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Material Culture, Theoretical Culture, and Delocalization

Collection, laboratory, and theater – all face the unavoidable problem of moving the specific, tangible reality of a highly refined local circumstance into a wider domain, if not of the universal, at least out of the here and now. In the study of science, simply recognizing the inevitably local origins of science has been an enormous accomplishment, perhaps *the* signal achievement of science studies over the past twenty years. But we then need to understand, again in specific terms, how this locally-produced knowledge moves, how – without invoking an otherwise unexplained process of 'generalization' – scientific work is delocalized. My work over the last years (e.g. *Image and Logic*)[1] has aimed at this goal, folding the local back on the local, so to speak, by asking how the local cultures of science link up through the piecewise coordination of bits of languages, objects, procedures. I have in mind much more austere and less grand ideas than the 'translation,' 'transmission,' or 'diffusion' of pre-existing meanings. Instead, my focus is on the way bare-bone trading may occur between different subcultures of science, or between subcultures of science and bits of the wider world in which they are fundamentally embedded. In this picture, neither language nor the world of things changes all of a piece, and talk of world-changing *Gestalt* shifts give way to the particular building-up of scientific jargons, pidgins, and creoles. These trading languages become important as do shared bits of apparatus or fragments of theoretical manipulation. Building on this picture, this piece is an exploration of how the conditions of theorizing, making, and experimenting generate new structures at their boundaries. It aims to get at what one might call local de-localization.

1 Peter Galison. *Image and Logic. A Material Culture of Micropolis*. Chicago: University of Chicago Press, 1997.

It was 1993, and superstrings, the 'theory of everything,' was the rage. Arthur Jaffe, a senior member of the physics department at Harvard and for several years chair of the mathematics department, along with Frank Quinn, a mathematician at Virginia Tech, penned the following in the pages of the *Bulletin of the American Mathematical Society:*

> Theoretical physics and mathematical physics have rather different cultures, and there is often a tension between them. Theoretical work in physics does not need to contain verification or proof, as contact with reality can be left to experiment. Thus the sociology of physics tends to denigrate proof as an unnecessary part of the theoretical process. Richard Feynman used to delight in teasing mathematicians about their reluctance to use methods that "worked" but that could not be rigorously justified.[2]

Jaffe and Quinn quickly added that mathematicians, unsurprisingly, retaliated: as far as they were concerned, physicists' proofs carried about as much weight as the person who claimed descent from William the Conqueror with only two gaps. Nor has tension between cultures been restricted to the axis of theory/mathematics. Albert Einstein and Paul Dirac famously derided putative experimental refutations of major theories, and experimentalists have never hesitated to mock what they considered to be the aimless speculation of theorists. One cartoon, widely circulated in the physics community during the 1970s, portrayed a balance scale with thousands of offprints labeled 'theory' heaped on one side, outweighed by a single paper marked 'experiment' on the other. Beneath these cross-currents of jibes and jests lie substantive disagreement about what constitutes an adequate demonstration, and, ultimately, a clash over whose pilings sink sufficiently deep to stabilize further construction. What vouchsafes knowledge, and for whom?

Like Jaffe, I find it useful to talk about the difference in cultures between the interacting groups that participate in physics. In fact, as we look around the national and international laboratories – now and throughout the last two centuries – the diversity of such cultures is striking: there are electronic engineers, cryogenic engineers, experimenters, computer programmers, field theorists, phenomenologists, just to name a few.

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2 Arthur Jaffe and Frank Quinn. "Theoretical Mathematics. Toward a Cultural Synthesis of Mathematics and Theoretical Physics." *Bulletin of the American Mathematical Society* 29. 1-3 (1993): 5.

What motivates talk of 'cultures' or perhaps better, the subcultures of physics? Part of the appeal of the distinction between cultures is driven by historical concerns. To address the question 'Why does this happen there and then?' we want to identify affiliations between certain activities inside the walls of the laboratory and others outside, all the while being careful not to exaggerate the distinction between 'inside' and 'outside.' The world of the electrical engineer fashioning circuitry for the central tracking detector at a major colliding beam detector is a world apart from that of the theorist who may eventually be a consumer of its data. By contrast, the electrical engineer may well share a world with other electrical engineers also concerned with shielding their delicate printed circuits from massive pulses of X-rays – engineers, for example, preparing electronic devices to survive a nuclear battlefield. A condensed matter theorist may have more to say to a quantum field theorist than to his or her own condensed matter experimentalist colleagues as they struggle to lay atom-thin films of metal, or build new ceramics.

Anthropologists generally understand the cultural to embrace not only social structures *per se*, but crucially the values, meanings, and symbols associated with them. Now it is true that the term 'culture' has always and continues to reside in disputed anthropological territory. Clifford Geertz, Marshall Sahlins, Gananath Obeyesekere, for example, sharply disagree on how constraining, how overarching a 'culture' is. But the important point for the historico-philosophical characterization of the production of science is that we cannot pretend that meanings, values, and symbols are mere window dressing. When a mathematician derides a computer-based demonstration as a horrendous violation of the very idea of mathematics, when a theoretical physicist recoils from renormalization, pronouncing it a 'trick': when an experimenter asks, in shocked tones, if future generations of experimentalists will get their data from 'archives' rather than through the concerted application of screwdrivers, soldering irons, and oscilloscopes – at these and other moments like them, *values*, always present, have surfaced. Meanings too can differ: when theorists speak about a particle, say an electron, they may, through usage, deploy a concept quite distinct from the usage of 'electron' spoken of by an experimenter.

It is useful to separate subcultures of physics on more philosophical, specifically epistemic grounds. We can ask: what is it, at a given time that is required for a new particle or effect to be accepted among theorists (in contrast to the requirements that must be satisfied among experimentalists)? This can be stated more precisely: what, at a given time and place, are the *conditions of theoreticity*? What is it that a theory must exhibit for it to count as reasonable even before it faces new experiments? From time to time these requirements change, and we can specify the circumstances of these alterations. A theory in many branches of physics is not out of the starting gate if it is not relativistically invariant, if it does not conserve charge. Such constraints may not be forever. For generations, *conservation of parity* was such a rigid requirement. (Conservation of parity is the demand that any process allowed by physical law ought also to allow a mirror-reflected image of the same process.) Parity, along with time reversibility, fell as absolute demands, leaving behind only approximate conservation laws. Renormalizability (the demand that a theory be constructed with a fixed and finite set of parameters that can then be used to predict to arbitrary precision) was another such broken constraint. Thought to be a rigid and exact stricture on theory in the early 1970s, by the 1980s renormalizability too reappeared as an approximate constraint.

In a similar way, we can speak of *conditions of experimentality*, focusing on the constraints that allow (or disallow) forms of laboratory argumentation. How are probabilistic arguments to be treated? Would a single instance of an event be considered a persuasive demonstration? Are witnesses necessary to secure experimental closure? Do experimental results without error bars count? Or, if results do include errors, how much statistical power is demanded? How much and what kind of knowledge can be deferred to other fields, literally or figuratively 'blackboxing' component parts of the experiment?

Conditions of instrumentality can also be distinguished as those constraints that delimit the allowable form and function of laboratory machines themselves. These conditions governing the material culture of physics may be of different temporal structures: there are broad, long-lasting classes of instruments (picture-producing instruments or statistic-producing instruments). Such classes provide constraints of the longue durée, as they set out the conditions under which (for example) picture-producing apparatus will be judged distortion free, or by which statistics-producing instruments will be assessed as having a certain loss rate. Then there are middle-term conditions on 'species' of instruments that may achieve legitimacy as knowledge facilitating objects (bubble chambers or optical telescopes or spark chambers). And at the short range of temporality there are the individual tests and conditions that certify individual instruments, *this particular* bubble chamber, or even more specifically this particular bubble and its production of *this particular* bubble chamber photograph. All such conditions of possible theorizing, experimentation, and instrumentation are temporal: they change with time; the dynamics of those changes constitute some of the most interesting and difficult questions facing the study of science.

Taken together, the historical, anthropological, and philosophical concerns suggest that periodization is a far more complicated business than might be suspected from older models of the philosophy of science. We ought at least look to see if the rhythms of change in one domain (experiment, for example) are the same as that of another (theory, for example). That is, instead of assuming that theory, instruments, and experiments change of a piece in one great rupture of 'conceptual scheme,' 'program,' or 'paradigm,' we would do better to see how the various practice domains change, piece by piece. Dates of conceptual breaks such as 1905 (special relativity), 1915 (general relativity), 1926 (non-relativistic quantum mechanics), and 1948 (quantum electrodynamics) may have been points of discontinuity in theory; they were not so in the development of the material culture that surrounded instrumentation and experimentation. And while there may be good reasons not to jettison widely accepted experimental practices at precisely the same moment the community is entertaining the radical reformation of theoretical practice, none of this is to say that co-periodization *cannot* occur. But it is to say that co-periodization ought be shown, not assumed to follow from the dictates of antipositivist philosophy of science in any of its forms.

An objection springs to mind. The separation of theory and experiment holds good in many branches of twentieth-century physics. Certainly this sociological division is so in atomic, cosmic-ray, nuclear, and particle physics, but also astrophysics, planetary physics, plasma physics, condensed matter physics. In each of these domains separate societies, meetings, and reprint exchange networks have long existed. But what of areas of inquiry where the separation is incomplete or nonexistent? What of the physics of the broad middle of the nineteenth century where a James Clark Maxwell, a Heinrich Hertz, or a Lord Kelvin could hardly be classified as a pure theorist or a pure experimentalist? And what of whole domains of other kinds of science, biology, to take perhaps the most powerful example? Does it make sense to speak of separate cultures of experimentation and instrumentation in such instances? The question could be rephrased: are these clusters of practices in experimental work tied to practices outside the laboratory differently from the way theory is situated? An example might be found in the introduction of nuclear magnetic resonance just after World War II. At least for Robert Pound and Edward Purcell, though they worked

	conditions of instrumentality			
		conditions of theoricity		
conditions of experiment				
time →				

Fig. 1: 'Intercalated' Periodization.

theory 1	theory 2	theory 3
observation		
time →		

Fig. 2: 'Positivist' Periodization.

both theory and experiment, their theoretical efforts drew on classical electrodynamics and basic quantum mechanics – both long since established – while their instruments and procedures drew heavily on then-recent wartime radar developments. I would argue this: there is no universal answer to the question of whether it pays to speak of distinct cultures of theory and experiment in the absence of sharp sociological lines between the groups. Everything rides on how bundled together certain practices are – and that cannot be settled in advance, but only in the thick of historical inquiry.

The periodization picture sketched here might be represented in Figure 1, designating *intercalated* practice clusters. Looked at more finely, even 'theory' ought to be broken up in a similar arrangement, with some forms of theoretical practice lasting for the long term, while others are of shorter duration. These same considerations hold good for instruments and experiments. Such a scheme contrasts directly with two others. In one (fig. 2), denoted in a too-rough designation as 'positivist,' the view is that observations build aggregatively and continuously, while the level of theory breaks seriatim. Because observation builds cumulatively and intertheoretically, many followers of logical positivism and logical empiricism held it in special esteem.[3] Theory in a sense builds on the foundation of observation, and numerous metaphorical systems have designed to capture this primacy of the neutral observation language. In the other metaphorical scheme – designated (also crudely) as 'antipositivist' – the scheme of Figure 2 is stood on its head: now instead of viewing a neutral observation language as primary and theory as secondary, the reverse is true (fig. 3). 'Theory' is every-

3 Peter Galison. "Aufbau/Bauhaus: Logical Positivism and Architectural Modernism." *Critical Inquiry* 16 (1990): 709-52.

observation 1	observation 2	observation 3
theory 1	theory 2	theory 3
time →		

Fig. 3: 'Antipositivist' Periodization.

where, as Benjamin Whorf, Thomas Kuhn, Paul Feyerabend, N.R. Hanson and so many others taught us. To enforce the notion of a conceptual scheme, antipositivists assumed the equivalent of the co-periodized picture of Figure 2. When theory changed, it precipitated a break in meaning that extended 'all the way down.'

Suppose we stay with the argument presented so far, conservatively focusing on that sector of twentieth-century physics in which the separation of cultures of theory, experimentation, and instrumentation are reasonably well defined. Still, a new and more serious objection arises: if the culture of theory, for example, is really all that distinct in its contextualization, meaning, values, and argumentative structure from that of experiment, how do the domains relate at all? Reformulating the problem we could say this: to escape from the problems of noncommunication that arise from the confrontation of various block-periodized 'conceptual schemes' we moved to specify more local, intercalated subcultures of physics. Doesn't this just multiply the original problem, leaving us with hundreds of incommensurabilities, ruptures, revolutions, or epistemic breaks where before we had a few?

Behind this objection is a picture of language that is fundamentally holistic. 'Mass,' 'time,' and 'space,' are thought of as fixed terms, fully specified along with their connotations in one conceptual scheme (the 'paradigm of classical or Newtonia physics') and carrying with them a particular set of instruments and experimental procedures that are only understandable in terms of that conceptual scheme. Equally fully specified, or so goes the argument, is another, incompatible conceptual scheme (the 'paradigm of Einsteinian physics') in which 'mass,' 'time,' and 'space' have utterly different meanings. Because Einstein and Newton and their respective followers 'speak different languages' any putative communication between them amounts to little more than puns, a homophonic happenstance. Out of this picture come some of the most famous metaphorical structures used to capture the radical untranslatability of languages: *Gestalt* shifts and systematic visual-perceptual misconstruals on the basis of prior conceptions. If *Gestalt* shifts or total shifts of conceptual scheme are the model for what happens at the boundary between languages then indeed, the repartition of schemes like the positivist periodization of Figure 2 into the antipositivist periodization of Figure 3 may be of use historically, but analytically we advance not one inch.

What does happen at the boundary between cultures, where people face one another? Do people, in fact, translate with the sudden, *Gestalt*-like character of the duck-rabbit switch? Here we can learn from the burgeoning field of anthropological linguistics, a field that at least in one of its forms has dealt extensively with the historical and structural development of *trading languages*, highly specific linguistic structures that themselves fit between two or more extant full-blown languages. Very roughly a 'trading jargon' or 'foreigner talk' designates a few isolated terms used to facilitate inter-linguistic communication, 'pidgin' refers to a more developed language, with sufficient structure to allow more complex modes of exchange between speakers. Generally, pidgins are characterized by more regularized phonetic, syntactic, and lexical structure than the 'parent' languages that the pidgin links. For example while one of the parent languages may carry multiple consonant clusters (CCC), pidgins tend to be routinized into consonant-vowel-consonant (CVCV) form. Pidgins may – but do not necessarily – develop into full-fledged creoles, where 'creole' designates a language with sufficient structure to allow people to 'grow up' within it. Creoles, unlike pidgins, can be a *first* language. Intriguingly, it seems to be the case that among our linguistic capacities is the ability to shift the register of the language we speak: we are able to restrict vocabulary, regularize syntactic as well as phonetic form.

On this view, linguistic borders are not the thin-line mathematical idealizations that they appear to be in the *Gestalt*-switching picture of the antipositivists. Instead, linguistic borders appear as thick and irregular, more like the creoles one in fact finds in border regions in many areas of the world. So it is, I argue, in the *trading zones* between theories or between experiment and theory, or between a physics subculture and an engineering subculture grounded in the industrial or military traditions in contact with physics. Here I would suggest that we drop the attempt to gloss the interaction among the electron theorists Lorentz, Abraham, and Poincaré with Einstein in the early twentieth century as 'Classical' physicists 'talking past' 'Relativistic' physicists, a forced *Gestalt* switch between old and new notions of 'mass,' 'space,' and 'time,' with language shifting as a whole. Instead, we ought to examine the ways in which each of these physicists actually went about coordinating their theories with the results of experimenters like Kaufmann and Bucherer. For despite these philosophical protestations about incommensurability, these laboratory ventures were precisely aimed at comparing the various electro-dynamic theories. How, they asked, did the deflection of fast electrons as a function of velocity come into contact with (for example) Abraham's and Einstein's specific theoretical proposals? Or consider the interaction of experimenters and theorists around track images in bubble chambers. While each may have very different ideas about adequate demonstration, or even about the nature of particles, it is nonetheless often possible for both groups to come to common ground of 'interpretation' – e.g. 'this particle decays here to two lighter particles, one of which escapes and the other is absorbed.' The point is that some meaning gets stripped away in the trading zone where theory meets experiment, where engineering meets theory, where, in general, the scientific subcultures encounter one another. What thrives in the interstitial zones is neither trivial nor purely instrumental, it is a form of scientific exchange language.

In the interstitial zone I have not distinguished sharply between locally shared terms and mathematical-syntactic relations on one side, and the material objects on another. This is deliberate: we ascribe meaning to machines as surely as we do to mathematical symbols. And in new material and functional contexts, the meaning of machines can alter as well – we need only look around the laboratory to see a myriad of technologies now performing functions (and carrying meaning) a long way from their site of origin. For this reason, it may be helpful to think of the process by which a computer logic circuit, vacuum tube, or clock mechanism becomes modularized as a form of pidginization. But because this process is not, at least in the first instance, purely linguistic, it is helpful to think of the production and elaboration of such common objects as 'wordless pidgins' and 'wordless creoles.' Wordless grammar corresponds to the rules of combination allowed for these objects – circuit design rules, for example. Similarly, an element in such a wordless interlanguage (should we call it the analogue of a noun?) corresponds to the useful notion advanced by Leigh Star and James Griesemer of the 'boundary object,' an entity participating simultaneously in two or more fields of Inquiry.[4] Here a cautionary note is worth

4 Peter Galison. "The Trading Zone: The Coordination of Action and Belief." Paper presented at TECH-KNOW Workshops on Places of Knowledge, their Technologies and Economies. UCLA Center for Cultural History of Science and Technology. 1989. Susan Leigh Star and James R. Griesemer. "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Verterbrate Zoology. 1907-39." *Social Studies of Science* 19 (1989): 387-420.

sounding. To speak of wordless creoles is not to commit oneself to a position in which concepts can be extricated from language altogether; I mean to emphasize that pieces of scientific objects are often transferred without words.

Throughout this discussion, I intend the pidginization and creolization of scientific language to be treated seriously just the way one would address such issues for language more generally (that is, not as a 'model' or 'metaphor'). First, physicists themselves regularly refer to problems of language in terms of idioms, meaning compatibility, and translation. There are books such as *Computers as a Language of Physics* and physicists characteristically speak about 'putting this current algebra argument into the language of field theory' or ask 'can you express that relation in the language of effective field theory?' Second, the standard account of science in terms of 'conceptual schemes,' 'epistemic breaks,' and 'paradigm shifts' presupposes and relies on talk of meaning change between terms; the argument presented here simply says that the account of linguistic work at the boundary is oversimplified and unhelpful. For example, 'translation' as used by Kuhn to describe scientific change explicitly borrows from notions of translation from ordinary language: "in time," says Kuhn's expositor Paul Hoyningen-Huene, "the members of one group will be able to translate (in the everyday sense of 'translate') portions of their counterparts' theory."[5] Finally, 'model' and 'metaphor' talk presupposes a radical break between scientific and ordinary use of language, and philosophical attempts to enforce such a dichotomy have notoriously foundered. The point I want to make is this: the characterization of different registers (such as jargons, pidgins, and creoles) is helpful in distinguishing between different modes of scientific and non-scientific uses of language. My intention is to expand the notions of interlanguages to include both the discourse of scientific and non scientific utterances, and both material and abstract systems. It is not to 'apply' the results of one field to another.

How literal is the notion of a 'trading zone'? At one level, I have in mind the most literal sorts of spatialized scientific practice. Laboratories, scientific campuses, often reflect architects' and scientists' expectations of intellectual proximity and meeting points in the walls, hallways, and stairwell landings. Conferences, informal meetings, visits, present other transient sites for face-to-face interactions. If one is trying

5 Paul Hoyningen-Huene. *Reconstructing Scientific Revolutions.* Chicago: University of Chicago Press, 1993. 257.

to understand the development of the bubble chamber one would do well to register the circumstances that in the 1950s cryogenic engineers from the Air Force were meeting with staff from Lawrence Berkeley Laboratory. If one is trying to understand the development of computer simulations, it is essential to track the direct encounter of mathematicians, physicists, weapons designers, and statisticians with one another in a series of conferences from UCLA to Endicott, New York. In the end, however, the issue is one of coordination and regularization of different systems of values and practices; if that takes place outside of any spatial configuration, this too is of interest. In a typical colliding beam experiment, the full set of 500 or so experimenters may never be in the same place at the same time. Many will never meet at all. Webs and computer links bind interlocking laboratories and subgroups together and conventions, standards, and procedures are often established with no single point of authority or geographical center. Exchange zones are written into architectures, but the architectures may not be physical and spatial.

As such considerations suggest, there are many avenues opened up by the less restrictive image of substantive as opposed to mathematically thin boundaries between the subcultures of physics. And in the conceptual sphere, the stripped-down view of shared, specific meanings as opposed to total translation between full languages may offer a better vision of how knowledge moves in and across boundaries. Other questions arise as well. One could follow the development of a highly restrictive inter-field into a full-fledged 'creole' as in the gradual articulation of physical chemistry or biochemistry out of highly localized shared techniques.[6] Or one could examine instances in which the interlanguage more or less died off, as did the eighteenth century boundary field of iatromechanics – or, in more recent times, many of the myriad of unification schemes such as those that would (à la Millikan) join cosmic ray physics to the genesis of higher elements in deep space; or Einstein and Weyl's notion that they would be able to unify *directly* electrodynamics with general relativity. As in linguistic boundaries, there is nothing to block the attempt to put languages together, but at the same time there is no guarantee that either social or intellectual coherence will follow. Some pidgins get resorbed back into

6 John W. Servos. *Physical Chemistry from Ostwald to Pauling*. Princeton: Princeton University Press, 1990. Robert E. Kohler. *From Medical Chemistry to Biochemistry*. Cambridge: Cambridge University Press, 1982.

one of the 'parent' languages, some stabilize as pidgins, and some grow into full-fledged creoles, and then ultimately into a language, full stop.

Here I would like to explore a different point: *delocalization*. The problem is this. Over the past years, we have again and again seen how tied specific laboratory practices are to their conditions of origin. An instrument made in one spot is often difficult to replicate in another without the bodily transport of things and people. Paper instructions are often not enough. We have learned from scholars like Harry Collins how necessary it is to attend to the site-specificity of particular materials, skills, and resources available at a given time and place: the little tricks of the trade that made one group of people able to build a certain kind of laser where others could not. Eventually, though, these objects do travel, the lasers, prisms, accelerators, detectors, tubes, and films – hence the problem. If the original production of scientific knowledge is so reflective of local conditions – whether they are craft techniques or religious views, material objects or forms of teamwork, how does *de*localization take place?

One solution, referred to by Simon Schaffer as the 'multiplication of contexts,' accounts for delocalization by a process is which the original context is imposed elsewhere. Methods are devised which "distribute instruments and values which make the world fit for science."[7] One pictures maps of imperial expansion, the dark black arrows of distributed context radiating outward like the footsteps of a conquering army from Oxford, Cambridge, London, or Paris to newly-acquired sites elsewhere. To be replicated, air-pumps required a particular set of machines, facticity, and witnessing to be in place. To be enforced values and methods of standardization stamped their mark on distant sites. Multiplying *these* contexts was a precondition for replication, and the lines of power that designate the creation of those conditions extend from center to periphery. Similarly, Bruno Latour focuses on ways in which the world is modified to make it possible for an instrument, even one as simple as a clock, to "travel very far without ever leaving home."[8]

A different solution, building on the notions of discursive and wordless pidginization within trading zones of limited exchange, would

7 Simon Schaffer. "A Manufactory of Ohms." *Invisible Connections. Instruments, Institutions, and Science*. Ed. Robert Bud and Susan E. Cossens. Bellingham, Washington: SPIE Optical Engineering Press, 1991. 23.

8 Bruno Latour. *Science in Action*. Cambridge: Harvard University Press, 1987. 251.

put greater emphasis on two features of the 'transfer.' First, it would stress the *activity* of interpretation that takes place on the receiving end of the objects, techniques or texts. Varying Latour's apt phrase, scientific practices can also travel to very *different* homes. However imposed the more powerful set of techniques might be, the site of their application fundamentally alters the way those technologies manifest themselves. Many creolists insist, for example, that French creoles can only be understood if one abandons the attempt to view them as merely simplified French, and instead considers the combination of the lexical French structure with a variety of syntactic elements from African languages. Just such a nuanced view is needed in the domains of material and theoretical cultures – pieces of apparatus can circulate without the whole, devices can move without their scientific contexts, functional specifications can move without a trace of their original material form. An abbreviated example: working against low-flying attack fighters, radar engineers during World War II designed a 'memory tube' that would store radar returns and cancel out the signal of any stationary object – leaving only the plane. In travelling to a different context not long afterward, the tube became a recirculating memory to store information for the early computers. Again, cut loose from its 'meaning' the device is then appropriated by particle physicists who want to use it to locate the position of a passing particle (by measuring the time it takes for a particle-induced pulse to reach the end of the tube). We have, in a sense, a hermeneutics of material culture. At every stage of such multiple transfers we need to ask *both*: How does the stripping-down process occur by which local circumstance is removed? And then how does the re-integration into a *new* context take place?

If the first feature pointed to is *activity*, the second is *locality*. Elements of the meaning of a scientific practice are pared away. Theoretical physicists drop many properties that they ascribe to an electron or quark before they bring those notions to the bubble chamber scanning table at which they meet the experimentalist. The embedding of 'electron' in a quantum field theoretical description, or a particular unified theory might be deleted altogether. Conversely, before encountering the theorists, the experimentalists bracket many of the concerns about the nature of the film, the optics and compression pattern of the liquid hydrogen. The tracks being discussed carry, in this interaction, a shared local meaning. Experimentalist and theorist can pore over the scanning table, gesturing at the tracks and arguing interpretations: 'this kaon here splits into two unseen gammas, which then produce these two electron-positron pairs over here.' But such exchanges, though widely shared, are not pieces of a universal protocol language interpretable in *any* epoch of physics. Against the positivists: there is no 'neutral observation language' in the activity of track interpretation – what goes on in the process of sorting out the particle decay scheme does not in any way, shape or form constitute a 'pure' observation to be positions against 'hypothesis.' Against the conceptual-schemers: we do not find a homogenized amalgam in which experimentation and theory become a single, undifferentiated whole in which every experimental statement is utterly fixed by the theory to which it is inseparably attached. The pidginized hybrid of the language of track interpretation has *both* elements of theory and of experiment, while recognizing the self-maintained distinct identity of each. A pidgin is neither a linguistic passe-partout nor a subset 'baby-talk' of a 'full' language; it facilitates complex border interactions. Coordination around specific problems and sites is possible even where globally shared meanings are not.

The history of physics can be profitably seen as a myriad of such productive, heterogeneous confrontations. Field theorists met radio engineers in the American radar laboratories of World War II. British cosmic ray experimentalists encountered colloidal chemists in the production of nuclear emulsions sensitive enough to 'photograph' all known elementary particles. Such heterogeneity continues. One collaboration joining mathematics and physics expertise began a 1980 *Physics Report* by recalling the halcyon days in which Newtonian mathematics and Newtonian physics could develop together, only to separate under the pressures of specialization:

> a formidable language barrier has grown up between the two. It is thus remarkable that several recent developments in theoretical physics have made use of the ideas and results of modern mathematics … The time therefore seems ripe to attempt to break down the language barriers between physics and certain branches of mathematics and to re-establish interdisciplinary communication.[9]

This exchange was bilateral: physical techniques from field theory were solving problems in algebraic geometry, and mathematical tools were at the root of the 'string revolutions' that, for many late-twentieth century physicists, promised a final unification of gravity and the short-range forces that held matter together. When algebraic geometry 'travelled' to the physicists, it was often precisely by *shedding* many of the values with which it was practiced in mathematics departments. To the horror

9 Tohru Eguchi, Peter B. Gilken, and Andrew J. Hanson. "Gravitation, Gauge Theories, and Differential Geometry." *Physics Reports* 66 (1980): 215.

of many mathematicians, 'speculative,' 'intuitive,' and 'physical' argumentation were supplanting the hard-won rigor of mathematics proper. When these practices travelled, some of the mathematically constitutive values associated with them stayed behind.

This observation – that meanings, values and symbols often stay home or switch identities when scientific theories and instruments travel – lies at the heart of the alternative (exchange language) picture of delocalization I have in mind. Donald Glaser, inventor of the bubble chamber, desperately hoped the device would 'save' small-scale physics (and the life that went with it) from the onslaught of Luis Alvarez's factory-laboratory. But substantial portions of Glaser's original device – stripped of those material components that were tied to the small-scale – were reappropriated into the massive chambers that became the very symbol of Big Physics. Alvarez's sector of the Lawrence Berkeley Laboratory. This is not to say that values played an inessential role in the constitution of particle physics at any stage, it is, instead, to say that the particular guiding values altered radically as they shifted from a defence of individual craft-style work to a form of scientific life that emerged from the massive nuclear weapons and radar projects of World War II.

What then is the relation between these two accounts of delocalization – 'multiplication of contexts' and 'exchange language'? Let us return briefly to the relation of algebraic geometry to quantum field theory in string theory. For there the practitioners of both sides were roughly equal stature – such an exchange did not resemble (for example) the relation between technicians in Alvarez's laboratory to the Nobel Prize winning physicists who ran various groups. And perhaps here lies a clue. In examining situations in which the balance of power was maximally unequal – it might well be the case that one group could impose a fuller set of contextualized values along with specific practices on the other. In other words, we might keep in mind that in terms of power relations, there are two interesting limits to the confrontations of languages. At one extreme, the two languages enter into contact in states of roughly equal power. Linguistically, such situations typically result in pidgins in which the lexical admixture of the two languages is markedly heterogeneous. This is of great interest from the scientific standpoint, because we are often faced with situations of this type. String theory, for example puts quantum field theorists on one side and algebraic geometers on the other: a situation in which the balance of power is roughly equal. At the other extreme, in which one group is far more powerful than the other, very different linguistic structures might be expected. For example, in very unequal balance of power, it is common to find that the lexicon emerges overwhelmingly from the superordinate language and a regularized, restricted syntactic structure from the less powerful language. It is also well documented that in very unequal situations, pidgin languages can be reabsorbed back into the superordinate language. Such instances occur in the domain of science and technology. In large-scale collaborative ventures, such as the Manhattan Project, one sees sectors of almost every possible power relation, from the Du Pont (engineer run) effort in Chicago to the Los Almos (scientist run) laboratory.

We are now in a position to understand the relation between the interlanguage and context-multiplying accounts of delocalization. Context multiplication is the limit case of interlanguage coordination just when the power imbalance is so pronounced that the recipients' constitutive values and technical practices were thoroughly subordinated, or where relevant local values did not enter.

Let me end with one final thought. In a sense, the last fifteen years of studies in the history and sociology of science have left us with a powerful set of tools for understanding the local origins of scientific ideas, practices, and methodological precepts. But awkwardly we have grafted to this local description, a picture of language (broadly conceived) that remained global, rigid, and holistic. No wonder we often end with a peculiarly bad set of choices. At one extreme we anchor our notion of science in a global picture of language imagining that moving machines and ideas is automatic, completely ignoring the contextualized circumstances in which scientific work originates. At the other, we imagine that practices are so tied to local circumstances that we either descend into a radical nominalism in which no one is 'really' talking to anyone else, or we wrongly conclude that the full context of origin is packed off, kit and caboodle, to every distant site of application. If, as the anthropological linguists are trying to teach us, meanings don't travel all at once in great conceptual schemes, but rather hesitantly, partially, and nonetheless efficaciously, perhaps, for those studying the development of science, there is an exit from this impasse.

WORKS CITED

Eguchi, Tohru, Peter B. Gilkev, and Andrew J. Hanson. "Gravitation, Gauge Theories, and Differential Geometry." *Physics Reports* 66 (1980): 218-393.

Galison, Peter. "The Trading Zone: The Coordination of Action and Belief." Paper held at TECH-KNOW Workshops on Places of Knowledge, their Technologies and Economies. UCLA Center for Cultural History of Science and Technology, 1989.

Galison, Peter. "Aufbau/Bauhaus: Logical Positivism and Architectural Modernism." *Critical Inquiry* 16 (1990): 709-52.

Galison, Peter. *Image and Logic. A Material Culture of Micropolis*. Chicago: University of Chicago Press, 1997.

Hoyningen-Huene, Paul. *Reconstructing Scientific Revolutions.* Chicago: University of Chicago Press, 1993.

Jaffe, Arthur and Frank Quinn. "'Theoretical Mathematics': Toward a Cultural Synthesis of Mathematics and Theoretical Physics." *Bulletin of the American Mathematical Society* 29. 1-3 (1993): 1-13.

Kohler, Robert E. *From Medical Chemistry to Biochemistry.* Cambridge: Cambridge University Press, 1982.

Latour, Bruno. *Science in Action*. Cambridge: Harvard University Press, 1987.

Leigh Star, Susan and James R. Griesemer. "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39." *Social Studies of Science* 19 (1989): 387-420.

Servos, John W. *Physical Chemistry from Ostwald to Pauling*. Princeton: Princeton University Press, 1990.

Schaffer, Simon. "A Manufactory of Ohms." *Invisible Connections. Instruments, Institutions, and Science.* Ed. Robert Bud and Susan E. Cossens. Bellingham, Washington: SPIE Optical Engineering Press, 1991.