FOREWORD

Four Riddles of Physics and Epistemology

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Arpita Roy provides the latest of a series of ethnographic ventures into high-energy physics, in this case primarily at CERN. Coming in part from mentorship of J. P. S. Uberoi, the eminent anthropologist-philosopher at the Delhi School of Economics, she attempts to revisit a number of old and new riddles that physics poses for general epistemology, trying to get beyond the presuppositions with which we conventionally talk about physics and nature. Earlier variants of this desire have been provided by the juxtapositions of cultures with other presuppositions or with other traditions of scientific discovery, whether in anthropology or in the history of science (as in Uberoi's rereading of Goethe's science as a counter tradition in Western and global science). Roy attempts to do this more rigorously through living with and debating physicists as they try to explain their work.

Chapters 3 through 5 provide fascinating case studies. Chapter 3 is about left- and right-handedness (of the body, of the spin of particles), an old anthropological topic since the studies of Robert Hertz in the overlapping terrains of what are nature, culture, natural symbols, symmetry, the limits of topological transformations, and perhaps the challenges of viewing biochemical

cascades in multidimensional space. Chapter 4 is about the 2008 explosion in the Large Hadron Collider, which, in its forensics and reconstructions, provides exposure of the materialities of highly speculative science experiments. It is an old verity in engineering that one often learns as much from the failures of systems as from when they work according to plan; in any case, failures help demystify the shiny surfaces and secrets of complexity, revealing the nature of work and imagination. Chapter 5 is about CERN's efforts to allow access to artists-in-residence. This is no longer as novel an experiment as it seemed some thirty years ago, and there are now many art-science projects, which, when most productive, are not just metaphorical publicity to support the arcane mysteries of technical knowledge for the general public but are also experimental spaces where some of the constraints of science can be relaxed to see what happens and if, thereby, new insights can spark the scientific imagination.

The book begins in chapters 1 and 2 with the excitement surrounding the discovery of the Higgs boson in 2012, a long-anticipated international quest of theoretical collaborations in international physics, and the building of such transnational experimental machines as the Large Hadron Collider, which are beyond the means of national governments but are nonetheless matters of competition to provide centers of physics activities. CERN was part of the post–World War II effort to rebuild Europe's scientific infrastructures and political establishments. The Higgs boson, like many of physics' high energy particles, is a weird object: although evidence of Higgs bosons have been "found" or "produced," Roy tells us, they can never be directly observed in particle detectors that produce the evidence for them, partly because they decay so rapidly into two photons and partly because they are hard to differentiate from background photons.

Readers will bring to chapters 1 and 2 different backgrounds and their own understandings of the philosophy of science, which may make Roy's arguments more or less difficult to accept, and I suggest that after a brief review (which I will try to present below), one delve into chapters 3 through 5 before attempting to come back and clarify, in one's own terms, chapters 1 and 2. Chapters 3 through 5 can just be read on their own. But they too gain something from the most interesting challenge Roy poses in chapters 1 and 2 stemming from her observations about the finding/production of the Higgs Boson.

We recognize the Higgs Boson by its "signature," "models anticipating its existence," human judgment (of what are bosons and what background), and significance. It is this last, in particular, that is most interesting (and least predictable). What role, Roy asks, "will the signature or discovery of the Higgs particle play in the next few decades?" At issue, in conventional terms, are the Standard Model, or new frontiers in the work of physicists, terms that are of primary interest to physicists. But for the rest of us, perhaps the interest is rather the ways in which science is always open-ended. *Unfinished nature* is Roy's term for the open-endedness of scientific discovery. It resonates with my own "Emergent Forms of (Un)Natural Life"—explorations in synthetic and systems biology that provide new arenas of similar/different epistemological issues as particle physics. Similar/different is what those physicists learned who entered biology in the mid-twentieth century in the hopes of "solving" the mysteries of the genetic code as if they were cryptographies. As the historian Lily Kay demonstrated, the "code" proved resistant to them and was "cracked" instead by microbiologists, illustrating, as Kay put it, that genetics was not simply a code but complex cascades of regulatory systems that often mutate and generate the new: genetics and genomics had to be expanded into many

different "omics" if we were going to understand much about biology (and indeed ecology, habitats, and human and nonhuman interactions). Hans-Jörg Rheinberger would call the experimental method in biology a generator of surprises, more like the open systems of Ludwig von Bertanlanffy and the deconstructive reading and writing of Jacques Derrida, whose work Rheinberger both translated and used in his account of the discovery of protein cascades.

RIDDLE 1: SIGNATURES AND WHAT ROLE WILL THE HIGGS BOSON PLAY IN THE NEXT FEW DECADES?

Roy writes, "On July 4, 2012 . . . two of the [four] collaborations at CERN, ATLAS and CMS, announced that they had discovered a new particle in the mass range of 125 gigaelectron volts (GeV) consistent with the Higgs boson predicted by the Standard Model." This was popularly glossed as the discovery of the Higgs particle. Riddle 1: as Roy nicely puts it, while anticipated, "it is not foretold what precise role it will play in the next few decades" even as the researchers who discovered it won the Nobel Prize in 2013. Indeed, what is its significance?

The Higgs boson, an "elementary" particle theorized long before it was experimentally found, was one of the great international quests at the Large Hadron Collider (LHC) at the CERN particle accelerator, built in 2012 to consolidate European scientific infrastructures in quasi competition and quasi cooperation with accelerators in Japan and the United States. It was, like the earlier accelerators at CERN *a political and political economy project* as well as a scientific one—*and an epistemological one*. One can count "generations" of precursor scientific experimental apparatuses that

involved both different ideas about how to "tinker with" versus "machine-tool" high-precision instruments (Traweek 1988) and different epistemologies about how particles can be identified (Galison 1997). Traweek contrasts American physicists, who are often farm boys used to repairing and inventing machinery, who believe int their personal tinkering abilities; and Japanese physicists from more upper class backgrounds who depended upon machinists to work for them. Galison provides a history of the competition between physicists who privileged a singular visual trace (a "golden event" or image) and those who privileged statistical pattern recognition (or "logic").

For some 'seeing is believing' continued, but a golden event can't show you anything (at best is an indexical trace); statistics can tell you things more robustly, but you can't see them.

Even this initial bit of twentieth-century history of high-energy physics has given sociologists and anthropologists material to think about how science is patterned in different historical, cultural, and epistemic traditions. Two key moments in this initial history are Sharon Traweek's closely detailed account of physicists in two training traditions that simultaneously seems to reflect larger industrial patterns, with Japanese physicists relying on machinists, just as Japanese small supplier firms power their larger international industrial ones (ethnographically described by Kondo, 1990); and Galison's account of the development of instruments in particle physics from "bubble chambers" modeled on alpine cloud chambers or visible models of dynamics of patterns, not unlike wind tunnels or wave machines, evolving into tracks left on "detectors" of "invisible" events whose logic (and reality) is demonstrated by their statistical "signatures." Galison shows how, slowly, with competition between the epistemologies and capacities of different instruments, what eventually clinched belief in truth (of a model) or existence (of a particle) merged:

logic and image coalesced; theory and experiment overlap and interrogate each other. Knorr-Cetina adds a useful contrast within this ethnographic tradition, arguing (along with Galison) that physics has become inextricably the work of large-scale teamwork (and historical sociologists like Mario Biagioli would show how the politics of citation worked in apportioning credit among team members for, among other things, professional promotion, fund raising, and access to machines), in sharp contrast to the continuing role of the scientists' own "good hands," intuition, and "body" in molecular biology, a field that since then has also rapidly become statistical big science. .

Arpita Roy brings us back to some basic questions about signatures, models, and realities. Although Higgs bosons have been "found," Roy tells us, they can never be directly observed in particle detectors, in part because they decay so rapidly into two photons and in part because they are hard to differentiate from background photons. The background photons are given off as electromagnetic radiation by accelerated particles when forced to orbit in the circular trajectory of particle accelerators, used to generate bosons in high-speed particle collisions. This differentiation between the photons in boson decay and the background photons, she says, is a *matter of judgment* by physicists. The experimental detection of the Higgs boson can be done only indirectly, through other particles produced in wake of its decay such as photons, leptons, and hadronic jets. So, are the bosons objectively real? Here the language around the issue becomes a bit philosophically fuzzy. As she quotes Ian Hacking, the question is ill-formed: as any pragmatist, logical positivist, or operationalist (to name just a few of the overlapping reflections of physicists about the philosophy of what they are doing) would say. *Of course it is real, as long as it does productive work*. (To call this "socially constructed," as sociologists of science of a certain

stripe, faddish in the 1990s did, is not a very helpful substitution of jargon, providing a false or merely linguistic sense of precision.) Euclidian geometry was socially constructed and was considered universally true, until the presuppositions were changed and non-Euclidean geometries became not only theoretical potentials but also practical tools. It is this *level of presuppositions* that Roy wishes to access, and an account that does so would be quite valuable. Unfortunately, the complications of using ordinary English to describe her concerns lead into old-fashioned formulations such as intrinsic versus extrinsic relations in science (e.g., logic or verification through experimentation versus funding and political patronage). Roy describes her ambition "to disclose not so much the discovery of a new fact, as the discovery of new ways of thinking about a fact." She attacks an old-fashioned (and mischaracterized) variant of ethnography as naïve description as if it was finished fact-finding and insists on the inescapability of any ethnographer today that "fieldwork among a community means attending to persons and things, learning from them, and re-uniting knowledge." This is not "beyond the pale of description," as she claims in the introduction, but a necessary component of it. (And has been so since at least the 1920s, if not back to Vico and Fustel de Coulange.) The credibility of Roy's comments about physicists' understanding of their own presuppositions depends upon our being able to credit her accounts of ethnographic method as well. She writes, "One of the great puzzles for sociologists and philosophers is to explain how scientific discoveries and innovations constitute a break from a given empirical context." This is the old issue of how models function between logic and phenomenology. In anthropology, perhaps most productive were the distinctions between etic and emic perspectives (provided by linguistic analysis) or between generative deep structures and ordinary surface structures as in

the classic example of language speakers who recognize correct versus incorrect grammar but cannot provide the rules. In physics the riddles (Roy's nice term) have more to do with the logics of mathematics (of various kinds and presuppositions) and the conventional realities or agreements among practitioners.

She writes, "A fundamental premise of this book is that the context of discovery in particle physics cannot be explained by any compilation of empirical results since it expresses itself in the heuristic of conceptual possibilities," involving always the relationship between the framing of problems and the "facts" gathered to support or disconfirm them. This is true not only in physics. The production of a topology of conceptual possibilities might be an enlightening project, but if that is what Roy has in mind, she doesn't quite get there.

She nicely quotes Marx on the Greek arts and epics as bound to social forms of development (their time and place), yet they "and still give us (who are located in other times and places) not only artistic pleasure, but norms and models that cannot be rivalled." Roy sees this as disjunctive; I do not. Indeed, as she says in the following sentence, mistaking this "riddling problem" as distinctive of science (I would think it distinctive of aesthetics), how natural science (or aesthetics) "is integrally bound up with the social milieu, while it preserves its distinctiveness and promotes the expansion of knowledge, which furthers discovery and invention and even experimentation."

Roy wishes to argue that scientists are (usually) unaware of the presuppositions that they operate with, but Roy operates with her own presuppositions, especially that one can code scientific presuppositions as having relations of fact/value, subject/object, and theory/practice. That these words are everyday usages seems empirically true, but suppose that the term "subject" includes the engineering apparatus and one comes close to the Heisenberg

indeterminacy principle that her (Roy's) presuppositions refuse. This is a riddling problem (if not a mistake) on the part of the ethnography (not the science) or of the descriptive account, and perhaps of the theory of binaries—where do binaries come from, if not from always changing linguistic discriminations? Indeed, even she says that "the power to make things real and active belongs to the domain of concepts," i.e., linguistic tokens, which go in and out of fashion. It may certainly be true that, as Thomas Kuhn tried to argue, concepts are smaller units locked into paradigms of thought and paradigms change only when better ones (able to explain more facts with fewer anomalies) are devised (note the active human verb "devise," as in devising a play out of actors' multiple experiences and interpretations or devising an experiment out of multiple scientists' earlier experimental schemes). Of course, in science, purely subjective feelings, perceptions, and sensations are marginalized, *but not as a matter of discovery*, only as matters of verification by multiple scientists. At times she wants it both ways: "There is no reason why we should be coy in approaching the technical concepts to discern features of subjectivity or contingency, and then we may be fully justified in regarding natural science as a normative activity or social construct like any other."

Roy wishes, as this last sentence indicates, to bridge the "gap" between C. S. Lewis's two cultures (the sciences and the humanities), an old topic of discussion from the late nineteenth century, perhaps best overcome in Wilhelm Dilthey's notion of intersubjective and dialogic communication, making all thinking public, not private or hidden, open therefore to modes of confirmation or reframing (socially constructed, if you insist, while growing from the "internalist" developments of previous work and also using various kind of pidgin languages where different fields of expertise with their different conventions must find ways of communicating).

In this context, then, "riddling problems" is not a bad frame for challenging scientific problems, both as they play out conceptually and as they are channeled by their funding, accrediting, and other (sometimes frozen and sometimes reassorting) social environments.

RIDDLE 2. LEFT HAND, CHIRALITY, CHIROPTICAL OBJECTS, AND SURREALISM

Chapter 3 might be read as a symphony integrating four different sorts of melody or logic: left-right symbolism; chirality (a geometrical property that an object or structure and its mirror object, or enantiomer, are not superimposable by any translation or rotation, something Leibniz tried to address with his topology); chiroptical phenomena (rotational features that are quantifiably measurable unlike the pure geometrical chirality); and René Magritte's (or other surrealists') provision of an epistemological parallel in visualization methods.

It is a stimulating tour through the boundary land between commonsense puzzles of left-handed gloves and other such asymmetries (while right and left hand look the same, a left glove cannot fit on a right hand, except if it is turned inside out), on the one hand, and, on the other, more fundamental findings by physicists that elementary particles have spin (or are left- or right-handed or spin clockwise or counterclockwise, also called negative and positive [right-handed] helicity). In particle physics this asymmetry is further made puzzling by the fact that while for every particle there should be an anti-particle ("symmetry"), in fact after the big bang, more left-handed particles were produced. (One theory is that as the universe expanded it cooled and

couldn't keep up the symmetry of particle–anti-particle pairs, leaving an asymmetry of more left-handed ones carried by weak electromagnetic forces). Richard Feynman and others have suggested this might be a future way of communicating to aliens elementary notions of orientation, so important in biology on earth.

Chirality (handedness) is fundamental to cells, DNA, and proteins and is an important feature in artificial nanoparticles and new materials. What is so interesting about this asymmetry (from the micro level of particles to the relatively macro level of asymmetry in biology such as right and left hands) is the geometrical property that an object or structure and its mirror object (enantiomer) are not superimposable by any translation or rotation. Leibniz tried to construct a topology to deal with this issue (identified by Kant, that the orientation of the two hands could not be reduced to metrical properties). Chirality is a geometrical feature, but biological processes also are constituted by *chiroptical* properties, measurable by chiroptical spectroscopy in various kinds of rotation, dispersal, refractions, polarization, and phases. These help constitute embryological processes defining organ orientation and placement.

Roy, appropriately given her fieldwork at CERN, pays most attention to the puzzles of chirality and provides a short history of the experiments of the early post–World War II period "atom smashers" in laboratories at Berkeley (the bevatron), Brookhaven (the cosmotron), Birmingham (the synchrotron), and CERN (the proton-synchrotron). These machines produced subatomic particles with which theories about the beginnings of the universe could be explored, a program that the search for the Higgs boson continued. But she also intriguingly hints at a Lévi-Straussian (or structuralist) account of the near universality of left-right symbolism in cultural systems, what anthropologists once suggested might be "natural symbols." A too-easy solution

to natural symbols would be to say that physicists have provided a natural substrate to explain the asymmetries (what is sinister or left and what is right or moral); a more likely solution, as Roy suggests, is the idea of holistic systems of symbolic tokens, part of linguistic system formations themselves.

In a very nice final move in the chapter, Roy allows the physicist Luis Álvarez-Gaumé to guide her in a discussion of René Magritte's 1937 painting *La reproduction interdite*, meaning ambiguously either "reproduction forbidden" or "impossible reproduction," the latter being more apt. It is a painting of a man standing in front of a mirror, but instead of seeing his front in the mirror, we see a second slightly awry variant of his back. To his right on a table is a copy of Edgar Allan Poe's novel *The Narrative of Arthur Gordon Pym of Nantucket*. The book is painted realistically, as we would expect to see it, unlike the man and his image, but the novel is full of the quirks of an unreliable narrator. It is as if Álvarez-Gaumé is acknowledging the weirdness of particle physics, with its breaking of normal symmetries, parities, and the like, and indeed with remaining open questions, dependent on things we cannot see directly (requiring indirect observation, cleaning of data, and refining of models and theories). It is a bit odd that in chapter 5 Álvarez-Gaumé's openness to art-science synergies of different but mutual exploration becomes attenuated if not denied: it is as if he has split selves or has stepped into Magritte's painting.

RIDDLE 3. NUTS AND BOLTS: TUNNEL EXPLOSION AND REPAIR

On September 19, 2008, just nine days after the new LHC had been inaugurated, a gas explosion suspended operations for

many months, amid intense competition with the Tevatron at Fermi National Accelerator, in Illinois, and fears that this setback might cost CERN the chance to find/produce the evidence of the Higgs boson first. The explosion released two tons of helium gas, breaking magnets and displacing others from their moorings. The liquid helium containers, which were punctured by an electrical arc when an interconnect splice between magnets disintegrated, were supposed to keep the magnets and tunnels at a 1.9 Kelvins (or −271° Celsius) operating temperature. After the accident, it would take time to bring the tunnels up to room temperature to allow for repairs.

Chapter 4 is, or can be read as, a real "case study" of industrial failure, or as Charles Perrow classically formulated it, "natural accidents," involving more or less "tightly coupled" social components of complex engineering projects. The STS scholars Diane Vaughn and Constance Perrin followed up regarding the 1986 Space Shuttle *Challenger* disaster and the 1979 partial meltdown of the Three Mile Island nuclear plant and other brittle nuclear safety cultures. Vaughn focused attention on the internal power relations among engineers, administrators, and manufacturers that allowed for a managerial risk decision to be taken that proved fatal. The physicist Richard Feynman became the iconic figure in the forensic inquiries and hearings by simply holding up a rubber "O" ring that failed (it could not expand in 32 degree weather as the rocket heated up) and should have been recognized as likely to fail (as engineers warned), and that ipso facto threw harsh light on the rosy probabilities of failure and safety that NASA used in its public relations. Constance Perrin attempted to provide nuclear power stations with "safety cultures" that have been pioneered in other critical mission arenas such as surgical anesthesia and airplane cockpit design. She ran a two-year seminar in the MIT STS Program to explore

these safety cultures and their challenges while doing fieldwork in nuclear facilities in the United States and Europe. Among her warnings, again, was a sociological one, that a primary challenge needed to be to increase the relative prestige of safety officers in the plants, whose status was normally at the bottom and whose recommendations were treated as routine and unimportant housekeeping. The industrial accident at Bhopal in 1984 and the failure at Chernobyl in 1986 have become classic cases in what the sociologist Ulrich Beck called "risk society," a second-order systemic formation in modernity that works to remove the "means of perception" from individuals and hides risks among a variety of bureaucratic processes, not unlike the way in which Franz Kafka analyzed how mining industries hid their risky practices from the view of insurance companies so that they could shield accidents and death from liability. The failure of the Daiiche Fukushima nuclear plants in the 2011 tsunami was largely caused by misrecognizing that heights of tsunamis in the future would increase and placing the backup generators in basement rooms rather than higher in the facility.

Accidents such as these and others become case studies for the fragility of complex systems but also moments exposing the competition and relative standing and power among theorists, experimentalists, and instrument makers. This is a classic framing often attributed to Thomas Kuhn (does theory lead science, or does experiment?), then refined in the history of twentieth-century physics by Galison's inserting the importance of instruments (as described above). But accidents also force the recognition that more players are involved and could be critical positive safety monitors, including regulators, policy managers, and even ethnographers. It is indeed in the postaccident debates, grumblings, and corridor complaints that Roy says she first truly understood the sharp divisions between getting back to work

(theory), getting the instruments up and running, and rethinking experimental strategies (especially timing of repairs and experiments to not allow the Americans to get ahead). The analogue to the *Challenger*'s O ring here was the disintegration of an interconnect splice in turn attributed to the mundane (if crucial) lack of care in soldering. Among the riddles Roy's work presents in this chapter is what effect a Diane Vaughn, Constance Perrin, or Charles Perrow might have had if their forms of attention and accountability checking been in place? As we learn from such case studies, these are not simply technical matters (best practices, check lists, maintenance checks) but also matters of power competition among many different kinds of actors, who on the surface all seem to want to work in seamless cooperation toward common ends but whose assertion of interests throws obscuring shadows over equally important but less powerful ones. (No science and engineering without the peopling of these fields.)

RIDDLE 4. NULL RESULTS: EXPERIMENTAL AND ETHNOGRAPHIC FAILURES, REVISING THEORY

In 2000 CERN created several three-month residencies for artists. Such residencies have become quite popular in various science and engineering settings over the past several decades. The expectations and outcomes can be quite varied, and one of the affordances is that expectations are kept loose. Still, as with laboratory experiments, experiments in the arts do not always live up to expectations. Roy's one-time advisor at Berkeley, Paul Rabinow, once wrote a controversial book on his ethnographic failures in Morocco in which he questioned the entire enterprise as a colonial misadventure. He subsequently redeemed himself,

first, by expanding his field of vision to the nature of French colonial modernity and, later, by a finely crafted ethnography of a molecular biology laboratory and the making of PCR (polymerase chain reaction), which was becoming a fundamental laboratory tool, in which he questioned the lavish credit we give the scientist who is given patent rights to an idea, as opposed to the laboratory craftspeople who put in the long hours of actually making an experimental system function accurately and regularly, versus thirdly those who turn the tool into a marketable package that can be sold off the shelf (without special knowledge) and accrue the commercial value. Rabinow's account sounds not unlike Roy's case study above.

Roy's account of CERN's art residency program, however, sounds much like Rabinow's *Fieldwork in Morocco* book, oddly without a sense of what art-science collaborations can achieve. Part of the problem, perhaps, is the riddle: Is there an analogy, interdependence, or milieu of understanding between art and science? Many accounts of the arts and sciences at the fecund turn of the twentieth century note the crossover weltanschauung that was transforming from a mechanical universe to one recognizing probabilistic relationships, reflected in surface indeterminacies, inflected as well by mediating instruments whose capacities or affordances were changing and by observer-observed interactions. Some accounts of the arts and sciences today attempt to do something similar. Roy gestures at this, but with the unsatisfactory and empty term "postmodernism," a term from commercial branding efforts in architecture in reaction to overemphasis on streamlined functional aesthetics or, worse, from Alan Sokal's 1994 self-promoting silly hoax whose only stakes seemed to be whether he could fool an editorial board at a non-peer-reviewed journal to print his attack on STS as if it were a challenge to the doing of science. (I well remember the physicist Steven

Weinberg, who was being recruited by Sokal allies to attack STS, openly disappointing them by pithily saying that STS was not the enemy; the enemy was members of Congress who would not fund science appropriately. This was around the time of the defunding of the Superconducting Super Collider in East Texas and a growing sense within the senior physics community that their generation-long command of funding processes in Washington, DC, present since World War II, was coming to a close, not least because physics had been replaced by biology as the lead science for the coming century.)

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Not all art-science initiatives generate results beyond the metaphorical or superficially pretty, but the best do. Some of these depend on technological innovation, such as Doc Edgerton's famous photographs of drops and splashes or bullets and bombs caught midaction by repeatable electronic flashes or, more recently, the extraordinary large-scale images of cellular processes that hang on the walls of the MIT Cancer Research Center, such as tracking nanoparticles across the blood-brain barrier, snapshots of tumor growth, or inspiration and respiration in a petri dish. Some art-science works do actual pedagogical work, such as the bioart of the Australians Ionatt Zurr and Oran Catts, in which they expand the work of the tissue-engineering lab into wider ecological realms, asking what kinds of social work are required to accomplish sustainability beyond an art installation's already-complicated importing of negative pressure hoods and other sterilizing protections. Or again, the physics department at Princeton a number of years ago gave a residency to an artist, and I helped bring him to MIT, who wanted to capture the sounds of the interior of a nuclear reactor: part of the project

was to get the help of the machinists and material scientists to devise a recorder that would not melt in the reactor. The specifics of the project can be variously evaluated, but a key goal was to get scientists and engineers to work together in novel ways and see if they could learn new things. As to the dismissal of doing a walking tour through the tunnels at CERN, I can only counter that my visits to the tokomak and nuclear power plant at MIT made a profound impression that was useful years later in understanding the plume physics of the fires raging in the Western United States, Canada, Siberia, Australia, and elsewhere and that will increasingly become part of the new forms of scientific research on fire as the climate continues to change. This is critical work for surviving in the future, work that Adriana Petryna calls "horizon work." It is work that depends upon more than the predictability of old models and in that sense is a challenge to old science and demand for developing new science.

Much of art-science relations involves the innovation of visualization and now sonic technologies, a theme that I have been stressing in my teaching and exploring with colleagues like Joseph Dumit, who studies how scientists manipulate visualizations (and now movement) in order to see more clearly, or often just for publicity that requires the public to learn how to read visualizations for the information, not just the aesthetics. The covers of scientific journals are a rich source of visuals, but they often obscure as much in the name of prettiness as they reveal in the name of information. This, like coding, will become an increasingly important form of literacy.

Even if Roy's experience with CERN's artist-in-residence program was unsatisfactory, I urge readers to take the chapter as a placeholder or a challenge to look around their science and engineering environments for edifying initiatives and experiments. Experiments are just that: trials. Some work; many do

not. Some work for some audiences; others work for other audiences. As science and engineering education becomes more and more project- and team-oriented, the artistic literacy side of creativity will grow. MIT's Media Lab stands as a powerful example of this sort of emerging pedagogy, as do similar endeavors at the Swiss ETH, Singapore's CREATE laboratories, and elsewhere.

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