Competition and the death of science

It is natural that research should be competitive, but there is now ample evidence that competition is too fierce for many people's peace of mind and for the health of the scientific enterprise as a whole.

COMPETITIVENESS in science has done wonders for both the quantity and the quality of discovery in the past few decades. Of the quantity, there can be no doubt; familiar statistics of the steady growth of the literature, of the proportions of deserving grant applications that grant-giving agencies cannot support and of the demand for space in reputable journals are ample proof that the river of discovery is in flood. Quality, most easily assessed with hindsight, is also steadily improving in at least one important sense: competitiveness ensures that people take immense care not to make too much of the data they have gathered for fear, afterwards, of seeming fools. That makes the literature duller than it needs to be, but that may be a virtuous fault.

Nor is it surprising that competition should be common even in science. The edifice of discovery may be built from abstract ideas, but the builders are only human. Most probably, the present competition for research grants, laboratory space, the brightest graduate students and so on is merely an extrapolation into modern circumstances of the primaeval instinct to compete for edible resources. If the outcome is the marvellously deepening understanding of what the world is like that we now enjoy, who, except perhaps the spouses of the postdoctoral fellows whose laboratory lights burn all the night, can reasonably complain?

Of course, this coin has another side. There is little doubt that the competition to succeed in discovery, or at least to be seen to succeed, explains the rash of perversions of the literature in the past 15 years or so. Outright fraud may be comparatively rare, the magnification of achievement by making thinner slices of one gobbet of discovery is not. Nor is the deliberate misreferencing of other people's work in a manner that magnifies the importance of one's own. Most people know the dangers, many have suffered from them.

The side-effects of competitiveness are not simply a contemporary phenomenon, of course. The great clash between the newtonian and cartesian schools lasted for much of the eighteenth century. The foundation of the atomic theory in the early decades of the following century was not free from skulduggery. But by the early decades of this century, it seems to have been implicitly recognized that outright expressions of competitiveness may damage not only people but even the progress of science itself. That is when it became established that people with discoveries to boast of should answer serious enquiries about them, explaining even to their competitors exactly how they had succeeded. What follows is mostly a lament for the passing of those genteel decades, based mostly on this journal's recent and often distressing experience.

The reasons why good manners have recently been debased are readily understood, and have much (not all) to do with the publication process. In the old days, when the scientific enterprise was much smaller, people's achievements, while embodied in publications, made their reputations mostly by word of mouth. Neither Maxwell nor J. Willard Gibbs would have felt compelled to publish something every other month. But now, when a publication record is almost the sole determinant of reputation and thus, by way of research grants and promotions, of success and self-esteem, competitiveness centres uncomfortably on the journals.

Journals (even this one) abet competitiveness, often willingly but sometimes unwittingly. In essence, they compete among themselves (by means of the service they offer contributors) for what they consider to be the most interesting articles and then, by their decisions, endow what they publish with a degree of importance not necessarily commensurate with its intrinsic worth. Increasingly, the competitiveness of journals and authors centres on the time and even the timing of publications.

Here is an example of what can happen. On 13 November last, Dr Manuel Perucho and his colleagues at the California Institute of Biology at San Diego offered *Nature* a manuscript describing the deletion of nucleotides in tracts composed of alternating A and T residues from the genomes of cells in a subset of human colon carcinomas. Afterwards, it emerged that essentially the same manuscript had been seen and refused by two other reputable journals. In the usual way, the manuscript was sent to two referees (avoiding at the authors' request a noted expert in the field). Each expressed enthusiasm, but at the same time suggested important ways in which the findings could be strengthened. The manuscript was therefore returned on 12 January with a note saying we would be happy to consider a resubmission if the authors were so inclined. (They were, and another version, including data on other kinds of repeats and a range of tumour stages, arrived a month later.) By mid-March, after further review and some drastic pruning, Perucho was told that his paper would be published.

Then came a bombshell of sorts. On 7 May, three articles announcing essentially the same result appeared in the journal *Science* (Peltomäki, P. *et al.* **260**, 810; Aaltonen, L.A. *et al.* **260**, 812; and Thibodeau, S.N. *et al.* **260**, 816). The first two articles are recorded as having been received on 8 April, and were accepted for publication within the week; one of the senior authors of the first two articles was Perucho's excluded referee. The third article, equally interesting, had originally been submitted a month earlier. In mid-April, Perucho was telephoned by *Science*, seeking information about his work for a general article on the three articles, each of which (from internal evidence) must have entailed several years of work. Perucho's article appeared in *Nature* on 10 June (and will repay reading).

No blame attaches to *Science* or the authors of these papers for having rushed them into print; given the chance, *Nature* would probably have done the same. But there are three casualties. First, Perucho, who feels he has been robbed of the glory that was rightly his. Second, the interest of the discovery has been diminished by all the fuss; the cancers whose cells carry shortened repeats are differently distributed in the colon from others, and metastisize less frequently, for example, while if Petrucho is right in believing that the underlying fault may be a mutation of a DNA repair gene, the ramifications of that may be exceedingly important.

The third casualty is science itself. So much zeal is now invested in being first, and so much emotion is spent if seeming to be second, that people's effectiveness must be undermined. But that is the least of the harm done. Will Perucho in future be as open in talking about his work at meetings as he has apparently been? Will his younger colleagues not regard his experience as a proof that they must be even more zealous competitors, secretive until their discoveries are published, then ruthless in winning recognition for them?

The only lasting remedy is that the link between publication records and reputations should somehow be broken. Academic institutions have the chief responsibility. Journals have also an important part to play. If good manners are entirely banished by competitiveness, neither will have a long future.

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