"The best papers are the boldest"

John Grace

One of my closest friends – let us call him Roger – told me that his published papers were read by only one other person, a French scientist working in the same field. Indeed, Roger has been brushing up on his French so that he can publish at least some of his work in that language, making it easier for the other scientist to read about his latest discoveries. Together, they have been gradually pushing back the boundaries of knowledge in an obscure and little-known area of ecology. However, their papers are published in highly specialized journals and read and cited by no one but themselves. He showed me some of these papers. They were erudite, scholarly, and written in a lucid style. Did he mind the small amount of attention given to his work by the wider scientific community? No, he didn't. Not a bit. His work has been absorbing, often challenging, and satisfying; he has made definite progress, and he is on excellent terms with his French colleague. She had invited him to visit her laboratory but he declined, mostly out of bashfulness. They have even talked about writing a little book, but the thought that it might not sell caused both of them to relegate the project to the backburner. Nevertheless, communication had occurred, and the scientific record had been extended. And who knows how important these findings may become in the future?

Why do scientists bother to publish? A disinterested member of the public or a member of government might be forgiven for thinking that science exists to advance society's needs (Figure 1). While "making the world a better place" is both an aspiration and an outcome of scientific activity, in my view, it is not the major motivation for scientists to publish. In a recent survey of biological and medical scientists, about half said they publish "to communicate knowledge", while rather fewer responded with "to demonstrate productivity" or "to establish prestige" (www.scienceboard.net). If this sample is representative, then Roger and his French colleague may be typical scientists; however, all those polled were members of The Science Advisory Board, an organization with a strongly altruistic motivation.

Those whose motivation is "to establish prestige" are less likely to be members of The Science Advisory Board, as they are driven by the urge to make a conspicuous impact rather than by scientific curiosity or the need to communicate. Their approach is more red-blooded: they publish widely and think strategically about publication, sending their boldest material to *Science* and *Nature*. Of course, they are encouraged to do so by the academic leaders of their institutions, who reward them with promotions and who may even put them forward for prizes. I have discussed this matter at length with a rather high-ranking colleague whose work is read by many; let us call her Rebecca. She assures me that the best papers are the boldest, as they make the most ripples. Gratification for the likes of Rebecca is akin to goal-scoring, and she has lately taken a deep interest in calculating the citations of fellow scientists and comparing them with her own. The competitive type of person that this mode of operation requires is not as uncommon in ecology as one might suppose. As Tennyson pointed out, nature is "red in tooth and claw", and ecologists know nature well.

Most of us have Rogers and Rebeccas among our friends and colleagues. It is not my purpose in this article to recommend one approach against the other, as it is obvious that both styles of working are important, and one complements the other. Moreover, one cannot rationally select between Roger-type or Rebecca-type behavior, as these are mainly determined by temperament rather than training. Nor do we want to place ourselves on the Roger-to-Rebecca continuum or try to shift our own natural position, an experiment that might go badly astray. Rebecca is a highly competitive individual whose first few publications included one in *Nature*; she doesn't suffer fools gladly, but at the same time this lack of tolerance is accompanied by a razor-sharp intellect that can cut through layers of wooly thought like a surgeon's knife. Roger, on the other hand, is not at all bold; in fact he is rather tentative, although what he does is so well executed that his friends

Figure 1. *The noble aspirations of science, as written on this public building, may not always coincide with aspirations of individual scientists; yet curiosity-driven and ambition-driven research will often lead to the service of humankind. The inscription is at the entrance of the Crew Building (built in 1930) of the University of Edinburgh, UK.*

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think the time will come when others will need his work to provide a foundation for their own.

My main purpose is to argue that the Rogers of the world need protection because they will otherwise become extinct in today's struggle for existence, which has become overdependent on rapid publication in top-flight journals and the winning of grants. It will be harder for them to gain tenure, and when external reviewers look at the published output of the department they will be perceived as "the problem". Rogers tend to occupy the least trendy areas, and they keep to themselves. On occasion they can become quite cross on matters of ethics and standards, but generally they do not shine in committees and their contributions to staff meetings are few in number and often seem naïve. However, they are involved in outreach and give lectures to schools, an activity which increases student recruitment. Importantly, they frequently have the knowledge to do the technical things that others need. They are often the ones with the best knowledge of the flora, or they understand experimental design better, or they can tell you how to change the lamp on the atomic absorption spectrometer, or show a research student how to cut thin sections of a leaf (an important skill which is no longer widely taught). There are Rogers amongst the ranks of taxonomists and plant anatomists and other important yet shrinking disciplines where published output and the citation rate of journals are not very high (Krell 2000). In many such technical areas, which form the foundations of what the rest of us do, citation is scarcely relevant. But without the Rogers, where would we be?

My thoughts have turned to consider these things following several recent events. I have been on several Chair appointment committees recently. Of course, at this level, individual brilliance is paramount, and we recognize it immediately from the publication list. However, I notice a growing trend towards the use of metrics based entirely on citation scores to measure academic performance, and even national scientific performance (King 2004). Indeed, we expect our individual and institutional research excellence may soon be assessed using such metrics, thus avoiding the tedious and time-consuming business of assembling lists of best publications (in the UK we are again busy preparing for the Research Assessment Exercise; http://rae.ac.uk/). One of the favored indices is the *h* index, suggested in 2005 by Jorge Hirsch of the University of California. For those who haven't heard of the *h* index, it is defined thus: "The *h* index is the highest number of papers a scientist has that have each received at least that number of citations" (Ball 2005).

I tried this out on colleagues and then on some famous ecologists, and at first I rather liked it. There are a few problems in calculating the index, especially when the person has a common name, has changed his or her name, or has moved around from institution to institution. Some difficult names are very hard to look up accurately, as are people who are inconsistent in the use of their initials. I thought my own name was uncommon, but there are several people listed who share it, three of whom are ecologists. We probably all need personal registration numbers to surmount the ambiguities! However, putting these difficulties aside, if you score an *h* index of 30, for example, you are really rather good. Absolutely top people in biology make it to 100. Then I applied it to the ranking of applicants for a prestigious Chair at one of our top universities, and I decided I didn't like it any more. It is strongly correlated with age, as it integrates citations. Thus, it positively discriminates for older scientists and does not reward clever youngsters. I was told early on in my career that when recruiting for the department we should look at "muzzle velocity", defined as the published output in top journals in the first few years of the applicant's career, because this is an excellent predictor of lifetime performance. It sounds reasonable, but has the research been done? Probably not.

Finally, I have that nagging feeling that no list of publications, no citation metric, no counts of papers in *Science* and *Nature*, and not even muzzle velocity can ever measure the scientific worth of a person.

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