BOOK REVIEWS

Science by Worst Cases

The Golem. What Everyone Should Know About Science. HARRY COLLINS and TREVOR PINCH. Cambridge University Press, New York, 1993. xii, 164 pp., illus. $19.95 or £10.95.

This book is an attempt by two well-known sociologists to reveal to the utterly unsuspecting general reader some of the true workings of science. Collins and Pinch have made their names as brave explorers of frontier and fringe scientific claims. Their methodological position is one of radical symmetry: failed or dubious scientific claims should weigh as heavily in our view of science as successful ones. Their interviews with proponents and opponents in current controversies have provided valuable insights and shown that scientific judgment is indeed a complex affair.

The Golem contains a set of case studies, most of these well known to the science studies community, here rewritten for a popular audience. This slim volume builds its argument out of the following scientific episodes: memory transfer in planarian worms, the relationship between theory and experiment in relativity theory, cold fusion, Pasteur's germ theory, gravitational radiation, the sex life of whiptail lizards, and missing solar neutrinos. An immediate question arises concerning this particular selection of cases: how representative are they of science in general? We are told explicitly that conclusions about individual cases also apply to science as a whole; this claim is reinforced in every chapter.

In view of this, a reviewer may as well stay away from the details of particular episodes, however charming or provocative, and ask instead, What is the gist of the game being played in books such as this? The reasoning seems to be that if it can be shown that scientists for various philosophical or other reasons "cannot" come to agreement on the basis of facts or experimental results, then it "must" be social factors that are actually guiding the production of scientific knowledge. We have here nothing less than a battle between the Two Cultures: at stake is the potential victory of sociology over experimental science!

Does this all now mean that an impending doom, a sociological golem, is hanging over science? Let us examine more closely the sociological hubris involved. For the reader to buy into the book's central argument (that facts cannot settle controversies), he or she would have to trust Collins and Pinch's accounts of what "really" happened in the scientific episodes ("We are simply going to describe episodes of science. . . . We are going to say what happened" [p. 2]). It is hard to see how these stories can avoid becoming facts themselves, constantly appealed to by the authors. Paradoxically, the same persons who tell their readers that facts cannot settle scientific controversy seem to be using the very method they dismiss in citing these stories to boost their own case within the framework of an ongoing controversy in the sociology of science. (Not all sociologists share Collins and Pinch's sociological reductionist views.)

The authors say that they "have tried to level out the scientific mountain range which rises up as a result of the forces of celebratory history" (p. 141). Does this mean that the supposedly well-established body of scientific knowledge is as contestable as the claims about the sex life of whiptail lizards? This is, indeed, what the authors urge the reader to believe. What passes for established science is inherently contestable, it is based on mere "agreement." To illustrate this (under the subheading "science education") the authors bring up the following example: schoolchildren get different results measuring the boiling point of water. This is just like frontier science: look, scientists also get different results! And the teacher then helps the children reconcile their results, "transmuting the clumsy antics of the collective Golem Science into a neat and tidy scientific myth" (p. 151). What we are not told is that scientists have succeeded in exactly specifying the boiling point of water because there is an objective reality corresponding to it: the case is closed, as any tea drinker knows. Nor are we told that we rely on exact measurements of regularities in the real world in order to be able to build such things as the Challenger (whose disaster the authors do not fail to bring up as an example of failure of *science*—not of technology or of organizational factors).

Collins and Pinch argue that "matters of fact are inseparable from the skills of the scientist used to produce them" (p. 116). But does this really demonstrate, as they seem to believe, that facts have nothing to do with scientific agreement? What is most important for scientists is for their results to hold up in the long run, that is, for them to be *right*. This is what the whole scientific endeavor is geared toward. No wonder that scientists are constantly assessing one another's experimental skills and overall credibility: it would be embarrassing and unproductive to lean on or defend claims that are likely to be incorrect. If the aim of all well-documented, seemingly "extra-scientific" talk is that scientists be as right as possible in a particular case, then the nature of scientific agreement is not purely cultural at all: it is connected to views of how things "really" are and the best way to portray this. Surprisingly often, and despite all the intricacies documented by Collins and Pinch, scientists are right. Why is this? It seems oddly obstinate for sociologists to tell readers that "Nature poses much less of a constraint than we normally imagine. . . . Science works the way it does, not because of any absolute constraint from Nature, but because we make our science the way that we do" (p. 138).

The authors in effect exclude Nature as a factor in scientific knowledge, in favor of Culture. One reason for this extreme position may be a (misconceived) interpretation of the professional interests of sociologists. Another appears to be a vague political goal: a wish to "democratize" science by exposing it as nothing but negotiation and inviting the layperson to be constantly skeptical of scientific experts. The authors make an explicit connection between their epistemological and moral and political position at the very end of the book: "Contested forensic evidence is like contested scientific evidence everywhere; it is like the science described in this book. It is contestable" (p. 147). But it is not clear that it would be advantageous for the public to cultivate the systematic distrust of science Collins and Pinch urge, because then also important, well-founded criticism may be brushed aside. Here one would have liked to see a more thorough discussion of the relationship between (different types of) science and politics.

In genre, this book represents a point near the exhaustion of a paradigm of science-bashing that has flourished since the early 1970s or so. This paradigm has yielded important insights, but it is now at a point where sweeping claims are backed up by a relatively small set of selected case studies that are cited over and over (perhaps in this way being made "harder"-sounding as facts?). The question remains, Why are these unyielding cases presented as representative of science: solar neutrinos instead of the (indisputable!) splitting of the atom,

memory transfer in worms instead of the discovery of DNA as the material of heredity, and cold fusion instead of superconductivity? The strong way to prove the point Collins and Pinch are concerned to make would be to take a seemingly obvious, or quickly resolved, case and show that it was, after all, Culture alone that was responsible and that Nature had nothing to do with it. I am not really recommending this, of course, since I do not think it will help us better understand science. Epistemologically, it is high time to turn the question from What's wrong with science? to How come science works at all? This would seem more fruitful than the continued fight against an idealized picture of science that nobody really holds any longer.

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American Plants

Flora of North America North of Mexico. Flora of North America Editorial Committee. Vols. 1 and 2. Oxford University Press, New York, 1993. Vol. 1, Introduction. xiii, 372 pp., illus. $75 or £40. Vol. 2, Pteridophytes and Gymnosperms. xvi, 475 pp., illus. $75 or £45.

Botanical workers yesterday and today. *Top*, Marcus E. Jones and N. L. Britton. *Bottom*, Laurie Lang drawing ferns and Deborah Kama demonstrating TROPICOS to Bruce Parfitt. [From *Flora of North America North of Mexico*, vol. 1]

The seeds of this project, so to speak, were first sown in 1965, perhaps as a result of North American botanists' seeing the first volume of the now-complete *Flora Europaea*. If the Europeans could cooperate successfully on such a collective venture, why couldn't the North Americans? The first incarnation of the *Flora of North America* (*FNA*) project incubated for several years, then died in 1973 when a squabble between the National Science Foundation and the Smithsonian Institution resulted in neither organization's offering financial support for the project. Two subsequent attempts to revive the project in association with the Man and the Biosphere project and the National Park Service also failed. The fourth and last revival was initiated in 1982, housed at the Missouri Botanical Garden, and for the first four years was supported by the Garden and other participating institutions; in 1988 external funding was secured and the project acquired a sound financial base.

The introductory volume to *FNA* is a free-standing one containing essays on diverse topics related to the flora and vegetation of North America and the history of the development of botanical knowledge of the continent. The second volume, which covers pteridophytes and gymnosperms, is the first of a projected 12 volumes that will describe the vascular plants and bryophytes that grow "naturally" in the continental United States, Canada, Greenland, and the French St. Pierre and Miquelon islands. Thirty collaborating institutions in the United States and Canada are involved, as are "hundreds" of botanists; this binational effort should be completed in 12 years.

The first volume contains an interesting account of various individuals important in developing current knowledge of the North American vascular flora (why equally colorful bryologists are omitted is unexplained). Portraits of many of these individuals are included, drawn from the extensive collection of the Hunt Institute. These portraits bring this history to life. Marcus E. Jones, horsewhip and plant press in hand, covered wagon in the background, wears a facial expression signaling his "defiance" that helped end Harvard botanist Asa Gray's "domination" of western American botany. Jones's visage shares a page with that of easterner N. L. Britton, placidly seated in his herbarium but no less an antagonist of the taxonomic tradition of Gray. The colorful Reverend E. L. Greene is credited, along with Jones, with ending Gray's domination of western botany, yet otherwise the two men were hardly allies. In his obituary of Greene, Jones referred to him as "the pest of systematic botany." The authors lament the lack of a biography of Greene, this "western dissident," yet he is well served by the perceptive accounts of him as a person and as a botanist in *Landmarks of Botanical History* (1983) edited by F. N. Egerton.

The circumscription and sequence of flowering-plant families in *FNA* will follow those of A. Cronquist, who provides an interesting commentary on his widely