process is similar to what Goldstein describes at *eLife*: When reviewers strongly disagree, the editors typically communicate the key points of the reviews back to the reviewers and ask whether a consensus view can be reached. It often can, in which case one or both reviewers modify their original report. When it cannot, we seek mediation from a third reviewer or from an editorial board member who is an expert in the field.

Much of this process occurs behind the scenes, and far more often than anyone would like, disagreements between reviewers persist despite anything that editors can do. The process used by *eLife* sounds highly worthwhile, and insofar as mediation results in consensus, its editors deserve all possible credit and support. Meanwhile, the editors at the *Physical Review* journals (and I am sure elsewhere) annually meet to propose, refine, and from time to time implement modifications to the peer-review process.

I will be interested to follow the progress of the *eLife* model and open, online, and other review approaches.[1] Ultimately, however, I believe that conflicting reviews may be inevitable and that peer review, like democracy, is the worst system imaginable, except for the alternatives.

Reference

1. See, for example, *Nat. Commun.* **6**, 10277 (2015); D. Rennie, *Nature* **535**, 31 (2016).

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Peer review as conflict

I read with interest Melinda Baldwin's article "In referees we trust?" on the historical development of peer review (PHYSICS TODAY, February 2017, page 44). She is not the first to suggest that William Whewell should be considered the inventor of peer review. I did not take the trouble to respond to Alex Csiszar's comment to that effect in *Nature* last year,[1] but I now see that this curious idea could become entrenched just by being repeated. It is important to distinguish between explicitly establishing the idea of peer review and more or less systematically applying it.

Although Baldwin writes that Henry Oldenburg "rarely consulted outside opinions," the Council Minutes of 1 March 1665 explicitly note "that the *Philosophical Transactions*, to be composed by Mr Oldenburg, be printed on the first Monday of every month, if he have sufficient matter for it, and that the tract be licensed by the Council of the Society, being first reviewed by some of the members of the same."[2] It made sense that the Royal Society did not want to see printed opinions that could bring shame on its reputation. Every book was to be checked by members before being given any imprimatur. Hence in June 1664, the council decided that "in case Mr Hooke's microscopical observations should be printed by order of the society, they might be perused and examined by some members of the society."[3]

In any case, one did not have to wait as late as the 1830s to see peer review of scientific papers at work, since already in the 1760s the Paris Academy of Sciences had its own publication committee that reviewed papers and made explicit comments before publication. It even often required authors to cite previous work on a given subject as a condition of publication. For example, a corresponding member of the Academy, Pierre-Toussaint Navier, who submitted a paper on the dissolution of mercury in acid, was asked by the members of the committee to "add the citations mentioned in the referee's report."[4]

As for Max Planck, he did not like, as editor, to reject papers from colleagues and much preferred to suggest ways to render them publishable. Baldwin should have mentioned the work of Lewis Pyenson, who studied in detail how Planck handled his task as editor of *Annalen der Physik*.[5]

The mechanism of peer review evolved over centuries, obviously, and became very formal only in the middle of the 20th century. But before promoting Whewell as the inventor of that practice, historians would do well to extend research beyond the British islands. Also, there were many scientific organizations and journals in Europe before the 1830s.

References

1. A. Csiszar, *Nature* **532**, 306 (2016).

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.