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EDITORIAL

Scientists, Papers and Journals: Coping with Rejection

A long time ago Charles Dodgson (more famously known as Lewis Carroll) described Man as an 'animal that writes letters'. If he were with us today he might describe scientists as 'an animal species that writes papers'. Writing a manuscript, describing new results, not always earth shaking, and sending it off for publication in a journal is an almost routine activity amongst scientists, especially those involved in academic research. Researchers fall into many categories, but discerning observers might generally agree that three broad classes are identifiable. There are compulsive authors, who can hardly wait to describe a result or finding, however incremental. Writing comes easily to these scientists, who are generally quick to brush off criticism from referees and editors. It is their energy and optimism that keeps journals alive. There is a second group of authors, who are careful in their choice of work to be publicly disclosed and who prefer that every paper be based on a substantial body of results. This group invariably consists of scholarly, but cautious individuals. Indeed, the onward march of science depends greatly on their contributions. Finally, there are the self-proclaimed perfectionists, who intend to produce that one jewel of a scientific publication, which will forever alter the contours of their disciplines. This class of researchers rarely manage to put pen to paper (or more accurately in today's context, fingers to the keyboard). They are generally recognisable in the community as critics of all their fellow researchers. This group often resembles literary and music critics who neither write nor sing.

Even thirty years ago, the scientific literature seemed to consist of a largely useful body of knowledge on which further advances would be based. The number of journals and papers in every discipline and sub-discipline appeared manageable, with conscientious scientists being able to keep up with much of the published work in their fields. But, times have changed and changed dramatically. Scientists are now routinely judged by the number of publications they produce, the impact factor of the journals in which they are published and their influence on the field, as measured by the number of citations they accumulate. Of these parameters, the citation count may be the most reliable indicator of scientific impact, especially when examined several years after the appearance of a paper. Faculty members in academic institutions need to show evidence of productivity as judged from a list of publications, in order to secure career advancement. Students and postdoctoral researchers need to display a significant number of publications to move forward in their careers. Institutions are ranked worldwide on the basis of their published output and even countries are constantly compared. Unsurprisingly, science journals have mushroomed and the scientific literature is a vast ocean, with each new paper being an infinitesimally small drop.

The rise of scientometrics has completely transformed the way the average academic scientist views the practice of science. Assessments of individual performance are increasingly based on apparently quantitative indicators, with the Hirsch *h*-index rapidly gaining ground as a favoured metric. Young academics worry about *h*-indices at a time when they should hardly be worried about a parameter that is almost guaranteed to improve with age. Indeed, aging scientists who decline in every other way are encouraged by a metric that must normally show a positive trend or at best remain at a steady level. Many prolific authors have already begun to devise ways of gaming a system that places an undue reliance on quantitative metrics for assessing scientific quality. The burgeoning scientific literature is growing at a rate which is unsustainable, driven by the desire to maximise publication numbers and citation counts.

Can a parameter like the *h*-index be used to predict the future course of a scientist's career? For the uninitiated, a scientist with an *h*-index of 10 will have at least 10 papers listed in his curriculum vitae, which have each been cited at least 10 times in the literature. A recent commentary in *Nature* advances an intriguing view that a formula can be constructed, which permits the prediction of the *h*-index of a scientist as a function of time (Acuna, D. E., Allesina, S. and Kording, K. P., *Nature*, 2012, **489**, 201). The authors have assembled a dataset of '3085 neuroscientists, 51 *Drosophila* researchers and 151 evolutionary scientists' for whom they 'constructed a history of publications, citation and funding'. In putting together this dataset, the authors imposed a few restrictive conditions in filtering an initial sample of 34,800 neuroscientists, 2000 *Drosophila* researchers and 1300 evolutionary biologists. The final set required that those included had an '*h*-index greater than 4 (to exclude inactive scientists)'. The publications analysed were published after 1995, a restriction required 'because electronic records are sparse before 1995'. The authors included must have 'published their first manuscript in the past 5–12 years'

and 'were identifiable in *Scopus*'. The *Nature* note will undoubtedly attract a wide readership as there are three formulae presented that should allow readers, with even minimal computational abilities, to predict their *h*-indices for the following year and for five and ten years in the future. In keeping with the modern frenzy for quantitation, *Nature* has provided an online facility for readers to try it out for themselves, with the caveat that the projections 'are probably reasonably precise for life scientists, but likely to be less meaningful for the other sciences'. The authors suggest a potentially dangerous use for their formula, arguing that it 'is particularly useful for funding agencies, peer reviewers and hiring committees who have to deal with a large number of applications'. In pushing the quantitative method they note that 'statistical techniques have the advantage of returning results instantaneously and in an unbiased way'. Even as I suppressed a feeling of unease, that the authors may have overstated their case and produced a formula that would be welcomed by science administrators, I was encouraged by their concluding sentences. While acknowledging that their 'findings and predictions may not alleviate scientists' angst over their careers, the results offer some comfort by showing that the future is not so random'. They note, somewhat comfortingly, that 'the occasional rejection of a paper may feel unjust and indiscriminate, but in the long run, such factors seem to average out, rendering *h*-index trajectories relatively predictable'.

Rejection is an occupational hazard for authors. While experienced authors display admirable resilience in searching for journals that will publish their work, the urge to seek out 'high impact' journals ensures that rejection is a common experience for most scientists. Having just received a rejection letter, for what I imagined was an extensively reviewed and revised manuscript, my spirits lifted slightly when I came across a news item that drew attention to a study that concluded 'that rejection improves eventual impact of manuscripts'. The story begins on a cheerful note: 'Just had your paper rejected? Don't worry – that might boost its ultimate citation tally' (Ball, P., *Nature News and Comment*, doi:10.1038/nature.2012.11583). The paper being highlighted, which appeared online a few days ago, analyses the 'flows of research manuscripts among scientific journals' in an attempt to 'reveal hidden submission patterns (Calcagno, V. *et al.*, *Sciencexpress*, 11 October 2012; http://dx.doi.org/10.1126/science.1227833). The authors conducted a difficult exercise in attempting to track the, sometimes tortuous, history of manuscripts before they appeared in print. Their focus, predictably, was in the area of biological sciences where the literature has been expanding at a phenomenal rate. Their strategy was to contact 'the corresponding author of virtually all research articles published between 2006 and 2008 in 16 subject categories of Biological Sciences (923 journals)' and to ask them to answer a direct and simple question: Was the article 'first submitted to the publishing journal and, if not, the name of the journal previously attempted'. This survey yielded the 'submission history of 80,478 articles (37% of all enquiries)' from which the authors go on to 'reconstruct the network of manuscript flows among scientific journals'. They note that 'this network can be used to learn more about publication strategies and perceived journal importance than is available in citation networks'. Ferreting out the often unknown and sometimes forgotten history of published papers may seem a curious exercise, at first glance, to casual readers. Calcagno *et al.* however reach conclusions that must spark interest in scientists intensely focused on the race to publish and to publish in high impact journals. As anticipated, high impact journals, *Nature* and *Science*, predictably, amongst them, were key nodes in the network. Surprisingly, the authors discovered that the high profile journals 'published a higher proportion of papers that had been previously submitted elsewhere than did more specialized and lower-impact publications', leading Philip Ball to conclude that 'rejection improves eventual impact of manuscripts'. Reading this was like balm to my wounded ego, still smarting from recent rejection. Clearly, my colleagues and I would now be well advised to raise our sights and aim higher. The Calcagno paper reveals interesting facts about scientists and their journals. They note that, 'overall 75% of all published articles were first-intents, with a range of 67% to 87% across subject categories. None of the journals … sampled was found to be purely recycling manuscripts rejected from other journals. Thus most articles were initially targeted to the journal that would eventually publish them.' Experienced authors, tempered by early experiences of rejection, undoubtedly exercise better judgement in their choice of journals to which they submit their manuscripts. Limiting risks and adopting a pragmatic approach in submitting manuscripts is presumably a widely adopted strategy.

The authors pose some interesting questions. 'Does submission history reflect the intrinsic "quality" or, in a more quantifiable way, the impact or utility of articles following publication?' Armed with their formidable dataset of prepublication histories, Calcagno *et al.* turn to the *Web of Science* to examine citation counts for papers for about 3–6 years, following their appearance in print. Their findings may encourage authors struggling with the process of resubmission after rejection. Curiously, 'resubmissions were significantly more cited than first-intents published the same year in the same journal'. Every reader experienced in the game of publishing papers will undoubtedly have his or her own explanation for these findings. The authors go on to gently suggest that 'these results should help authors endure the frustration associated with long resubmission processes and encourage them to take the challenge'.

Science watchers armed with the technologies of the information age will undoubtedly continue to provide new insights into the behaviour of scientists and their journals.

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