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CULTURAL CHOREOGRAPHIES OF SCIENCE

Sharon Traweek

One of the goals of this issue, I was told, was to "coordinate views about the current state of the contests between social rationality and scientific rationality." That seems to me completely laudable, if a bit difficult, due in particular to that pesky verb *coordinate*. The word brings to my mind Descartes's orthogonal measuring lines and the social niceties in the late 1950s of getting the colors of one's clothes aligned or even getting the lines of our high school drill team perfectly straight. It seems to emphasize the disciplined side of life. I was reading a lot of papers just now and trying to discipline my thoughts into classifying student work under five of the first six letters of the alphabet and then inscribing my judgment into little boxes with the correct marking implement. As you can probably tell, I am keener on the other goal of the issue: "Drawing attention to the cultural prejudices inscribed in the very epistemology of scientific inquiry." However, I would be much happier if we could drop the word *prejudices* and replace it with something less prejudicial.

I prefer to draw attention to the cultural choreographies embodied in scientific inquiries. In *Choreographing History*, edited by Susan Foster, several of us wrote about how social, intellectual, political, scientific, economic, art, and cultural histories are enacted and performed, produced and consumed by human bodies moving through specific places.[1] Of course, these embodied actors perform their moves in ways that they and others around them understand. Their movements might be rigid or fluid, formulaic or inventive, but they are enacted in the context of cultural codes that make them decipherable to most everyone around them, just as most of the readers of *Social Text* could probably navigate the pedestrian traffic at midday on the sidewalks of big cities. We know our way around gatherings of the sort of people who read *Social Text*. We know the gestures, the tones of voices, the styles, the rhythms, the jokes, the texts, the details that make a difference. I think we should know more of the moves made by scientists, engineers, and physicians as they get around their worlds.

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Ex Cathedra Voices

Many would argue that Thomas Kuhn's *Structure of Scientific Revolutions*, published more than thirty years ago (in 1962), launched the new empirical researches into the practices of scientists. (Certainly there were several others who were building similar arguments against the older notions about scientific research.)[2] Nonetheless, Kuhn's work did not, in fact, disrupt the familiar litanies about science. Until the late 1970s, most historical, sociological, and philosophical investigations about science, technology, and medicine continued to assume and celebrate but did not investigate the notion that scientists had invented a perfect way of knowing, quite free of all human constraints.

I want to recapitulate, perhaps too tersely, a few of those older notions we usually first encountered in the pages of high school and undergraduate textbooks, or perhaps in museums and on television, that often survive in our minds.

⋆ Until Galileo invented experimental research, almost all important discussions about the phenomenal world were conducted as theoretical debates.

⋆ Galileo's ideas were rejected by the Vatican because they challenged Catholic religious beliefs of the time.

⋆ Francis Bacon developed the idea of a laboratory and codified the procedure for research now called the scientific method.

⋆ The printing press made possible the accurate reproduction and circulation of experimental data.

⋆ Isaac Newton invented the idea and the means of using mathematics to analyze experimental data.

⋆ Scientific method is based upon skepticism.

⋆ Scientific method identifies and controls all variables in an experiment.

⋆ Scientific analysis is mathematical analysis.

⋆ Scientific knowledge is amassed progressively and cumulatively.

⋆ Scientific theories and data are rejected when subsequent efforts at replication fail.

⋆ New scientific theories are accepted because they explain more experimental data more economically than their predecessors.

⋆ Scientific thinking and methods are incompatible with religious thought and feeling.

⋆ Scientific reasoning proceeds by deduction and induction; hypotheses are deduced from existing experimental data and experimental data are tested against hypotheses inductively.

⋆ Scientific research is made objective by eliminating all biases and emotions of the researchers.

* Scientific research is neutral with respect to social, political, economic, ethical, and emotional concerns.

* Scientific research has an internal intellectual logic; there is an external social, political, economic, and cultural context for science that can only affect which scientific ideas are funded or applied.

* Improvements in the quality of human life and the duration of human life during the past two hundred years are due primarily to the application of scientific discoveries.

* Technology is applied science.

* Basic research and applied research are easily differentiated.

* There is a significant rate of "social return" on scientific research.

In their most rudimentary telling, our traditional notions about science are usually recited in the cultural forms developed by the medieval European Catholic Church: a list of saints' (geniuses') lives, their miracles (discoveries), and holy sites (laboratories). These reverential stories can be found easily on television (especially the Discovery Channel and public broadcasting stations), invoked as documentaries on science and technology, complete with authoritative male voice-over, offering instructive and amusing examples of Derrida's notion of "absent presences."[3]

New Voices

All of these conventional ideas about science have been very powerfully challenged during the last thirty years of scholarship by anthropologists, economists, historians, sociologists, philosophers, and others.[4] To continue in these beliefs is to signal that one is unfamilar with the massive body of scholarship that has undermined them. The main way in which these ideas are still taken seriously in today's research is to inquire as to the conditions for the circulation of such beliefs in certain specific cultural political economies. Oddly, the people I find most attached to this set of beliefs are faculty in the humanities and students in undergraduate science and engineering courses. That would only be a curiosity if there weren't such serious consequences: those old ideas make it very hard to think carefully about sciences and technologies and their practitioners and consumers.

My own exposure to the notion that these ideas were inappropriate descriptions of scientific work came from scientists engaged in what later became Nobel Prize–winning work. Excellent scientific, engineering, and medical work is being conducted that can in no way be described with the old litanies. When asked why the old ideas are still so widely circulated, these scientists reply that what they really do is too complicated to explain to the public, students, and even lesser scientists. Besides, they muse, what scientist would want to stop doing research long enough to write about stuff like that? (They are all demure when I ask if they might be benefiting from the extramural circulation of those litanies.)

Research on these human activities called science, technology, and medicine has changed from hagiographies (lives of "geniuses") and lists of miracles ("great discoveries" and "inventions") to careful scrutiny of the practices of those who engage in such work. The research is conducted for many reasons: to improve scientific, technical, and medical education and research; to improve policies concerning the funding and application of scientific, technical, and medical research; to organize more effectively scientific, technical, and medical institutions; to understand ways of producing knowledge that is efficacious; to understand the social and cultural organization of the production, distribution, and consumption of knowledge, locally and transnationally, and so on. This research has been conducted in many countries using a very wide range of inquiry modes and analytic strategies. The primary sites for this research include North America, northern Europe, the U.K., Scandinavia, India, Japan, Australia, and Brazil.

Of course, some of the research has been conducted by people who are hostile to science or ignorant about science, just as some anthropological fieldwork or historical archival work or literary interpretation is conducted by researchers who feel hostility to their subjects of inquiry, and some fieldwork and archival work and cultural interpretation is conducted by people who do not learn much about certain crucial practices of the people they study. This work is usually not too interesting to other researchers. In science, technology, and medical studies as a whole, I would hazard as a guess that 50 percent of the researchers have bachelor's, master's, doctorate, and/or medical degrees in science, engineering, or mathematics. Since many of the readers of this journal probably are involved in bestowing these degrees, we know (approximately) what they do and do not mean. In anthropological fieldwork, we often are not already initiated/socialized/experts in the practices we set out to study; in fact, we often think that this would make it difficult to study the shared assumptions and practices of the group under study. That, of course, does not mean that we remain ignorant about the knowledges regarded as crucial in these communities.

There are, by now, some very widely accepted "findings" of thirty years of research on scientific, engineering, and medical practices.

⋆ There are many practices called "science" by their practitioners, not one such practice; there are many methods called "scientific method" by their practitioners, not one such method. That is, each research subfield has its own distinctive research practices. Hence, the proper terms are plural: *sciences* and *scientific methods*.

⋆ The forms used in scientific writing have converged and have not varied significantly over the last couple of centuries. For example, all references to the agency of the scientists involved in the research is minimized. The written presentation of findings have become quite stylized and terse; it would be almost impossible to reproduce an experiment based upon the information provided in scientific articles. I strongly doubt that an article that fully discloses the complete process of conducting an actual experiment or even a "thought experiment" would be published in any field. The purpose of publishing scientific articles is to announce findings and to lay claim to a discovery, and for that purpose a succinct and formulaic literary economy suffices. In some fields the writing of scientific articles is often assigned to the person in the research group with the least status; the power of the claim is not established by a distinctive or original way of writing. In fact, claims are made in a formulaic mode.

* Access to scientific knowledge is highly restricted. That is, there is restricted access to different stages of training; to funding, positions, publication, conferences—the whole infrastructure of knowledge production/consumption; to networks of active researchers; to tacit knowledge—the crucial craft knowledge that is never written into articles but without which one could never really understand or reproduce an experiment; to the groups that define present and future priorities for problems, methods, research equipment; and to the process of establishing and revising reputations of researchers. There are different levels of access: that is, one can get access to certain levels of training, but not others, get no access, get full access and yet accomplish nothing significant, gain access to research sites, but only as a helper, and so on.

* Problem selection is a process highly subject to the available resources.

* Experimental equipment constitutes signals; scientists adjudicate whether those signals correspond to significant information about the phenomenal world.

* The more capital intensive the research process the less likely the research community is to endorse funding research that replicates other experiments. In the most expensive research there is no replication; in such fields data instead are corroborated. Similar data generated by very differently designed experiments with very different forms of data analysis are taken as especially corroborating.

* Adjudicating which experimental data to take as facts and which theories to take as important is a collective process conducted by those who are tacitly empowered with the authority to participate; it does not include all practicing scientists in the particular field.

* Closure of debates about the status of data and theories is not accomplished with definitive findings as to their truth status, but with a consensus that certain data and/or theories are more useful to more of the practitioners who are entitled to participate in the debate.

Discussion about scientific, technological, and medical practices unfortunately have relied too often on formulaic, if cherished, general statements about what science is or isn't.

* The forms of reasoning conducted in research communities as they interpret the signals from their research equipment recapitulate all the known forms of human reasoning.

* Mathematical analysis is a very limited aspect of research. For example, in my fieldwork during more than twenty years among particle physicists engaged in experimental work, the practitioners usually report that they engage in mathematical analysis an average of about three hours per month. By contrast, there is a great deal of time spent in accurate enumeration and measurement.

* For a few centuries scientific arguments have been probabilistic, not causal; some would say that since calculus became widely used among scientists, their mathematical analyses have been approximations.

* Being conducted and constructed by groups of human beings, scientific, technological, and medical practices and ideas are necessarily social and human. Because those practices and ideas are about the phenomenal world, they often, but not always, also require an engagement with that world. What constitutes a satisfactory engagement with the phenomenal world is necessarily open to debate among the practitioners.

* The definition of science is made by those who are empowered to offer resources for work they consider scientific; for example, the work funded by the NSF, SSRC, NIH, or NIMH is science.

There is more, but this list is already far too long. I merely wanted to point out what has already been asserted, debated, and widely adjudicated to be the case about scientific, engineering, and medical practices by researchers in the fields of sci/tech/med studies. Do some people still disagree with some of these findings? Sure. Does that change the fact that most researchers take these statements as a sort of boring baseline of shared knowledge in the field? No.

Did I rehearse these lists in order to open debate on them here or to provide you with bibliographic essays about any of them? No. Nor was my goal in rehearsing all this to urge us to spend a lot of time trying to get our beliefs about the old litanies in alignment. Discussion about scientific, technological, and medical practices unfortunately have relied too often on formulaic, if cherished, general statements about what "science" is or isn't. So why is it that so many people have such turgid notions about science, engineering, and medicine, often spoken with either an ex cathedra voice or a pounding clenched-fist-in-the-face voice? That is the big, interesting question, appropriate for cultural, psychological, historical, political, feminist, economic, anthropological, and social research. At the moment I want my terse list of what a generation of researchers around the world has learned about those kinds of theories about science to help us get beyond the old scripts. I also hope that the second list will open another sort of discussion.

I am eager to see us discuss our different modes of inquiry, our different modes of producing ideas, our different modes of adjudicating, our different modes of training, our different modes of problem selection, our different modes of writing, our different ways of making sense, and whether this diversity is interesting. I would like for us to teach this. Certainly, some of us sometimes like to think with coordinates, means, norms, lines, boxes, parts, and categories. I know as many people in the humanities and arts who think like that as I do in the sciences, engineering, and medicine. In fact, we all realize that this mode of thought has quite a history in human affairs. The orders of Le Notre's gardens, the periodic table of elements, St. Benedict's guide for governing monasteries, Kyoto's grid plan, and Descartes's arguments have some obvious parallels. They are spatial and temporal orders; they create grand views and privileged sites; they create remote sites and borders; they create insides and outsides. Whatever doesn't fit into the grid contributes to disorder, to mess. The law of the excluded middle prevails.

Of course, there are some variants within this mode of thought. First, there are dyads, triads, and quadrants. Just itemizing some crucial instances could keep us all busy for a semester. For openers, here are the usual dyads: objective/subjective, reason/emotion, positive/negative, and good/evil. Then the triads chime in: thesis/antithesis/synthesis, father/son/holy ghost, Lévi-Strauss's triangles, and induction/deduction/abduction. Booming quads are coming to the fore: north/east/south/west, Cartesian coordinates, not to mention the little analytic boxes built by Mary Douglas, A. J. Greimas, and Jürgen Habermas. Then there are the great charts of hierarchies with bifurcations and branches. Linnaeus gave us great diagrams of that line of thinking, as did Darwin, the kinship theorists, and the decision modelers. Of course, the dyads, triads, quads, and decision trees all have their analogs in poetry, music, painting, sculpture, architecture, dance, and prose. I think it can be great fun to run any old idea through the gamut of twosies, threesies, and foursies, with a finale of Busby Berkeley–style ascending and descending of hierarchical steps.

A singular focus on simplicity, stability, uniformity, taxonomy, regularity, and hierarchy can, of course, be limiting. Furthermore, every way of making sense has its cognate forms of obsession. Certainly, there is an aesthetics of purification that can linger over the ways of the mind and body I described in the last paragraph. Swirling around with Occam's razor, slicing away what cannot be categorized, leaves more than order behind. At this point some of my students always say: "What else is there?" I am always fascinated that they have not been taught any other language for thinking carefully.

Well, what else is there? To begin, I left out the ones: the singularities and the universals. Just naming them begins to reveal one of our problems. Just how did we get to believing in those peculiar singular generics: science, man, woman, state, justice, evil, god, love, truth, beauty, logic? Why is it, in our time, in our country, in our academies, considered so very blasphemous to add an *s* to those words? Why is it so horrifying to suggest that we might think more interestingly, and perhaps more carefully, if we stopped, just for a while, using any singular generics. Just saying this in a seminar once led to a philosopher announcing that in the future he would refuse to be in the same room with me.

On another occasion I was asked to be the discussant for a presentation by an eminent anthropologist who is sometimes called a postmodernist. I sometimes like his work very much and took care in preparing my comments. As I read and re-read his paper I thought something unusual was going on in his argument. Using some notation left in my head from mathematical logic courses I began to map his ideas; then I tried using some symbols from philosophical logic classes. After that I explored some techniques of rhetorical analysis I had learned in graduate school. All three strategies gave me some insights but did not locate my vague sense of unease. Then I used a device I had hit on in graduate school. I slowly went through the piece putting a little line through every singular generic and I found the problem. The generics were not randomly distributed; they clustered and they disappeared. They piled up whenever he tried to address certain topics about women. Those topics made his argument disorderly, and singular generics were waved over the mess like a protective fetish. Naively, I suggested that he think about not using any singular generics; I thought it would lead him to strengthen an otherwise very interesting paper. He was not amused.

So what is so sacred about the singular generics and what is so outrageous about wanting to defer them, even if just for a moment? I got a clue in Japan. In Japanese there are no definite and indefinite articles, no *a* and *the* to differentiate *a cat* from *the cat*, and without them one cannot differentiate the singular generic *cat* either. There is no way to distinguish *state* from *a state* or from *the state*; *mass* is the same as *a mass* and *the mass*. As I read the drafts of scientific papers written in English by Japanese physicists I needed to explain to them why their uses of *a*, *the*, and neither *a*/nor *the* seemed inappropriate to me. I could not remember the grammatical explanations for the terms, so I was left trying to explain the differences for physics, but I knew that the Japanese had done perfectly interesting physics for a century without recourse to the singular generic, the indefinite article, and the definite article. Obviously, it was not necessary for science.

All that led me to think about what I could remember about the same distinctions in Latin, Italian, and French, languages that in different decades of my life I have once had some skill in reading. I believed that the grammatical rules about these distinctions are not the same in those languages, but I was not certain. And yet when I returned to the United States, I found people just as annoyed with me for making my plea that we skip the singular generics, that we "just say no" to beauty, truth, logic, science, man, woman, state, justice, evil, love, violence, sex, music, art, poetry. . . . Maybe William Blake was right that god(s) and all the rest are in the particulars.

Monotheism and Morphing

What links cultural, gender, and social studies of science, technology, and medicine of the last thirty years? Certainly, there has been no singular theory, method, or way of defining our questions. What can the empiricists, nominalists, postmodernists, feminist epistemologists, actor-network-theorists, post-Althusser/post-Gramscian Marxists, systems analysts, chaos theorists, discourse analysts, ethnomethodologists, postcolonialists, constructivists, and so forth, among us possibly have in common? Collectively (even though most of us would decry being thrown together for even a moment), that whole generation of research can definitely be said to have dislodged the notion of singularity about science and technology, not to even mention their difference. That is the sin we have committed together.

The other singularities have gone too. European arts no longer set the world standard; the "human condition" is no longer defined in Europe or North America alone. Beauty, truth, and logic have multiplied and dispersed. Some have just begun to notice that the same has happened with science. What is the name of that obsession for singularity and unity, for an order that does not divide, for a world of symbiotic union, for a world that begins and ends with an indissoluble ego? What is the name of the rage against a world of particular plurals? Is it like the rage that some felt against a heliocentric universe or the rage that others felt against a Darwinian world? Why should there be only one way to think well, only one way to have fun with our minds? Why is mental monogamy required? Are we still fighting about monotheism, Manichaean fallacies, and Albigensian heresies?

Does thinking without singularities mean we cannot think carefully about ourselves, other human beings, and our phenomenal world? Not only are we doing it, we already know how to be playful and graceful as we think, dance, and sing about ourselves, the other humans, and our world. What is the name of some of these other ways of making sense? I always look to the students in artificial intelligence, graphics, and music to answer that one: I know that they can already talk powerfully about complexity, composition, instabilities, variations, transformations, irregularities, patterns, morphing, and diversity in performance, research design, equipment design, images, software design, scores, and data analysis. They feel these approaches are just as aesthetically and intellectually compelling as some of those physics students do about equilibrium. The law of the excluded middle isn't always interesting and it doesn't always hold, especially in the best compositions. There are new ways to think within and about our sciences and technologies. Let's dance.

Notes

1. During the last fifteen years there has been a great deal of research on the mutual production of bodies and cultures. See, for example, the bibliographies in Susan Foster, ed., *Choreographing History* (Bloomington: Indiana University Press, 1995), including that for my "Bodies of Evidence: Law and Order, Sexy Machines, and the Erotics of Fieldwork among Physicists." The following is a tiny sampling of these cultural studies of embodied actors: Jean Comaroff, *Body of Power, Spirit of Resistance: The Culture and History of a South African People* (Chicago: University of Chicago Press, 1985); Robbie Davis-Floyd, *Birth as an American Rite of Passage* (Berkeley: University of California Press, 1992); Julia Epstein and Kristina Straub, eds., *Body Guards: The Cultural Politics of Gender Ambiguity* (New York: Routledge, 1991); Brigitte Jordan, "Technology and the Social Distribution of Knowledge," in *Anthropology and Primary Health Care*, ed. J. Coreil and D. Mull (Boulder, Colo.: Westview, 1990); Emily Martin, *The Woman in the Body: A Cultural Analysis of Reproduction* (Boston: Beacon, 1987); Sherry Ortner and Harriet Whitehead, eds., *Sexual Meanings: The Cultural Construction of Gender and Sexuality* (Cambridge: Cambridge University Press, 1981); *Representations*, special issue on "The Cultural Display of the Body" (no. 17, winter 1987); Peter C. Reynolds, *Stealing Fire: The Atomic Bomb as Symbolic Body* (Palo Alto, Calif.: Iconic Anthropology, 1991); Elaine Scarry, *The Body in Pain: The Making and Unmaking of the World* (New York: Oxford University Press, 1985); Michael Taussig, *Mimesis and Alterity: A Particular History of the Senses* (New York: Routledge, 1992); *Women and Performance: A Journal of Feminist Theory*, special issue on "Feminist Ethnography and Performance" (vol. 5, no. 1, 1990).

2. For the best-known positions see Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Chicago University Press, 1962), and *The Essential Tension* (Chicago: Chicago University Press, 1977); Karl Popper, *The Logic of Scientific Discovery*, 3d rev. ed. (London: Hutchinson, 1968). For summaries and statements of the other positions see Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970); and Ernan McMullin, ed., *Social Dimensions of Science* (Notre Dame, Ind.: University of Notre Dame Press, 1992). Some of the other participants in the debates of the time include, in addition to the work covered in Lakatos and Musgrave and in McMullin, Paul Feyerabend, *Against Method* (New York: New Left

Books, 1975); Norbert R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958); Gerald Holton, *The Scientific Imagination* (Cambridge: Cambridge University Press, 1978); Robert Merton, *Social Theory and Social Structure* (New York: Free Press, 1957); Norman Storer, ed., *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago: Chicago University Press, 1973); and Derek J. de Solla Price, *Big Science, Little Science* (New York: Columbia University Press, 1963).

For an introduction to earlier debates about the production of scientific knowledge, see Gaston Bachelard, *La formation de l'esprit scientifique: Contribution à une psychanalyse de la connaissance objective* (Paris: Librarie Philosophique J. Vrin, 1980); Pierre Duhem, *La chimie; est-elle une science francaise?* (Paris: Hermann, 1916) and *German Science: Some Reflections on German Science: German Science and German Virtues*, trans. John Lyon (La Salle, Ill.: Open Court, 1991); Georges Canguilhem, *A Vital Rationalist: Selected Writings from Georges Canguilhem*, ed. François Delaporte (Cambridge, Mass.: MIT Press, 1994); and Ludwig Fleck, *Genesis and Development of a Scientific Fact* (Chicago: Chicago University Press, 1979). Some would add Emile Durkheim's reflections on "Science as a Vocation," in his *Selected Writings*, ed. and trans. Anthony Giddens (Cambridge: Cambridge University Press, 1972), although in my opinion it only invokes the conventional litanies.

For a compilation of the kind of pragmatic research about science, technology, and medicine during the 1950s, 1960s, and first half of the 1970s, see Ina Spiegel-Rosing and Derek de Solla Price, eds., *Science, Technology, and Society: A Cross-Disciplinary Perspective* (Thousand Oaks, Calif.: Sage, 1977).

For an introduction to the cultural, social, and gender studies of science, technology, and medicine developed since the mid-1970s, see Traweek, "An Introduction to Cultural, Gender, and Social Studies of Science and Technology," *Journal of Culture, Medicine, and Psychiatry* 17 (1993): 3–25, in a special issue on "Biopolitics: The Anthropology of the New Genetics and Immunology, edited by Deborah Heath and Paul Rabinow. See also Stanley Aronowitz, ed., *Technoscience, Power, and Cyberculture: Implications and Strategies* (New York: Routledge, in press); Laura Nader, ed., *Naked Science: Anthropological Inquiry into Boundaries, Power, and Knowledge* (New York: Routledge, in press); Constance Penley and Andrew Ross, eds., *Technoculture* (New York: Routledge, 1991); and Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992).

3. Jacques Derrida, *Of Grammatology* (Baltimore, Md.: Johns Hopkins University Press, 1976).

4. See note 2.