

Loose ends



by Sydney Brenner

As we are all coming to learn, any old list of publications is just not good enough when looking for a job or applying for grants. Candidates' publications are now subjected to microscopic scrutiny but unfortunately not for their scientific content. What is looked for is with whom you write papers and where these are finally published. Today, God would never get a research grant. One member of the committee would deny it on the grounds that the work had been done a long time ago; a second would confirm this by noting that it had never been repeated. Rejection would be clinched by a third member pointing out that, to top it all, the work was published in an unrefereed journal.

I now see many lists where the papers are divided into those published in refereed journals, followed by reviews, abstracts, meetings proceedings, etc. In the old days, neither abstracts nor meetings proceedings were even mentioned, and reviews were included only if they contained an original way of looking at the subject. The best publication list I have ever seen was that of a candidate for some official post who was engaged in defence research. The first two papers were: *Landing aeroplanes on aircraft carriers I & II, Restricted circulation*. The remaining items, numbered 3 to 9, were labelled "Secret". I would have been tempted to inflate the list of secret publications to 19.

The position of your name in the list of authors is very important. Most people seem to go from being the first author on a paper to the last author, without ever writing one by themselves, much as it was said of someone that he went from being a promising young man to a distinguished old man without ever passing through the age of accomplishment. I note a change in the last few years, with the senior author's name appearing increasingly at the head of the list. This should be done only if you are well known, as it carries the risk of you being labelled a recently graduated research student.

The journal in which the paper is published is perhaps the most significant. I have heard seriously discussed that a scoring system should be introduced so that papers in, shall we say, *The Oklahoma Journal of Poultry Engineering* would get 10 points whereas those in *Nature Chicken Genetics* would get 1. I hasten to add that here we would be looking for low-scoring candidates. This would make life simpler for busy committee members but something analogous to vintages would also need to be introduced. Was 1972 a better year than 1989? The most alarming development is that citation rating seems to be taken very seriously. We all know that the most cited papers are those that contain a widely used recipe or method.

There is also good evidence that most authors citing the paper have never actually read it but simply copied it from the references of another paper. I once went to look up one such paper and could not find it, because a mutation had occurred in the page number at one point in a readily traceable lineage.

A particularly ludicrous example of futile citation analysis may be found in *Current Contents* of December 5 1994, where the precursors of modern structural biology are purported to be traced by the author, Eugene Garfield, who invented this type of analysis. By following citations from a starting group of papers that have structural biology as a keyword he produces a list of the 17 "core papers in the field of structural biology" among those that were the most frequently quoted during the period of 1981–1993. There is also a matrix of co-citation frequencies which is supposed to reveal the "hidden structure" of this field, in terms of its connections to immunology, biochemistry, molecular genetics, and so on.

Now, if you know something about the field and the contents of the papers, the structure is by no means hidden but obvious. A couple of the 17 "core" cited papers deal with methods of analysis. A few others describe the determination of new macromolecular structures or of sequences that suggest the occurrence of structural domains. You will understandably find references to zinc fingers and to leucine zippers and, not surprisingly, an analysis of co-citation frequencies among the 17 papers shows that the pair of papers most frequently co-cited happen to be the two on zinc fingers. Most of the remaining papers are simply references from the immunological or biochemical literature to proteins that were being studied during the period, such as lymphokines or proteins involved in gene regulation. Much the same is true of the supplementary list of additional highly cited core papers given in the article. And, whereas I can understand how these kind of papers come to be included, given the method of compiling the list, by no stretch of the imagination can most of them reasonably be considered to be the most important "precursors of modern structural biology".

Before we develop a pseudoscience of citation analysis, we should remind ourselves that what matters absolutely is the scientific content of a paper and that nothing will substitute for either knowing it or reading it. We should also recognize that citation often tells us more about the sociology of science than about the science itself. In rapidly developing subjects, the lifetime of the average paper is exceedingly short, perhaps only months, before it utterly vanishes, never to be referred to again. I have been told that in physics only a handful of papers more than 25 years old are still being cited. It must be very gratifying to have a paper in this class, but better still is to be the author of work that is so well known that it doesn't require a literature citation. If in writing a paper now on DNA one cited Watson and Crick (1953) it would probably be regarded as part of an elaborate joke.