

Book Reviews

A General Economics

Stealing into print: Fraud, plagiarism, and misconduct in scientific publishing. By MARCEL C. LAFOLLETTE. Berkeley and Oxford: University of California Press, 1992. Pp. viii, 293. \$30.00. ISBN 0-520-07831-4. JEL 93-0778

"Now the klieg lights are on," writes Marcel LaFollette. "What began as private discussions among professionals stands unshielded in the spotlight of national politics" (p. 2). Her book, which has made a considerable and justified splash, surveys the scandals in science that have brought camera crews and congresspeople pushing into the lab—or rather into the editorial office, her focus being on scholarly publishing, especially journals. LaFollette does not believe, as claimed by the New York Police Department and by the shills for big science, that fraud is a matter of a few bad apples. She offers what amounts to an economic explanation: the gains from big science are so big, and the number of people involved so large, that comity has broken down. Can economics itself be far behind?

"Fraud occurs when an author, editor, or referee makes a false representation to obtain some unfair advantage or to injure deliberately the rights or interests of another person or group" (p. 41). The NSF estimated recently that 2,000,000 scientific articles are published annually in 200,000 journals. The detection of fraud is therefore difficult, and difficult for the scientists to believe: "the community often engages in wholesale denial of the problem's significance" (p. 2ff). The denial stage is chronicled by LaFollette in embarrassing detail. Time and again the establishment figures in science, and often the very journals *Science* and *Nature* (denying most vehemently), have had to eat their words. As the journalists William Broad and Nicholas Wade put it, science has a "rotten barrel."

LaFollette makes a useful distinction between the whistleblower—an insider who cries

foul—and a new breed, the nemesis, an outsider to a research team who exposes its fraud, "attempting singlehandedly to punish unethical behavior through dogged publicity." The pair at one of the National Institutes of Health, Walter Stewart and Ned Feder, is a doublehanded version of Nemesis. We could use a few of them in economics.

The book is compulsively readable, a gripping story filled with news from the science wars. LaFollette's source and to some degree her subject is the science press, which she covers in a rhetorically sophisticated way. The implied audience for the book is someone outside scholarship, or someone just beginning, who will be surprised that referees do not double check everything in a submitted paper or that journals do not certify as true and original everything in a published paper. (And yet, as LaFollette notes with astonishment, when the *Journal of American History* in 1986 stopped checking "the accuracy of every footnote citation in the Indiana University Library" it *apologized*. Economists take note of what real scholarship means.)

LaFollette does not offer much in the way of closure for the problem of fraud—although she can hardly be blamed for not solving a problem that lies at the heart of science as a community. No economist would object to her mild suggestion that "Mistakes or errors within the system may simply represent a price paid for a free market in scientific information" (p. 4). LaFollette observes that a routine of blind refereeing is recent in all the sciences (the routine started in sociology, introduced a quarter century ago to increase the objectivity of reports). She quotes a chemist suggesting we initiate nonanonymous refereeing: "If science is really supposed to be the most honest game in town then why not referee our publications above board?" (p. 128). But she rejects the suggestion for a "journal centered data audit" on grounds that do not seem persuasive. After all, in economics we have become alerted to the problem

adopted self-interest as a guide to morality. If there is progress, a lot of the naughty behavior is going to be in aid of arguments that in the end prove mistaken, such as Kelvin throwing his weight around in an attempt to squash Darwin. Or worse, such as Karl Pearson inventing neopositivism and eugenics in the same program. So fraud, with other scientific misbehavior, is not eliminated merely by *being scientific*.

In charging or defending fraud the scientific establishment has misbehaved often enough to suggest a generalization. "Power tends to corrupt" is a catchy way to put it. The Nobel laureate David Baltimore attempted in 1988 to ruin the reputation of a whistleblower from his laboratory (he later apologized). The rhetoric Baltimore deployed is that she was a publicity seeker, a charge the establishment always uses against inconvenient people. Yet as LaFollette points out, "Baltimore himself turned out to be a skillful manipulator of press coverage" (p. 153). The recent furor over cold fusion brought out the worst in the establishment physicists, who were granted free access to the columns of the *New York Times* for making lengthy complaints about the uses of publicity by Pons and Fleischman. (The episode is not discussed by LaFollette, because fraud was not the issue; if you think it was an open and shut case of bad science, read Eugene Mallove's book *Fire from Ice*.) Note well: even the first-rate, or maybe especially the first-rate, scientists engage in rhetorical thuggery. They get accustomed to being believed every time they open their mouths. The temptation to cash in the rhetorical capital is hard to resist. The mathematician from Yale (a good example of a nemesis figure) engages in a witch hunt against a political scientist nominated for the National Academy. Where have you seen such behavior in economics? Being Scientific, no more than Getting Referee Reports, is no guarantee of virtue in science.

And yet, says the editor of the *New England Journal of Medicine* (Arnold Relman, who comes across as an establishment mensch), "the whole system is based on trust. You can't run science the way you run a criminal investigation" (LaFollette, p. 127). That's right. LaFollette points out that

in retrospect, it may seem remarkable that no one questioned [John] Darsee's phenomenal rate

of production [as a researcher in cardiology in 1981 at Harvard Medical School] or asked to see his data, but such a level of trust among co-workers is normal and, indeed, encouraged among scientists. (p. 9)

The philosophers Stephen Toulmin, Rom Harré, Hilary Putnam, and others have concluded that scientific truth is a matter of persuasion within a speech community: "Knowledge claims," writes Harré, "are tacitly prefixed with a performative of trust" (Harré, p. 90).

The trust means that science is a matter of human persuasion. But the persuasion in science, like the persuasion in the economy, is not self-correcting, if the notion is that we can rely entirely on greed without attention to love. Many economists miss the point, and have missed it since Hobbes. As Adam Smith understood, an economy that gets 90 percent of its fuel from greed nonetheless requires the other 10 percent to warm the market, the other 10 percent which he called in his other book "moral sentiments." So too in science.

The implications for graduate training are not that we need courses in ethics (a while ago the Harvard Business School was given 20 million for such purposes, all the ethics that money can buy). We need to expose students to standards of scholarly integrity harder to meet than "Have a career." That's going to be hard for the science of selfishness, where doing well is identified with doing good. An essential part of the good in science is a public provision, on which the baddies find it easy to free ride. Perhaps it's time to start thinking about the free riding on integrity in economic science. Start with LaFollette and keep going.

DONALD McCLOSKEY

University of Iowa

REFERENCES

- COOLEY, THOMAS F. AND LEROY, STEPHEN F. "Identification and Estimation of Money Demand," *Amer. Econ. Rev.*, Dec. 1981 71(5), pp. 825-44.
- DEWALD, WILLIAM G.; THURSBY, JERRY G. AND ANDERSON, RICHARD G. "Replication in Empirical Economics: The *Journal of Money, Credit and Banking Project*," *Amer. Econ. Rev.*, Sept. 1986, 76(4), pp. 587-603.
- FARLEY, John and Geison, Gerald L. "Science, Politics and Spontaneous Generation in Nineteenth-Century France: The Pasteur-Pouchet Debate," *Bulletin of the History of Medicine*, 1974, 48, pp. 161-98.

- HARRÉ, ROM. *Varieties of realism: A rationale of the natural sciences*. Oxford: Basil Blackwell, 1986.
- MALLOVE, EUGENE F. *Fire from ice: Searching for the truth behind the cold fusion furor*. New York: Wiley, 1991.
- MARKS, JONATHON. "Scientific Misconduct: Where 'Just Say No' Fails." *American Scientist*, July-Aug. 1992, 81, pp. 380-82.
- THOMAS, LEWIS. *Late night thoughts in listening to Mahler's ninth symphony*. New York: Viking Press, 1983.