the absence of an engaged interlocutor from another, distinct group. Here, I do not mean the atomic physicist presenting her work within the nanolaboratory to the surface chemist and virologist. Instead, think of the presentational, reflective, informal work among such practitioners as they walk away from the laboratory, talk over lunch, or begin to sketch out a paper or presentation. This, I would argue, is the limit situation that gives rise to what Harry Collins has dubbed interactional expertise (see, e.g., Collins and Evans 2002). Collins considers an interesting case—the verbal, unmathematical discourse of the gravitational wave physicists. Interactional expertise is a very interesting notion, one the too-rigid canonical social studies of knowledge has (in my view) dismissed too quickly. Collins's neo-Turing argument is strong and persuasive (the test is this: Can an outsider learn enough vocabulary and characteristic ways of speaking to simulate a "real" gravitational physicist as long as no mathematics, hardware manipulation, or calculations are involved?). It seems to me absolutely right that it is possible to learn to interact in such conversation in a way that is familiar and recognizable to all practitioners—without, in the process, learning the others' mathematical or detailed experimental or craft competencies. The capacity to carry on a professional-level, informal conversation about gravity waves is precisely analogous to particle theorists' ability to speak in the more regularized, stripped-down manner of out-talk scientific pidgin.

Early-twenty-first-century experimentalists working on the billion-dollar detector at the Large Hadron Collider at CERN had to converse with the electrical, cryogenic, and structural engineers. To do so, the experimentalists needed to know how to move their vocabulary, parameters, and calculation devices into a form that a sophisticated technical person (who was not a physicist) could grasp. Gone from the experimentalists' local concern were the details of the supersymmetric partner to the photon, the hypothetical "photino"; gone too were the detailed physics of the supposed Higgs particle. These physicist-with-engineer discussions were much more about gas characteristics in the detectors, the failure rate of circuits, and the degree of radiation hardening against the appalling environment in which the detectors had to live. In all trading zones, there is always such a shift of register as each of the participating groups creates an out-talk suitable for communicating with the others.

I take interactional expertise to be the capacity to speak in a specific register, an acquisition (by an outsider) of a form of pidginized out-talk used by physicists, for example, to speak with one another with a minimum of ancillary knowledge. Stripped from these conversations are a wide variety of other kinds of talk: detailed calculations or proofs from differential geometry on one side, particular issues of materials, instruments construction, experimentation, or engineering on the other. Though it repeats a theme I keep hitting, it is important to emphasize again that regularized and stripped-down out-talk is not a lesser version of something else; rather, it is a register of scientific interaction that is supple and effective in its domain. A creole is not a poor version of a "parent" language. In precisely the same way, scientific out-talk is neither identical to the technical language from which it originates, nor a diminished version of it. The skills of someone versed in interactional expertise represent one specific register of scientific language. Regularized, demathematized out-talk is then a third limit of the interlanguage performance within a trading zone.

Trading Zones: Why Now, Where To?

Historians, political scientists, and sociologists regularly think of the Cold War in terms of international confrontation, domestic political repression, and the arms race. But we have only touched the surface of how the long war from 1939 to 1989 shaped—and effectively froze—aspects of the academy. I'd like to suggest, all too briefly, that the post-Cold War disciplinary map is in a state of intense rearrangement, one unparalleled by any developments since the immediate postwar years of the late 1940s. This set of shifts has made modes of coordination between and among long-established fields immediately pressing—contexts in which trading zones are conspicuously present.

It is easy to think of our universities as highly stable, unchanging fixtures of the world, so old as to be part of the distant, unremembered past. But the academic forms we know from the present are much more recent than the antique founding statues and plaques that adorn the university gates. The world of internationally connected science, liberally funded by national agencies and open to an increasingly diverse population of students and faculty, is a creation of the years just after World War II. So too are the system of national laboratories, competition for contract funding, and the construction of government-owned and corporate-operated laboratory facilities. In the United States, the Atomic Energy Commission, the National Science Foundation, the bulk of the National Institutes of Health—just to name a few—also rose in the shadow of the Cold War. Besides these large institutions, the departments of universities achieved a new kind of fixity. True, atomic physics gave way to nuclear physics, nuclear physics yielded to nucleonic structure studies, and the interior resonances of protons and neutrons eventually yielded to particle physics. But physics from the 1920s through the 1970s remained recognizably physics—the basic courses, the ethos of training, the divisions between theory and experiment, the prestige hierarchy of pure over applied work.

Over the course of the Cold War, the essential integrity of the basic departmental division of knowledge stayed in place, even if new departments would sometimes appear, such as computer science and biochemistry. Since the fall of the Soviet Union, some of this fixity has been eroded. New flows of funding bolster different kinds of research—startups, intellectual property, venture capital—and all have blurred the lines between the pure and applied sciences. Cryptology went from being a concern of the national security state to a Web-based industry. Nanoscientific groups often maintain two Web sites, one for their academic work, another for the corresponding startup. Universities encourage and even participate as stakeholders in the acquisition of patents and their deployment in new ventures. All of this means that an increasing number of students emerge from their doctoral studies with a very different experience of disciplinary formation. In many arenas of nanoscience, wherever one began—atomic physics, surface chemistry, electrical engineering—collaborators come from all three domains. Clean rooms, visualization facilities, and fabrication devices are all shared. Joint appointments have become increasingly common between physics and biology, physics and mathematics, physics and chemistry, and so on down the line.

This world of shared space, technique, training, and authorship has increasingly obviated the spell of the "pure." The terms "pure physics" or "pure biology" ring false. More than that, the very idea of pure science—as more prestigious, important, or consequential than applied science—has lost traction. In the midst of string-theoretical work, the challenge that an investigation is "not physics, just mathematics," doesn't much move the postdoc—nor does she find herself distressed by the idea that nanoscale devices are "just engineering," and therefore not truly physics.

Thus, everyday scientific work already militates for attention to crossover domains, and away from "pure" languages, theories, and disciplines. But if trading zones have helped us grapple with pressing boundary work after the Cold War, it can also suggest ways to look freshly at the other periods in which disciplines were in flux: in that postwar moment, for example, or in the late nineteenth century.

If the Cold War crystallized disciplinary divisions, its historiography also froze certain debates so deeply that they seemed to be inevitable features of any historical inquiry. Historians of art split (in what long seemed an irresolvable struggle) between the social theory of art, which aimed to tie the making of artworks to social conditions, and formalism, which wanted exactly the opposite—to make the painting speak on its own by the analysis of color, brushstroke, and other formal features. Literary historians similarly divided between close readings of texts and situating them contextually or theoretically. And historians in history departments fractured into those defending military, diplomatic-political, or high-cultural or intellectual history (from above) on the one side, and those after social or cultural history (from below) on the other. What powered these arguments and made them more than intraprofessional disputes was the long, ferocious battle over Marx. Behind historiography was politics—the hard-fought left/right politics that intensified during the Cold War.

In the 1950s, 1960s, and 1970s, the history of science spun its own version of Marx-anti-Marx: internalism versus externalism. Internalists prized the autonomy of science, its freedom from outside circumstance. Internalism was meant to be a giver of law to itself, dependent (when done right) on nothing and no one. Externalism, by contrast, took scientific content to be nothing more than the surface waves caused by the deeper currents of class, psychology, institutions, or technology. Yet tracking the flow of space, techniques, language, and standards offers a way to eschew this false choice between total autonomy and total dependence. Pushing locality all the way down to scientific techniques, languages, and values offers a way to address practices as they form among the sciences and between the sciences *and* the worlds of work that abut them.

There are many ways to carry on with research on trading zones and interlanguages, and it would be the worst kind of self-refutation, were I to try to set out what should or shouldn't be the "right" way to deploy such concepts. The trading zone offers a set of tools, not a doctrine. In that spirit, I want to gesture just a bit at the ways that the idea might have some rather practical consequences. Some, happily, are explored in this volume. But let me draw an example from elsewhere—from the work of Boyd Fuller (2006 and forthcoming), whose study of water use battles in California and Florida exemplifies some of what I have in mind.

Fuller began with conflict. The stakeholders in recent debates over the Everglades were more than diverse—federal and state regulators, tribal groups, environmentalists, and agricultural interests "exploded" in some of their early attempts to interact. Their values were irreconcilable, their desires askew. Fuller showed that these actors neither subscribed to a common worldview about the meaning and significance of wetland water supplies nor threw up their hands in despair at the clash of values. Instead, without abandoning their own deeply held values, the groups were able to establish terms of negotiation around a *delimited* set of water management recommendations. (For additional examples of trading zones involving scientific and technical policy, see the chapters in this book by Jenkins, by Gorman and Werhane, and by Collins, Evans, and Gorman.) Over a very broad range of battles from power-generating stations to fisheries, we have scientists and practitioners struggling to find common—but restricted—interlanguages. It would be powerful if we could understand more systematically *why* some disputes can be productively advanced through the formation of delimited trading zones, while other such attempts fail. If we could figure that out, our understanding might lead us to strategies to encourage positive outcomes. Here, it seems to me, is a theoretical problem that bears on the most practical side of trading zone work today.

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