
Cordelia's Love: Credibility and the Social Studies of Science

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This article assesses the current state of research on the credibility of scientific claims and makes some recommendations about the lines along which future historical and sociological inquiry might most constructively proceed. It sketches how credibility has emerged since the 1970s as an important focus for the social studies of science; it offers an appreciation of the scope of the problem involved in giving an explanation of credibility; it warns against the temptations of overambitious theorizing about how credibility is accomplished; and it provisionally identifies distinct predicaments in which the resources for establishing credibility may systematically differ.

When King Lear decided to take early retirement, he announced his intention to divide up the kingdom among his three daughters, each to get a share proportioned to the genuine love she bore him. Each is asked to testify to her love. For Goneril and Regan that presents no problem, and both use the oily art of rhetoric to good effect. Cordelia, however, trusts to the authenticity of her love and says nothing more than the simple truth. For Lear this will not do. Truth is her dower but credibility has she none.

Cordelia, we should understand, was a modernist methodologist. The credibility and the validity of a proposition ought to be one and

I thank Michael Lynch for much useful conversation prior to writing and Steven Epstein for criticism of an earlier draft. This paper was developed from an oral presentation to a Workshop ("Doing is Believing: Credibility and Practice in Science and Technology") sponsored by the Department of Science and Technology Studies at Cornell University, 21-23 April 1995, and I am grateful to members of that audience for many comments and suggestions.

the same. Truth shines by its own lights. And those claims that need lubrication by the oily art thereby give warning that they are not true. In this sentiment, Cordelia can be celebrated as a neglected forerunner of such plain-speaking English anti-rhetoricians of the seventeenth century as Francis Bacon and Robert Boyle. Use of the arts of persuasion handicapped rather than assisted the perception of truth: it was, Boyle said, like painting "the eye-glasses of a telescope" (Boyle 1772, p. 304). Bacon urged that all persuasive "ornaments of speech, similitudes, treasury of eloquence, and such like emptiness" be "utterly dismissed" (Bacon 1858, p. 254). The "truth of knowing and the truth of being are one, differing no more than the direct beam and the beam reflected" (Bacon 1857, p. 281).

Yet if Cordelia embodies the modernist ideal, Lear represents obdurate reality. Lear makes a mistake such as we are all liable to make. He does not see truth shining by its own lights, and he confuses the pure glow of truth for the artificial brilliance lent by the arts of persuasion. The recognition of truth ought to be simple. The truth of knowing and the truth of being perhaps ought to be the same, but in practice we can never be quite sure that they are. Cordelia loves her father, but she does not evidently understand him as the imperfect being he is. By contrast, the late modern sensibility understands Lear—"human, all too human"—but it is Cordelia who puzzles us. How could anyone not only believe that truth is its own sufficient recommendation, but also consequentially act on that belief?

The changing place of credibility in the understanding of science tracks our move away from Cordelia's innocence. Once upon a time, so the story goes, students of science, too, believed that truth was its own recommendation, or, if not that, something very like it. If one wanted to know, and one rarely did, why it was that true propositions were credible, one was referred back to their truth, to the evidence for them, or to those methodical procedures the unambiguous following of which testified to the truth of the product. Alternatively, if one wanted to know, and one usually did, why false claims achieved credibility, one pointed to an assortment of contingent circumstances that caused people to hold dear what was in fact worthless. That is to say, once upon a time pronouncements of validity were considered adequate responses to questions about credibility. And, indeed, it would be a very narrow and pedantic view of the matter to refuse to recognize that, for most students of science and, so far as we know, for most laypeople, they still count as such.

Credibility and the "Big Picture"

It was David Bloor ([1976] 1991) who made the disjunction between validity and credibility into a maxim of method in the social studies of science, and so it has become for those few specialist scholars who work in this idiom.¹ Within this practice it is both appropriate and interesting to ask why it is maintained, by an individual late modern scientist or by a layperson, that there are such things as neutrinos, or that the pathological signs of Alzheimer's disease include neurofibrillary tangles. The answer to such questions might well include, for example, routine deference to authoritative sources of expert knowledge, just as it might be if one asked why seventeenth-century philosophers and laity maintained the historical reality of Christ's miracles.² All propositions have to win credibility, and credibility is the outcome of contingent social and cultural practice.

Accordingly, again for those persuaded by this argument, the study of the grounds and means of credibility vastly expanded—from the explanation of false claims to the explanation of all knowledge-claims, whether deemed true or false. The study of credibility then became simply coextensive with the study of knowledge, including scientific knowledge. In sociological terms of art, an individual's *belief* (or an individual's claim) was contrasted to collectively held *knowledge*. The individual's belief did not become collective—and so part of knowledge—until and unless it had won credibility. No credibility, no knowledge.

Credibility has indeed been increasingly identified as a fundamental topic for the social studies of science. And the condition for its emergence is just the (partial and local) loss of credibility of the grand old narratives that exempted scientific truth from the need to win credibility. The study of credibility—for those persuaded of Bloor's general point—has expanded to fill the space vacated by the defeat of the grand narratives. So it might be more proper to say that, insofar as we are concerned with scientific knowledge, credibility should not be

1. "Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility. It is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic." [Barnes and Bloor 1982, p. 23]

2. Sociologists of scientific knowledge have long stressed the process of socialization as, so to speak, a "default explanation" of actors' beliefs (e.g., Barnes 1972, p. 272): "The human actor adopts a way of life largely determined by his culture and the position he occupies within it; many of his beliefs and most of those crucial to the acquisition of further beliefs, will be found empirically to have been received in socialization processes. Theories of the socialization process will eventually provide the answer to most problems in the adoption of beliefs."

referred to as a "fundamental" or "central" topic—from a pertinent point of view it is the *only* topic.

The social studies of science is seemingly going through a period of infatuation with topics of this sort, topics that are not special areas of empirical inquiry—the sort of thing you might do if you studied a discipline or a period of time or a specific set of contextual social relations—or special "factors" that bear on scientific knowledge—sources of patronage, the use of instruments, considerations of the use of science in supporting social hierarchy. Rather, what seems to fascinate many of us now might more properly be seen as the presuppositions or necessary preconditions of any possible body of knowledge. So, for example, we have the recently popular study of the "spaces of science" or the soon-to-be-very-trendy study of the "embodied" nature of science—I plead guilty on both counts (Shapin 1988; 1994, chap. 4; Ophir and Shapin 1991). Neither space nor bodies should strictly speaking be regarded as "factors": no space, no science; no bodies, no science. And so too with credibility. Science, like finance, is a credit-economy: these are activities in which, if you subtract credibility, there is just no product left: neither a currency nor a body of scientific knowledge. Skepticism in science is like a run on the currency, and the exact equivalent of the scientific fraudster Elias Alsabti is the Barings barrow-boy Nick Leeson.³

We are urged these days—especially in the history of science, but also in sociology—to rise up above our particularism and to retrieve "the big picture" (see, e.g., Secord 1993), but the picture framed by the unqualified study of credibility is just too big. It leaves nothing out. So, for a focus on credibility to do any particular work, some distinctions have to be made. First, some points of methodological principle can be set out. Then, the problem has got to be characterized in some useful way, and this I want to do by alluding to recent work in the field that can serve as exemplars of the kinds of things we might attend to and how we might get to grips with the problem of credibility in particular instances. Finally, despite the caveats about a picture that is too big, I want nevertheless to conclude by tracing some recurrent patterns that might help us recognize *classes* of credibility-predicaments and the tactics of credibility-management that seem pervasively pertinent to those classes.

3. For relevant history of credit-economies, see Schaffer (1989; 1993); for limits to skepticism, see Douglas (1986) and Shapin (1994, chap. 1); for treatment of fraud and dishonesty in relation to science as a credit economy, see Shapin (1995b).

Maxims of Method for the Study of Credibility

Three points of methodological principle: first, if we say that scientific claims have always got to win credibility, then that makes them like the claims of ordinary life, and like those of other specialized practices that have the task of establishing whether claims are the case or not. This principle means that we can make use of many of the resources and procedures that feature in academic inquiries about other practices. Take, for example, the law. Legal proceedings are all about the establishment or erosion of credibility. In law courts utterances are systematically monitored for their veracity, and, as Augustine Brannigan and Michael Lynch have noted (1987), legal proceedings are framed as explicit inquiries into the veracity of what is said. What is understood to be at issue is not a notion of philosophic Truth but of something like "truthfulness," an adequate assurance about the case on which a verdict and penalty are sufficiently warranted—or, as they said in the seventeenth century, a "moral certainty." If that kind of certainty, and that quality of truthfulness, are adequate for understanding how the law works, then perhaps they are relevant to understanding credibility in science, as in fact they are for such versions of science as seventeenth-century English experimental philosophy.⁴ Lynch has suggested (Lynch and Bogen, in press, chap. 1) the use of the term *truthing* to describe the mundane processes by which credibility is established in the law and similar practices. *Truthing*, with that resonance, suggests the processes of securing credibility without the neon glow induced by verificationist, confirmationist, or similar versions of Scientific Truth.⁵

Although there are formal writings treating the credibility of witnesses, in the main, law-court assessments of credibility derive from inferential practices that flourish in everyday life—including inferences from the standing of the witness and from postural, gestural, and linguistic manner. Again, as Brannigan and Lynch note (1987, p. 116), "there is no jurisprudence describing how such inferences are to be made." The procedures for establishing truthfulness are inchoate; they are not formalized; and, perhaps, they are not formalizable.

4. For historical studies of the link between legal and scientific fact-making practices in early modern England, see, for example, Martin (1992), Sargent (1989), and Shapiro (1969).

5. See, in this connection, Barbara Herrnstein Smith's powerful argument (1994) against both the effective existence and the desirability of transcendental ("objectivist") standards of judgment in the law; and, for the relevance of her arguments to science studies debates over relativism, see Gieryn (1994, pp. 342–45).

Knowing how to recognize truthfulness is knowing your way around a culture. And, as Mary Douglas has repeatedly argued, the procedures by which a culture distributes credibility, like those by which it perceives risk, are so bound up with its moral life that one can give an adequate account of the culture by describing its techniques of credibility- and risk-management: "Credibility depends so much on the consensus of a moral community that it is hardly an exaggeration to say that a given community lays on for itself the sum of the physical conditions which it experiences" (Douglas 1975, p. 238). There is no state of affairs outside the culture that uniquely determines what will be believed is the case within it.⁶

The second point follows from this. In principle, there is no limit to the considerations that might be relevant to securing credibility, and, therefore, no limit to the considerations to which the analyst of science might give attention: The plausibility of the claim; the known reliability of the procedures used to produce the phenomenon or claim; the directness and multiplicity of testimony; the accessibility and replicability of the phenomenon; the ability to impute bias to the claimants or to assess risks being taken in making the claim; the personal reputation of the claimants or the reputation of the platform from which they speak; knowledge of the friends and allies of claimants, including their personal reputation and power; calculations of the likely consequences of withholding assent; claimants' class, sex, age, race, religion, or nationality and the characteristics associated with these; claimants' expertise, including the means by which that expertise becomes known; the demeanor of claimants and the manner in which claims are delivered; minute aspects of the life-histories of those assessing claims and their knowledge of the life-histories of those making them (Shapin 1994, chap. 5). Again, in principle there is no reason why an inquiry into the grounds of scientific credibility might not find itself concerned with the investment portfolios of individual scientists (did Martin "Cold Fusion" Fleischmann own stock in a palladium mine?) or what they eat in the morning (does a medical researcher warning against the risks of dietary cholesterol turn out to eat a "full English breakfast" every day?).

Any aspect of the scene in which credibility is accomplished may prove to be relevant, and the relevance of *nothing* can be ruled out in

6. As later discussion will make quite clear, I do not here suppose that cultures are necessarily homogeneous with respect to their credibility-judgments or with respect to their sense of how credibility ought to be secured. (Nor, of course, does Mary Douglas.) On the contrary, it seems a prudent maxim of method to presume that all cultures recognizable as such nevertheless contain conflicting credibility-managing schemes.

advance of empirical inquiry. From which the third point of methodological principle follows: there should be no such thing as a *theory* of how credibility is achieved, at least in the sense of one of those grand theories that would offer an adequate formula for how it is done regardless of setting and the nature of the case at hand. In any particular case the resources and tactics relevant to the achievement of credibility are likely to be very diverse, and a different array of resources and tactics is likely to bear on different types of case. For that reason alone we ought to be suspicious of simple and global credibility-stories of whatever sort.⁷ Finally, the description or explanation of credibility has got to specify the credibility of *what* and *for whom*: as I will briefly indicate, credibility-predicaments vary in interesting ways according to the nature of the claim and according to the relationship between who claims and who is meant to believe.

Metonymy, Induction, and Risk

One way of getting to grips with the scope of the problem we have in giving an account of credibility is to recognize credibility as embedded within what one might call—with some license—a *metonymic* (or “standing-for”) relationship. At the most basic level, that relationship is evident when I say that “I do all the cooking in my household” and expect you to accept that claim as a fact about me. So all testimony about states of affairs stands in a metonymic relationship to those states of affairs, and the condition of your knowing about these things—otherwise unavailable to you—is your accepting the legitimacy of that relationship. Accordingly, for all the knowledge you have of those states of affairs that you have not yourself experienced you are dependent on some practical resolution of the problem of credibility.

The same is the case when the claims in question have the character of *inferences* from one state of affairs to another. For example, when in 1648 Pascal sent his brother-in-law up the Puy-de-Dôme carrying a barometer, that climb of some 1,000 meters produced a drop in the level of mercury of three inches. In order for this state of affairs to stand for the general phenomenon of the air’s weight—which is what it was supposed to stand for—at least three, and very probably many more, metonymic relationships needed to be credited, apart from the credibility of testimony about the event itself. First, the behavior of mercury-in-glass had to be accepted as standing for the weight of the

7. I have strongly argued (e.g., Shapin 1994, chap. 1) that “trust in people” is an *ineliminable* feature of the credibility of factual claims. Yet I also make a distinction between necessary and sufficient conditions of credibility and acknowledge that the identification of which people will count as trustworthy is scenically variable.

atmosphere above that part of central France; second, that what obtained in this vertical region of space might be extrapolated to stand for the kind of thing that would happen were one to go higher than the Puy-de-Dôme (or lower than the earth's surface); third, that what happened to the barometer then and there would happen, *ceteris paribus*, to other competently designed barometers at other times and places (and to this one on some other occasion). Likewise, when Robert Boyle put a barometer in the air pump and then exhausted the air, its behavior was meant to stand for what would happen were one to walk a barometer up to the top of the atmosphere. Without these metonymic relationships being credited, there would be no philosophic *point* to what was done. In practice, the natural philosopher does not care what happened to this mercury in this piece of glass apparatus on this day and at this place, except as they support inferences to the relatively nonlocal and nonspecific. The local and the specific are not the *point* of these experiments; the philosopher cares, for example, about the *atmosphere* or about the *mechanical nature of the universe*. But in order for specific findings to be *about* the atmosphere or *about* the universe the credibility of these standing-for relationships have to be accepted. Elements in these relationships are not logically connected, and the metonymic connections between them are defeasible in principle.⁸

Similarly, in Trevor Pinch's (1985) work, the detection of surplus Ar³⁷ atoms in a vat of dry-cleaning fluid is meant to *stand for* neutrinos emerging from the Sun, while, in Bruno Latour's (1988, pp. 87–93) account, the result of Pasteur's controlled field-trials on sheep at Pouilly-le-Fort was meant to *stand for* what would happen to vaccine-protected natural populations of sheep spontaneously exposed to anthrax bacilli (cf. Geison 1995, chap. 6). In each case, that to which scientists naturally have, or have worked to secure, effective access is intended to stand for that to which they cannot, or do not yet, have access.⁹ Put

8. For recent accounts of these, and similar, passages of early modern pneumatics, see, for example, Shapin and Schaffer (1985) and, especially, Dear (1990; 1995, chap. 7).

9. Here I can only hint at some differences in sensibility between my account and Latour's important model of the stabilization of claims through enrolling, controlling, and the constitution of "obligatory points of passage." Since I insist on the potential open-endedness of the resources for managing credibility, Latour's presumption of pragmatically maximizing actors strikes me as a bit too schematic and lacking in detail—too much like a global theory. Moreover, the Latourian model accounts for stability without the apparent invocation of a *normative order*. As John Law nicely noted some time ago, economic (and militaristic) models tend to be hollow because of their reluctance to acknowledge actors' pervasive concerns for maintaining the interactional order and the role of such concerns in economies of credibility (Williams and Law 1980, pp. 312–14; cf. Shapin 1995a, pp. 307–9).

another way, that metonymic relationship is a way of pointing to the *scope* of science: scientific claims—only provided they achieve credibility—act as a shorthand for the natural world. Then we forget, or are obliged to ignore, the defeasible metonymic relationship and accept the claims as simply corresponding to the real states of affairs that are their reference and their point. And put still another way, deciding on the adequacy of the relationship between findings and what they stand for is just the problem of induction—impossible to justify in logical principle, routinely solved for all practical purposes a million times a day.

So far I have treated this standing-for relationship in very abstract terms. Yet, as we know, the credibility of that relationship is often a highly consequential and a highly politicized affair. Consider, for example, the phenomenon of *testing*.¹⁰ Donald MacKenzie's (1989; 1990) research on nuclear missile guidance systems notes that no missile has ever yet flown on the north-south polar trajectory it would have to take in the event of United States–Russian nuclear war, and only one missile seems ever to have been fired tipped with a live nuclear warhead. Accepting that the results of east-west test firings were credible versions of what would happen when live missiles were fired north-south—and there were important technical reservations about accepting that—was enfolded in Cold War military and political realities.

Consider also the vast range of testing activities involved in the modern pharmaceutical industry and in environmental monitoring and protection, together with the political and legal apparatuses that are fed by test results and that in turn prescribe the adequacy, pertinence, and reliability of test procedures. Billions of dollars depend on the credibility of clinical drug trials as standing for the efficacy and safety of drugs when administered to nontest populations, and thousands of lives depend on whether trials of AIDS drugs and vaccines are designed—in my colleague Steven Epstein's nice terminology (appropriated from Dr. Alvan Feinstein's clinical medical usage)—in a "fastidious" or in a "pragmatic" mode (1993, p. 421). The political and economic interests mobilized around the credibility of such tests are massive. And those interests are pertinent at practically every stage of test design and reporting, and of policy inferences from those tests.

10. Several of the general points about testing developed here have already been concisely noted by MacKenzie (1989, pp. 411–14) and Pinch (1993), to whose work I am obviously indebted.

Many years ago, in another incarnation, I worked as a jumped-up laboratory technician (in fact, a summer intern) in a United States government agency that the present legal climate discourages me from naming. Our unit was testing a range of chemicals and drugs for possible human mutagenicity and carcinogenicity: tranquilizers, pesticides, cosmetics, etc. For test systems we used a then relatively well-established type of human tissue culture—inspecting the cells, after exposure, for chromosome breakage—and the new Ames test, then being developed at another government laboratory. This used *Salmonella typhimurium* bacteria unable to grow on histidine-free plating medium whose back-mutation to the ability to survive on unsupplemented medium containing the substance under study would signal the production of point mutations. What happened in this sensitive bacterial test system was taken to signal possible dangers in human exposure. Our group was interested inter alia in possible human genetic damage inflicted by tetrahydrocannabinol (the active component in marijuana), LSD—this was, after all, the 1960s—and also caffeine. As it seemed to members of our small cytogenetics unit, the evidence implicating caffeine—in roughly the same concentration reaching your gonads after drinking a cup of strong coffee—was persuasive, while that pointing to the cellular risks associated with smoking marijuana and ingesting LSD was dubious. As it transpired, in subsequent divisional discussions work pointing toward the danger of coffee was deemed at most ambiguous and unconvincing, while the evidence establishing the risks of marijuana and LSD was considered scientifically secure.

Two points: the first is that *absolutely everyone* involved in the discussions—at least at the fairly low levels to which I was privy—understood as a matter of course that there was a congenial credibility-environment for claims about the risks of marijuana and LSD while economic and political realities would work strenuously against the public credibility of claims about the dangers of coffee. Accordingly, agency deliberations about the credibility of the different test regimes were political through and through. The second point is that at no stage in the formal discussions I witnessed leading to this outcome was anything *unscientific* said. Nor need it have been. For there was sufficient “play” between the test situation and possible in vivo effects for relevant skepticism to be expressed about the caffeine metonymic relationship, and, of course, sufficient grounds of confidence in the pertinence of the marijuana and LSD systems. It is proper usage to say that the legitimacy of inductive inference from in vitro to in vivo was

conceded or contested on scientific grounds *and* on political grounds, yet no one was obliged to depart from a recognizably scientific idiom to give politics a grip.¹¹ I offer this anecdote both as a typical late modern instance of the politicization of credibility-judgments and as a warning about simplistic ways of understanding the relationship between political interests and technical judgments.

Furthermore, it would be a mistake to think of test situations as having only *one* outcome whose credibility is to suffer the vicissitudes. Consider the relationship that seems very widely to obtain between the credibility of a claim, on the one hand, and the significance and scope of the claim, on the other. In Pinch's (1985) work, solar neutrino scientists had the option of *choosing* among a number of claims, all of which might be deemed to "follow from" experimental findings. One could say that one had observed "splodges" on a graph—which is practically undeniable but uninteresting—or that one had observed a certain number of Ar³⁷ atoms in a vat of dry-cleaning fluid—somewhat more deniable and interesting—or, finally, that one had observed solar neutrinos—very deniable and very interesting. The series ascending from "splodges" to solar neutrinos progresses along axes—as Pinch says—extending from low to high evidential significance and low to high externality.

What do you say you observed? If you say you saw "splodges," the likely credibility will be high, but the likely interest in such a claim will be low. Moving up the axes of externality and evidential context is to take, and be seen to take, a *credibility risk*—critics can pick away at the gap between elements in the metonymic relationship—but also to bid for rich credibility-rewards. Accordingly, what Pinch in effect offers is a framework for describing the moral economy of risk and reward in the relevant community. And, of course, such decisions can also take place in even more intensely politicized arenas. Sheila Jasanoff's recent work on the U.S. Environmental Protection Agency (EPA) documents the EPA's shift down the axis of evidential significance as it came under increasing pressure of political skepticism in the 1970s—from stating, as she says, "substance X . . . is a carcinogen to giving intricate explanations of the process by which it came to that factual conclusion" (Jasanoff 1992, p. 202). The EPA could secure widespread credibility on the condition that it made its processes of inductive in-

11. I refer above to "formal discussions" because laboratory-bench informal conversation sometimes consequentially contrasted "scientific" to "political" (or "economic") considerations. That state of affairs only tended to change when discussions acquired a more "official" character.

ference publicly visible. It responded to political and economic forces by reducing its own exposure to credibility-risk.

"Authorized" and "Conversational Objects"

In such cases credibility arises in part from actors' *judgments* of risk and rewards, and from actors' beliefs about the credibility-economy into which claims will enter. However, the conditions of credibility also flow—it might be said—from the nature of the phenomena or concepts themselves, or, more accurately, from the environment in which they are produced and in which they live out their careers. The English social theorist Zygmunt Bauman has recently noted that the late modern world is thickly populated with entities about which the authority to speak resides solely with very highly specialized, and very highly bounded, communities. Only certified physicists—and indeed physicists of a certain specialty—can pronounce on the existence and characteristics of intermediate vector bosons, and only very highly specialized astronomers can speak credibly about the existence and characteristics of pulsars:

The matters dealt with by physics or astronomy hardly ever appear within the sight of non-physicists or non-astronomers. The non-experts cannot form opinions about such matters unless aided by—indeed, *instructed*—by the scientists of the field. The objects which sciences like these explore appear only under very special circumstances, to which no one else has access: on the screen of a multimillion-dollar accelerator, in the lens of a gigantic telescope, at the bottom of a thousand-foot-deep shaft. Only the scientists can see them and experiment with them; these objects and events are, so to speak, a monopolistic possession of the given branch of science (or even of its selected practitioners); the monopoly has been assured by the fact that the objects and events in question would not occur if not for the scientists' own actions and the deployment of resources those scientists command; and thus the objects and events are, by the very nature of their appearance, a property unshared with anybody who is not a member of the profession. Monopoly of ownership has been guaranteed in advance by the nature of scientific practices, without recourse to legislation and law enforcement (which would be necessary were the dealt-with objects and events in principle a part of a wider practice and hence accessible to outsiders). Being the sole owners of the experience which provides the raw material for their study, the scientists are in full control of the way the

material is construed, processed, analysed, interpreted, narrated.
(Bauman 1992, p. 71)¹²

And so long as these things are not taken to be, as Hobbes said, "contrary to any man's right of dominion," specialized scientists have massive control of the conditions of their credibility (Hobbes [1651] 1968, p. 166). These objects and events are enormously consequential for late modern social and political action, for they include such things as the composition of the protein coat of HIV, the chemical combinations occurring between CFCs and ozone, and the nucleotide sequence of the gene controlling muscular dystrophy. Without them—or, more precisely, without the actions and understandings mobilized around their credibility—late modern society would not be recognizable as the thing it is. Yet the conditions of credibility of such things depend to such an extent on a certain form of economy that it is tempting to recognize a distinct class of what might be called "authorized objects."

Not all scientific objects have that authorized character and those conditions of credibility. Consider, for example, the concepts and phenomena of much human and social science. The credibility-economy of such notions as "the unconscious," "psychic regression," "having fixations," "being in denial," or "suppressed memory"—just to take some quasi-Freudian locutions—depends, as Graham Richards has effectively argued (1989, pp. 85–90), on the extent to which they are vernacularized and, therefore, the extent to which they actually come to constitute the phenomenal base to which they refer. If people, as it appears many of them now do, make sense of their lives by organizing them with respect to such notions, to that extent, Richards says, human science has regenerated and recreated *human nature*. There was a time when people did not *have* "unconscious reasons" or see things as "phallic symbols." Now they have and now they do. The credibility of such notions is secured on different conditions than those pertaining to subatomic particles and DNA sequences, so that it might make sense to refer to them as "conversational objects." The ability of specialist communities to speak authoritatively about conversationally constituted subjects is circumscribed by the judgments and decisions of the objects of study. Bauman suggests (1992, p. 73) that, for such

12. There are obvious resonances here with Bachelard's ([1934] 1984, p. 13) account of the "phenomeno-technology" of modern science: "The first achievement of the scientific spirit was to create reason in the image of the world; modern science has moved on to the project of constructing a world in the image of reason. Scientific work makes rational entities real. . . ."

reasons, the states of affairs predicated by human and social scientific inquiry have a credibility "handicap"—they can't be authoritatively established in the same way as quarks. Yet it is possible that many notions flowing from expert human and social scientific culture enjoy a credibility *advantage* by virtue of being high-toned versions of locutions already present—in some form and to some degree—in lay culture. To make this point about "conversability" is not merely to pick out some features of *interpretative* human science: arguably, *causal* and deterministic conceptions in human science share the same conditions of credibility, since lay actors also routinely use causal notions to make out their own and others' behavior.¹³

Comparing the Economies of Credibility

In 1982 Barry Barnes and David Edge (p. 233) wrote that as yet, "we know very little about the basis of credibility: the importance of the problem is matched only by its complexity and its comparative neglect." And they warned against those "superficial accounts" and "mythologies" that were best regarded not as attempts to describe and explain the current distribution of cognitive authority but to legitimate it. It was in this connection that the grand narratives of reason, reality, and method were so pointedly criticized. In the social studies of science we have come some way since then. We possess an increasing range of empirical studies of how scientific claims win credibility, and many of us now appreciate the complexity of the task involved in giving an account of that credibility.

Following early work by Yaron Ezrahi (1971), Barnes and Edge (1982, p. 238) suggested a preliminary categorization of the "major modulators of credibility" obtaining in the relationship between experts and the modern public in democratic societies. These included the relationship between scientific accounts and prevailing social and cultural beliefs; the relationship between scientifically generated technologies and prevailing social values; the forms of accessibility of a

13. The scientific "boundary objects" described by Star and Griesemer (1989)—objects whose properties are decided through transactions between actors in several "social worlds"—might possibly be treated as complex intermediaries between my "authorized" and "conversational objects." Also, it should not be concluded that "conversational objects" only populate the *Geisteswissenschaften*: recent scholarship seems to establish their pertinence to the career of medical ontology (e.g., Jewson 1974; Helman 1978; Rosenberg 1992, pp. 9–31), while Dear's (1995) work on early modern natural philosophy draws attention to changing conceptions of "experience"—from that which is commonly available to that which requires authoritative testimony.

specific science to the public; and the extent of expert consensus. Give attention to this list of modulators and you will, Barnes and Edge argued, capture important considerations shaping the credibility of expert claims among the public.

This still seems to be pretty good advice. Take account of these sorts of things and you will probably have a decent chance of constructing a rich and interesting story about the conditions of credibility between experts and laity. However, these are not the only conditions of credibility in which we might be interested, and I conclude with some brief speculative remarks about what might be called the *vectors* of credibility and the credibility-economies that arguably obtain along different vectors.

Up to now in science studies most attention has been paid—as Barnes and Edge indicate—to the *public* credibility of expert claims. And rightly so, for there is no doubt that this vector has the greatest practical significance. Yet credibility has other vectors, and the credibility-economy that obtains between experts and laity may not obtain elsewhere. So, for example, some recent science studies work—including my own (1994)—has been mainly (though not exclusively) concerned with the economy of credibility *internal* to scientific practices, while other work—notably Theodore Porter's (1995)—has focused on the economy of credibility *between* scientific and technical groups in modern differentiated societies. Obviously, the state of credibility-management in one vector bears on credibility-economies in others—and this is why, for example, Ezrahi and others are right to note the pertinence of expert consensus and dissensus to lay judgments. But suppose, for the purpose of stimulating discussion, one makes a speculative stab at some distinctions.

Within such small interdependent groups as the “core-sets” of specialized scientific practices, the economy of credibility is likely to flow along channels of familiarity (cf. Shapin 1994, chaps. 5–6 and pp. 409–17). The practitioners involved are likely to know each other very well and to need each others' findings in order to produce their own. Here, the immediate fate of one's claims is in the hands of familiar others, and the pragmatic as well as the moral consequences of distrust and skepticism are likely to be high. In such social settings the analyst should take care not to explain the achievement and maintenance of mutual credibility too aggressively. In a world characterized by familiarity—whether in lay or expert society—taking each others' claims at face value is *normal*, and it is distrust, skepticism, and the demand for explicit warrants for belief that need specially to be justified and

accounted for. It is, indeed, hard to conceive how small groups of familiar others could long maintain their cohesiveness were the situation otherwise.

At the apparent opposite pole is the credibility-economy obtaining between expert groups and laity. Here, as such social theorists as Niklas Luhmann (1979) and Anthony Giddens (1989) have recently argued, the resources of familiarity for addressing problems of credibility are absent or impoverished. We look instead for formal warrants of credibility—institutional affiliation or standing, the observance of explicitly framed methodical procedures, the display of expert consensus, and the like. Accessibility can cut both ways in such an economy. On the one hand, where we have independent access to the “facts of the matter,” we may be able to use that knowledge to gauge the claims of experts. On the other hand, the representation of expert knowledge as far beyond lay accessibility can serve as a recommendation for its truth. So in *Absolutely Fabulous*, the Sloane Ranger Catriona reads the impenetrable technical prose on a jar of anti-wrinkle cream and concludes: “I don’t know what this means, but it’s forcing me to believe it.”

Then we come to the economy of credibility obtaining *between* expert groups in modern differentiated societies. This is the world so ably and richly described by Theodore Porter (1995). It is an economy in which, if shared belief is to be secured and maintained, it must travel great distances—in both physical and cultural space. This economy also substantially lacks the resources of familiarity, while it possesses an array of inducements to distrust and skepticism. Here a major recommendation for belief is the public display of the *discipline* to which claimants and their claims are subject. If I *can* impute bias and interest, and if that imputation does not produce moral and pragmatic disaster, why else should I believe you except because of a convincing display of the disciplining of bias and interest to which you have been subjected? It is in this connection that the language of quantification and of method has its consequential task in the making of credibility. “Quantification,” Porter writes, “is well suited for communication that goes beyond the boundaries of locality and community” (1995, p. ix). I am sure this is quite right—indeed, I have tried to develop parallel arguments about the language of disinterestedness in both seventeenth-century experimental philosophy (1984) and early nineteenth-century cerebral anatomy (1979)—while reference to “communication beyond the boundaries of community” puzzles. Wherever one has shared knowledge, there, I would suggest, one has a form of “community” where distrust—unlimited in principle—has in fact reached a *conventional limit*. Why *accept* the language of quantification as an ade-

quate solution to problems of distrust, since in principle further distrust is always possible?¹⁴

The world of "community" is not, perhaps, that readily circumscribed. So I am not convinced that the social theorists' description of the economy of credibility between experts and laity is adequate. If I ask myself how it is that I came to believe a whole range of expert scientific and technical claims—probably the majority of those whose truth I now accept—I discover that I was told these by familiar others, often in a setting of face-to-face interaction: by people whose names I know (or knew) and whose characteristics I know (or knew)—teachers, professors, physicians, nurses, plumbers, mechanics, colleagues. Moreover, it is well to remember that experts commonly stand in something like the position of lay members with respect to the claims of *different* expert groups. The resources of familiarity are not so easily dispensed with, even in the late modern world, and even with respect to the credibility of esoteric scientific claims.

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14. The fact that I now happen to accept certain quantitatively expressed claims about the relative safety of air travel, or the yield of investments, may indeed have something to do with my sense that statistical propositions are more reliable than anecdote or assertion. After all, I spent many years acquiring that sense, in educational institutions, coached (and occasionally coerced) by the embodied authority of teachers, training me to accept the "just-so-ness" of numerical manipulations. The credibility of such claims also proceeds from the contingent fact that I am currently unable—lacking the necessary expertise—effectively to discern bias in specific modes of statistical inference or in the presuppositions on which specific data sets were assembled. Yet an item in tomorrow's newspapers may convince me that *these* quantitative claims were biased and misleading, thus potentially feeding a general sense that numerically expressed claims may be no more reliable, disinterested, or objective than anecdote. See also Gieryn (1994, pp. 333–35) for a utopian element in Porter's otherwise excellent account of the relationships between quantification, democracy, and the disinterestedness of accounts.

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