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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.