

# Publishing and assessing mathematics

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## Browsing the journals

“The past is a foreign country — they do things differently there”, said L. P. Hartley. One thing they do differently is the dissemination of mathematics, which was more localised and more leisured than at present. The Oxford mathematics don Pedby in Max Beerbohm’s *Zuleika Dobson* had achieved a European reputation for his work on short division of decimals; now any piece of mathematics is instantly available worldwide.

Cauchy went through a period of intensive work on group theory; he would write a long, discursive paper for the *Comptes Rendus* every week, and expect to see it in print the next week.

The style of writing was very different too. As a graduate student, I spent a lot of time reading Burnside’s *Theory of Groups of Finite Order*. He would launch into several pages of close argument, at the end of which he would state his conclusion as a theorem. I have the impression that he would not have expected his readers to look ahead to see what was coming!

Now, things are much tighter. Any mathematician can tell you horror stories of papers which languished for years in the publication process. I wrote two articles and a short tribute for a journal volume dedicated to a mathematician for his 80th birthday; the volume came out just in time for his 85th.

As to the way we write, we train our students to write strictly in “theorem-proof style”. Proofs must be clearly labelled as such, and should follow, not precede, the theorems; some journals even enforce the use of an end-of-proof symbol. It is usual to have the content of the paper summarised in an abstract.

Of course all this is necessary. I don’t know by what factor the volume of mathematics published per annum has increased since the time of Cauchy and Burnside, but it is completely impossible for any mathematician to read more than a small sample of the papers in her field now. For dabblers like me, who work in several fields, things are even worse. At least, reading the abstract and the statement of the main theorem is usually enough to tell me whether I need to read the paper.

My older colleagues typically spend a period each week looking at the new journal issues in the library. Even older volumes are well worth browsing. My own observations suggest that, if I take a journal volume from the library shelf to read a paper, the chance that it will contain at least one more interesting paper is very high. But my current students prefer to sit at their computers reading journals on-line or printing out preprints from various websites, and in some cases are quite reluctant to go to the library at all.

One thing that is lost here is an element of serendipity, which is crucial to research, in my

opinion. Perhaps a paper on a very different subject will contain the clue to the problem I am stuck on at the moment.

Is all this a problem that needs addressing? It seems to me that it is related to three rather serious issues which are on the horizon for mathematics now.

### **Information overload**

Where do I look for information about my field? The electronic catalogue in my library includes fourteen journals with “algebra” in the title, to say nothing of the *Journal of Group Theory*, *Semigroup Forum*, *Theory and Applications of Categories*, and so on and on.

Then there are the non-specialist journals such as *Crelle’s Journal*, *Advances in Mathematics*, and national or local journals ranging from the *Michigan Mathematics Journal* to the *Journal of the European Mathematical Society*. Some of the best papers appear here, so I can’t neglect them.

There are simply not enough hours in the day for a researcher to monitor the flood of new publications.

What about a keyword search? We have no agreement about what are relevant keywords. Apart from its widespread use in algebraic geometry, the term “scheme” is used with an unrelated meaning in combinatorics and statistics. Even in these subjects, authors can’t agree on the definition! It would not surprise me to learn of other unrelated uses of such a handy term.

We are on the horns of a dilemma here. Any method of searching or filtering this information which produces few enough results to be useful is also likely to miss out on some very relevant material. We can guarantee that what the search engine serves up to us will have a

high proportion of nuggets, but we will have very little idea about what we have thrown away unpanned.

### **Open access**

The second problem is open-access publishing. This new revenue stream for publishers took longer to reach us than some other subjects; it has been an issue in medical sciences for several years. The idea is simple. The results must be freely accessible to everyone. But someone must bear the cost, and publishers are not charities. So authors should pay.

It is now not uncommon for a mathematician to receive a letter from the publisher, when a paper is accepted, saying that on payment of a sum of money (typically a four-figure sum in US dollars), the paper will be available to all readers without charge. (This is not the same as page charges, which have been with us for some time. Page charges were always optional, and were a couple of orders of magnitude smaller.) We are supposed to feel good about this; we are making a donation to research in countries where the universities cannot afford the very high subscription charges that most publishers impose. The very name seems to echo such trends as open-source software and give us a warm glow.

New journals are starting up which make such payment a condition of publication. If this were to become common, a few prolific authors could break the budget of a small mathematics department.

The average mathematician laughs when asked for such a sum, and says “I can put it on my web page, or on the arXiv, for nothing; why should I pay?”

A recent news item had the remarkable effect of making me feel, against my instincts,

a little sympathy with open-access publishing. The issue was debated by the US Senate; it seems that the Senators were lobbied by traditional publishers claiming that this would be the death of refereeing and the rise of junk science. Of course, as long as somebody pays, journals can continue refereeing papers just as they do now (or more precisely, just as we do, for free, as another contribution to the publishers' income). So this was just a diversionary tactic.

But the scare tactic might be relevant to the alternative view, the one which advocates just putting everything on the arXiv and letting everyone read it there. Since this already happens (even published papers now cite preprints on the arXiv), it seems we may have been caught unawares. Do we have junk mathematics already?

Can you trust a paper on the arXiv? Can you trust a paper in a refereed journal, for that matter? Even such papers contain mistakes!

This is a serious problem in most parts of science. How can I trust a paper reporting an experimental result? Only by repeating the experiment myself to check the conclusions. Even if I have time, this may be quite beyond my resources. In parts of science where the theoretical basis for prediction is weak, I can't even know whether the result is reasonable unless it is in my specialist field.

Mathematicians, of course, have a different test. We can read the paper and check its claims ourselves (in effect, do a personal refereeing job). So is there a problem? Well, yes. In Cauchy's time I could have sat on the sofa and read the current *Comptes Rendus* from cover to cover. This is no longer feasible, for two reasons.

Even a single result may be out of reach. One of the most well-used theorems of re-

cent times is the Classification of Finite Simple Groups. The proof has only recently been completed, but the result has been used for many years because of a premature announcement. But the proof covers very roughly ten thousand pages. I have often used the theorem, but I am simply incapable of checking the proof. I trust it because each piece has been read by many people. Important theorems such as Fermat's Last Theorem or the proof of the Poincaré conjecture will continue to receive such scrutiny, whether as formal refereeing or not. But a moderately interesting but quite long paper on the arXiv? Is there any estimate of how many readers such a paper might have?

We can't really have quality control without refereeing. Also, quality control depends on there being a definitive version of each paper, rather than a system where the paper can be updated at any time (attractive as that might be to authors, especially if a mistake is pointed out).

## Assessment of research

This brings me to my second point, the assessment of mathematical research. We can't avoid this; our livelihoods depend on decent assessment of our work. The days when researchers were supported by family fortunes are gone, and we depend on funding agencies who are under many pressures, including shortness of funds and public accountability.

How is mathematics assessed?

The only sensible way is for someone to read it, and make the kind of judgement that referees make: is it correct? is it important? (and is it well-written? but the funding bodies seem less interested in this). At present, the best assessments do at least approximate

to this ideal. (But the review panels are made up of people whose time, like ours, is limited, and so they tend to regard publication in a refereed journal as some kind of quality assurance, so that they can give a lighter touch. For this reason, the rules sometimes specify refereed publications.)

The current UK model of research assessment invites each researcher to submit four papers published during the review period. The panel read and judge the papers. At least, this is what used to happen; now they read a proportion of the papers (at most half) and assume that this allows a fair judgement of the others. This still involves each panel member reading several hundred papers over one summer; a very substantial commitment of time by people who are themselves among our best researchers.

This scheme does not go down well with the funding bodies, since such judgements are subjective. “Transparency” has become the watch-cry; those who utter the word seem unaware that it is not synonymous with “fairness”. The funders would prefer an “objective” formula for their decisions. This requires numerical measures of the quality of a piece of mathematics.

Enter the metrics: citation index, impact factor. Couldn't we judge a paper on these numbers? In many parts of the academy, this already happens. Is maths really different?

A paper may be cited in order to refute it. In most of science, this is at least partly positive: the paper stimulated further investigations which showed that the original hypothesis is not confirmed. This is how science progresses, after all. But in mathematics, it is true to a first approximation that if a paper is wrong, it is worthless.

More seriously, the timescales are quite in-

appropriate for us. For the citation index, a window from one to two years after publication is examined. To our colleagues in science this might be reasonable: publication times are fast, and after two years the subject has moved on or died. Mathematicians look on with blank incomprehension at such debates. A paper which generalises my theorem is not likely to be in print within two years; and if my theorem is of any worth, people will still be citing it after twenty years (or two hundred, in the case of Gauss; or nearer two thousand, for Euclid.)

A further problem is that any introduction of formulaic assessment will have the result that researchers will aim for the assessment targets rather than for real research quality (and we believe we know what that is!) In the first Research Assessment Exercise in the United Kingdom, papers were simply counted, each paper weighted in inverse proportion to the number of authors. Apart from discouraging collaborative research, this caused a big upsurge in “salami-slicing”, which could have been foreseen by anyone but the civil servants who designed the exercise. It was necessary to put some effort into undoing these effects. But the lesson has not been learned, of course.

## **A common problem**

But at least we and our assessors have a common problem: how do you recognise good mathematics, without spending a lifetime on it?

Some pointers already exist. There are completely free journals on the web, run entirely by volunteers, which manage to achieve a high standard. (In my field the Electronic Journal of Combinatorics is one such.) There is the Wikipedia model: anyone can publish, anyone

can edit. This would not be appropriate for mathematics, where as we have seen a definitive version of a paper is required.

A halfway house is to invite comment on a paper (which may include refutation, or corrections). This is available in electronic journals now, but in my experience is not much used. Another proposal involves an archive with a facility for readers to vote.

Meanwhile, the learned societies continue to publish journals at reasonable prices, even though they depend on publishing for the vast bulk of their income. They are anxiously watching the publishing free-for-all and trying out different ways of dealing with it: starting their own specialist electronic journals with guaranteed high standards, allowing free access after an initial period of subscriber access, and so on.

Is there a better way? Is there a good solution to any of these problems? Answers on a postcard please; maybe the next Maths Matters column can publish the best . . .