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Coping with peer rejection

Accounts of rejected Nobel-winning discoveries highlight the conservatism in science. Despite their historical misjudgements, journal editors can help, but above all, visionaries will need sheer persistence.

Not many people spend tens of thousands of dollars to tell the world that they were robbed. But that is what Raymond Damadian and his company did last week when he discovered that he hadn't won the Nobel Prize in Physiology or Medicine, and complained in full-page advertisements in *The New York Times* and other prominent newspapers (see page 648). He claims in his advertisement that he should have shared the prize won by Paul Lauterbur and Peter Mansfield for their work on magnetic resonance imaging.

Whatever the merits of Damadian's case, the episode highlights the difficulties in assessing ground-breaking work. If it is controversial 30 years after the event, one can imagine the divergences of opinion that arise over truly innovative research before history has had a chance to consider its verdict — when papers are submitted to journals and applications sent to grant funding panels. In the latter case, researchers can keep their ambitions masked, stating goals that are predictable extensions of previous work, thereby — as cynics would have it — maximizing the chance of funding. But in the case of journals, they have no choice but to stake their genuine claim.

Some funding agencies, to their credit, are setting out to encourage riskier, visionary applications. The US National Institutes of Health has a new roadmap that includes a Director's Challenge with precisely that aim. The European Commission is also setting up a fund for visionary research, the New and Emerging Science and Technology programme, which will shortly issue a call for proposals (see www.cordis.lu/nest/home.html).

Regrets

What of the journals? *Nature*, while proud of its content over the years, has a confession to make about this year's medicine Nobels. Not so long ago, presciently pleased with having published Lauterbur's work, we celebrated it along with other *Nature* greats in a promotional campaign. Lauterbur politely wrote in to point out that we had published it only after he had appealed against a rejection.

In case anybody runs away with the idea that *Nature* is unusually culpable in this respect, they can look at a collection of rejections experienced by Nobel winners that neatly illustrates the hurdles they had to overcome to publish their work. Juan Miguel Campanario, a physicist at the University of Alcalá in Madrid, Spain, has compiled a list of more than 20 Nobel laureates' rejections by many journals, and recollections by many more of resistance by their peers (see www2.uah.es/jmc).

Not all of *Nature*'s Nobel-winning casualties are totally embarrassing for us. Our notorious rejection of the Krebs cycle in 1937 is partly mitigated by the fact that we said we would publish it once several weeks' congestion was out of the way, only for Krebs to take it elsewhere. In some cases cited by Campanario, we are accused only of having the nerve to force the authors to shorten their papers.

But there are unarguable *faux pas* in our history. These include the rejection of Cerenkov radiation, Hideki Yukawa's meson, work on photosynthesis by Johann Deisenhofer, Robert Huber and Hartmut Michel, and the initial rejection (but eventual acceptance) of Stephen Hawking's black-hole radiation. Hindsight is always perfect. But we can take comfort, however dubious, from the fact that our unmitigated embarrassments are but a minority in a substantial list of journals' historical misjudgements.

We can take more respectable comfort from a little-celebrated positive accomplishment of editors, which is to champion submitted papers in the teeth of referees' (and sometimes colleagues') resistance. One such submission, according to his Nobel lecture, came from Thomas Cech. The three referees ("outraged enzymologists", as Cech described them) all opposed the idea that self-splicing RNA could be a catalyst, but *Nature* published it nevertheless.

Reasons to publish

A straw poll of Nature journals' editors confirms that risk-taking and hopefully enlightened acceptance by editors persists — although whether at the Nobel level it is too early to say. Confidentiality prevents us from being specific, but papers in (for example) stem-cell development, cell signalling networks, genetic linkage to disease, telomerase dysfunction and extrasolar planets were accepted for publication in recent years despite significant scepticism, and were subsequently well cited.

Other examples, for instance in mammalian evolution and muscle crossbridge dynamics, were published with editors and referees suspecting that their conclusions were probably wrong but giving the papers the benefit of the doubt because there were no insurmountable technical objections and they seemed important. Such cases have proved stimulating for their fields, even though (in at least one case) the conclusions, as techniques have improved, have indeed required revision.

What are the morals of these tales? Certainly we need a diversity of good journals. The laureates' rejected papers ended up being published somewhere respectable. And in particular there is perhaps some continuing virtue, along with the pitfalls, in the old élitist model of learned-society journals. The *Proceedings of the National Academy of Sciences*, for example, has published innovative papers that had failed to be appreciated by editors elsewhere, because the authors were academy members and so were able to publish by right.

This is strikingly reminiscent of perhaps the most celebrated editorial judgements of all, in *Annalen der Physik* in1905. That was the year in which Einstein published five extraordinary papers in that journal, including special relativity and the photoelectric effect. The journal had a great editor in Max Planck. He recognized the virtue of publishing such outlandish ideas, but there was also a policy that allowed authors much latitude after their first publication. Indeed, in journals in those days, the burden of proof was generally on the opponents rather than the proponents of new ideas. One might also remember just how exceptional Nobel-winning discoveries tend to be. By and large, peer-filtering has strong virtues.

Nevertheless — a final moral — rejected authors who are convinced of the ground-breaking value of their controversial conclusions should persist. A final rejection on the grounds of questionable significance may mean that one journal has closed its door on you, but that is no reason to be cowed into silence. Remember, as you seek a different home for your work, that you are in wonderful company. ■