An evolved conspiracy

Scrutiny of the reviewer-establishment axis suggests that the anonymous reviewing system is both harmful and unnecessary

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To be refused publication of a new discovery is a bewildering shock to the beginning scientist. Referees are supposed to despise error and cherish novelty. In fact they have suppressed important discoveries. F. W. Lanchester's circulation theory of aerodynamic lift was held up for ten years. J. J. Waterston's work on the kinetic theory of gases anticipated Maxwell by 12 or 13 years, and Boltzmann by 21 years. It was published, 47 years after submission, only because Rayleigh found the manuscript in the archives of the Royal Society. It was "nothing but nonsense, unfit even for reading before the society," according to a referee. Publication of Krebs's citric acid cycle was delayed also. (The present editor of *Nature* says that Krebs was not refused outright, only told that other journals might publish his article sooner. But dates of submission show that *Nature* was, at the time, publishing very quickly.)

Ingram Pinn

Why should an apparently counter-productive institution like reviewing exist? It does not and cannot succeed in its supposed function of protecting journal readers against error. Reviewers, chosen by editors, are seldom as well matched to articles as the eventual readers. Reviewing's real function is what it does successfully, to deny innovators direct access to publication.

People fear change. It lowers the value of anyone who does not exploit it. It puts us all on a down escalator, where we climb just to stay even. Innovation occurs faster than society will use it, perhaps faster than it can possibly be used. As the guilds controlled progress in the middle ages, so the scientific and technological establishments slow the pace of change to a rate they can accommodate to. Reviewing is part of the mechanism for doing this.

When people cooperate for an unacknowledged purpose their association is called a conspiracy, yet suppression of novelty by review is not a plot cooked up between referees and the establishment. But conspiracies can arise by evolution instead of by design, with the members falling into their roles by accident and finding them congenial. The establishment gives referees great power over other peoples' lives. The referees repay the establishment by suppressing new discoveries. It is not necessary that either side understand the arrangment.

Reviewers reject good ideas because reviewing inflates their egos and puts peoples' careers in their hands. Being anonymous, they cannot be called to account. This combination of exaltation and power would warp anyone's behaviour.

Original work is often sketchy, the writing brash and sometimes confusing. When it is not misunderstood it excites jealousy. Of course there are then good reasons why it must be rejected. The result is familiar to most scientists, a rejecting review compounded of error, insult, and sometimes brutality, with little chance of a rehearing and none of retribution against the reviewer.

Scholarly review is such an effective barrier to novelty that a new idea can seldom be announced to the world until it has first been sold to the establishment. Most innovators are ill-suited to promotional work, and begrudge the time and effort. Many ideas die at this stage. The innovator must buttonhole important people to enlist their support, or perhaps apply for a grant to study his innovation. If successful, this gets the idea on to the grapevine, which is its announcement to and acceptance by the world. Publication is a formality, like a letter confirming a telephone call. By thus standing astride the channels of communication the establishment maintains its rule and regulates progress for its own convenience.

Accepting that the rate of progress must be regulated, need it be done this way? The review system is poisoning the atmosphere in science. Must we keep it? If innovations were freely published the establishment would still decide which of them to develop. With the power of the purse, does it need the gag as well?

The conventional answer is that a reviewing system that accepts all innovations must let through a flood of junk. In my experience as a reviewer I found no junk, just right ideas and wrong ideas. In explaining to authors where they went wrong the power to reject was only an embarrassment.

Can a reviewer who wants this power be trusted with it? At most, reviewers should be able to delay publication for a six month "second thoughts" period, and impose a reasonable length limit. If a reviewer felt strongly that readers should be warned he could have his signed comments published next to the offending article. A reviewer who only advised could remain anonymous.

Mass circulation journals like *Nature* and *Science* would still have to reject papers, or their issues would be the size of telephone books. But for the specialised journals the quantity of submitted manuscripts might well decrease and the quality improve. With publication itself no accomplishment, and carrying no presumption of quality, each article would stand on its intrinsic value, and the rewards for quantity would be much less that at present.

The first journal to try this system might even get too many manuscripts. Its principles would require it to refuse all contributions for a while. Problems with repeating contributors who would monopolise the journal should be dealt with when they arise, not by making elaborate restrictions at the beginning.

This need not be the only experiment. A journal that actively solicited rejected articles and printed good ones beside foolish parts of the rejecting reviews might illuminate the status quo. If scientists once realise what they are doing to themselves with the review system, they may think of ways to let innovators publish their ideas and discoveries without having first to promote them. □