Genius in a bottle

Augustine Brannigan

Changing Order: Replication and Induction in Scientific Practice. By H.M. Collins. *Sage: 1985. Pp.187. Hbk £20, $25; pbk £9.95, $12.50.*

COLLINS argues that facts are like model ships in glass bottles: the ships are bits of knowledge and the bottles are truth, and once the ships have been assembled, they look like they've always been there and could never come out again. His task has been to study how shipbuilders — the scientists — construct and deconstruct what goes in and out of science. He focuses on the reconstruction of the TEA laser, the gravity waves controversy and disputes over the claims of paranormal phenomena. The work is based on interviews and observations of researchers in each of these areas over the past decade or so, and most of it has already appeared elsewhere. This volume highlights the similar patterns of reality construction and the way conceptual order has been achieved in these areas.

Like most British social studies of science, Collins's approach is brazenly relativist. Order is achieved not because nature dictates what theories work and which do not, but because the language, models and concepts of science shape how we investigate and experience the world. While this is not new, Collins's work is attractive because he focuses on one of the cornerstones of objectivism: the actual process of replication in science. This is crucial since it is argued that the replication of an effect recommends the independence, the objectivity of the finding. For Collins, what is intriguing is what *counts* as a replication (or falsification) can be very controversial. Generally, unless one is attempting to disconfirm a hypothesis, there is little incentive in exact or identical replication. Also, since publications never spell out (and arguably cannot spell out) all the contingencies in experimental research, replication failures are open to challenge for not being identical. This raises the experimenter's regress:

> What the correct outcome is depends upon whether there are gravity waves hitting the earth in detectable fluxes. To find this out we must build a good gravity wave detector. . . . But we don't know that we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until . . . and so on *ad infinitum* [p.84].

The most compelling chapter describes the replication of the TEA laser by Robert Harrison (then at the University of Bath) with, we gather, the amateur assistance of Collins. It demonstrates that the replication did not follow an algorithm of inference (nor does induction generally); that the expertise acquired in creating something the first time through is largely tacit and marked by trial and error; that knowledge transfer in publications is quite incomplete; and, finally, that when the procedure works or the model crystallizes the previous sequence appears immediately to be forgotten and the account of how the discovery was made is reconstructed as if it followed an algorithm or recipe, with the experience of contingency and trial and error written off as "human" error. Induction becomes idealized like the ship in the bottle. Science education fosters this picture by directing attention exclusively to completed ships, and ignoring the first-time-through experience.

Change occurs in the relativist world by the construction of novel conceptual systems. These must have an empirical basis, and a conceptual plausibility, yet for Collins what could count as proof is not dictated by a direct grasp of nature, but the negotiation of claims in the institutional or conventional framework. "What counts" is mediated by preferences for different methodologies, different procedures for data analysis and the reaction of the "core set", the cluster of relevant experts. The credibility of a candidate discovery is negotiated through various replications and the scientific community's use of the innovation. Nature does not limit unambiguously what counts as fact, for the relevance of empirical observation is already imbued with constructs from previous theories. Even the checks on experimenter regress are problematic. For example, the calibration of the sensitivity of gravity wave detectors by introducing electrostatic stimuli to measure their responsiveness entails *a priori* assumptions about the properties of the waves which equate them with other forms of electromagnetic radiation, which, for Collins, puts "constraints on [Weber's] freedom to interpret results" (p.105). However, I think it must be conceded that the acceptance of some such conventions provides positively for the accreditation of evidence, even for, and perhaps especially for, the relativist. The suggestion that gravity waves could have survived beyond their demise in 1975 by use of *ad hoc* hypothesis, and that this would be consistent with the spirit of science, while the destruction of them by failures to replicate was somehow motivated by non-scientific intrigues is not a proposition that is likely to receive much sympathy.

The book gives a vivid sense of the contingent nature of research and is generally a good read, though Collins frequently has a penchant for irreverence and seems to be writing for an introductory audience. My main misgiving surrounds his view of the extra-scientific factors which influence change in science:

> For scientific culture, the mediating role of the core set, its laundering of 'illegitimate social interest', and its transubstantiation of social contingency into methodological propriety, along with its privacy, explain the paradox of reification [p.145].

In my view, the core set of experts is unfairly cast as a sort of irrational masonic lodge. No particularly strong evidence is advanced to support this view (though I fear we are into the experimenter's regress?) Also, the idea that illegitimate factors influence the adoption of innovations is not established by discussion of the attribution of character, competence and credibility in the weighing of evidence.

Finally, it seems odd for a relativist to complain that some fact is a reification. This appears to betray a subterranean objectivism, for whatever else could facts be for relativists but reifications? Though the first five chapters are at times brilliant, the final chapter, which deals with the theoretical crux of the matter, how change is accomplished, does not quite get all the pieces into the bottle. □

Augustine Brannigan is Associate Professor in the Department of Sociology, University of Calgary, Alberta T2N 1N4, Canada.

New in paperback

Time's Arrows: Scientific Attitudes Toward Time, by Richard Morris. Publisher is Touchstone, New York (an imprint of Simon and Schuster), price is $8.95. The book was reviewed by David Park in *Nature* **314,** 687 (1985).