CHAPTER 11

SCIENTIFIC CULTURES

PETER GALISON

WITH IT OR WITHOUT IT: THE CONCEPT OF SCIENTIFIC CULTURE

After Newton, physicists struggled against the concept of force. They hated it. What *was* the force law, $F = ma$, supposed to mean? Did it define force in terms of acceleration and mass? Or were force and acceleration intuitively satisfactory concepts, making the equation define mass? So detested was "force" that a perfectly plausible history of mechanics of the past 300 years could be written as one long battle to annihilate the very idea. Energy would take its place; or if not energy then the minimum laws of Lagrange, the equations of Hamilton, or the brackets of Poisson. There were notions of virtual displacement to supplant force; there were physical explanations of the concept and mathematical ones that seemed to capture its import without its substance. Anything to extirpate this dreadful idea that still stank of alchemy 200 years after Newton tried to clean off his own massive involvement with the Green Lion. And yet, despite all these displacements, substitutions, and buryings, the concept of force guided physics like a sextant across the sea changes of the discipline. Force was there in classical gravitation and mechanics, electrostatics, and electrodynamics; it was still present in the very names "weak force" and "strong nuclear force." In fact, force was just about everywhere in physics with the exception of general relativity and quantum field theory. Force is the worst of ideas and the best, detested, championed, ineliminable—an idea with embarrassing ancestors and enough adored illegitimate offspring to populate the sciences of modernity.[1]

"Culture" is to the interpretive side of anthropology and sociology what "force" has been to physics. We hate it; we cannot do without it.

For much of the nineteenth century, "culture" labeled something absolute, certainly not an entity that was different in different places (though one might have more or less of it). After the First World War, that changed; as Geertz put it: "Instead of just culture as such one had cultures—bounded, coherent, cohesive, and self-standing: social organisms, semiotical crystals, microworlds. Culture was what peoples had and held in common, Greeks or Navajos, Maoris or Puerto Ricans, each its own."[2]

What did Geertz want? I take it he was, throughout his work, rebelling on the one side against universalism, the view that—through a biologistic reductivism, a structural scientism, a sociological functionalism, or a layer-cake picture of human nature—one could obviate local culture. On the other side, he rejected a fragmentary nominalism that strips the world of meaning by refusing to acknowledge the systemic nature of meanings within local cultures.

> We do not know, really, how to handle this, how to deal with a world that is neither divided at the joints into ingredient sections nor a transcendent unity—economic, say, or psychological—obscured by surface contrasts, thin and concocted, and best set aside as inessential distractions.[3]

Against universalism and nominalism, Geertz has defended a going-togetherness of particulars, a finite set of connected meanings. In defending this locally holistic position, he built on a long tradition of interpretive social science, from Vico and Weber through his own studentship with Talcott Parsons. This tradition aimed for plausible cohesiveness, not logical necessity; its ideal was more that of a persuasive interpretation of James Joyce's *Ulysses* in which the elements of the view held together than the axiomatic deduction of Euclid's *Book II*. What Geertz has added to this vision of culture (in my view) was a dramatic expansion of the domain of particulars to be understood: not just rituals, mythologies, and ceremonies as text, but also politics, history, and play. These issues matter for philosophy because philosophy shares these concerns and the debates that eddy around them. Geertz saw Thomas Kuhn as an ally and for good reason—Kuhn aimed to depict science in communities, to make commitments based on exemplars and not on axioms, and to expose the different ways the world was constituted through the paradigms articulated by Aristotle, Newton, or Einstein. These ways of seeking, these Kuhnian paradigms, are about as close as one can come to the scientific analogue of Geertzian, thickly described cultures. For Kuhn, participants in one paradigm pass another as if, as Kuhn liked to say, they lived in different worlds. Kuhn pitted his work against the view that science aimed for a single, unitary truth toward which theories progressed. He opposed universalizing schemes of falsification, verification, or confirmation. In the first instance, he fought against the universalism implied by protocol or observation sentences standing outside all theories and litigating among them. (After a long discussion about Carnap's neo-Kantianism and conventionalism, Kuhn once told me that he was glad that he had misinterpreted Carnap because otherwise he would not have written *the Structure of Scientific Revolutions*.)

Gananath Obeyesekere, in a sense, was the most recent apparition of the opponent Geertz has been sparring with for 40 years: the universalist. This time, however, the defense case for transcultural does not wear the robes of biology, psychology, linguistics, or structuralism, but rather those of a widely shared "practical reason." Practical reason, Obeyesekere has argued, cuts across lands and peoples. There is a universal capacity for reasoned judgments that Obeyesekere calls "practical rationality"; it is a rationality that is not coterminous with being a stakeholder in a particular set of beliefs (he has been perfectly happy to see practical rationality in magic, medicine, or spirit possession). No, it is instead a generalized capability to think in certain ways, to engage in debate arguments and judgment formation. "The notion of practical rationality...I believe," says Obeyesekere, "links us as human beings to our common biological nature and to perceptual and cognitive mechanisms that are products thereof."[4] Geertz demurred; neither this attempt to formulate transcendental rules of comportment nor any of the other extratemporal, extraspatial counters to "cultures" persuaded him. They explained far too little about the Cook-epoch Hawaiians' world (or, so Geertz believed, our world) for that matter. Transcultural practical reason stands in opposition to Geertz's historicized cultures as Carnap's transtheoretical protocol sentences stand outside Kuhn's historicized paradigms. Or put another way: Obeyesekere's practical reason critique of autonomous cultures resembles the many more recent attempts of the science wars to shut down a perceived epistemological relativism by invoking a universal rationality that is not particular to place, time, or practitioner.

The conflicting intuitions behind the cultural specificity of ways of thinking and some form of practical universal reason are both clear—and in their own ways compelling. From Franz Boas to Geertz, there is a powerful desire to impart dignity and coherence to other people's ways of acting and seeing. Among historians, the idea has resonated powerfully, though their "peoples" are more usually less exotic and longer dead. To capture that "otherness" in a persuasive form is what much of the study of science has been after. Not just now in the work of science studies, but for the past decades—this is what Alexandre Koyre wanted to get at in his Newtonian and Galilean studies—the internal compelling logic of other systems of thought.

On the other side, there is an equally powerful desire to show that other peoples in other places and at other times were, at bottom, like us. "Otherness" (in the view of the universalists), far from setting peoples on an equal footing, inevitably relegates those others to a "native" inferiority. And the dignity that is due peoples (read in the present instance: scientists) of the past or in other traditions is best paid by acknowledging the fundamental similarity of ways of thinking, reasoning, and doing.

Boundaries, Thin and Thick

At the root of this divisive, long-running culture debate is, I believe, a dubious picture of sites and boundaries that is shared by both sides. In particular, there seems to lie behind the holistic picture of culture, the notion that it extends over a definite space and population and that there are well-defined and fixed boundaries that pick out its edge. Since this well definedness seems to individuate the cultures one by one, particular "scientific cultures" (Kuhnian paradigm-based communities) pick up metaphysical weight. "Living in different worlds" becomes a typical locution, a move adopted by a certain segment of science studies that accepts the Kuhn picture as phenomenologically accurate and takes it upon itself to ground each "world" on a functionally defined social basis.

Unfortunately, this hermetic, crystalline conception of scientific culture is historically, sociologically, and philosophically problematic. With or without the functional explanation of it, science studies does not need to assume that technical cultures are, each one, so strictly bounded and so purely ordered.

In particular, two problems emerge from the hermetic-crystalline conception of scientific culture. First, the airtightness of the picture makes change impossible except as a sudden and total flip. You cannot bend, stretch, or distend a diamond—you leave it as it is or you cleave it down a fault line. Second, the demand for purity makes the hybridization of scientific cultures impossible to understand. That is, even if we accept (and I do not) the supposedly disjunctive switch from phlogiston chemists to oxygen chemists, the assumption of crystalline ordering fails in any way to account for the emergence of mixed practices—biochemistry, chemical physics, physical chemistry, biophysics, bioinformatics, and nanoscience, to name but a few.

For historians of science, technical work does not typically splinter into sectors of pure and absolute autonomy. The movement in and out of Newtonianism strikes historians as not even approximately one of sudden transitions and long periods of inner stability. Sociologically, the "community" that is supposed to support a particular scientific program or paradigm—and the simplistic functionalism that takes the science to reflect a homogeneous "need" for this or that scientific object or theory—carries little weight among sociologists. Philosophically, even among the work classed as "Newtonian" (or "Einsteinian" or "Darwinian") there has never been a core set of tenets accepted that is common to all. More formally, there is no necessary and sufficient set of conditions that picks out all and only "Daltonian atomists," for example. There has not even been a single exemplar under whose sign all the work was conducted. There were relativity theorists who believed in the ether and those who did not, relativists who subscribed to the substantiality of space–time and those who refused it, and relativists who took cosmology as an example problem solution and those who utterly rejected it.

Considerations like these propelled me toward the study of fluctuating, dynamic boundaries, boundaries with substantive dynamics that not only can generate new disciplinary and argumentative structures, but also can rearrange the disciplines that nominally gave rise to them. This region of linguistic, argumentative, instrumental, and experimental hybridity is what I have called the trading zone. In this zone, things can be exchanged—that is scientific procedures, symbols, and materials can be traded. Local coordination is possible even when there is not the slightest gesture toward global agreement. If I hand you what for me is a salt shaker and you offer me a book, we do not at all have to agree on their full meanings and associations. We just have to agree to the coordination needed for the exchange. Bit by bit, such local agreements can expand. Just like jargons, pidgins, and creoles that mark everyday exchange languages in boundary regions the world over, so scientific groups are constantly forming local systems of exchange, working out ever more elaborate exchange languages, for example, to allow atomic physicists to be able to speak to surface chemists when they want to do something new with the rolling of carbon nanotubes.

Let me give two brief examples of trading zones that, I believe, illustrate both the usefulness of the culture idea *and* the necessity of altering it, so that it is not a structure frozen outside time. First, take the boundary between mathematics and physics. Up through the 1970s, almost all particle physicists took the contemporary discipline of mathematics to be skew to their concerns. Almost all of the mathematical tools needed by quantum field theorists had existed for a half century or so—some elementary group theory, a few bits of the calculus such as Fourier and Laplace transforms, some linear algebra. These are, in fact, pretty simple bits that physics students master in the first two years of college. When the research fields of mathematics and physics touched during those years—in mathematical physics—the physicists took the mathematicians to be little more than a cleanup squad. Mathematical physicists returned the compliment, taking their occupation to be setting to rights and rigor the sloppy moves of their applied colleagues.

Then, for the several decades beginning in the mid-1980s, this mutual mistrust changed. From a sidebar to a central storyline of fundamental physics, string theory climbed to a position high on the prestige heap. Along the way, theorists struck an uneasy, immensely productive accord with the mathematicians. It was an accord that meant that a new field began to grow in the now very thick boundary between math and physics. But even more importantly, the new field then began to reshape the conduct and self-definition of physics even domains away from this widening boundary.

One can trace, in detail, how professional identities, values, demonstration strategies, and explanatory standards—even ways of speaking—have, bit by bit, been cobbled together into a subdiscipline of its own. Differing standards of proof, divisions over the definitions of specific terms, scientific jargons, pidgins, and creoles—welding together a hybrid form of reason was a long and difficult process. But that assemblage was not without cost.

When Ed Witten, string theory's clearly dominant voice, aimed to get a grant for an advanced joint education program combining physics and mathematics, *both* fields struck back. One prestigious referee from the National Science Foundation acknowledged the scientific importance of what Witten and his colleagues had done, but nonetheless shot this reply: "[M]y conscience would not rest if I did not record those doubts here, even though I am fully aware that my opinion is highly contrarian." The referee continued:

> I tend to think that the most conspicuous development of the last decade is the training of a generation of very bright young theorists who know and care more for geometry and topology than for the standard model and current experimental efforts to discover the next step beyond it. Since I am convinced that the key advances in physics emerge from physical rather than mathematical insight, I must view this as a negative development. I think that theoretical physics would be in better shape if this group of very capable people had been taught to practice research with better balance between physical fact and mathematical intuition.[5]

Ultimately, this evaluator's greatest concern was not for the mathematicians but rather for the physicists, especially "young ones," whom the program "would tend to subvert." Mathematicians, the referee continued, were a tool, but one that must be secondary to the concerns of a fundamental physical nature. Proposal rejected. One sees here very vividly why the idea of defending a "scientific culture" is by no means too strong a description—values were at stake and not just results. The training of the young, the tending of right intuitions, the balance of care directed in and out of the group's main concerns, the cultivation of a proper attitude toward data.

A mark of the importance of the new field: physicists were not the only ones to protest. One mathematical response to the unlocking of mirror symmetries (the details not important for our purposes here) and similar string theory successes was a full-tilt emulation by some senior mathematicians of physicists' style of work. It was a route, two math department chairmen cautioned, that was strewn with landmines. Imitation by mathematicians of physicists' habits had

> happened without the evolution of the community norms and standards for behavior which are required to make the new structures stable. Without rapid development and adoption of such "family values" the new relationship between mathematics and physics may well collapse. Physicists will go back to their traditional partners; rigorous mathematicians will be left with a mess to clean up; and mathematicians lured into a more theoretical mode by the physicists' example will be ignored as a result of the backlash.[6]

Here again, the signs of cultural concerns are strewn throughout the passage—it is not just about everyday rationality, it is about "family values" of mathematics and physics (cognitive), "standards of behavior." (With more time, we would do well to pause here at the "family" conceit.)

There is a rejection by physicists and mathematicians who considered themselves "*Pur sang*." But looked at historically, even the "parent" disciplines had themselves moved from hybridity to purity in historical time. Their names speak their biographies: quantum field theory, for example, wears its mixed lineage on its sleeve, coming as it did from an amalgam of turn-of-the-century classical electrodynamics and the nonrelativistic quantum theory of the 1920s. Algebraic geometry, currently perhaps *the* most central field of mathematics, was not too long ago itself an admixture of, as its name suggests, algebra and geometry.

A sign of culture in flux, these expressions of anxiety about disciplinary boundaries signal more than the presence of "scientific culture." They are signs of *changing* values, symbols, and procedures. It is a moment, a crucial moment, that is captured neither by calling in some protocol language nor by invoking universal practical reason. At the same time, we do no better to invoke the stability, boundedness, and autonomy of self-standing "microworlds." These are scientific worlds in transition, borrowing, exchanging, and mutating. I do not mind at all the use of "scientific cultures," but these cultures cannot be described, even approximately, as infinitely malleable unit ideas, utterances, or practices. Nor are they well captured by the pluriholism of crystalline blocks delineated from each other by zero-width boundaries.

Dynamic, thick boundaries are important not only in the gestation of new fields (like string theory) but also to reconstruct even more established domains, like physics, where technical, philosophical, and scientific cultures cross. There is an old problem that had been bothering me for many, many years—it is this: from 1902–1909, Albert Einstein worked in a patent office on electromechanical devices—precisely the time of his most intense physics work. Were the two kinds of activities on coils and currents merely puns of one another—worlds apart, so to speak?

A full account would obviously be much more complex, but schematically there are three scientific-technical cultures in play. First, both Poincaré and Einstein's workplaces were involved in technical work that bore directly on the coordination of time. Poincaré was a lead scientist in and by 1899 in charge of the French Bureau of Longitude. The bureau's principal job was to set clocks around the world (in place X) to be in sync with Paris, so that the Paris–X longitude difference could be calculated. It was a task both crucial to the administrative needs of empire (through the making of maps) and vital to the symbolic play for dominance among the Great Powers. Einstein at the Swiss Patent Office found himself in the midst of a surge in devices to send coordinated time down the railway tracks—so that passengers could coordinate their connections and trains could synchronize their watches well enough to coordinate traffic and avoid catastrophic collisions.

Second, both Poincaré and Einstein were powerfully drawn to scientific philosophy: Poincaré participated in and published in this nascent field along with a cluster of former polytechnicians; Einstein and his friends met together as the ambitiously named but modestly provisioned "Olympia Academy" (three guys, cheese, wine) where they analyzed many texts at the boundary between philosophy and science (including Poincaré's). Both scientists, the established Poincaré and the young Turk Einstein, were fascinated with a new scientific-philosophical account of the nature of time.

Third, both Poincaré and Einstein had plunged deeply into the physics of the electrodynamics of moving bodies. Since the day of James Clerk Maxwell, and even before, physicists had assumed that electric and magnetic forces existed in a universal bath of a scarce and difficult-to-perceive ether. How, both Poincaré and Einstein wanted to know, should one analyze the behavior of electric and magnetic fields when one is moving in that all-pervasive ether? Out of this brew of concerns—from the electrodynamic, the practical technological, and the philosophical—emerged a new concept of time.

Take Poincaré (the Einstein story carries some important structural parallels). In 1898, in an article about the philosophical foundations of time, he brought to bear the standard longitude-finder technique and concluded that simultaneity ought be *defined* this way: two events are simultaneous if they occur at the same time as measured by two distant, identical, and coordinated clocks borne by longitude finders. Coordination was to be done as the longitude finders, in fact, did it everyday in their far-flung work: by sending an electrical signal from one clock to another, taking into account the time the signal took to arrive. So if finder X sent finder Y a signal, and X's clock struck noon and the signal took a second to arrive at Y, then Y would set her clock at noon plus one second. Philosophically, the new concept was powerful: it offered the possibility of transforming the very idea of simultaneity from a metaphysical notion ("mathematical, true, absolute") to one that depended in its very core on a *procedure*, a protocol, that could be laid out with rulers, clocks, and signals. Practically (for longitude finders) the simultaneity procedure remained a tool—an essential tool deployed every day—but a tool nonetheless. But as of 1898, nothing in Poincaré's work indicated that he imagined the new time concept to demand changes in physics per se.

The exceptionalism of physics ended just a few years later when Poincaré realized that the signal-and-clock definition of simultaneity could make sense of recent work in the electrodynamics of moving bodies. Over the years that followed, Poincaré shuttled between these different sites: the simultaneity procedure fitted into conferences and publications on cartography, electrodynamics, and philosophy. To the philosophers, the reformed simultaneity became an exemplar of a concept grounded in procedure. To physicists, after Einstein, it became a fundamental starting point for relativity theory. And to the practical folks, the pith-helmeted explorer soldiers who telegraphed time from the Andes to Paris and back, the procedure was their bread and butter.

This convention, this concept—simultaneity grounded in signal-based clock coordination—gives us a very concrete example of scientific exchange. Here is a simple procedure passed between domains that signified quite differently in each of its contexts. Was clock coordination *essentially* a piece of longitude finding, a piece of physics, or a piece of philosophy? The question is unacceptable. It was all three. In the heterogeneous cooperation that marks so much of scientific work, devices, procedures, and equations are mobile—but not infinitely so. Clock-coordinated simultaneity "worked" in this particular triple intersection. Would it have worked similarly in Newton's Cambridge back in the 1660s? Of course not. Einstein–Poincaré clock coordination would have corresponded not at all to a world without telegraphs, without the articulated problem of electrodynamics, and without a philosophy in motion toward the sciences.

This small but significant example of procedural simultaneity illustrates again how a scientific utterance, a procedure, can be finitely local—that is neither infinitely transportable nor locked in one microworld crystal. It shows how Geertzian coherence *does* help us understand how things fit together and, at the same time, pushes us toward a world of *finite* exchange, hybridity, and fragmentation.

Conclusion: Finite Exchange

The concept of a scientific culture can continue to serve us well—if we bear in mind its limits. Its single best function is to remind us insistently of the importance of interrelationships among practices and meanings, of the ways in which meanings, values, and symbols hold together in particular sites. Scientific culture leads us away from a fragmentary universalism, away from the ever-receding promise of a *characterstica universalis* that will unlock knowledge everywhere and for all time. But the pitfalls of the culture concept are equally clear; it is easy enough to reify a set of practices into something indelible, inevitable, to render a scientific subculture into a sealed microworld marked by fixity, holism, and cohesiveness and isolated from everything and everyone else by a mathematically thin but poreless boundary.

To treat science as made up of such island empires is to exclude *in advance* some of the most interesting features of science in its historical development—and present condition. It is not hyperbolic to say that all of the most productive domains of science today—domains as heterogeneous as nanotechnology, string theory, and bioinformatics—are fields where the boundaries between the older disciplines are thick and the whole is changing at an astonishing rate. To find the tools to handle science in flux is not the matter of a moment—perhaps it is not surprising that no single metaphor will do. Here might be the start of a series that could be of use: instead of limiting our imaginative space to heaps of sand *versus* ordered, isolated crystals, we might reach deeper into the materials that have more recently proven of world-changing importance. Within any electronic device are bits of silicon—not in its pure crystal state, but in that condition that the condensed-matter physicists know as "amorphous semiconductors." These have two characteristics: *locally* they exhibit very strong order, but as one goes further from any particular spot, the ordering breaks down—a distant atom is *not* positioned as one expects (from crystal-like reasoning) relative to an atom nearby.

Perhaps scientific cultures are a bit like this. At any given disciplinary location, the world does indeed look quite Geertzian. Concepts, values, symbols, and meanings hold together. They are not mere assemblages. But as one moves further away in the space of practices, conformity to the structure breaks down. All sorts of exchanges are constantly in motion; purity is post hoc dream.

Now, as a very old proverb has it, no metaphor ever walks on all four legs, and this one is no exception. Order parameters are very simple in physical space—compared with the immense complexity of the space of symbols and meanings that we have in view as we confront human activity. But perhaps these starting ideas, ideas of exchange languages, trading zones, and local order might prove of use.

Having learned much from the anthropology of Clifford Geertz, the study of science might pay back, in some small measure a debt long due. If it can, that would be a great pleasure. I miss Cliff. I wish he were here.

NOTES

1. Early versions of this essay were presented as a comment on Clifford Geertz's *Available Light* (unpublished), APA, San Francisco, March 30, 2001 and at the American Sociological Association, August 17, 2003.
2. Clifford Geertz, *Available Light* (Princeton, NJ: Princeton University Press, reprinted 2001), pp. 248–49.
3. Ibid., p. 250.
4 Gananath Obeyesekere, *The Apotheosis of Captain Cook: European Mythmaking in the Pacific* (Princeton, NJ: Princeton University Press, 1997), pp. 20–21.
5. Referee Reports for NSF DMS 9505939. For more extensive discussion of the string example, see Peter Galison, "Mirror Symmetry" in M. Norton Wise, ed., *Growing Explanations* (Durham, NC: Duke University Press, 2004), pp. 23–64.
6. Jaffe and Quinn, A theoretical mathematics. *Bulletin of the American Mathematical Society*, Volume 29, Number 1, July 1993, pp. 1–13.