Math matters

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Never mind the quality, feel the width[1]

Mathematics (including Statistics) at UQ sits within the School of Physical Sciences, together with Physics and Earth Sciences. This year the School is due for its first five-yearly review, and it has also been selected as a guineapig within the University for an associated exercise in the assessment of research quality. We are not quite sure what this will involve, but there has been some discussion among the troops of performance-indicators for research, including citation counts and impact factors for journals.

There is no doubt that the *Web of Science* and now *scholar.google.com* are very useful research tools. Furthermore, most people enjoy using them to find out who if anyone is citing and perhaps extending their work (or that of their colleagues!). And who among us haven't felt the disappointment of finding that a piece of our published work that we consider interesting and of high quality, is sitting like a dead duck in the water, creating not the slightest ripple of interest as measured by citations?

But are citation counts and impact factors useful in the assessment of research quality? Few issues cause more animated discussion when they are raised in the Maths Common Room. In particular, it is clear that most mathematicians and statisticians are openly hostile to the use or publication of citation counts, although there are some enthusiasts. My feeling is that the degree of hostility drops off as the research area of the person involved becomes more applied – not surprising perhaps, because citation rates are typically greater in more applied areas – and it is hard to escape the conclusion that mathematicians in general would be much more relaxed about citation counts if only our counts were, on average, much larger than they are, in comparison with those of other scientists.

It is clear enough that heads of mathematics departments would be very unwise to suggest the use of citation counts as a measure of research quality amongst their staff. Unfortunately, in our pre-review situation at UQ we probably cannot afford to ignore them completely, if only because some of our colleagues in Physics and Earth Sciences are justifiably proud of their own citation records and will insist on including them in our review document as evidence of research quality. Indeed, there are certainly some in Mathematics here who feel the same way.

More generally, it seems clear that the Federal Government is determined to afflict us all with some sort of research-quality exercise, so it's perhaps timely for all of us to think about how we are going to handle these issues in the near future. We may hope that all such exercises will be carried out in a more sophisticated and meaningful way than by the mere collection of citation data, but I am not confident that we will get away with saying that impact factors and citation counts are irrelevant or inappropriate for mathematics. The bean counters may see it as incongruous that mathematicians, the most numerate of scientists, have the

[1]The title of this note was used for a British TV comedy in the 1960s, but it has an earlier, uncertain origin.

strongest objection to the use of these concepts. And an argument that it's because we are mathematicians that we can see the weaknesses of attempts to quantify the quality of research, is unlikely to cut much ice unless we can back it up with careful reasoning. If our citation counts and the impact factors of the journals in which we publish become perceived in Government circles as unimpressively low, it may become that much harder for us to argue that research in mathematics is important and relevant.

The first objection that is commonly raised against citation counts is their unreliability. Invariably, someone will say that they know of at least N citations of their works that don't appear in the *ISI* database. This is a problem for mathematicians mainly because our counts tend to be small. I don't hear the biologists or theoretical physicists that I mix with complaining much about the reliability of citation counts, and that is presumably because their counts are typically much larger and a few missed citations don't affect their totals as much. Citation counts, or average counts per paper, might be a more reliable indicator of quality for our department as a whole, or for sizeable research subgroups, rather than for individuals, if we could collect the data. But then we would need the data for similar groupings elsewhere for comparison. In this connection, I see that UNSW announces proudly on its School of Mathematics web page (and indeed, why shouldn't it?) that it ranked fourteenth in the world on the basis of citations of its papers in mathematics over the ten-year period 1991-2001. They go on to claim that they are "number one in Australia for both quality and quantity," so they at least are confident of the reliability of citation counts when used for their whole department in this way.

Another common objection to citation counts for individuals is that a paper may be widely cited because it contains faulty conclusions. This one I regard as a furphy. Researchers whose counts have been inflated in that way would be well known to their colleagues, and seem to me rather unlikely to gain much benefit from their sins. This is not to dismiss the importance in research, if one gets something wrong, of getting it wrong in an interesting way. Self-citations are also a cause for objection to citation counts, but this too is not a very strong objection, I believe.

There are better reasons for not giving citation counts too much weight. One is that token citations are common, in the applied sciences especially. These are there because the citing authors feel they should be there for completeness, whether or not they are really building on the results of the papers they cite. Such citations tend to be rather vaguely described and to appear in nested groups: "Much research has recently focussed on blah [m–n]."

This often happens with what could be called, somewhat unfairly, bandwagon research. A bandwagon gets under way when someone, usually a well-known big player in a field, opens up a new area with a breakthrough paper, and hordes of smaller players quickly concentrate their attention upon it and explore every possible variation. I am reminded of piranha. It is a common phenomenon in the applied sciences, but much less common in mathematics.

I don't want to be too critical of bandwagon research because it is after all the main way in which the cutting edge of scientific research keeps advancing: unpredictable leaps ahead, followed by backing and filling. And which researchers should be judged to be doing the more valuable work? The ones who get on a bandwagon where the hot topics are obvious but the competition is fierce; or the ones who are off in a quiet corner, trying to find original problems of their own and chipping away in an area that's of current interest to only a few colleagues around the world, with little or no danger of being scooped, with little pressure to obtain results quickly, but with the hope and dream of a big breakthrough?

Certainly the former are more likely to be rewarded with higher citation counts.

The main objection to the use of citation counts is surely that it tries to trivialise the whole research exercise and to measure by a few numbers the quality (whatever that means) of an enormously complicated human endeavour. That is the real reason we are uncomfortable with it. However, I fear that it will be difficult to convince Government bean counters with this argument. Reducing the measurement of research quality to a few numbers is exactly what they would like to do, and unfortunately their power and influence seem to increase steadily.

Why is it that citation counts are lower for mathematics? An obvious reason is that the bibliography of the average research paper in mathematics is shorter than for many other disciplines. My very rough estimate, based on looking at a handful of research (not review) papers on the *Web of Science*, is that a typical paper in pure mathematics cites ten or fewer sources; a typical paper in applied mathematics about twenty; a typical paper in theoretical physics about thirty; and a typical paper in the biological sciences about fifty. (I'm sorry but I didn't have time to look at papers in statistics as well.) It's not so clear what is the reason for this wide variation. People tell me it's that each field has its own culture, even to the extent of judgements as to what is the real purpose of a bibliography – a guide to background for the reader, or a comprehensive listing of relevant sources – but I don't find this very convincing. Perhaps research problems in mathematics, especially pure mathematics, tend to be more narrowly defined, so that fewer background papers need be cited. Perhaps it's that journals in more applied areas tend to require authors to provide introductions accessible to a wide readership. Undoubtedly there is also the matter of how 'hot' is the relevant research area. I have had limited experience of bandwagon research on a really hot topic, but enough to know that other authors on the bandwagon get very angry if you fail to cite what they consider to be their relevant papers or even their preprints.

Whatever the reasons, citation counts are lower for mathematics and we may have to deal with that when research quality is being assessed. A useful suggestion that has been made to me is that for each of our research subgroups, rather than looking at citation counts, we might identify the top ten or so journals, and then report how many publications we had in those journals over the past five years. This is a good idea, and we shall probably adopt it, giving each subgroup the option of including citation data as well if they wish.

Then comes the question of how we identify the top journals in a research area. This is probably best left to the people in each subgroup to decide, but we could look at impact factors which, on the face of it, can help to identify the top journals in any field. As far as I could tell from the *ISI* database, the five top-ranking journals in pure mathematics (based on 2003 data), with their impact factors, are

Bulletin of the American Mathematical Society (3.65)

Journal of the American Mahtematical Society (2.46)

Communications on Pure and Applied Mathematics (2.25)

Acta Mathematica (1.79)

Annals of Mathematics (1.51)

I don't know if any of the pure mathematicians among us would dispute that these are five of the very top journals, although I am sure that some might change the order, or have a slightly different top five. Candidate top fives or tens for different research sub-areas could perhaps be identified in this way, using impact factors, without causing much argument.

The problems with impact factors, as with citation counts, arise when people start to attribute too much weight to them.

When commenting on research quality, colleagues in physics seem to talk a lot these days about how many '*Physical Review Letters*' or '*Natures*' so and so has, presumably because these journals have very high impact factors (7 and 31, respectively). And a senior colleague in the biological sciences with whom I have published, is now reluctant to use *Journal of Theoretical Biology* as a journal of first choice because of its "low" impact factor 1.55. A culture seems to be developing in science generally that pays a great deal of attention to impact factors as well as to individual citation counts, and I fear that as mathematicians we may have trouble with both in the coming years.

I was slightly surprised to discover from the Web that there is an active field of study of the measurement of research quality called *scientometrics*, and even a well-established journal by that name. The immediate reaction of a colleague when I told him this was, "What's its impact factor?" So I checked: It's 1.25, which is higher than the impact factors of most mathematics journals. Perhaps this one observation, more than anything I have said above, shows how silly it would be to attach a lot of weight to this whole business.

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