

2. M. B. Hall, *Henry Oldenburg: Shaping the Royal Society*, Oxford U. Press (2002), p. 84.
3. M. Biagioli, *Emergences* **12**, 11 (2002), p. 29.
4. J. E. McClellan III, *Specialist Control: The Publications Committee of the Académie Royale des Sciences (Paris), 1700–1793*, American Philosophical Society (2003), p. 33.
5. L. Pyenson, *Ann. Phys. (Berlin)* **17**, 176 (2008).

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Fred Hoyle wrote in his 1994 autobiography, *Home Is Where the Wind Blows: Chapters from a Cosmologist's Life* (page 159):

Referees are permitted by editors and learned societies to remain anonymous, a practice that has always seemed to me objectionable, if not indeed corrupt. Corrupt it certainly is in some cases. It is wrong that an unknown person or persons should have access to new work several months in advance of anybody else, and the more important the work, the greater is the scope for shenanigans. It is not unknown for a referee to contrive the rejection of a paper and then to make use of what he has been privileged to read. On the other hand, a scrupulous person may be inhibited from following up his own independent ideas as a result of being asked to comment on similar ideas in a paper by someone else. I am told that Wolfgang Pauli was inhibited, in essentially this way, from publishing what today we call the Schrödinger equation.

Can this last statement be corroborated?

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Many thanks to Melinda Baldwin for her article on peer review in the February 2017 issue. I'd make one correction, though.

Baldwin closes by asking us scientists to consider whether the purpose of peer review can "be fulfilled by reports from two or more referees." I'd replace "two"

with "one." My field, astronomy and astrophysics, has, for as long as I am aware, assigned only one referee for each paper submitted to its journals, such as the *Astrophysical Journal* and *Astrophysical Journal Letters*.

When I was a graduate student, the editor of *Astrophysical Journal Letters* told me that the community was a small group of basically good people who mostly got along, and that besides, 90% of all letters were ultimately accepted anyway. He and the other editors have my respect and admiration. But he was wrong. Scientists are human beings, they have biases and personal grudges like anyone else, and the fight for jobs is as brutal now as ever. The integrity of the system can no longer be maintained with a single referee, if it ever could.

I have seen first-hand how the current system can fail. My first two submissions, in 2000 and 2001, were summarily rejected. Still respecting the process after the first rejection, I dutifully waited nearly two months for the report from *Astrophysical Journal Letters* on the second, and when the report arrived, it was obvious that the referee had not actually read my paper.

Meanwhile, two famous authors of a lengthy submission to the main journal on a related topic demanded—and received—a new referee when the original one failed to provide a report within four weeks. That event was well known, quite the gossip in the community, and we knew that it would not have happened but for the names attached to the paper. Reliance on a single referee made the editor vulnerable to undue influence.

Later, the technical details that got my first paper rejected did not stop similar papers by others from getting published, including by a well-known researcher who had earlier critiqued my approach as being unsound. I was young and trusting. I know better now. Publication, I now realize, is inherently adversarial.

With name recognition and arm-twisting, you can publish papers from crazy to crackpot. Being the first to publish is supposed to mean something. I'm not sure it does anymore.

It's not even clear what peer review signifies. When I was first asked to referee a paper, I was appalled to find that the journal had no guidelines for reviewers. Ask five referees what their job is and I'm sure you will get five different answers.

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Journal editors should take note of the resounding success of the far more equitable arXiv. You do not have to pay to publish. You do not have to pay to read it. And there is no referee. Some junk gets posted, sure, but at least the reader can decide what is junk.

Anonymous peer review is not a bedrock scientific and Enlightenment principle. The Enlightenment and the science it birthed valued fairness, equality, free inquiry, and openness, not anonymously blessed publications hidden behind paywalls.

The current system needs to be examined critically. I believe in the antiseptic properties of sunlight, and I think all peer reviews should be made public. We need a broader discussion about the meaning of peer review. And I think having a single referee is far worse than two, three, or even none.

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► **Baldwin replies:** Yves Gingras is right

that we ought to distinguish the modern peer-review system from the more general idea of having colleagues give opinions about a book or paper. Because my focus was on tracing the origins of the former, I do think it was legitimate to highlight William Whewell's innovation. However, perhaps it would have been better, and more precise, to say that Whewell was "arguably the inventor of systematic refereeing."

The term "peer review"—itself a creation of the 20th century—is so weighted with modern connotations that I think historians must be cautious when bestowing it on practices from the past. I agree with Alex Csiszar that the internal approval practices of 17th- and 18th-century scientific societies were significantly different in both form and purpose from modern refereeing, in part due to the dramatic changes in scientific publishing that took place in the 17th through 19th centuries.

Gathering such a wide range of practices under the umbrella of "peer review" seems misleading to me, and it risks creating the false impression that the refer-

eeing system has been a consistent part of science since the Scientific Revolution. My hope, in the article, was to show that the story is much more complicated.

I regret not mentioning Lewis Pyenson's excellent article on Planck's work at the *Annalen der Physik*, and I recommend it to PHYSICS TODAY readers.

For Vitaly Matsarski's question, I consulted a handful of fellow historians but was unable to confirm the story about Wolfgang Pauli. I would welcome further correspondence on the subject.

Finally, Peter Williams's letter is not the only one we received that expressed frustration with the current workings of the peer-review system. As historians work to write a more complete and nuanced history of peer review, one hope I have is that past changes in refereeing practices might shed light on how the process can change in the future to better serve the scientific community.

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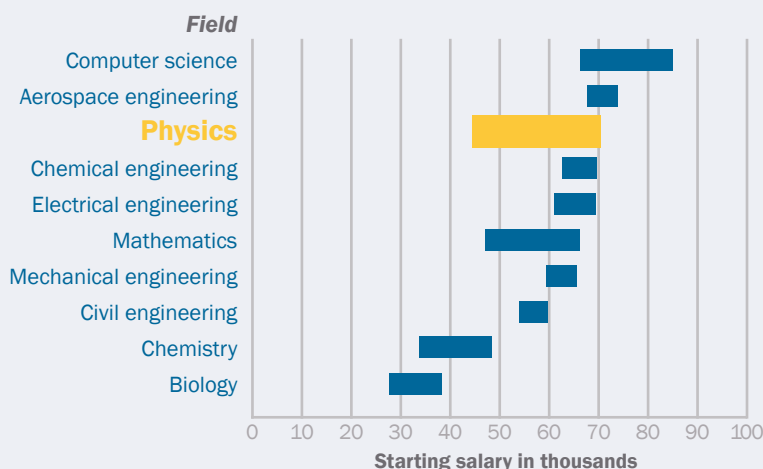
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