

CULTURAL
BOUNDARIES OF
SCIENCE

credibility on the line

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Preface

I have always been fond of maps. My mother insists that I learned how to read from a street map of Rochester, New York, where I grew up. From an early age, I spent hours drawing maps of imagined cities, crude ones at first in thick black lines, then more sophisticated efforts in colored pencil, water colors, rapidograph pens, transfer letters—and most recently, with graphics software on my Mac. I still make time for creative cartography and love to arrange lines and colors into fictional urban spaces for no other purpose than my own amusement. I also collect city street maps (more than 300 and accepting donations), less for directions than for art.

Still, it took the greatest sociologist of all time to suggest how this passion for maps might allow me to see *science* in a different and fruitful way. In the 1894 novel *Tom Sawyer Abroad*, Mark Twain narrates a conversation his hero has with Huck Finn as they float above the midwestern countryside in a fantastical balloon:

“Tom didn’t we start east?”

“Yes.”

“How fast have we been going?”

“Well, you heard what the Professor said when he was raging around; sometimes, he said, we was making fifty miles an hour, sometimes ninety, sometimes a hundred . . .”

“Well, then, it’s just as I reckoned. The Professor lied.”

“Why?”

“Because if we was going so fast we ought to be past Illinois, oughtn’t we?”

“Certainly.”

"Well, we ain't."

"What's the reason we ain't?"

"I know by the color. We're right over Illinois yet. And you can see for yourself that Indiana ain't in sight."

"I wonder what's the matter with you, Huck. You know by the *color*?"

"Yes—of course I do."

"What's the color got to do with it?"

"It's got everything to do with it. Illinois is green and Indiana is pink. You show me any pink down there if you can. No, sir, its green."

"Indiana *pink*? Why, what a lie!"

"It ain't no lie; I've seen it on a map and it's pink."

"... Seen it on a map! Huck Finn, did you reckon the States was the same color out doors that they are on the map?"

"Tom Sawyer, what's a map for? Ain't it to learn you facts?"

"Of course."

"Well, then, how is it going to do that if it tells lies?"¹

Anybody who has hung around the sociology of science lately will recognize at once what Tom and Huck are fighting over: what *is* the relationship between nature "out doors" and its representation on a map—or in a scientific fact? Perhaps nobody has ever been as naive as the old-fashioned realist Huck Finn: no gazetteer or scientific theory mimes reality literally, without mediation or translation or interpretation or contextualization. But for Huck, verisimilitude must reign, absolutely.

Sociologists of science have found lots of stuff that intervenes between reality and map, signified and signifier, object and image, referent and representation, nature and knowledge. Merton's pioneering studies of the social structure of science situated scientists in a constraining and enabling milieu of institutionalized norms, reward and evaluation systems, and communication networks—all of which were assumed to shape their representations of nature. With the emergence of the sociology of scientific knowledge in the 1970s, attention focused on social processes that were demonstrably connected to the content of scientific belief, and it soon became apparent just how substantially nature outdoors underdetermines its inscriptions. Interests, rhetorical tropes, power, identity, hands-on practices, tacit skills, instruments, experimental systems, and (as a catchall) culture are now standard ingredients in sociolog-

1. Mark Twain, *Tom Sawyer Abroad* (New York: Oxford University Press, 1996), 17–18. Bibliographic note: for primary historical documents, full references are given in footnotes when they are introduced for the first time in a chapter; for the secondary literature, short identifying references are given in the notes and full references can be found in the bibliography.

ical studies of the construction of scientific knowledge. What happens to nature in all this kitchen work depends upon the chef you ask: for some, nature is a seasoning thrown in to flavor the social meat and cultural potatoes; for others, nature is what is finally brought to the table, what gets ladled into bowls, either thick stew with chunks of social left in or thin broth after the "meat" is methodically strained out and discarded; still others never bother to pick up any nature at the market—it is social down to the bottom of the pot.²

After almost three decades of sober sociological and historical inquiry into how scientific knowledge gets prepared and why scientists accept some accounts of nature as provisionally and incompletely true, this much can safely be said: "Les faits ne naissent pas dans des choux."³ Lingered debates over relativism versus realism, over the fraction of a scientific theory caused by natural versus social forces, over the existence of chairs at 30,000 feet have become stale, even a little moldy. Possibly the time is ripe for sociologists to look at science from a different vantage—not upstream at facts in their making, but downstream at their consumption. Scientists, their expertise, their claims and material artifacts eventually leave laboratories and technical journals and make their way out into the rest of the social world, where they are called upon to settle disputes, build airplanes, advise politicians, ascertain truth. And they do so with a special authority that begs for sociological explanation: Why is science so widely trusted? Why do we turn so often to scientists for help in reaching personal or policy or corporate decisions? Why do we provide copious public patronage to support more scientific research? Why is science conferred the legitimate power to define and explain nature and other realities?

The answers will not be found upstream, I suggest, but down. Nothing in the practices of scientists at their benches, nothing in their skillful mangle of gadgets or critters, nothing in the literary machinery that translates inquiry into facts on a page can alone explain why science is trusted (in so many and varied situations) to provide credible and useful accounts of nature. Or, more precisely, upstream science substantially un-

2. Merton's work is gathered up in his *Sociology of Science*. Constructivist sociologies of scientific knowledge are reviewed in Barnes, Bloor, and Henry, *Scientific Knowledge*; Lynch, *Scientific Practice*, chaps. 2–3; Shapin, "History of Science" and "Here and Everywhere." A review of both orientations is found in Zuckerman, "Sociology of Science." A sample of recent work in the field is provided by Jasanoff et al., *Handbook of Science and Technology Studies*.

3. A double translation is needed: "Facts are not born in cabbages"—or, for Americans, they don't come from storks. Bruno Latour, "Y a-t-il une science après la guerre froide?" *Le Monde*, January 18, 1997.

derdetermines the epistemic authority that marks its consumption downstream. Just as the constructivist sociology of scientific knowledge found that nature outdoors was an incomplete cause of scientific belief, so is science as practiced in labs and journals an incomplete cause of its power, prestige, and influence in society *tout court*. What is missing? If nature is socially constructed, so is science: the practices, skills, texts, achievements, and potentials of scientists are wrapped up in layers of discursive interpretations as they make their way downstream to respectful waiting publics. It is in these mediating representations of what science is or what scientists do that sociologists will find a robust explanation for the predominance of science these days in settling questions about the real.

I take one other thing from Twain. In the same way that Huck learned about Indiana from a map, people all over learn about science from maps of it. The layered interpretations that surround scientists and scientific facts with a special believability often come in a rhetorical form best described as cartographic. "Science" becomes a space on maps of culture, bounded off from other territories, labeled with landmarks showing travelers how and why it is different from regions of common sense, politics, or mysticism. These cultural maps locate (that is, give a meaning to) white lab coats, laboratories, technical journals, norms of scientific practice, linear accelerators, statistical data, and expertise. They provide the interpretative grounds for accepting scientific accounts of reality as the most truthful or reliable among the promiscuously unscientific varieties always available. Maps of science get drawn by knowledge makers hoping to have their claims accepted as valid and influential downstream, their practices esteemed and supported financially, their culture sustained as the home of objectivity, reason, truth, or utility. Maps of science get unfolded and read by those of us not so sure about reality, or about which accounts of it we should trust and act upon.

These cultural cartographies of science-in-culture are historical phenomena, with a local and episodic (rather than transcendent) existence. The same concatenation of interests, identities, discourses, and machineries that come together to make scientific knowledge have also come together to shape representations of science itself in a contextually contingent way. If I am correct that sociologists will find an explanation for the epistemic authority of science in its cartographic (re)constructions for public consumption, they will *not* find in those interpretative representations that science is any single thing (or even a small and consistent set of qualities). What science becomes, the borders and territories it assumes, the landmarks that give it meaning depend upon exigencies of

the moment—who is struggling for credibility, what stakes are at risk, in front of which audiences, at what institutional arena? It is exactly this pliability and suppleness of the cultural space "science" that accounts for its long-running success as the legitimate arbiter of reality: science gets stretched and pulled, pinched and tucked, as its epistemic authority is reproduced time and again in a diverse array of settings.

Such an argument calls for detailed examinations of local and episodic constructions of science, highlighting the different cultural spaces science becomes in order to serve diverse pragmatic ends. The book ahead consists of five historical cases of the cultural cartography of science: they are written fresh but over an unbearably long period, not exactly in the same style, and arranged chronologically by when they were penned. I introduce the five cases with a chapter of theory mongering (where concepts such as "credibility contest," "epistemic authority," and "boundary-work" are defined and anchored), and the book ends with some reflexive horrors brought on by the so-called science wars. Each episode starts out with a struggle for credibility: somebody somewhere seeks to ride science into the public's trust or support or vindication. It is never easy: somebody else challenges their credentials as scientist, their skills, their procedures, their potential for making a truly better world. The maps start to fly, as contestants create distinctive cultural worlds—with discrepant locations and features for science—in order to convince those downstream that their claims about nature are credible or pertinent and that their practices are worthy of esteem or trust or patronage. There are winners and losers of course, but through it all and over historical time, the connection is reproduced between science (a fuzzy set if there ever was one) and the legitimate power to define the real.

Sociological attention is centered on how the boundaries of science are episodically established, sustained, enlarged, policed, breached, and sometimes erased in the defense, pursuit, or denial of epistemic authority. As knowledge makers seek to present their claims or practices as legitimate (credible, trustworthy, reliable) by locating them within "science," they discursively construct for it an ever changing arrangement of boundaries and territories and landmarks, always contingent upon immediate circumstances. When people outside the laboratories and technical journals dispute the authority or credibility of claims and knowledge-making practices, what does science become then and there? The selective attribution of this or that characteristic to science cannot be explained by what science "really" is at the bench or in a journal, but only by the pragmatic utility of any given borders and territories for the pro-

tection or expansion or denial of scientific authority over the facts. In other words, the question to be asked of any cultural map is not "Is it accurate?" but "Is it useful? If so, by whom, for what?"

"Science" is a cultural space: it has no essential or universal qualities. Rather, its characteristics are selectively and inconsistently attributed as boundaries between "scientific" space and other spaces are rhetorically constructed. The longstanding question, "What unique, essential, and universal features of science justify its authority in politics, law, media, advertising, and everyday reckonings of reality?" should be replaced, I suggest, by this more tractable question: "How do people sustain the epistemic authority of science as they seek to make their claims and practices credible (or useful) by distinguishing them from unworthy claims and practices of some nether region of *non-science*?" Science is a symptom of the legitimate power to decide reality—its edges and contents disputed, moved all over the place, settled here and there as decisions about truthful and reliable claims are acted upon by jurists, legislators, journalists, managers, activists, and ordinary folk. Representations of science—where it is, and where it is not—have less to do with the cultural realities they supposedly depict, and more to do with the cultural realities they sustain.

This book was written from many places, and the more I ponder the sociological importance of "place" these days, the more I believe that it matters *where* I was—when I thought, when I wrote, when I anguished. The journey has been long and twisted; no map could possibly be detailed enough to show all the people who have helped me along the way. The influence of my teachers at Columbia University is palpable still. Robert Merton and Harriet Zuckerman worried little about the substantive content of my sociology, but demanded only that I execute it as well as I could. Their lives and works offer a model of scholarly excellence—for me, and for so many others—and our friendship through years of divergent paths measures a shared commitment to the norm of organized skepticism. In New York, Peter Messeri and I argued forever the possibility of defining science, and Astrida Butners showed me how to survive the City.

My trailhead is probably best located on the banks of Cazenovia Lake, not far from Syracuse, New York, where Donald Campbell (a much missed friend) convened an extraordinary gathering of historians, sociol-

ogists, and philosophers of science. I went there a Mertonian, and left . . . something more. Steve Woolgar recognized my inchoate babblings as "boundary-work," and Karin Knorr Cetina helped me find my own way (so did Sal Restivo).

Most of the struggle happened at the Department of Sociology at Indiana University, which has provided for two decades a salutary climate of vigorous challenge and warm encouragement. I thank my local colleagues one and all (including some who are no longer local: David Brain and Ron Giere). David Zaret has heard every single idea of mine ad nauseam, though I shall never convince him why constructivists board airplanes with as much (or as little) confidence as he does. I bored my students even more with endless cultural cartographies of science, but they were kind enough not to show it: Steve Zehr, George Bevins, Anne Figert, Mitch Berbrier, Alyssa Kinker (who collected a ton of stuff for chapter 5, all to be recycled in due course), Barb Halpenny, Todd Paddock, Emanuel Gaziano, Walter Jacobs, Joe Tatarewicz, Karen Rader, Brad Hume, and everybody in Science Club. I have learned a thing or two about life from Jim Capshew—once an undergraduate in my very first class at Indiana, now a faculty colleague, always an inspiration.

Material for chapter 3 was collected while my family and I survived a blustery spring at Ard Carrach, Lydia Forbes's home in Carradale, Kintyre, Scotland. I traveled to Edinburgh's Science Studies Unit, where David Edge got me into the Combe archives, Barbara Edge told us how to stay warm, and Steve Shapin explained everything phrenological. There were beneficial side trips that year in Britain, to the reflexive fun-house at York (Mike Mulkay and Malcolm Ashmore), and to Bath (Harry Collins). The theoretical chapters were written in Ithaca, during a year at Cornell's Department of Science and Technology Studies. Sheila Jasanoff insisted that the book make an argument. Bruce Lewenstein and I (with Dougan's help) cooked up the Cornell Cold Fusion Archives that enabled chapter 4 (in which Trevor Pinch caught some sloppy errors).

For many years, *Cultural Boundaries of Science* has had a peripatetic existence—a road show, if not traveling circus. I thank my hosts and helpful audiences at the many universities where I have delivered bits and pieces in colloquia and conferences: Virginia Tech (twice!), Darmstadt, Iowa, Cornell, Minnesota, New York University, Illinois, MIT, Rensselaer Polytechnic Institute, Pennsylvania, Notre Dame, and St. Andrews Presbyterian College. Chapter 2 was helped along by a grant from the National Science Foundation, which did not mind at all a study of itself. Thanks to J. Merton England for help with the NSF archives.

influence, and possibly abundant material resources (cash, equipment, bodies-and-minds) needed to make still more truthful tales. The legitimate right to have one's reality claims accepted as valid or marginally useful is no plum at all if everybody enjoys it all the time. Epistemic authority exists only to the extent that it is claimed by some people (typically in the name of science) but denied to others (which is exactly what boundary-work does).

So ordinary folks seek out cultural maps to locate credibility; fact makers produce maps to place their claims in a territory of legitimacy—but such cultural cartography also has consequences for the spaces mapped out. In credibility contests, the epistemic authority of “science” as a cultural space is chronically reproduced. Familiar reasons are given for why science is a preferred source of knowledge about nature even as the allocation of that legitimacy among practitioners, methods, and claims remains to be disputed, negotiated, and maybe eventually settled by mappers and audiences. Starting out as an authoritative but otherwise largely featureless terra incognita, science is then given particular (but non-aligned) borders and territories, landmarks and labels, in order to enhance the credibility of one contestant's claims over those of other authorities or scientists *manqué*. The epistemic authority of science is in this way, through repeated and endless edging and filling of its boundaries, sustained over lots of local situations and episodic moments, but “science” never takes on exactly the same shape or contents from contest to contest.¹⁸

18. This constructivist explanation for the epistemic authority of science stands in contrast to (1) Durkheimian efforts to explain the credibility of scientists in terms of functional necessity: “That [science] was born indicates that society needed it. . . . For the harmonious development of a complicated social order the co-operation of reflective intelligence soon became indispensable. . . . It was, then, a result of certain collective ends that science was formed and developed” (Durkheim, *Moral Education*, 70); (2) political economic efforts to explain the authority of science by its benefits for capital or another ruling class: “Science cannot escape capital and has been subsumed under the dialectic of the production of needs and capital (use value and exchange value)” (Aronowitz, *Science as Power*, 40); (3) state-centered theories to explain the authority of science by its utilities for governance (the legitimization of policies, for example): “The power of science has become part of the power of the modern state because it gains power in the hands of policymakers” (Mukerji, *Fragile Power*, 202; Mukerji's argument is more sympathetic to constructivism than this brief extract might imply); (4) attempts to explain the cultural authority of science in terms of its alignment with or embodiment of other widely endorsed values, as when Yaron Ezrahi suggests that science and technology are “harnessed” “to ideologically defend and legitimate uniquely liberal-democratic modes of public action” (*Descent of Icarus*, 1); (5) explanations that identify some single watershed event (“the” scientific revolution, the bomb, Sputnik) that ratchets in a degree of epistemic authority that science subsequently never loses. These other possibilities have their merits, but share two flaws: first,

In this sense, it is a little misleading to speak of the “epistemic authority of science” as if it were an always-already-there feature of social life, like Mount Everest. Epistemic authority does not exist as an omnipresent ether, but rather is enacted as people debate (and ultimately decide) where to locate the legitimate jurisdiction over natural facts. Such spatialized allocations—to science provisionally and workably bounded, or to some other putative worthy source of knowledge and guidance—are local and episodic, extant then and there for interpretative finding-one's-way or for practical fact-based decisions but also for seizing/denying the spoils of credibility contests. The cultural space of science is a vessel of authority, but what it holds inside can only be known after the contest ends, when trust and credibility have been located here but not there.¹⁹

The stakes—authority, jobs, fame, influence, nature—create big incentives for some cultural cartographers to (re)draw the boundaries of science one way, just as others then have good reason to counter with maps of their own. Little wonder that the shelf life of any particular representation of the boundaries of science is short to vanishing, even if the epistemic authority of science-in-the-blank endures. The spaces in and around the edges of science are perpetually contested terrain: cultural maps are the interpretative means through which struggles for powerful ends are fought out—the right to declare a certain rendition of nature as “true” and “reliable.” Prima facie evidence for the permanently unsettled cultural space of science is the ubiquitous boundary-work that can only be hinted at in the five episodes to come. The universe of such credibility contests divides into three genres, each an occasion for a different sort of boundary-work.

Expulsion. The first genre defines a contest between rival authorities,

they presume that “science” has a stable and distributed meaning for all institutions, classes, or states seeking to use it, thus obviating analysis of its contingent interpretative constructions; second, they attach too little theoretical significance to the idea that scientists (and many others) have a stake in the seizure or defense of epistemic authority, and that they adjust meanings of science to pursue that stake. These nonconstructivist interpretations assume that something immanent about scientific practice or scientific knowledge causes powerful forces in society to grant science epistemic authority: agency (i.e., boundary-work) gets dwarfed by impersonal structural forces.

19. Roger Chartier has a fine sense of the back-and-forth relationship between institutionalized cultural categories and those momentary, contingent, discursive, and pragmatic enactments of spatial classifications that make up boundary-work: “We [must] accept the schemata found in each social group or milieu (which generate classifications and perceptions) as true social institutions,” but at the same time it is important to realize that “behind the[ir] misleading permanence . . . we must recognize not objects but objectifications that construct an original configuration each time” (*Cultural History*, 6, 46).

each of whom claims to be scientific. All sides seek to legitimate their claims about natural reality as scientifically made and vetted inside the authoritative cultural space, while drawing a map to put discrepant claims and claimants outside (or, at least, on the margins). Real science is demarcated from several categories of posers: pseudoscience, amateur science, deviant or fraudulent science, bad science, junk science, popular science.²⁰ Boundary-work becomes a means of social control: as the borders get placed and policed, “scientists” learn where they may not roam without transgressing the boundaries of legitimacy, and “science” displays its ability to maintain monopoly over preferred norms of conduct. Expulsion often pits orthodox science against heterodox, mainstream against fringe, established against revolutionary—but of course the issue in dispute is who and what belongs on which side. Neither side wishes to challenge or attenuate the epistemic authority of science itself, but rather to deny privileges of the space to others who—in their pragmatic and contingent judgment—do not belong there.

Expansion. Boundary-work also takes place when two or more rival epistemic authorities square off for jurisdictional control over a contested ontological domain.²¹ Those speaking for science may seek to extend its frontiers, or alternatively, spokespersons for religion, politics, ethics, common sense, or folk knowledge may challenge the exclusive right of

20. As a sampler of studies on various kinds of non-science, see, on the possible fraud of Sir Cyril Burt, Gieryn and Figert, “Scientists Protect Their Cognitive Authority”; on the paranormal, Hess, *Science in the New Age*; on popular science, Hilgartner, “Dominant View of Popularization”; on parapsychology and fraud, Pinch, “Normal Explanations”; on borderline science, Wallis, “Science and Pseudo-science” and *On the Margins of Science*; on the boundary between science and common sense, Derksen, “Are We Not Experimenting Then?”; on politics and science, Moore, “Organizing Integrity.”

21. The idea that professions—or institutions, like science—compete for jurisdictional control over the provision of vital tasks is developed in Abbott, *System of Professions*, a work that has been of enormous utility for my concoction of a cultural cartography of science, and which I consider further in Gieryn, “Boundaries of Science.” However, Abbott flimflams a bit on matters at the core of constructivist challenges to all varieties of structuralist sociology. He admits rightly that “human realities are both subjective and objective,” but then calls “objective” the actual practices of doctors with their patients, and calls “subjective” representations elsewhere of those first-time-through practices. Even though these representations of professional jurisdictions are real and consequential—to investigate the subjective qualities of jurisdictions is thus to analyze the mechanisms of professional work itself—Abbott’s parsing out of “subjective” and “objective” belies his wish to see in the actual practices of doctors or lawyers or accountants the weightier “cause” of their professional dominance: “Once a group enters the competition, what matters for us is not what it claims to be, but what it actually is. . . . Autonomous change in the objective character of the task transformed the profession” (*System of Professions*, 82, 39). I am inclined to put greater weight on how downstream interpretations construct the actual practices of scientists in the course of settling issues of epistemic authority.

science to judge truths. On these occasions, the interpretative task is not to distinguish real science from ersatz, but rather to distinguish science from (or identify it as) one of the less reliable, less truthful, less relevant sources of knowledge about natural reality.

Protection of autonomy. A slightly different kind of boundary-work results from efforts of outside powers, not to dislodge science from its place of epistemic authority, but to exploit that authority in ways that compromise the material and symbolic resources of scientists inside. When legislators or corporate managers seek to make science a handmaiden to political or market ambitions, scientists put up interpretative walls to protect their professional autonomy over the selection of problems for research or standards used to judge candidate claims to knowledge. In the same way, when the mass media take upon themselves the task of distinguishing genuine scientific knowledge from putatively less responsible claims, scientists whose claims were made suspect will redraw the cultural map to restore a monopoly over such cartographic efforts to those inside science—autonomy of a different kind. Finally, scientists will draw boundaries between what they do and consequences far downstream—the possible undesired or disastrous effects of scientific knowledge—in order to escape responsibility and blame (which often come coupled with intrusive demands for accountability or restriction).²²

The boundaries of science have not, historically, been set in amber because—in the first instance—nature does not allow but one order of understandings, and therefore those serving up discrepant realities can draw discrepant cultural maps to legitimate their claims as uniquely credible and useful. As contestants for credibility pursue, deny, expand, constrict, protect, invade, usurp, enforce, or merely justify the epistemic authority of science (however bounded and landmarked), cultural maps get drawn and drawn again. But what determines victory in any specific contest? There are no fixed or general determinants of persuasive cultural cartography (“science” becomes the winners’ map only after the dust has settled)—in part because of a “cartographic regress.” When people face multiple and discrepant claims about nature that are located in different epistemic spaces, they need still other maps to assign authority over the task of mapping. “Second-order” boundary-work uses a map drawn primarily to locate authoritative accounts of nature also to locate credible cultural cartographers—and often to dismiss a rival’s map as unskilled or

22. Anne Figert and I develop this point for the Challenger explosion, in “Ingredients for a Theory of Science in Society.” The locus classicus is Vaughan, *Challenger Launch Decision*.

misleading or deceptive.²³ This interpretative layering of boundary-work is not a rare event but probably the norm in credibility contests, as the episodes ahead will suggest. The authority to represent both nature and culture, both facts and maps, is simultaneously contested, negotiated, and (in the end) distributed.

WHAT SCIENCE IS

Perhaps it is clearer now why those involved in credibility contests have strategic reasons to draw lots of culturescapes with variously located places for authentic and authoritative understandings of nature. But is it really the case that cultural spaces for science are drawn incommensurably each time out, or that competing maps during one credibility contest have no likenesses whatsoever? Has there been no convergence at all, over the three or more centuries of science and of its representation, toward a small and consistent set of features able or necessary to distinguish scientific knowledge, methods, and practitioners from everything else in the cultural world? Are there no limits on what characteristics may be attributed to science in order to endow it with distinctive epistemic authority or to restrict, exploit, or reallocate its supposed legitimacy over questions of natural fact?

Doesn't the referent science "behind" all those representations of it—the science happening first time through in laboratories and field stations, in journals, at professional meetings—constitute a reality to which contestants for credibility can appeal in order to justify their own distinctive mappings of epistemic authority? Unfortunately not. If all relationships between referent and representation were merely *mimetic*, so that representations reflected (better or worse) the thing itself, credibility

23. No particular map can carry the day, on all occasions, for all audiences: such a definitive map of science, with fixed and unambiguous boundaries and absolutely stable landmarks, would make all subsequent efforts to redraw science-in-culture into tilts at windmills. Some maps no doubt appear less "real" as depictions of the cultural terrain of objectivity, investigative skill, authenticity, expertise, and practical utility. For example, maps that appear weirdly idiosyncratic—like those upside-down geographical maps where South America is on top (weird, at least to North American eyes)—are less likely, *ceteris paribus*, to convince others that this is where epistemic authority is really located (if only because map readers suspect that some hidden motive or plain ineptitude is causing the map to look nothing at all like their baseline expectations). Similarly, a map that arrays cultural practices and institutions in ways unflattering or downright insulting to the epistemic travelers' own line of work—for example, locating "lawyers" in the "Valley of the Ambulance Chasers" in a court presentation—will probably diminish its perceived accuracy and utility (at least for the attorneys and judges).

contests would not be the chronic and ubiquitous phenomena they have always been: just look straight at nature to know who is telling the truth about it. Even Thoreau spotted the impossibility of that! The problem is not that there is no "real science" behind the cartographic representations, but that there are too many "real sciences." And even when all those sciences are added up, they still together do not allow either the sociologist or the players themselves to know *a priori* what science will look like on the next occasion for its mapping. But neither does it make much sense to think of science-on-the-map as just made up any old way. Boundary-work is constrained by the several "real sciences," but not determined by any or all of them.

Sociological constructivism is not nihilism: an absence of hills from that map of San Francisco does not mean that they do not exist (try telling that to the tired tourists looking up). Nor can everything on a map be found outdoors: the border between Illinois and Indiana, so clear on Huck's mental map, is invisible to hikers in the woods crossing over from one state to the other. Whatever science might be first time through, it is both too much and too little to determine its place on a cultural map. Maps must not only simplify, distill, and reduce their referents, but then reconfigure, distort, and embellish them. It is not immediately obvious where to look for science-the-real-thing in order to check out the accuracy of a certain cultural map—or at what, at whom, and when. Science—as practiced, as written up in technical papers, as regulated by norms of research conduct—has a robustness, a plenitude, a scale that defies complete mapping. Selections from this real science must be made by cultural cartographers, and they are—strategically. And even if an exhaustively authoritative rendering of science as practiced could be drawn, it would not tell travelers lost in the wash of contending truths to find out what they want to know: there is nothing on a scientific instrument, a fact, a statistic, or white lab coat that says "true" or "trustworthy" or "credible." Such labels are added to the reality of first-time-through science in the course of making the map useful for getting around—like inventing different colors to distinguish the states visually.

Moreover, science-the-referent is not embodied only in these first-time-through practices, instruments, research materials, facts, and journals; it has several other realities too. Science gets sedimented as an ordinarily tacit space on most everybody's mental map of culture, a bit of the cognitive schema we use everyday to navigate material and symbolic lands. Science also exists in codified bureaucratic procedures, as when