

# The New Publishing Scene and the Tenure Case: An Administrator's View

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When I received my Ph.D. back in 1981, the rules were simple. You published in peer reviewed journals (impact factors, though introduced by Thomson Reuters in the sixties, were not yet in everybody's vision), and that's about all you needed to know.

As I aged, I did not lose my passion for mathematics but became more involved with administration, becoming first chair, then dean, then provost, and now chancellor (I hope, like Palpatine, to be promoted to emperor soon). In this new capacity, I find myself in charge of the last and often key evaluation in the tenure process, and the many changes that have occurred recently have made this process more challenging than in the past. The case of mathematics, in particular, presents three new challenges that I will briefly discuss here.

## **Electronic Publishing**

Though many journals now live in cyberspace (I rarely go to the library to read a paper; rather I do so at home on my computer), we are still able to offer a first rough evaluation of the value of a

paper by looking at its placement. However, a new trend has emerged that makes this more difficult. In the last several years, the number of papers that are simply posted on arXiv, for example, has grown significantly, and it is not infrequent for a CV to contain papers that only appear on arXiv. Should one disregard them because they have not been vetted through the revered peer review process?

This seems hardly fair. After all, we are supposed to read the papers, not to blindly accept the referee's opinion. Those of us who have been involved in refereeing have plenty of horror stories to share. I once recommended a paper for publication, and the journal sent the authors a heavily redacted subset of my review, as an excuse not to publish the work. I have also seen many less than professional reviews, where a paper is accepted by somebody who clearly has not really read the paper or where a paper is rejected by somebody who is prejudiced against a certain field.

Thus we cannot count only on placement, and we need to actually read the works before us. Well, this is easy to say for us in mathematics, but (I hope you will forgive my arrogance) how can we expect a dean or provost from the humanities to actually read a paper in mathematics or in physics? So, the advice I would give to any faculty is to certainly use electronic databases such as arXiv (particularly useful to protect priority when working in a rapidly developing field), but I would also

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encourage authors to move their work steadily from arXiv to refereed publications in good quality journals. This brings us to the second issue.

### Impact Factors and Other Metrics

When I was young, I sought publication in journals that I knew were reputable because I had read interesting papers in them. Thus, for example, I published my dissertation in the *Memoirs of the AMS* because that's where my scientific grandfather (Leon Ehrenpreis) had published his. Now, whenever I choose a publishing venue, I look at its metrics in MathSciNet. When I review tenure files, I see reports that include, for every journal, a variety of metrics, including, for the candidate, a citation list that tells me how many times his work has been cited. How relevant are those numbers? Taken individually, every one of those numbers is irrelevant. For example, it is very well known that impact factors can be manipulated and that they are not really suitable for individual evaluations. In this case, it has been said, the h-index (another measure based on citations and introduced also by Thomson Reuters) is a more useful instrument. In my opinion, one has to exercise caution and prudence. A publication in a journal with very low impact factor should not condemn a tenure case, just as a single publication in a high impact factor journal should not be considered the stamp of approval on the case. More important is the balance, and especially (when I look at faculty from different disciplines) the realization that different bibliographical and stylistic conventions render cross-disciplinary comparisons untenable (for example, *Nature* and *Science* have impact factors in the 30s, while the most prominent mathematics journals have impact factors between 2 and 3). My recommendation, which I am sure is already being taken into account by most administrators, is to look at the balance of the résumé. If none of the papers appear in a good journal (the definition of "good" being subjective, but I believe we would find substantial agreement among experts), then we do have a problem. Either the papers are not very good or the author is lazy (and prefers to send papers to journals where acceptance is more readily obtained) or the author has low self-esteem. The decision as to whether the lack of prestigious journals is an indication of low quality or timid behavior is a burden that the chair, the dean, and the provost need to bear. We get now to the last issue I want to discuss.

### Multiple Coauthors

In biology, chemistry, physics, and most other natural sciences, there is a long tradition of publications with multiple authors. Because of the natural need for scientists to work in large teams, we have now a well-established convention. The first author is the one who did the majority of the work

in the paper. The last author is the scientist who is responsible for the laboratory where the work is being done. Thus, these are the two prestigious positions in an article. Everybody in the middle has probably contributed in a more limited way (if at all) to the paper. It is quite possible that the middle authors are just members of the lab and their contributions are minimal. In mathematics we behave differently, in what appears to be a more "democratic" fashion. Authors are almost invariably listed in alphabetical order, and an unaware dean or provost may incorrectly interpret the value of the contributions of his or her faculty. The alphabetical usage is now being extended to other disciplines, as we see, for example, in the recent articles which contain the first results from the European Large Hadron Collider: a cursory look at arXiv will show articles with literally hundreds of authors in alphabetical order. One wonders how such articles may be evaluated in the course of a tenure process. This did not used to be a significant problem, but the average number of authors on a mathematical paper seems to be growing each year. If one takes a cursory look at MathSciNet, it will be evident that the large majority of papers in 1955, for example, were single authored. Compare this with 2005, where the large majority of papers have at least two authors. In fact, a recent article in the *Notices* shows that while in the 1940s more than 90% of the articles in mathematics were single authored, now the percentage has declined to about 50%, and more than 10% have three or more authors. How are we to judge our candidates for tenure? Once again, I make a plea for balance and coherence. An author who has no single-authored publications may raise some suspicions, but if (s)he has many different collaborators, this can simply be a sign of his or her collaborative style and preference. Generally speaking, I would advise a junior faculty member to establish his or her reputation with a few well-placed, single-author contributions, and then (if that's their pleasure) they should feel free to engage with other scholars and publish as they prefer.

It is clear that the nature and form of scholarly publication and scholarly discourse in general are changing and evolving, and our methods for evaluating scholarly work should grow and change with it.