

## My Word

# Rejecting arrogance

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Baseball players, works of art and rare postage stamps have value in the market place partially as a function of their rarity. What is it about human nature that drives some to chase a postage stamp or coin, precisely because it is rare, and others of us to crave the stamp of approval that comes with publishing a paper in a prestige journal?

It is not uncommon for prestige-seeking journals (or at least their editors) to flaunt their rejection rates as evidence of their superiority, and measures of university 'excellence' often include their selectivity. I have been troubled for some time by the confounding of the value of a scientific paper with that of the rejection rates of the journal in which it is published (not that this has kept me from submitting my papers to the most competitive journal I think will take it!).

One can ask the simple question: would the Watson and Crick *Nature* paper describing the structure of DNA be less important had *Nature* published twice as many other papers in the same issue, or in the same year? I think we would all agree that the paper describing the structure of DNA has intrinsic value that does not depend on the value of the papers that precede and follow it in the same issue, or in the same year. The logic that an individual paper has more value because of those not published next to it is as flawed as that which accords value to a paper because of the number of citations to the other papers in the journal in which it is published.

In the 'old days' — good or bad, as they may have been — rejection rates were a necessary outcome of page limits dictated by financial constraints of print publication and mailings. As we move further into an on-line

world, the economics of publishing are changing, and the need to restrict pages for financial pressures alone is disappearing. Indeed, future financial incentives will favor publishing more papers, not fewer. Will there remain an incentive to encode the value of a paper merely by restricting the number a journal publishes?

Some might argue that the time of the reader is so precious that we need others to signal for us the 'best' papers, defined as those published in the 'best' journals. In this argument, the absence and number of the rejected papers adds value to the accepted papers. Certainly it adds to their visibility, because the other papers are not present in the table of contents or the printed journal. But couldn't that be done as easily with a gold star such as we used to get in kindergarten for work well done? Already many journals have their versions of 'News and Views' type editorial material, designed to flag work thought to be of exceptional interest. And the success of 'Faculty of 1000' — an internet service which provides postpublication assessments of papers judged sufficiently interesting by members of the 'Faculty' — argues that there are successful models for the field to communicate value other than through the rejection rate of the journal in which a paper is published.

Some journal editors and publishers argue that there are very few 'good' papers, and it is their mission to keep the field 'pure' by not publishing inferior work. Before going further, I must make it clear that I believe strongly in the value of peer review. Many papers improve vastly as a function of reviewers' comments, and many technical, logical and statistical errors are spotted in the process. Reviewers often point out relevant literature that should be cited and discussed. That said, the review process operates best when reviewers are asked to think seriously about the data and what they mean, and works least well when reviewers are

asked to judge the potential importance of a paper. Hindsight is 20:20, but few of us (including reviewers and journal editors) make reliable assessments about the long-term value of most papers at the time the work is done.

The pursuit of publication in journals that measure value by rejection rate (and its corollary by Impact Factor) comes at great cost. It is not uncommon for a paper to have been seen by six or eight different reviewers as it bounces from a high profile journal to one of lower prestige, delaying it by months or even years. These delays, if they are not accompanied by improvements in the paper, are a violation of the social contract we, as scientists, have with the tax-payers who fund our work, to publish it in a timely fashion and make our work available to the larger community. Sometimes the paper improves continuously during this process, but ironically the least substantive reviews are sometimes provided by reviewers for the prestige journals, who are often trying to justify why a paper is or is not important, rather than thinking about what it has actually proven, or how it could be improved.

But most significantly, it sends the terrible message to our younger scientists that where a paper is published matters more than what it says. I constantly lecture my own graduate students and postdocs that they should rise above such considerations, and write detailed, careful and scholarly papers, and that the quality of their papers matters more than where they are published. Then I do my best to help them publish their work in the most selective journals when we have what I consider an important result. I guess my philosophical principles and logic do not make me immune to the siren lure of honor as defined only by the scarcity of the opportunity to publish in high-ranked journals.