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Valediction from an old hand

John Maddox steps down as editor of *Nature* from this issue, to be replaced by Dr Philip Campbell.

L. J. F. (Jack) Brimble had been editor of *Nature* for roughly 22 years (for much of that time, in unwelcome harness with A. J. V. Gale) when he died almost exactly 30 years ago, alone in his apartment in London's Dolphin Square. I met him only once, over lunch one day. His manner was gruff, as if he had spent a lifetime in the army; I later learned that he was capable of great generosity to colleagues and even acquaintances. But he was evidently also given to bursts of irascibility: a few years earlier, when I had been working for the *Manchester Guardian*, he had removed me from the list of those to whom page-proofs of *Nature* were distributed in advance of publication. My transgression had been to write something that could be (and was) taken as critical of the journal. Then, without further intervention on my part, the tightly rolled brown-paper packages started arriving again.

Then Brimble died, and I had a visit from the late Maurice Macmillan MP. That was a minor embarrassment. I was then the Co-ordinator of the Nuffield Science Teaching Project, housed in a basement in one of the more dingy parts of Bloomsbury. My office was a partitioned box with no windows, and we had no decent coffee cups (nor coffee). But Macmillan had said that he had wanted to talk about *Nature*, which we did. I can imagine, but cannot remember, what I said: probably "Strange to see a weekly journal with so little in the way of news", and I would probably also have boasted of having found my first job (at the University of Manchester) from a classified advertisment in *Nature*.

But then the conversation became more serious, and Macmillan seemed to be asking whether I would be interested in Brimble's job. I remember a flush of ambivalence about the prospect (which is not the best way to win the confidence of potential employers). It had been more than a year since *Nature* had abandoned the practice of appending to research articles and notes the dates on which it had received them. That seemed worse than merely bad, which I explained to Macmillan. "How big is the backlog?", I asked. "I will arrange for it to be counted", he replied. When we next met, he produced the answer: 2,000-odd. (The number was precise, but I have forgotten all but the leading digit; I almost decided not to take the job, which was then firmly on offer.)

Poor Brimble was not entirely to blame. The sight of the office revealed his problem. It was an open-plan space without much of a plan. A window facing West ran 10 metres along the room and the broad window-ledge supported the famous backlog. That was arranged in piles, one for each month, providing a histogram of Brimble's problem, soon to be mine. There were fourteen monthly piles when I first saw them.

The staff was tiny, and evidently too small. There were two editorial people, one of whom had been recruited on Brimble's death; he had failed the first-year examinations at the medical school at which he had been enrolled. The more senior, and late Brimble's right-hand man, was Richard Fifield, who has been for many years the managing editor of *New Scientist*. The hard work at *Nature* in 1966 was done by two secretaries, Jill Baker (who later became a director of the Macmillan company owning *Nature*) and Mary Scallan (who is married to Nicholas Wade, the science editor of the *New York Times*). The roster was completed by two middle-aged men: Roy Lincoln, who handled the mail (of which there was a great deal) and D. W. Wilkinson (but everybody called him "Mr" Wilkinson). His job was to put illustrations in order for publication, which meant ordering type from a typesetter and sticking it alongside the ordinate and abscissa of any graph that might be published.

What I most vividly remember from that period is the sheer hard work, which seems inseparable from the job. As soon as I had shaken hands with the owner, then Mr Harold Macmillan (later the Earl of Stockton), I began to moonlight at the *Nature* office. We had a refereeing system of sorts in action within a few weeks, and began refusing manuscripts on the strength of negative opinions or their sheer length. Anxious to put something newsy in the journal, I began writing leading articles, much to the annoyance of the late Ronald Brightman, who lived in Cheshire and who worked from miscellaneous pamphlets sent to him in a parcel (by Roy Lincoln) every week. Soon we had our first newshound, a beginner journalist called Nigel Hawkes who is now the science editor of *The Times* (London). The last backlog manuscript was published only after eighteen months.

Refereeing

The state of *Nature* in 1966 raises a raft of questions. Can, for example, a journal with such a reputation survive for long if scientific manuscripts are hardly ever refereed?

It is a good joke (which I have often used) that Watson and Crick's paper on the structure of DNA could not be

published now. It is only necessary to imagine what people would say if it reached them in the mail: "It's all model-building, just speculation, and such data as they have are not theirs but Rosalind Franklin's!" Some would complain that the sentence beginning, "It has not escaped our attention..." is entirely unsubstantiated, and must be an attempt to claim credit for developments in genetics that lie years ahead.

What happened when the paper reached the London office is easily imagined. Indeed, before sending it off, Sir Lawrence Bragg, the director of the Cavendish Laboratory in 1953, would have telephoned Brimble to warn him that the impending paper was important. Brimble might then have telephoned a few friends, perhaps including J. T. Randall, then the head of the Medical Research Council Biophysics Unit at King's College London, where both Franklin and M. H. Wilkins worked. It would not have taken them long to put together the account of Franklin's work, with its crucial demonstration of helical signatures in crystalline DNA. That paper appeared in the same issue as Watson and Crick, but after it.

These days, the idea that an editor should behave like that would be scorned. To have alerted the King's group to what was happening at Cambridge would be unethical. But whatever happened, rough justice was done; Wilkins shared the prize with Watson and Crick. (Rosalind Franklin's untimely death disembarrassed the Nobel committees from having more than the statutory three candidates on their hands.) Who can complain?

Confidentiality

This apocryphal reconstruction does not have to be true for it to illustrate the difficulties of the refereeing system now. One is the problem of the implied confidentiality. Journals ask referees to keep the contents of manuscripts confidential but have no way of policing their request. Referees are mostly honourable, but suppose one has been sent a paper whose conclusion is plausible and even arresting but unsubstantiated, and which the referee has recommended should not be published? Is it not then easier to talk about the idea or even to use it as the starting-point for an investigation of one's own? My guess is that the confidentiality of the refereeing process breaks down only when it is important that it should not.

For different reasons, during my first spell as editor of *Nature* (1966–73), there were two authors whose papers I decided never to send to referees. One was Louis Leakey, who repeatedly provoked otherwise reasonable colleagues to declare his paper not to be publishable and that he should spend the next three years working systematically through the fossils already stored in his many tea-chests; we would have missed much excellent palaeoanthropology if we had listened to them. The other was Fred Hoyle, who during the 1950s and 1960s entertained and instructed the readers of *Nature* on matters as different as the function of Stonehenge and the nature of the Universe. Most referees found him "unsound", often without saying why. But it does not require a referee to tell whether it is a reasonable hypothesis that terrestrial life stems from interstellar bacteria. There remains a place for the occasional unrefereed article.

The other threat to the health and well-being of science is the fierce competition among scientists for money, mostly in the form of research grants and appointments. There is no doubt that the competitiveness of the US system, now being widely imitated elsewhere, has stimulated the US research enterprise to be more productive of good science than would have been expected. But it has also sacrificed large numbers of postdoctoral fellows on the altar of achievement. It seems a waste of young talent that the academic life should be so intellectually bruising. The obvious danger is that the yardsticks of attainment (impact factors, for example) used in the United States are unthinkingly transferred elsewhere. But that seems all too likely.

Civility has been the other casualty of competitiveness, especially in the United States. Endless disputes about individual priority for discoveries are one almost trivial index of the underlying trouble. One incident in the past few months sticks in the mind. An author found that an article submitted to *Nature* was returned on the grounds that it answered only half the question posed, but was then invited to resubmit it when another article dealing with the other half of the question arrived by coincidence. The original author professed himself delighted, but failed to keep a promise to return his manuscript quickly. After a month's delay, it appeared that it was already in the press with another journal. The original author did not have to share the limelight with another.

The English language is another casualty. It used to seem that *Nature*'s contributors wrote clearly, but no longer. But has not science itself become much more intricate? That is true, but not so much as to justify the obscurity of most scientific texts. Nor is it that authors have become lazy; the accompanying figures are always excellently prepared, as are the typescript pages. The obscurity of the literature now is so marked that one can only believe it to be deliberate. Do people hide their meaning from insecurity, for fear of being found out or, in the belief that what they have to say is important, to hide the meaning from other people?

Misconduct

The scientific literature also suffers in ways other than the language its contains. Misconduct of some overt kind is on the rise. We all know why that is. Reputations rest on publications as never before, as do promotions and research grants. Evidently the pressure intensified remarkably between 1973 and 1980. During the spell 1966–73, there was hardly a case of scientific misconduct to worry about. By evil accident, one of the first pieces of mail I read when I returned in 1980 was a warning, from a British academic against the activities of a Jordanian called Alsabti, operating at Houston, Texas, at the time. It turned out that he had been publishing about one paper a month in undistinguished journals, citing very distinguished people as his co-authors. As the world knows, it is only now that the torrent

of scandal is slowing down.

There can be no more important goal for the research community in the next few years than to cut the link between publications and success that this pattern of misdemeanour implies. The extreme cases of fraud are less alarming than the present inversion of the link between the publications records of honest scientists and their success. If we agree that successful people publish a great deal because they have a lot to say, does it follow that people who publish a great deal are successful? Of course not, especially because journals have no means of ensuring that each published gobbet is of similar quality.

I believe that it is still true that almost every issue of every scientific journal contains something that will be surprising, even illuminating. But it is hard to avoid the conclusion that most of what is published need not be published urgently, or where it appears or even at all. That does not mean that the research enterprise is bankrupt, but that people cannot on the one hand complain that they cannot keep up with the literature and, on the other, insist that their survival depends on publication.

Discovery

None of this implies that the past 30 years have been one long battle with referees and aggressive authors. On the contrary, they have been filled with huge interest. Within a few months of *Nature*'s centenary in 1969, for example, we had Mark Ptashne's discovery of the *lac* operon (the first genetic regulatory element to be recognized), and the independent discovery by David Baltimore and Howard Temin that the enzyme reverse transcriptase can convert RNA into genetically equivalent DNA. And there was the discovery of the first pulsating star (now one of thousands). By 1972, people were talking of genetic engineering (or at least of a moratorium in the field for a year or so).

To compare the speed with which understanding is being deepened in the life sciences with what happened in physics in the 1920s is probably flattering to physics. Can there ever have been a time when there have been so many people pushing at an open door as there are now in the parts of biology derived from what was once called molecular biology? Sometimes it seems as if the intelligent statement of a question evokes the answer without much further effort.

With genome technology all the rage, it is natural that there should be a widespread expectation of still further improvements of health-care, at least in the rich countries of the world. That may be over-optimistic. The only certainty is that when the functions of newly discovered genes are better understood, there will be great opportunities for the pharmaceutical companies. Manipulating single genes by gene therapy may yet prove feasible, but manipulating groups of genes that are widely separated but must act in concert will be a headache for years to come. But these are not the important prizes. One is the improvement of crop plants by genetic technology and the other is the understanding of human (and mammalian) evolution that will flow from comparative analysis of the genomes of related species. It would be good to see the Human Genome Diversity Project given a fair wind in the years ahead.

What will happen to the physical sciences, now almost dormant in comparison with biology? Nobody should be disheartened. The overdue upheaval in cosmology will be immensely enlivening for us all, so too will be the understanding that must soon come of why the supposedly fundamental particles of matter in the Universe have just the properties observed, and not some others. These elements of the future are more easily described than they will be brought about, but some puzzles are not even simply stated. The old conundrum: how does the brain work? is not now as intractable as a few years ago, but the question of when life began on the surface of the Earth (or elsewhere) is as unapproachable as ever, enthusiasm for the problem notwithstanding.

Meanwhile, there is technology. The nanochip is on the way, as are the self-assembling electronic components modelled on what happens in cells, brain cells in particular. In due course there will be ways of assembling single large molecules that meet every specification. What use this will be to the world at large is clear only to the extent that the past has shown already what benefits flow from developments of just that kind. *Nature* will no doubt have a big part to play in these developments, as it is already doing.

Nature's other role, as a critic of the research community's manners and of the frequent misuse of science and technology, is something else again. The fuss that has been stirred up by recent developments in genetics will not quickly melt away. The crying need is for a better public understanding of what is being done, and why, together with a set of international agreements on the proper use of these new technologies. Global warming is here to stay, at least as a problem and probably as a phenomenon. Educating the young in novel ways, always with us, will become more taxing. And there remains the plight of the poor in rich countries and the poor in countries where all but a few are like themselves.

The scientific community needs several means of representing its legitimate interests to the governments and other public bodies, most of which are indifferent to them. The British government's lackadaisical transfer of the newish (for a government department) Office of Science and Technology to the Department of Trade and Industry is a particularly scandalous way in which to treat the scientific enterprise, especially when everybody in government is crying that the function of science is to create wealth, and quickly please. But there may be worse to come. Most industrialized countries are busily cutting spending so as to balance their books; science and higher education are easy targets, at least while they do not answer back. *Nature* has traditionally played a part in battles of this kind, and will no doubt continue in that vein.

For my part, I wish *Nature* and my successor, Philip Campbell, the success that they deserve, and thank my colleagues (past and present) for good fellowship over many years and a host of readers and contributors for correspondence that has always been illuminating and often entertaining.

John Maddox