Review of
"Prize Fight: The Race and the Rivalry to be the First in Science"
by Morton A. Myers

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The title of this entertaining and well-written book characterizes science in terms of two clashing sports metaphors: as a boxing match and as a track meet. But an even more apt title might have been one used for one of the sub-chapter headings: "Scientists Behaving Badly." The initial impetus for this work arose from one of the most visible and notorious conflicts in recent years, over scientific credit for the development of MRI as a major new medical technology. Myers, a radiologist at SUNY Stony Brook, had a ringside seat on a good part of the competition, and tells us that in delving deeper into the affair, he "uncovered a little noted but fundamental pattern among my colleagues involving self-interest, competitiveness, and the battle for recognition and reward. To be first is paramount. To be second is to be forgotten."

Despite the book's subtitle and the above quote, though, relatively little of the material is about a race to come in first. Most of it deals primarily with the allocation of recognition *after* success has been achieved, especially the problem of appropriately sharing credit between principal investigator and subordinates. That issue is prominent in the older (and perhaps less familiar to most readers of this journal) of the two detailed case studies that comprise well over half the book.

In 1943 soil bacteriologist Selman Waksman gave his graduate student Albert Schatz the project of trying to find a microbe exhibiting activity against the tubercle bacillus, one of the most antibiotic-resistant organisms known at the time, and of isolating the active agent, streptomycin. In a long and often ugly saga, we see how Schatz succeeded but was denied any share of the glory — Waksman was the sole recipient of the 1952 Nobel Prize in Physiology or Medicine — or of the financial rewards from the ensuing patent (although he was able to recoup some of that in a subsequent lawsuit).

For his other big story, Myers recounts the decades-long battle between Paul Lauterbur (on the chemistry faculty at SUNY Stony Brook for much of this period) and Raymond Damadian on the merits of their contributions to MRI. As Myers sees it, Damadian deserves credit for first recognizing that NMR relaxation times can distinguish different types of tissues, while Lauterbur (followed closely, and independently, by Peter Mansfield in the UK) introduced the use of inhomogeneous magnetic field gradients to produce images. Like Schatz, Damadian felt he was cheated of his due appreciation, culminating in the 2003 Nobel awarded to Lauterbur and Mansfield alone.

Both of these stories are very well told, and show evidence of an impressive amount of research, including substantial use of primary sources and author interviews. Myers is clearly trying (and mostly succeeding) to give even-handed accounts, illustrating many

examples of unattractive behavior on all sides; generally he does seem to somewhat favor the losers' side in both episodes. In the MRI case he suggests (as have others) that Damadian's exclusion may have had much to do with what *should* be extraneous factors, such as his abrasive personality and perhaps even his religious beliefs.

Besides the two central accounts, there are a number of much shorter vignettes about struggles for recognition, many emphasizing credit denied to students and colleagues. These include Rosalind Franklin vs. Watson and Crick on DNA structure; Lise Meitner vs. Otto Hahn on nuclear fission; Jocelyn Bell vs. Anthony Hewish on the discovery of pulsars; and a good number of less familiar examples. All of these are based on (mostly uncritical) synopses of secondary sources, and hence are rather less illuminating than the main stories.

Myers also considers several (somewhat) related topics: scientific malpractice and fraud, defects in the peer review system, relationships between science and art, and the representation of science in literature and popular media. He probably would have done better to leave out most of this discussion, which is quite superficial; each could easily receive (and indeed has received) full book-length treatment. Also their relevance to the main theme is at best tenuous. For example, Myers tries to connect problems in peer review of novel proposals to those of allocating credit: "If there is such limitation in recognizing groundbreaking initiatives, how can the process of establishing credit and priority — recognition and reward — be flawless?" But neither is his logic flawless: imperfect foresight need not entail equally poor hindsight.

Myers closes with some suggestions for reform that he thinks might help alleviate some of the problems addressed in the book, but they are not very concrete, mostly amounting to calls for clearer criteria and more transparency. Again, he tends to favor the underdog; for instance, he suggests: "[P]eer review has become an agent for the defense of orthodoxy and a constraint on creativity. A firm standard should be the degree to which a researcher's work threatens to disturb conventional beliefs." (But wouldn't faithful adherence to such a stance have ensured major funding for Pons and Fleischmann?!)

Should chemists read this book? Many of the stories, large and small, may well be new to them — the content is strongly centered on biomedical science — but one might have asked for a little more breadth of coverage. (Myers *does* give chemistry its due respect: "[A]ll biological processes are chemically based and mediated, and thus progress in medicine often awaits progress in chemistry.") And unquestionably this is an enjoyable read — although a good deal of the enjoyment may well fall into the realm of *Schadenfreude*. But I doubt that many readers of this journal will come away with any major new insights into the scientific enterprise, or agree with the author's claim that "A great secret of science has been revealed regarding its fundamentally ego-driven competitive nature." To most of us, I expect, that is hardly a secret.