



JFM in the electronic age

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I recall sitting at lunch with George Batchelor towards the end of the '90s when he mused how amazing it would be for *JFM* to reach volume 500. That was to happen in 2004, four years after George's death, shortly followed in 2006 by the Journal's 50th anniversary. Now here we are, a mere 20 years later, celebrating Volume 1000!

On the Journal's 25th anniversary (vol. 106, 1981), George wrote an article 'Preoccupations of a Journal Editor' reflecting on the shape of scientific publication through the lens of his experience with *JFM*. The final section of the article was devoted to the future of journals, including the possibility of an 'electronic journal', and it is prescient of many of the developments and their associated challenges that have occurred in the subsequent decades. I pick up that discussion from where George left off, at the dawn of what I consider to be the Electronic Age of scientific dissemination.

Others will have different perspectives and definitions of the start of the Electronic Age but for me there are two landmark events that can reasonably be considered as signalling that dawn. The first was the almost simultaneous launch in 1984 of the IBM PC/AT and the Apple Macintosh. To be sure, there were desktop computers before that, but they were mostly the preserve of hobbyists or used as smart terminals to mainframe computers. The significant processing power of those models and the extensive marketing around them made personal computers household commodities and, for academics, staple workhorses. That year also saw publication of *The T_EXbook* by Donald Knuth and, as it happens, my first experience of a laser printer. These innovations collectively began a quiet revolution in journal publishing in which, as George wrote, 'an appreciable part of the cost of typesetting [is] passed back to the first and only keying-in process' overcoming the 'formidable problems of standardization of the recording of mathematical formulae' and 'the [prohibitive] capital cost of the word-processor equipment.'

For me, the second landmark event occurred a full decade later when Comet Shoemaker-Levy 9 impacted Jupiter in 1994. The most astonishing thing was that images of the collision captured by NASA could be seen on my desktop computer using the World Wide Web (internet) once I had learned how to obtain a primitive browser by word of mouth. It is now a dim memory how we could only 'surf' the internet, having to explore link after link randomly, without the aid of a search engine. It is surely the internet,

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particularly with the aid of powerful search engines, that led to the democratization of knowledge (e.g. Wikipedia) and ultimately to the open-access movement that has so fundamentally affected the promulgation of research and posed significant questions regarding the nature of academic publication.

As it happens, 1994 is also the year that I joined the *Journal of Fluid Mechanics* as an Associate Editor. Almost immediately, there were discussions amongst the editors around the large committee table in the DAMTP (Department of Applied Mathematics and Theoretical Physics) about the threats and opportunities posed by the internet. Some of our colleagues believed that *JFM* would soon be pushed out by competitors if we did not become fully electronic by the end of the century, and Cambridge University Press (CUP) were similarly alarmed by the potential threat. As an aside, it is often forgotten or neglected that, although a not-for-profit academic publisher, CUP is subject to commercial pressures to maintain the income stream that supports important but loss-making academic publications in diverse fields, and is rightly concerned to remain viable. It is interesting to note that, in its first two years, the financing of *JFM* was felt as a real headache by its Editor and was loss-making for its publishers (see the articles by Keith Moffatt and Tim Pedley in this volume). By the time of our discussions in the 1990s, *JFM* generated more revenue for CUP than any of its other journal publications by a large margin, so its viability was of great concern. That *JFM* did not become solely electronic until the end of 2019 is due in large part to the continued commercial viability of its printed volumes, despite very many libraries moving to online-only subscriptions. Even George's college, Trinity College Cambridge, stopped taking hard copies at the end of 2006.

Nevertheless, the editors and publishers were not complacent and, as their first foray in the electronic age, worked towards making new volumes of the Journal available online from 1997 and creating a digital archive of existing volumes up to the end of 1996, forty years' worth of material. That latter task took a further decade to realize, in large part waiting for technology to improve sufficiently to create a useful product. Not only did a complete set of paper volumes have to be found, cut up and scanned as high-resolution images but there was also considerable work required to create a sublayer of textual information, using a combination of optical character recognition (that is, OCR) tools and manual re-keying of all the important meta data, so that the volumes could be searched.

The ability to search and re-search academic literature has enabled arguably the greatest impacts of the electronic age. The advantages are clear. I recall vividly spending an afternoon going line by line through the accumulated, printed indexes of *JFM* when I was a PhD student to see if anyone had published previously anything related to the problem I was working on. (It didn't occur to me to search other journals quite so diligently!) And forward referencing, finding out who might subsequently have written on the topic of a paper one was reading, was even harder, although our library did subscribe to the Science Citations Index (in print form of course). A side effect is more pernicious. Once searching is facilitated, so is counting. Recent decades have seen a growth in measuring the output both of journals (impact factors) and of individuals (H-indices), based on citations from other academic outputs, and has been commercialized, with additional products such as Altmetrics based *inter alia* on social-media attention. The appearance of objectivity that is created once a quality is assigned a number can lead people to undervalue expert judgment, and many arguments have been made against using such indices for the purposes of recruitment or promotion. In my opinion, there is an even greater danger. It is not possible to measure a system without affecting its trajectory, as any student of quantum mechanics understands. And systematic measurement can create perverse incentives, to the extent that science can be driven to become attention-seeking rather than problem-solving.

I was determined not to fill this article with statistics but find myself wanting to draw attention to one trend that is not much talked about. The average number of references included per paper of Volume 1, Issue 1 of *JFM* was 12, with a low of 3 and a high of 19. The average number in the most recent volume of *JFM* as I write is 55, with a low of 21 and a high of 90. It could be argued that this increase results from the much greater pool of accumulated knowledge over the past almost seventy years. But I wonder how much relates to the ease of searching mentioned above and a feeling that one should cite every paper whose abstract one has read, and how much relates to the fact that citations are now a 'currency', and that we owe it to our colleagues and to ourselves to be generous with it. Whatever the reason, I worry that as a scientific community we have strayed somewhat from the ideal that references are only included that contribute to the scholarly content of the article.

These sorts of considerations lay behind the recommendation that no more than 50 citations should be included even in the lengthy review articles written for *JFM Perspectives*, on which Paul Linden reflects elsewhere in this volume. I think that there is a strong argument that fewer citations are needed intellectually in this age of search engines powered by artificial intelligence (AI), but this sentiment is unlikely to find favour with journals who commercialize their impact factors nor with individuals so long as careers depend on such measures. The citations that are most important are those whose results are used directly and built upon and those that the new research corroborates or amends.

In 1982, as a PhD student, I spent six months visiting the Australian National University in Canberra and recall waiting two months after its publication for the latest volume of *JFM* to arrive by sea. The slow pace of distribution was simply the last step in a series of slow processes from submission to readership. Four copies of the single-sided, double-spaced typescript had to be submitted (one for the editor, three to be sent to referees), usually by second-class mail to reduce cost given the weight of paper involved. Referees were rarely asked in advance for their willingness to review a paper, which undoubtedly increased the time that manuscripts languished on people's desks before being reviewed. And typesetting for hot-metal presses, particularly of mathematical formulae, was a much slower process than all the electronic means utilized today. The average time from submission to publication for *JFM* articles was around 14 months when I was first an editor. To be fair to the processes just mentioned, it should be noted that approximately half of this time was the time that authors spent revising their papers, which was time well spent.

Given the long delays between new ideas being developed and being available 'publicly' (essentially just to academic libraries and individual subscribers to journals), it was very common for academics to share preprints of their work with colleagues they knew to be working on related topics. This was done partly to garner informal feedback to add to the formal referees' reports and thereby to improve the final publication, and partly just to keep one's community informed of progress. Such communities could be quite small and feel closed, although they were not closed by design: most academics are only too keen to have their work widely known. Separately, to have interesting papers immediately to hand in one's office and given that making photocopies from a bound journal was awkward and produced poor-quality images (particularly of photographic plates), it was widespread practice to distribute reprints to colleagues and many others who requested them, often using pre-formatted postcards. *JFM* would provide 50 free reprints to the authors for that purpose, and authors could (often would) purchase many more.

The dissemination of preprints has been greatly enhanced in the electronic age both privately by email and publicly on preprint servers such as *arXiv*, founded in 1991. For the scientific community, both professional and amateur, it is surely a good thing to have ideas

shared widely and freely in this way, stimulating discussion and potentially triggering new ideas. But, as with many things in life, the pure originating ideal can later pose significant questions and challenges. For example, what value should such preprints be afforded in the various indices discussed above? Today, they are not normally referenced in Web of Science but they are in Google Scholar, for example. In the days of private sharing of preprints, it was much more common to see '(private communication)' written against some idea that was referred to in a paper, and it seems to me that an article posted on a preprint server is a private communication in the same spirit: it is posted privately even though it is available publicly. As for discussions around whether preprints posted on public servers should be citable within other papers, it seems appropriate to bring attention to related ideas but not to build on them as if they were established. If the preprint is citable, is it therefore a 'publication'? Given that most journals, including *JFM*, will not republish material that is already published, they should perhaps not be considered as such. The counter argument, made by some individuals at least, is that they should 'count' as publications for the purposes of indices and research assessment.

Given that such online material is publicly accessible, we should consider not just the recognition value that may accrue to the author but, more importantly, what trust value can be placed on the articles by the public and by policy makers. Addressing the latter concern is the principal argument put forward for peer review; not that peer-reviewed papers are infallible but that there has been some expert scrutiny of the methodologies used and conclusions drawn. This is a hugely important service that rigorously peer-reviewed journals provide. I believe it to be the minimum necessary for the sake of scientific credibility but not quite sufficient for scientific discourse or society. Peer review is also hugely beneficial to the communication of science: If ideas are not conveyed clearly to an expert reviewer, how will they be found by a more general reader? Thorough revision in the light of constructive referees' reports is a duty on us all. And not everything that is new is worthwhile. Especially in an age of fast computing, new results can easily be generated by changing a parameter or two, but practitioners can do that for themselves. What is important and useful is when new conclusions are drawn, when new methodologies are developed, when new insights are made and when new applications are addressed. The best peer review focuses attention on such significant advances and enhances quality in those regards.

As much as good peer review adds intellectually to the quality of publications, attention to detail in production, from copy editing to typesetting to hosting online to archiving and connecting to external indexes, enhances the accessibility and communication of ideas. *JFM* is served admirably by CUP in these respects. For example, the seemingly trivial interference of re-lettering to ensure consistency of typeface between axis labels, figure captions and the body of text, which can appear in different fonts and sizes in submitted manuscripts, improves lucidity significantly. And there have been some major innovations made possible by electronic dissemination. The first was the hosting of video content, introduced in 2004, which allowed the presentation of continuous temporal evolutions that are so important in fluid mechanics. The most recent is *JFM Notebooks*, interactive objects that execute code and visualize data, allowing readers to explore different views of complex flow fields or adjust parameter values in a general computer model of some flow phenomenon.

The ease of distribution in the electronic age has facilitated public access to scientific information greatly, as mentioned briefly above. Rather wonderfully, huge swathes of the general public are drawn by scientific blogs and YouTube channels to further their general interests. More specifically, the internet allows people to learn about scientific advances, medical advances for example, that may concern them personally, and they surely have a

legitimate expectation to be able to access the primary research. These sorts of concerns fuelled the open-access movement, whose philosophical roots seem to go back at least to the 1940s (Wikipedia) but which really took hold in the electronic age. The drive to make scientific publications free to access posed challenges for established journals such as *JFM*, whose necessary income derived from subscriptions. The willingness of many funding bodies to support an alternative model of article processing charges (APCs), by which individual authors (their sponsors or institutions) paid for the costs of production and distribution upfront, encouraged an explosion of new journals unencumbered by commercial pressures of saleability. It is worth noting that *JFM* had such a strong subscriber base that to have replaced subscription income solely by APCs would have made the latter prohibitive. Debate about open-access publishing and how it should be paid for has literally raged throughout this century so far. Universal sharing of scientific ideas and insight, to inform and to be open to scrutiny, is surely ideal. The challenge to enable that without creating perverse or corrupting incentives is complex. I won't write more here except to say that *JFM* – its editorial board and its publisher CUP – has worked hard to maintain its core values while adapting to these challenges.

In closing, I return to the importance I attach to peer review and acknowledge the huge debt of gratitude we owe to each other within the community of fluid mechanics for the diligence, care and thoroughness devoted to refereeing articles submitted to the Journal. It is that, above all else, that has made *JFM* great over the past 1000 volumes and will sustain it into the future.

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