BOOK REVIEWS

Travails of Publishing

Stealing into Print. Fraud, Plagiarism, and Misconduct in Scientific Publishing. MARCEL C. LaFOLLETTE. University of California Press, Berkeley, 1992. viii, 294 pp. $30.

Just after I started writing and lecturing about scientific fraud, one of my colleagues at Caltech, a famous and much-honored scientist, came to see me about the matter. "Look," he said, "all that stuff you talk about, plagiarism, authorship problems and so on, that's not really important. The coin of the realm is scientific truth. If you fake data, you're counterfeiting the coin. That's a serious crime. The rest of it is just a matter of who's been handling the coin. It doesn't matter all that much."

Stealing into Print by Marcel LaFollette is a book written from the opposite point of view. Although subtitled "Fraud, Plagiarism, and Misconduct in Scientific Publishing," the book is not merely about that subset of scientific misdeeds that has to do with publishing. LaFollette covers the whole gamut of scientific misconduct, but she seems to think the really important kinds are the ones that show up in publishing. "Plagiarism," she tells us at one point (characteristically quoting someone else) "is at least as serious a misconduct in science as outright fabrication of data, if not more so." In another place she cites a journal editor with a taste for medical analogy classifying cases of scientific fraud as " 'benign' (e.g., loose authorship or repetitive publication), 'offensive' (e.g., selective presentation of data), and 'malignant' (e.g., plagiarism)." So much for the coin of the realm.

Nevertheless, the book is comprehensive. All points of view are represented, and the cataloging of types of misdeed is so extensive it includes exciting new forms of scientific misconduct I had never even thought of, much less committed. The documentation is massive. LaFollette seems to have read every article ever written about the subject (except mine). There are some 67 pages of notes and bibliography to go with just over 200 pages of text. And all this on a subject no one really knows much about. For example, the first and most important question about scientific fraud is whether it is very rare (as most scientists tend to believe) or quite common (as some journalists and a few congressmen seem to suspect). LaFollette tells us early on that she doesn't know the answer any more than anyone else does.

The book is written in clear, jargon-free prose, that, however, only occasionally gets us turning pages with a vivid story. In one, a 19th-century prize-winning medical essay was plagiarized, and translated into French and won another prize. That must have been some essay. Much later in the book there is a tale of a referee who (after a month or so) returned a paper saying he was working on the same subject. Out of the incident a priority dispute developed, which led to an investigation, which in turn concluded that the referee had actually stolen ideas and information from the manuscript. *Science* became embroiled in the dispute because it published the referee's article, even though by then questions about it had already surfaced.

LaFollette's prose also rises to something approaching emotion when she writes about the virtues and travails of her real heroes, the editors of journals. LaFollette, who is associate research professor of science and technology policy at George Washington University, was at one time editor of a journal called *Science, Technology and Human Values*. While there, she almost had to grapple with the question of whether to publish a notorious article by Walter Stewart and Ned Feder, the self-appointed fraud squad at the NIH (her dilemma was resolved when *Nature* finally agreed to publish the much-revised manuscript). The heart of the book is in fact about the science publishing industry (some 40,000 journals, worldwide!), its structure, its economics, its customs and practices, all examined from the point of view of fraud and misconduct. The system of peer review in all its various forms is described at length. Separate categories of misdeeds are introduced for authors, for referees, and (alas) even for editors. Nothing seems to have been omitted.

And yet, for all the book's comprehensive nature, it seems to me LaFollette has omitted the most important problem I know of concerning misconduct in scientific publishing. It works, more or less, like this: There is a journal (every physicist will know instantly which one I mean) that is so prestigious that many researchers feel their jobs or their grants depend on their publishing at least one article a year in it. It is prestigious precisely because it rejects a majority of the papers submitted to it. The editors of this journal are very far from being the romantic, avuncular figures LaFollette evokes. In fact, I doubt very much that they ever actually read any of the manuscripts submitted to them, any further than is needed to determine what pool of referees to send them to. They don't read them because they are greatly overworked and because nobody can understand more than a tiny fraction of the articles published by the journal, much less those submitted to it. Nevertheless, and in spite of being fully aware of the stakes of acceptance and rejection, they stoutly (and hypocritically) maintain that, aside from scientific validity, the criterion for acceptance is that the article be novel and urgent enough to be of interest to the journal's broad readership.

The referees must therefore make an ambiguous, not entirely scientific judgment in a high-stakes game in which the authors are usually known personally to them and are often competitors. Furthermore, the referee knows that the editor will not understand the technical details of the report that will be written. If the judgment is wrong or unfair, only the author will know, and the author won't know who wrote the report. The referee can depend on the editor's protection and support even if the review is guided by self-interest, professional jealousy, or other unethical motives, because the referee's unpaid help is essential to the editor and the author of a rejected manuscript has an obvious motive to be disgruntled. Referees are never held accountable for what they write, and editors are never held accountable for the referees they choose. For all of this to work, the referees would have to have impossibly high standards of ethical behavior, but nearly all referees have had their ethical standards corroded by themselves being victims of unfair referees' reports in the past when they were authors. Any misconduct that occurs under these circumstances is certainly committed by the referee, not the editor, whose behind is well covered. Nevertheless, the editors have managed to create a system in which misconduct is almost inevitable. Obviously, a very similar description can be made of many journals in other fields, and even of peer review of research proposals. The point is not that the editors (and contract officers) are bad people, but that peer review does not work when it is put under extreme stress, and these days everything is under stress. And yes, to answer the number one question I raised earlier, on the basis of anecdotal evidence and my own personal experience I think misconduct does happen, quite a lot.

The omission of this kind of systemic, or

collective, misconduct is, in its way, characteristic of LaFollette's book, which is more concerned with listing, enumerating, and cataloging than with producing new analysis or insight. Nevertheless, there is no doubt the book is valuable, for its bibliography among other reasons. The bottom line on it is this: I intend to use it next spring in the research ethics course we have recently begun teaching at Caltech. I'm sure it will be used in other such courses that are blossoming around the country.

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Technological Intuition

Engineering and the Mind's Eye. EUGENE S. FERGUSON. MIT Press, Cambridge, MA, 1992. xvi, 241 pp., illus. $24.95.

I recently sat in on an undergraduate engineering design course. Twenty students, mostly seniors majoring in engineering, were to design and build a walking robot. Most of them found the project very difficult. They could use mathematical techniques and computer programs to determine the details of motion and structure, but they had a hard time coming up with the design, imagining how the machine should work. They knew how to analyze, but not how to synthesize.

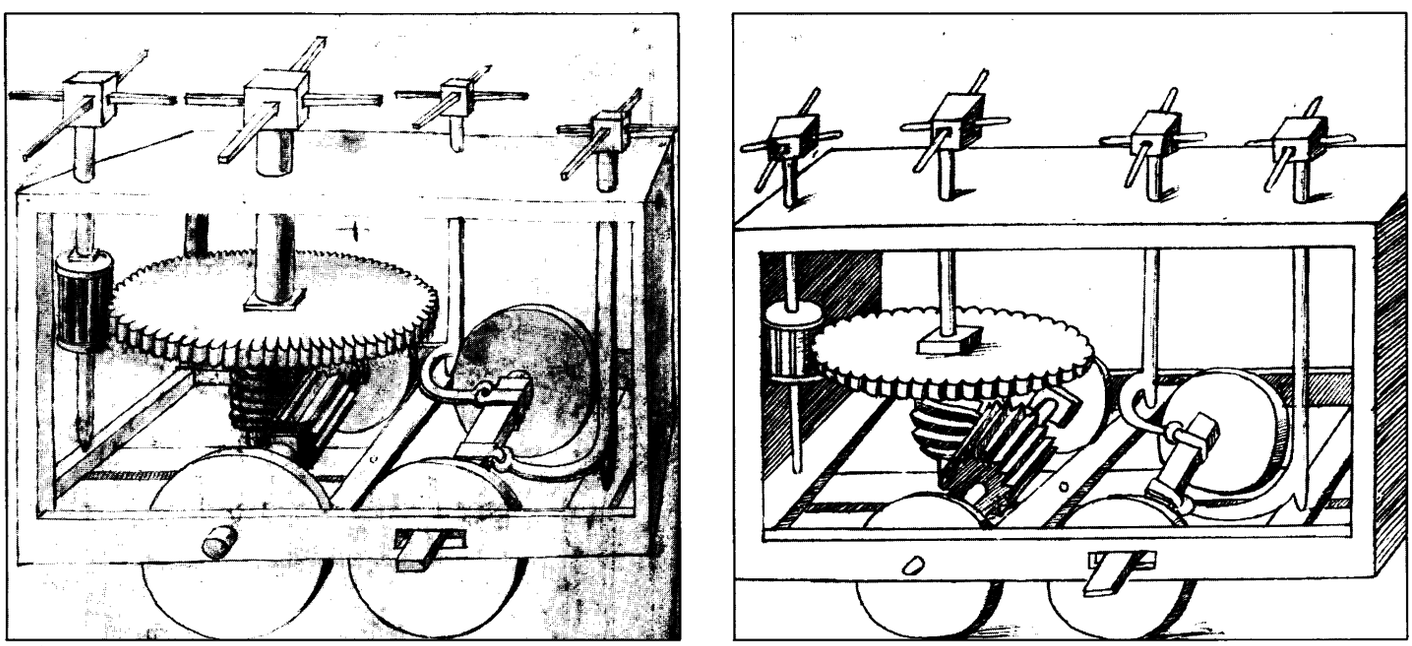
Eugene Ferguson, emeritus professor of the history of technology at the University of Delaware, explains why this is. Engineering education, he argues, has lost sight of the true nature of technological work. Engineering is not a scientific discipline. It is closer to art than science. It is non-verbal, creative, physical, and intuitive, based on experience of the real world, not on equations borrowed from the scientist.

A good engineer must have an "intimate, firsthand, internalized knowledge"—an "intuitive sense"—of technology. The only way to get this, says Ferguson, is to gain a "tactile and muscular knowledge" of moving machinery, materials, and fabrication processes. Students should visit factories and construction sites and get their hands dirty. But today, "engineering schools teach contempt, not admiration" for the people who actually build things. The 1952 Grinter Report on Engineering Education, for example, recommended that courses that taught skills or engineering practice be eliminated and replaced by courses in "engineering science." The move away from the real world was reinforced, Ferguson suggests, by the increased use of computers in engineering.

"By the 1980s," Ferguson writes, "engineering curricula had shifted to analytical approaches, so visual and other sensual knowledge of the world seemed much less relevant." He claims that the ensuing loss of "sound judgment and an intuitive sense of fitness and adequacy" and its replacement by engineering science has been responsible for many recent engineering failures, from the collapse of the Hartford Coliseum in 1978 to the myopia of the Hubble space telescope. "The successful design of real things in a contingent world," he writes, "will always be based more on art than on science."

This might seem the nostalgic screed of an curmudgeonly old-time engineer, upset with the rising prestige of science and the declining reputation of engineering. Be that as it may, the arguments throughout are based on some excellent history, a lifetime of thinking about technology, and a sophisticated, thoughtful, and provocative analysis of the nature of engineering.

The strength of the book is its analysis of engineering thought. The first step in design is visual, tactile thinking. Ferguson describes the extraordinary powers of visual imagination of great engineers. Elmer Sperry, for example, could visualize his gyroscopes as though they were hanging in the air in front of him. This visual facility depends on hands-on knowledge. "My fingers," wrote Walter Chrysler, "were an intake valve through which my mental reservoir was being filled."



A demonstration of the hazards of copying technical drawings. At left is an original drawing of a design for a carriage by Francesco di Giorgio, around 1470. At right is a copy made in the 1540s by a Sienese artist who was trained in the copying of such drawings. "Francesco's carriage was to be steered by moving the axle whose rectangular ends slide in slots on the near side of the carriage (and presumably on the far side also). The near half of Francesco's movable axle is attached to the J-shaped end of a capstan's vertical turning shaft. Note carefully the attachment at the end of the J, a loop fitting loosely in a loop attached firmly to the axle. The attachment on the far half of the axle is also a pair of loosely fitting loops. On the other hand, the copy shows two solid attachments instead of loosely fitting loops. Francesco's arrangement was clumsy, but it would work; the copyist's version would not." [From *Engineering and the Mind's Eye*]