

Commentary

High journal acceptance rates are good for science

Scientists occasionally complain that journal standards are too low and that journal acceptance rates are too high. The American Astronomical Society (AAS) journals that I publish in most frequently, the *Astrophysical Journal* and the *Astronomical Journal*, have an acceptance rate above 85%, as do the journals of many other professional societies. Therefore, nearly all significant astronomical results submitted to those journals that are not obviously fatally flawed are likely to be published. I am pleased with that state of affairs; it is the sign of a healthy culture of science, and astronomy is better for it.

One problem with the argument for lowering the acceptance rate is that it reduces the several dimensions of quality—correctness, importance, impact, flashiness, and others—to just one. The reduction inevitably leads, for instance, to researchers having difficulty publishing null results and similarly “boring” work. Such concerns are the source of the well-known issue of publication bias, in which the unlucky experimenter who gets a surprising (and spurious) result is more likely to get their work published. Thus the corpus of published work becomes skewed toward erroneous conclusions and makes for less reliable metastudies.

Indeed, a common perception in some subfields of astronomy is that *Nature* articles tend to be flashy but much less likely to be correct than papers in less selective journals.

The argument for higher standards also presumes that the editorial and refereeing processes are good measures of quality and that it is the poorer-quality papers that will end up being rejected. My experience with the refereeing process and discussions with scientists in other fields suggests that papers are at least as likely to be rejected for other reasons—among them scientific taste, politics, professional advantage, and science’s inherent conservatism.

A favorite example of such conservatism is a referee’s comment on Raymond Davis Jr.’s 1955 work that set one of the first upper limits on neutrino production in the Sun.¹ The referee complained, “Any experiment such as this, which does not have the requisite sensitivity, really has no bearing on the question of the existence of neutrinos. To illustrate my point, one would not write a scientific paper describing an experiment in which an experimenter stood on a mountain and reached for the moon, and concluded that the moon was more than eight feet from the top of the mountain.”² Such a critique must have seemed quite reasonable at the time. In retrospect, though, the paper marks an important milestone in our understanding of the neutrino and on Davis’s road to receiving the 2002 Nobel Prize in Physics.

In some fields, faculty positions and tenure are contingent on getting first-author papers published in extremely selective journals such as *Nature* or *Cell*. That puts tremendous pressure on researchers to cater to the perceived tastes of those journals’ editors and has even led to dramatic cases of scientific fraud, such as the Schön scandal (see PHYSICS



TODAY, November 2002, page 15). It also gives too much power for shaping the field to those editors, who have no fiduciary obligations to the discipline or its members. Astronomers and physicists are fortunate that our premier journals are largely run by professional societies, which are guided by such an obligation and where power to shape those fields properly lies.

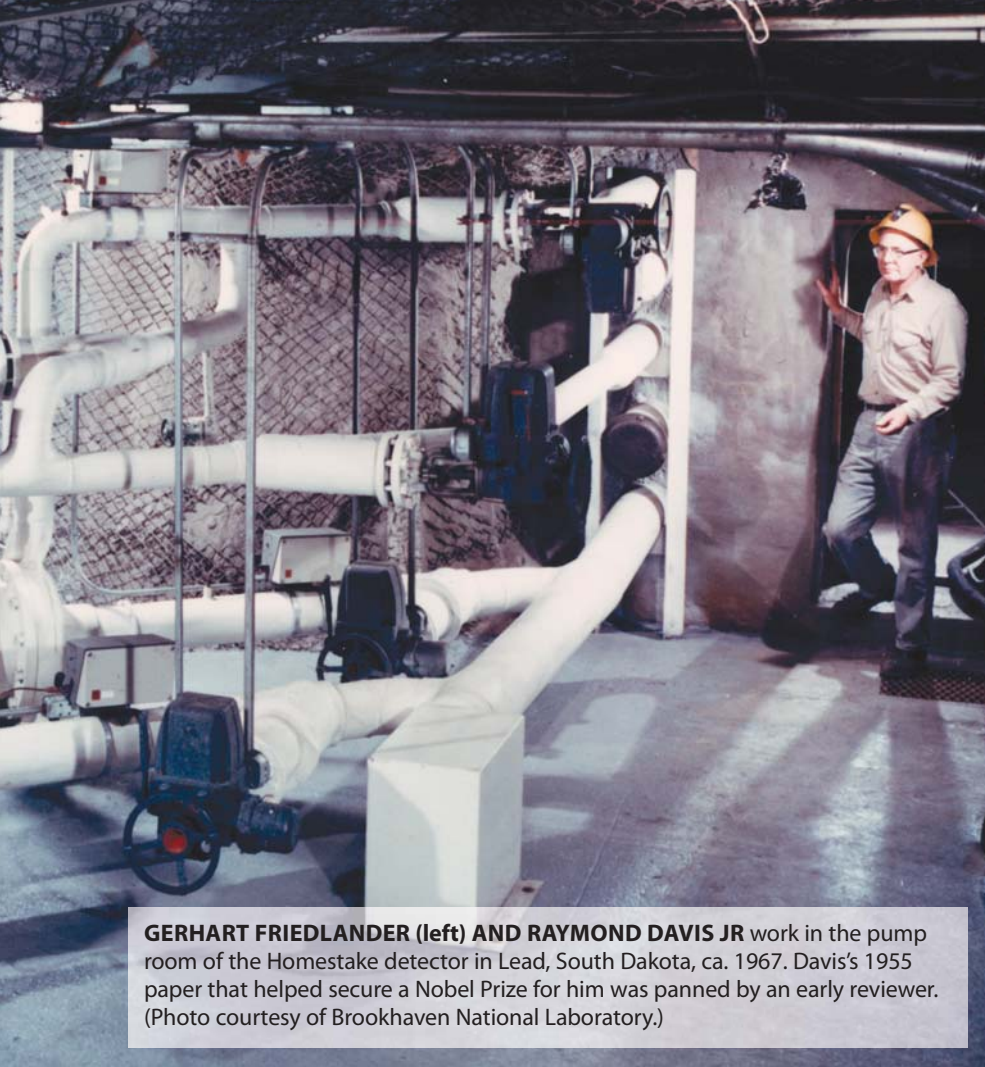
Another issue is where a field puts its efforts. In fields with very low acceptance rates, researchers spend (and, in my opinion, waste) a huge amount of time both refereeing their peers’ papers and revising and resubmitting their own. The system especially disadvantages graduate students and other junior researchers, who need to be published quickly to establish their careers and whose talents are best spent on their next project, not their last one.

Science is also best served when researchers feel comfortable taking chances and being wrong. In the words of legendary UCLA basketball coach John Wooden, “If you’re not making mistakes, then you’re not doing anything.” A pub-

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GERHART FRIEDLANDER (left) AND RAYMOND DAVIS JR work in the pump room of the Homestake detector in Lead, South Dakota, ca. 1967. Davis's 1955 paper that helped secure a Nobel Prize for him was panned by an early reviewer. (Photo courtesy of Brookhaven National Laboratory.)

lication scheme that attempts to prioritize correctness is less likely to produce the sorts of inspired, unlikely, and foundational results science valorizes. In other words, it's important that scientists be allowed to be wrong in the literature, as long as they have made no errors. Referees and editors do well when they respect the distinction.

To those who think journals should be more selective, I ask, Who is served by those ostensibly poor papers not being published? Gone are the days when scientists received and read entire paper copies of journals, and so might have had to waste time and shelf space on "bad" papers. Today, researchers who get papers rejected have no shortage of "lesser" journals to submit to, and they will eventually get published, including on arXiv.org, regardless of how much gatekeeping any individual journal does.

I do acknowledge the view that "prestigious" journals in science are important—for instance, as a signal to the public or media that a paper is especially noteworthy. Indeed, AAS has reoriented *Astrophysical Journal Letters* to serve that

niche in the marketplace of scientific journals and to better compete for high-impact papers with *Nature* and other prestige publications. I appreciate that in making that business decision, AAS has preserved the character of its other journals, so that it can cater to both markets.

I believe journals serve their disciplines best when they serve as journals of record, allowing scientists to document the work they have done. I also believe referees best serve when they act not as gatekeepers but as editorial consultants and independent voices that offer constructive criticism that improves submitted papers. High acceptance rates in the high-impact journals of the physics and astronomy professional societies help make those fields more efficient, fair, and productive. May it be so across science.

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LETTERS

Modern discovery in soft-matter physics

The 90th anniversary of *Reviews of Modern Physics* (RMP) is well celebrated in the February 2019 issue of *PHYSICS TODAY*—for example, in the excellent review of the late 20th-century topic of soft matter (page 38) popularized by the authors of its references, notably Pierre-Gilles de Gennes. The authors of the last two RMP references worked in numerous European countries and in Turkey, India, and North America. Even so, I was struck by the article's omission of a spectacular South American discovery in the 21st century, a discovery that in my view extends biologists' 20th-century qualitative considerations far up to the quantitative level sought by physicists.

Although the discovery of fractals associated with the solvent-accessible surface areas of folded protein segments was made by two Brazilian physicists, Marcelo Moret and Gilney Zebende,^{1,2} Lars Onsager's 1944 work had shown that long-range interactions at phase transitions are best described with fractals.³ Online protein data bases, including especially the genomic sequences data base, are by far the largest ever assembled.^{4,5} The Moret-Zebende discovery of 20 precise fractals in complex protein structures has far-reaching implications, including the demonstration of Darwinian evolution in protein families.² Such high precision immediately suggests that physicists may be able to achieve results of great medical value.

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