Forum

Tragic Loss or Good Riddance? The Impending Demise of Traditional Scholarly Journals

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Introduction

Traditional printed journals are a familiar and comfortable aspect of scholarly work. They have been the primary means of communicating research results, and as such have performed an invaluable service. However, they are an awkward artifact, although a highly developed one, of the print technology that was the only means available over the last few centuries for large-scale communication. The growth of the scholarly literature, together with the rapidly increasing power and availability of electronic technology, are creating tremendous pressures on journals. The purpose of this article is to give a broad picture of these pressures and their likely outcome, and to argue that the coming changes may be abrupt.

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It is often thought that changes will be incremental, with perhaps a few electronic journals appearing and further use of e-mail, ftp, etc. My guess is that change will be far more drastic. Traditional scholarly journals will likely disappear within ten to twenty years. The electronic alternatives will be different from current periodicals, even though they may carry the same titles. There are obvious dangers in discontinuous change away from a system that has served the scholarly community well [Quinn]. However, I am convinced that future systems of communication will be much better than the traditional journals. Although the transition may be painful, there is the promise of a substantial increase in the effectiveness of scholarly work. Publications delays will disappear, and reliability of the literature will increase with opportunities to add comments to papers and attach references to later works that cite them. This promise of improved communication is especially likely to be realized if we are aware of the issues and plan the evolution away from the present system as early as possible. In any event, we do not have much choice because drastic change is inevitable no matter what our preferences are.

Predictions and comments in this article apply to most scholarly disciplines. However, I will write primarily about mathematics, because I am most familiar with that field, and the data that I have is clearest for it. Different areas have varying needs and cultures and are likely to follow somewhat different paths in the evolution of their communications.

Growth of Literature

The impending changes in scholarly publications are caused by the confluence of two trends. One is the growth in the size of the scholarly literature; the other is the growth of electronic technology.

The number of scientific papers published annually has been doubling every ten to fifteen years for the last two centuries [Price]. Similar growth has been occurring in mathematics alone. In 1870 there were only about 840 papers published in mathematics. Today, about fifty thousand papers are published annually. The growth has not been even. A more careful look at the statistics shows that from the end of World War II until 1990, the number of papers published was doubling about every ten years [MR]. Growth has stopped recently, but this is likely to be a temporary pause of the kind that has occurred before.

The exponential growth in mathematical publishing has interesting implications. Adding up the numbers in [MR] or simply extrapolating from the current figure of about fifty thousand papers per year and a doubling every ten years, we come to the conclusion that about one million mathematical papers have ever been published. What is much more surprising to most people (but is a simple consequence of the geometric growth rate) is that almost half of these papers have been published in the last ten years. Even if the rate of publication were to stay at fifty thousand papers per year, the size of the mathematical literature would double in another twenty years. While this rapid growth is a sign of vitality of our field, it creates problems.

Scholarly publishing has some features that sharply differentiate it from the popular fiction or biography markets and make rapid growth difficult to cope with. Research papers are written by specialists for specialists. Furthermore, scholars do not receive any direct financial remuneration for their papers. They give them to publishers only in order to disseminate the information to other scholars. This means that radical changes are more likely to occur in scholarly journals than in mass market publishing, because the interests of scholars and publishers are different.

Scholarly publishing would face a minor inconvenience and not a crisis if the scale of this enterprise were small enough. If a university department were paying $5,000 per year for journals, it could deal with several decades of doubling in size and cost of the subscriptions before anything drastic had to be done. However, good mathematics libraries spend well over $100,000 per year just for journal subscriptions, and the cost of staff and space is usually at least twice that. Budgets that large are bound to be scrutinized for possible reductions.

Technological Advances

A doubling of papers published each decade corresponds to an exponential growth rate of about seven percent per year. This is fast, but nowhere near as fast as the rate of growth in information processing and transmission. Microprocessors are currently doubling in speed every eighteen months, corresponding to a growth rate of sixty percent per year. Similarly dramatic growth figures are valid for information storage and transmission. For example, the costs of the NSF-supported backbone of the Internet increased by sixty-eight percent during the period 1988–1991, but the traffic went up by a factor of 128 [MacKieV]. The point of citing these figures and those below is that advances in technology have made it possible to transform scholarly publishing in ways that were impossible even a couple of years ago.

Recall that about fifty thousand mathematical papers are published each year. If they were all typeset in TeX, then at a rough average of fifty thousand bytes per paper, they would require 2.5 GB of storage.

We can now buy a 9 GB magnetic disk for about $3,000. For archival storage of papers, though, we can use other technologies, such as optical disks. A disk with a 7 GB capacity that can be written once costs $200–$300. Digital tapes with 250 GB capacities are expected to become available soon. Thus the electronic storage capacity needed for dissemination of research results in mathematics is trivial with today's technology.

We conclude that is it already possible to store all the current mathematical publications at an annual cost much less than that of the subscription to a single journal. What about the papers published over the preceding centuries? Since there are one million of them, it would require about 50 GB to store them if they were all in TeX. Conversion of old papers to TeX seems unlikely. However, storage of bitmaps of these papers, compressed with current fax standards, requires less than 1,000 GB. This is large, but it is still less than 150 of the current large optical disks. For comparison, Wal-Mart has a database of over 1,000 GB that is stored on magnetic disks, and is processed intensively all the time.

Within a decade we may have systems for personal computers that can store 1,000 GB. Even before that, university departments will be able to afford storage systems able to store all the mathematical literature. This ability will mean a dramatic change in the way we operate. For example, if you can call up any paper on your screen, and after deciding that it looks interesting, print it out on the laser printer on your desktop, will you need your university's library?

Communication networks are improving rapidly. Most departments have their machines on Ethernet networks, which operate at almost 10 Mbs (millions of bits per second). Further, almost all universities now have access to the Internet, which was not the case even a couple of years ago. The Internet backbone operates at 45 Mbs. Prototypes of much faster systems are already in operation. Movies on demand will mean wide availability of networks with speed in the hundreds of megabits per second. If your local suppliers can get you the movie of your choice at the time of your choice for under $10 (as they will have to, in order for the system to succeed financially), then sending over the 50 MB of research papers in your specialty for the last year will cost pennies. Scientists might not like to depend on systems that owe their existence to the demand for X-rated movies, but they will use such systems when they become available.

Not only have information storage and transmission capacities grown, but the software has become much easier to use. Computerized typesetting systems have become so common that it is rare to encounter a manuscript typed on an ordinary typewriter. Moreover, scholars are increasingly doing their own typesetting. This trend is partially due to cutbacks in secretarial support, but is caused primarily by scholars preferring the greater control and faster execution that they obtain by doing their own typesetting. With modern technology, doing something is often easier than explaining to another person what to do.

Two centuries ago there was a huge gap between what a scholar could do and what the publishers provided. A printed paper was far superior in legibility to hand-written copies of the preprint, and it was cheaper to produce than hiring scribes to make hundreds of copies. Today, the cost advantage of publishers is gone, as it is far cheaper to send out electronic versions of a paper than to have it printed in a journal. The quality advantage of journals still exists, but it is rapidly eroding.

Preprints and Electronic Journals

Advances in technology allow for much more convenient dissemination of information. Preprints have already become the main method in mathematics and many other fields for experts to communicate their latest results among themselves. Electronics is making this process much easier. Two approaches are becoming common. One is for departments to set up publicly accessible directories from which anyone can copy the latest preprints by anonymous ftp. The other is to use preprint servers, with scholars sending their preprints to a central database. Wide use of these methods is a great boon to scholars, but it is extremely subversive of journal publications [Harnad3]. If I can get a preprint of a published paper free, why should I (or my library) pay for the journal?

The subversive effect of wide preprint distribution is bound to force changes on the traditional scholarly journals. Moreover, the changes could be sudden. For example, within one year the preprint server that Paul Ginsparg had set up for high energy theoretical physics became the standard information dissemination method in that area [Ginsparg]. It has since been adopted by other fields as well. Such sudden changes are common in high technology areas (as in the dramatic rise in popularity of fax machines, or the catastrophic decline of the mainframe) and could occur in journal publishing. During a future financial squeeze at a university, a dean might come to a mathematics department and offer a deal, "Either you give up paper journal subscriptions, or you give up one position." Today such an offer would not be considered seriously, because journals are still indispensable. However, in ten years or so, once preprints are freely available, giving up the journals is likely to be the preferred response.

Preprints have a deservedly different status than refereed journal publications. However, the new technologies are making possible easy publication of electronic journals by scholars alone. It is just as easy for editors to place manuscripts of refereed papers in a publicly accessible directory or a preprint server as it is to do so for their own preprints. The number of electronic journals is small, but it is rising rapidly.

I expect that scholarly publishing will move to almost exclusively electronic means of information dissemination. This will be caused by the economic push of having to cope with increasing costs of the present system and the attractive pull of the new features that electronic publishing offers.

The Interactive Potential of Electronic Publications

Because conventional print journals have been an integral part of scholarly life for so long, their inflexibility is often not appreciated. Most mathematical journals are available at only about one thousand research libraries around the world. Even for the scholars at those institutions, access to journals requires a physical trip, often to another building, and is restricted to certain hours. Electronic journals will make access available around the clock from the convenience of the scholar's study. It will also make literature searches much easier. For journals without subscription fees, access will be available from anywhere in the world.

Frank Quinn [Quinn] argues that the reliability of mathematical literature justifies extreme caution in moving away from paper journals, lest we be tempted into "blackboard-style" publishing practices that are common in some fields. He advocates keeping a strong distinction between informal preprint distribution and the formal refereed publications, even in an electronic format. I agree that mathematicians should strive to preserve and enhance the reliability of mathematical literature. However, I feel that Quinn's concerns are largely misplaced and might serve to keep mathematicians and other scholars from developing better methods for communicating their results. I feel a better solution is to have an integrated system that combines the informal netnews-type postings with preprints and electronic journal publication. Stevan Harnad has been advocating just such a solution [Harnad1], and has coined the terms scholarly skywriting and prepublication continuum to denote the process in which scholars merge their informal communications with formal publications. Where I differ from Harnad is in the form of peer review that is likely to take place. Whereas Harnad advocates a conventional form, I feel that a reviewing continuum that matches the publication continuum is more appropriate.

I will describe the system I envisage as if it were operating on a single centralized database machine. However, this is for convenience only, and any working system would almost certainly involve duplicated or different but coordinated systems. I will not deal with the software aspects of this system. There will surely be hypertext links, so that a click on a reference or comment would instantly bring up a window with that paper or comment in it, but the precise features are not important for this article.

At the bottom level of future systems, anyone could submit a preprint to the system. There would have to be some control on submissions, but it could probably be minor. Standards similar to those for *Abstracts of the AMS* might be appropriate, so that "proofs" that the Earth is flat, or that special relativity is a Zionist conspiracy, would be kept out. Discussions of whether Bacon wrote Shakespeare's plays might be accepted (because there are interesting statistical approaches to this question). There would also be digital signatures and digital timestamping, to provide authentication. The precise rules for how the system would function would have to be decided by experimentation. For example, one feature might be that nothing that is ever submitted could be withdrawn. This would help enforce quality, since authors submitting poorly prepared papers risk having their errors exposed and publicized forever.

Once a preprint was accepted, it would be available to anyone. Depending on subject classification or keywords, notification of its arrival would be sent to those subscribing to alerting services in the appropriate areas. Comments would be solicited from anyone (subject again to some minor limitations), and would be appended to the original paper. There could be provisions for anonymous comments as well as signed ones. The author would have the opportunity to submit revised versions of the paper in response to the comments (or the author's further work). All the versions of the papers, as well as all the comments, would remain part of the record. This process could continue indefinitely, even a hundred years after the initial submission. Author X, writing a paper that improves an earlier result $Y(123)$ of author Y, would be encouraged to submit a comment to $Y(123)$ to that effect. Even authors who just reference $Y(123)$ would be encouraged to note that in comments on $Y(123)$. (Software would do much of this automatically.) This way a research paper would be a living document, evolving as new comments and revisions were added. This process by itself would go a long way towards providing trustworthy results. Most important, it would provide immediate feedback to scholars. While the unsolicited comments would require evaluation to be truly useful, and in general would not compare in trustworthiness with formal referee reports, they would be better than no information at all. Scholars would be free to choose their own filters for this corpus of preprints and commentary. For example, some could decide not to trust any unrefereed preprint that had not attracted positive comments from at least three scholars from the Big Ten schools.

Grafted on top of this almost totally uncoordinated and uncontrolled system there would be an editorial and refereeing structure. This would be absolutely necessary to deal with many submissions. While unsolicited comments are likely to be helpful in deciding on the novelty and correctness of many papers, they are unlikely to be sufficient in most cases. There is need to assure that all the literature that scholars rely on is subject to a uniform standard of refereeing (at least as far as correctness is concerned), and at the same time control the load on reviewers by minimizing duplicate work. Both tasks are hard to achieve with an uncoordinated randomized system of commentary. A formal review process will be indispensable. There would have to be editors who would arrange for proper peer review. The editors could be appointed by learned societies or even be self-appointed. (The self-correcting nature of science would take care of the poor ones, I expect. We do have vanity presses even now, and they have not done appreciable damage.) These editors could then use the comments that have accumulated to help them assess the correctness and importance of the results in a submission and to select official referees. (After all, who is better qualified to referee a paper than somebody who had enough interest to look at it and comment knowledgeably on it? It is usually easy to judge someone's knowledge of a subject and thoroughness of reading a manuscript from their comments.) The referee reports and evaluations could be added as comments to the paper, but would be marked as such. That way someone looking for information in homological algebra, say, and who is not familiar with the subject, could set his or her programs to search the database only for papers that have been reviewed by an acknowledged expert or a trusted editorial board. Just as today, there would be survey and expository papers, which could be treated just like all the other ones. As new information accumulated with time, additional reviews of old papers might be solicited as needed to settle disputes.

The proposal above is designed to work within the confines of what we can expect both technology and ordinary fallible people to accomplish. It would integrate the roles of authors, casual readers, and official referees. The main advantage of this proposal is that it would provide a continuum of peer review that more closely matches the publication continuum that is likely to evolve.

The Future of Publishers, Journals, and Libraries

It is impossible to predict the date or speed of transition to a system like the one outlined in the previous section, but only because they will be determined primarily by sociological factors. The technology that is necessary for future systems is either already available or will be in a few years. The speed with which this technology will be adopted by scholars depends on how quickly we are prepared to break with traditional methods in favor of a superior but novel system. For example, how quickly will tenure and promotion committees start accepting electronic publications as comparable to those in traditional journals?

What would be the role of publishers in the projected system? Scholars can run electronic journals themselves, with no financial subsidies or subscription fees, using only the spare capacity of the computers and networks that are provided to them as part of their job. This is the model under which most of the current electronic journals in mathematics operate. There is more work for authors and editors in such a system than with traditional print journals, but advances in technology are decreasing the effort that is required. A major advantage of such a system is that the journal can be available free anytime and everyplace that data networks reach. However, the lack of copy editing that is likely to prevail in such a system may not be acceptable. I expect that what editing assistance might be required will not cost anywhere near what print journals cost, and so might be provided by the authors' institutions. If that happens, electronic journals can also be distributed freely. If such assistance is not provided, then subscription fees will have to be imposed, together with access restrictions to the information. However, to compete successfully with free preprint distribution and free journals, any subscription journals will have to keep their fees low. In any event, I expect that publishers will have to shrink.

Paper journals will have to convert to electronic publication or disappear. The role of paper is likely to be limited to temporary uses, and archival storage will be electronic.

Review papers are likely to play an increasingly important role, but they are written by scholars and can be published in regular electronic journals. On the other hand, short bibliographic reviews, such as are common in *Mathematical Reviews* and *Zentralblatt*, might be replaced by computerized searches, because the entire literature will be available on each scholar's workstation. This might mean the demise of *MR* and *Zentralblatt*. However, I suspect that they will survive, although they will have to change. They are inexpensive enough that they do not need to offer much extra service to justify their price. There will always be need for classifying papers, ensuring that all significant ones are reviewed, and keeping track of all the changes in the databases. Review journals are positioned to provide these services. Still, they will have to change. They will need to be accessible electronically, and will most likely be paid for by a site license fee, giving unlimited access to the database to all scholars affiliated with the customer institution. They will provide much more current information than is true today, because there will be no publication delays. The formats of reviews might vary from those used today. The main distinction from today is likely to be the presence of hypertext links from reviews to the papers and the commentaries associated to those papers. Combined with easy electronic access to the primary materials, review journals will then provide all the functions of a specialized library.

What about libraries? They will also have to shrink and change their role. The transition to the new system is likely to be less painful for them than for publishers. There is much more inertia in the library system, with old collections of printed material that will need to be preserved and converted to digital formats. Eventually, though, we are even likely to need many fewer reference librarians. If the review journals evolve the way I project, they will provide directly to scholars all the services that libraries used to. With immediate electronic access to all the information in a field, with navigating tools, reviews, and other aids, a few dozen librarians and scholars at review journals might be able to substitute for a thousand reference librarians.

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Roadkill on the Electronic Highway: The Threat to the Mathematical Literature

Frank Quinn

Introduction

There has been wide discussion of benefits and drawbacks of electronic publication. The benefits tend to be emphasized by users, particularly technically adept users with a tolerance for large volumes of information [Odlyzko], [Harnad]. Worries crop up among those involved in the infrastructure of publication: librarians, publishers, and editors [Franks]. In both cases mathematics is usually considered as one of many essentially similar branches of science or

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scholarship. Experiences in theoretical physics, molecular biology, or psychology are assumed to have direct relevance for mathematics. But, in fact, mathematicians do business in qualitatively different ways, and the differences make us more vulnerable to change. This article focuses on these differences.

A key issue is who should be served by publication, authors or readers? Is the primary purpose to establish priority and record the achievements of authors, or is it to be a useful resource for readers? Mathematical journals are genuinely useful: they are reader-oriented in ways that theoretical physics journals, for instance, are not. We have benefitted enormously from this, but our dependence on it exposes us to serious dislocation if this orientation changes.

I would like to thank John Franks, Arthur Jaffe, Andrew Odlyzko, and Dick Palais for their influence on this analysis.

Reliability and Usability

The mathematical journal literature is, by and large, reliable; much more so than in other sciences. Mathematical papers may be boring or useless, but they are nearly always correct (even experimental results are described with "mathematical precision"). And if one is interested then they are usually usable in the sense that techniques and details can be reconstructed. This is not universal. Most mathematicians know of papers that are wrong or opaque. But correctness is much more common than in other fields. A colleague has estimated that a third of the primary literature in biology is wrong. Imagine picking up a journal with twelve articles, and knowing that of these, four are likely to be seriously flawed. In mathematics it is unusual for even one to be in error.

Reliability has many consequences. Users from outside or from other fields in mathematics are significantly empowered by it. They may find the material hard to understand, like a legal contract with a lot of fine print. But after proper allowance for the fine print they confidently expect delivery of the goods. And they get the goods so routinely that many do not even wonder about checking details. A higher error rate would certainly make mathematics harder to use. Mathematicians also accept elaborate proofs by contradiction, which would be ridiculous if the ingredients were not completely reliable. In the other sciences information may be very good, but not absolutely reliable. And accordingly elaborate arguments are viewed with suspicion: the output is at best a hypothesis that should be tested.

Usability is also significant in several ways. It is important in the internal development of a field. Few things are ever absolutely complete, and to go further it is necessary to understand how previous understanding was achieved. Frequently, tomorrow's advances grow from the details of today's work. If details are incomprehensible or unavailable then this process is blocked. A usable record of detail is particularly important to students. And usability is particularly important when experts in one area find themselves needing material from another.

Mathematics is a seamless whole, but the limitations of human understanding lead us to see it as many separate research areas. *Mathematical Reviews* divides mathematics into about five thousand subjects, and this is a rough approximation to the number of areas. Most of these are small groups working in near isolation. Or we can think of them as communities distributed in time, "communicating" through the literature. In particular, much of mathematics does not fit the common image of a scientific research area as a large community exchanging ideas and checking each other's work.

There are several points to this. First, reliability of the methods and literature enables isolated groups to make steady progress. Indeed it encourages specialization. There is little benefit to replication or duplication, so mathematicians spread themselves out. Usability allows these groups to contribute to the larger enterprise when areas come together. If the quality of the literature were to be compromised, these practices would have to change. For instance, mathematicians would have to reorganize into fewer, larger groups to have the immediate interactions necessary to correct errors.

Reliability influences mathematical practice in other ways. For example, mathematicians are more likely to be familiar with the older literature, and to build upon it. When a literature is unreliable there is less benefit to knowing it. In such areas there is frequently a tendency to work only from preprints, and to depend on word of mouth to identify the good ones.

There are important exceptions to the familiarity of mathematicians with the literature. The best mathematicians often internalize their subject to such an extent that they seldom use the literature. Because they can rapidly assess the plausibility of new work, they often do not see reliability as a key issue. It is the rank-and-file who tend to know and depend on the literature: it enables them to contribute significantly to the mathematical enterprise rather than simply being camp followers of the great. Mathematicians of average ability stand to lose if reliability is compromised. An unfortunate corollary is that mathematics may not be well served on these issues by the leadership of outstanding individuals.

A final difference between mathematics and other fields is that we have less tradition of review articles: secondary literature that sifts and consolidates the primary literature. This is less necessary because the primary literature is more directly usable. Furthermore, review articles are less beneficial, because with fewer errors and less duplication to discard there is less compression.

In emphasizing the importance of reliability and usability we have implicitly taken the view of users. But the literature also serves authors by recording and publicizing their achievements. There is a tension between these two functions: authors tend to prefer fast publication and less rigorously enforced standards. Our conclusion is that mathematics has somehow evolved a user-oriented literature, and that the benefits are so profound that it has been (mostly) unresponsive to author-oriented pressures.

Reasons for Reliability

Reliability in mathematics is not an accident. Mathematics is unique in that its methods, when correctly applied, do yield conclusions that are completely reliable. It is true that Gödel has shown we cannot *prove* complete reliability. But several thousand years of vigorous testing have firmly established mathematical reliability as an experimental result. It is probably the most thoroughly tested conclusion in science.

More problematic is the fact that mathematics is done by people. Even the most reliable of methods can be used incorrectly. And usability, because it is as significant as reliability, depends on people for its meaning as well as its implementation. Therefore, the fact that these things actually happen is a consequence of social mechanisms, and is not inevitable.

We turn to a discussion of the social mechanisms leading to reliability and usability. There is, first of all (at least in the West and a few other areas), a long tradition of careful work and critical self-examination. Next, published papers are refereed. Refereeing catches many errors and often results in revision, which increases usability. These two practices reinforce each other. People write carefully because standards are high for acceptance into the literature. In turn, high standards are practical because people write carefully. This is a very beneficial equilibrium, but an unstable one, which could easily be disturbed.

A consequence of this equilibrium concerns the meaning of "publication". At present there is a relatively black-and-white distinction between published and unpublished work. This enforces standards. Authors must write carefully or remain unpublished. If there were a continuum of levels of publication, then standards would be less clear and would have less force. Authors would write to their own comfort level of quality and then negotiate the level of publication. Overall quality of the literature would decline, possibly dramatically. Unfortunately this strong published/unpublished distinction is an artifact of paper publication, and will disappear in the transition to electronic media unless it is deliberately maintained.

Another aspect of the quality equilibrium involves refereeing. Refereeing in any science can be thought of as a centralization of the "self-correcting nature of science." Referees do the first cut of checking, to reduce the load on users further down the line. A balance tends to evolve. Standards should be high, to reduce the burden on users as much as possible. But standards must be low enough that nearly all work can be brought up to the standard and go through the system. If standards are too high a "black market"of unpublished literature (including preprints and announcements) develops, and again a burden is passed on to users. Each area evolves its own compromise, ideally to minimize the burden on users. It follows that standards are adapted to individual research areas and are not interchangeable.

A final point is that these area-specific standards are social agreements among authors, editors, and referees. They evolve over an extended period of time. If these agreements are widely ignored, or if they lose credibility, they could dissolve and take a long time to reconstitute. Thus, standards that work should be cherished and protected. In a transition to electronic publication, great care should be taken to preserve standards and maintain credibility in them.

The Lures of Speed and Alternative Paths to Knowledge

There are several ways a desire for speed threatens the literature. The first is a craving for faster publication. In many areas there are already electronic preprint databases through which papers are instantly circulated worldwide. If the work is reviewed and corrected before it is frozen into the literature, then the additional exposure is a good thing. But there are pressures to regard this instantaneous circulation as publication. Information is transmitted instantly, so authors want credit instantly.

A second lure is a desire to speed up the research process itself. The understanding accumulated over decades and centuries is astounding, but on a day-to-day scale the pace can seem maddeningly slow. A major reason for such a pace is the insistence on reliability and usability in publication. It certainly would be possible to speed things up in the short run by relaxing standards. Authors want to stay in the flow of ideas rather than take the time to nail the last one down firmly. But the lesson of history is that this is counterproductive in the long run. Low quality work is in effect a debt: it must eventually be repaid, and usually with interest. There is a lack of enthusiasm for this, and a really big deficit can run people out of a field. Consistent high quality is the intellectual equivalent of a balanced budget.

The desire for speed is nothing new. Fast-moving fields have always engendered a sense of urgency. And there have been fields and times when giving a lecture at Princeton was considered tantamount to instant publication. But in the past the people who moved on too fast, or only lectured at Princeton, did not seriously damage the literature. Instead, they reduced their own long-term impact on mathematics. Now it is technically feasible to damage the literature.

Another hazard to the mathematical literature is a growing uncertainty about what should be counted as "mathematical knowledge". For example, one can determine that a number is very, very likely to be prime, or that a complicated identity is virtually certain to be true. These are useful conclusions. But no matter how high the probability, it would be dangerous to assert primeness or an identity as a mathematical fact without the caveat that it is not completely reliable. Our experience is that one often encounters low-probability events during elaborate arguments, and indeed many important mathematical developments are based on low-probability events. Therefore, an argument in which some steps are only probably true should be handled like arguments in other sciences. The conclusion is a hypothesis that may need further testing even to conclude that it is probably true.

Alternate paths to knowledge involve a greater reliance on intuition or direct visualization (see [Jaffe-Quinn] and the responses in the April 1994 *Bulletin*, particularly [Thurston]). Again, if this is presented without a warning that such knowledge is not completely reliable, and that it may not be reproducible, then it also threatens the integrity of the literature.

I want to emphasize that conjectures or intuitions or experimental conclusions *presented as such* are not "low quality" or unreliable in the sense used above, even if they are wrong. It is a false claim of knowledge, or a failure to provide usable details that is a de facto borrowing against the future labor of others, and that creates gaps in the literature.

These are not specifically electronic publication problems, but electronic publication is likely to weaken the barriers against defective information.

What Can We Do?

We have received a wonderful legacy from our predecessors: a remarkably reliable and usable user-oriented literature, along with customs and practices to maintain it. Will we pass the legacy on to our successors? Or will it be a casualty of the move to the electronic new world?

We could just relax and let the new age find its own equilibrium. The analysis presented here leads one to expect a substantial decline in reliability and usability of the literature. This would be uncomfortable, but would not fatally cripple the mathematical enterprise. It would put mathematicians of average ability at a disadvantage, but probably most advances happen at the top. It would force abandonment of smaller research areas in a reorganization into fewer and larger research groups. But most small research areas never really contribute in a vital way to the "big picture". "In groups" would develop private folklore about the hazards of their local literatures. Outsiders would be at a disadvantage, but most advances are made by insiders anyway. So the argument that quality is not a key issue is implicitly an argument that average mathematicians, small groups, and outsiders are expendable. In a sense this is true: theoretical high-energy physics even provides us with an example of an area with an unreliable author-oriented literature and all these problems, but which is far from dead. However the prospect is still unattractive, and would certainly be a significant reduction in "quality of life".

The first line of defense against loss of these benefits should be the maintainence of a strong distinction between preprints and material (paper or electronic) that has been officially accepted into the literature. This acceptance must remain a certification that the work is written to high standards of reliability, precision, and usability, and that it has been refereed. In other words, let us maintain the linkage between high standards and publication and keep the alternative sufficiently less attractive in order to encourage people to write to high standards. At present this distinction is an artifact of the rigors and expense of paper publication, so it must be deliberately supported if it is to continue. Some suggestions on how this might be done are given in [Quinn].

The second line of defense is to build as much feedback as possible into preprint databases. Originally, preprints had limited circulation, usually to a group that could provide verbal feedback to the author. Now preprints are distributed nearly as widely as the final publication. This additional exposure is a good thing if the other features are also extended. Mechanisms should be provided to encourage feedback from the new wider audience and to make this information available to others in this audience. This could be done by allowing readers to post comments or references to other work to a file linked to the preprint.

It has been suggested [Odlyzko] that such feedback mechanisms might in large part substitute for the current refereeing and editing system, whereby rendering the published/unpublished distinction unnecessary. This is unattractive in two ways. First, it relieves authors of the necessity to write to high standards to be published. Instead, they can write to their own comfort level, and let people complain if there are problems. Weak writing with a file full of complaints is not an acceptable substitute for good writing. Second, this practice passes on to users the burden of checking and replication now done by referees. A comment file would help, if the reader is sufficiently expert to understand it, and if it is not so polluted to be useless. But the burden is still on the reader.

Summary

The level of reliability in mathematical literature is impossible to achieve in the other sciences. This deeply affects both technical and social practices in mathematics and makes the literature exceptionally useful to readers. This reliability, and its benefits, are threatened in several ways by electronic publication. The transition probably can be managed to avoid loss of these benefits. But this will require a consensus in the community that these benefits are worth preserving and strong support for the necessary actions.

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