LEARNING TO SWIM IN THE PAPER FLOOD

I was delighted to read N. David Mermin's "What's Wrong with This Library?" (August 1988, page 9). I wholeheartedly support Mermin's suggestions for canceling journal subscriptions, refusing editorial positions and "think[ing] twice before writing yet another article." However, I think an additional change is necessary. As a young scientist (PhD in 1985) I often speak with my friends about how to find and keep a good (long-lasting) job. The prevailing wisdom is that a lengthy publication list is a key item.

I once heard a professor say that when he evaluates an applicant he looks up one publication (from the lengthy list) and bases his evaluation on the quality of the one article. However, this appears to be the exception; it's much more common to hear that someone was denied tenure because of too few publications, without any attempt having been made to judge the quality. The fact is that it's much easier to count (something most committee members have been able to do since age five) publications than it is to evaluate quality, especially since committee members are usually not intimately familiar with the subfield and sometimes not even familiar with physics.

Although applicants don't know whether they'll be judged on the quality of their work, I've never heard of anyone who was rejected because of too many publications. It's very difficult for us "young ones" to deviate from the "publish or perish" philosophy we've heard so much about. I could decide that principles are more important than getting a job and cut back on the number of my publications, but then I might very quickly find myself working at McDonald's and my principles left in a drawer somewhere.

I would like to add to Mermin's suggestions my own, directed toward those who have power in such matters. I suggest that when evaluating applicants for new jobs and tenure you judge purely by quality and never by quantity (at least without first checking the quality). You should immediately reject anyone who has published too many papers, and make a lot of noise about why that particular applicant was rejected. If you want to get carried away you could even go so far as choosing those with the shortest publication lists! If a short publication list were a bigger status symbol than a long list we would quickly see the library budget problem solved.

More seriously, when a job is advertised, applicants should be instructed to send along with their resumés their *two* best publications. No committee would ever read more than that number anyway, and the message would be clear to the world that quality is more important than quantity.

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8/88 *Golden, Colorado*

N. David Mermin's comments *re* the proliferation of physics journals, although very amusing, missed the main cause except as an afterthought in his last paragraph. I believe that the plethora of journals is due to the problem of too many papers, which in turn may be due to scientific promotions' being determined to a large degree by the quantity rather than the quality of publications. This excess of publications is deplorable not only because of the cost of maintaining libraries and the time spent perusing all those journals but also because of the productive time wasted in writing the superfluous articles. I think that the number of published scientific articles should be reduced by the following measures:

▷ Reduce the poor quality of papers by better refereeing (pay referees?).

▷ Reduce the number of published conference proceedings, since they generally consist of incomplete, duplicated and poorly refereed papers.

▷ Reduce duplication by encouraging publication of more complete reports instead of fragmented results—refer-

ees can help.

▷ Base the evaluation of scientists more on peer review of abilities.

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8/88

N. David Mermin's Reference Frame column shows that scientists are having to face the crisis that has developed in university science libraries. His analysis indicates that he has given a great deal of thought to the problem. I feel, however, that he dismissed the true underlying cause of the present situation. Why indeed do physicists (and scientists in general) publish so many articles? Why are they so ready to accept invitations to join the editorial boards of new journals?

I came across a very plausible explanation of the origin of this attitude in the North American university community (an attitude that has taken root worldwide with the rise of American influence in this century) in Ferdinand Lundberg's book *The Rich and the Super-Rich* (Bantam, New York, 1973, pages 520–522). Lundberg was actually repeating the ideas of Dwight Macdonald. Supposedly the process began around 1905 when the philanthropist Andrew Carnegie attempted to improve teaching by giving pensions to college professors. Not having sufficient funds for every institution that called itself a college, the Carnegie Foundation defined a qualifying college as one that had PhDs as department heads. The PhD at that time was a rare qualification given to scholars for significant contributions to research, often made over a lifetime of effort. The degree was not then a requirement for a university or college professorship. The Carnegie criterion created an immediate demand for PhDs. New programs were therefore introduced to produce them. Eventually supply exceeded demand, leading to competition. Another criterion had to be found to decide who would get jobs and research grants in the blossoming college industry. The obvious and easiest idea was to use the number of publications of the applicant. A new demand was thereby created. The then-existing journals expanded in size, and new journals took up the slack. With this second criterion getting out of control, some would argue that a new one is now needed—maybe the number of books a candidate has published. Imagine what this could eventually do to library budgets and size! Vita enhancement is thus the underlying cause of the library crisis.

Prior to 1905, research was the activity of a dedicated few who did it out of personal interest and often with a great deal of sacrifice. It would be naive to think that the scientific world was then ideal or to suggest a return to that condition. Nevertheless, the solution to our present predicament probably lies somewhere in that direction.

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10/88

N. David Mermin's notion seems to be that the best status is quo: Keep all the hoary old journals—*Physical Review*, *Physical Review Letters* and so on—coming through the library doors, but keep out any new upstarts, no matter how popular they prove. For example, he bemoans the fact that people in theoretical particle physics will insist on publishing in *Physics Letters B* (he doesn't name this journal, but it's easy to guess which one he's talking about). Their reason for doing so seems paltry to him: "Why do . . . particle theorists publish there rather than in *Physical Review D*? You guessed it: no page charges." He cannot conceive that many very gifted and famous European and third world physicists would rather resort to this dignified course of action than like some beggar check "I do not accept payment of page charges"—often the only option open to them given the very straitened condition of many a world-famous non-US institution.

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11/88

In his Reference Frame column, David Mermin addresses the question of how physical science libraries can make ends meet. Unfortunately, the way he deals with this important problem seems less than convincing. Mermin's two fundamental claims are that there are too many journals and that they contain less and less (worthwhile) information. For the first part of this credo he gives three rather naive arguments. The second claim remains unsubstantiated—understandably, as there is no easy way to define "important" information. Assumptions of comparable generality and with a similar degree of qualification would be that there are too many physicists and that they produce less and less (worthwhile)

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information. These two sets of statements may be related; they may even contain a grain of truth. However, to have credibility, they ought to be backed by a careful study relying on verifiable data and on accepted methods of investigation.

In looking for an explanation for the growing number of journals, we find seemingly conflicting opinions. For example, some people believe the scientific output (in the form of publications) is less than it should be, given the scientific manpower and funding.[1] On the other hand a study of the National Academy of Sciences seems to indicate that the average physicist now is publishing more papers per year than he did some time ago.[2] The problem plaguing physicists and their libraries is not only the increase in the flow of information corresponding to the increase, in recent decades, of manpower and research activity; it is also the question of how a physicist can retrieve the particular information he needs in his subfield. One means of attacking this problem is precisely the creation of new journals—either covering more and more specialized subjects or, in contrast, abstracting information. Another solution, which has been discussed by AIP,[3] is to introduce "user journals." Assembled by a large editorial board, these would form a sort of "reader's digest" for the various subfields. If no new journals are created, the volumes of the existing ones must become thicker until they subdivide (into sections A, B, C and so on). Information retrieval is not made easier in this way.

The draconian measure suggested by Mermin is to force "redundant" journals to expire. The flaw in this scheme is that there is no generally accepted criterion for "redundancy of a journal." Despite Mermin's appeal to the objectivity of science, unfortunately such objectivity remains to be discovered in the field of dissemination of information by journals. As an APS member, I greatly appreciate and admire the tremendous service provided by the society-run journals. Nevertheless, I cannot accept the idea that the bulk of physics journals should be edited by one or a few professional societies. This seems dangerous when one considers the effects of a possible eventual abuse of central power. Also, sometimes even the biggest countries cast provincial shadows.

Thus I doubt there exists a *general* solution to the problem of how libraries should deal with the proliferation of journals, one that takes into account the individuality of the members of our science community.

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David Mermin's column on the explosion of journal numbers and costs can be considered in a broader context and with an eye toward emerging technology. The broader context concerns the fact that even if an individual manages to read several journals he rarely reads any significant fraction of the information in a given one. Also, we always seem to miss articles in our fields we'd very much like to see, especially those in foreign journals. The emerging technology is electronic publishing. Our journal technology has essentially remained unchanged for centuries. It involves the mails, editors and set type, and is essentially a broadcast medium. The pathway between author and intended audience is inefficient. Here I'm not addressing general science, science news or the broad-brush review literature, but the specialist literature that most of us use most of the time.

The vision of the future I'd like to propose as an alternative to Mermin's contraction of the number of journals is one where we do the job we really want done, namely connect author and audience. We should keep the editorial and peer review system, which we generally benefit from, and discard the broadcast medium. We'd keep editorial boards, with their reviewing systems, and probably with their quirks and "prestige." But for the dissemination of an article, each of us would identify the generic types of articles, abstracts and so on—even science news—we'd like to see, and sign on to an information clearinghouse, such as AIP. Actual printing could take several forms. We could review articles electronically from databases and have hard copies of individual articles printed at our institutions. Or we could have hard-copy journals created for each of us, by interactive or automatic selection, by the same third-party printers who currently do the work. This might be expensive per page, but should be much less expensive per desired, read and used page. Given the proper general-science broadcast media and efficient abstract use, this approach should not lead to increased narrowness. After all, how often do string theorists read about the work of gallium arsenide researchers, and vice versa? The technology is close at hand, the economic driving force was described eloquently by Mermin, and we'd all like to get more of the information we want. Libraries could archive the information electronically, and this should lower their costs as well. Even the trees would be better off. We'd have to insure that there was total free access to the information—a great strength of the present system.

Rudolph Peierls's joke, quoted by Mermin, about publications expanding relativistically but not containing information could be modified by saying just that the signal-to-background ratio is decreasing terribly. The system I have described would improve this ratio, or at least enable us to see the signal in order to judge for ourselves its information content.

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Perhaps "what's wrong with this library" is that it is a library of the past, not of the present or the future.

Imagine that at the terminal in one's office, or at the terminal down the hall, one had immediate access to all titles and abstracts one or more days old, across the board of the entire bulk of literature; that one could, *if desired*, view the entire contents—illustrations included—highlight references and search a science citation reference index that was at most one month behind; and that one could, *if further desired*, generate hard copy at some printer nearby, so that one had something to grasp, carry around and keep one company at night.

The primitive beginnings of such capabilities are already in evidence. It behooves us to get behind further development of this jet, hop aboard and forget our concerns of fashioning a more comfortable and economical horseshoe.

B. D. Silverman
8/88 *Yorktown Heights, New York*

In his column "What's Wrong with This Library?" N. David Mermin called attention to the rapid proliferation of journals and the escalation of their cost. Scientific books have been following similar trends, which will make it impossible for research

professionals, let alone students, to buy their own copies. A European-published treatise I have been using for one of my graduate courses cost $45 in 1984; the same book, printed off the same plates, cost $65 in 1986 and $175(!) in 1988. The devaluation of the dollar could not possibly account for all of this trend.

A major factor would seem to be the copyright laws prohibiting copying of books and journals for library and class use. The particular book I referred to has 634 pages. Even at the high cost of 5 cents a page for bulk copying, the book could be copied for under $35.00. I should add that the original printing was by photo-offset, a low-cost process, but in any case that cost was amortized long ago.

Technical societies have been escalating page charges to authors to over $200 per page. The ability to charge this cost to sponsoring agencies, which the societies never fail to remind authors of, is no help, since agencies will not increase grants by, say, $2000 for a 10-page article. However, permission to continue the old practice of bringing copies to technical meetings would force the charges down.

I have no doubt that the cost of published books and journals will come down quickly once copying (for direct educational use, not for resale) is permitted, and I suggest petitioning our legislators to that effect. Let's give competition a chance here. Foreigners have been copying for a long time (and for resale at that); why should we subsidize foreign publishers?

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DAVID MERMIN REPLIES: Sarah Kurtz, M. L. Swanson and Clarence Gall make similar points about pressures on untenured faculty toward excessive publication. My experience in my own department (which I admit may not be typical) is that the single most important factor in promotion to tenure is the quality of direct scientific interactions with departmental colleagues—collaborations, seminar talks or informal conversations. Close behind (and of primary importance for the extradepartmental review process that follows departmental approval) are letters from outside experts, which evaluations also are based primarily on direct scientific interactions. It is no secret that an elephantine publication list is more likely to signify superficiality than breadth. Indeed, it is my impression that even among benighted deans and

provosts "publish or perish" has pretty much been replaced by "get funded or get lost." While in the broader sense this is not a step forward, it does at least diminish the pressure to publish junk, since funding decisions, in my experience, are also based more on a reviewer's direct knowledge of an applicant's work than on lengths of publication lists. I would in any event guess that untenured faculty are responsible for a very small part of the torrent of publications, which gushes forth from the very much more numerous tenured faculty (who may well have acquired unbreakable bad habits in their less secure early years) as well as from scientists in government and industry.

The problem I have with Swanson's suggestion about refereeing is that referees are already overworked, and given the tendency of authors to fight back, raising the rejection rate would increase the work load even more. Dropping the convention that one referees out of duty to the profession and introducing payment that was close to fair compensation for the work would increase the cost of journals far more than what one might save by reducing their size. So although it might help with the communication problem, it would not save library budgets. I agree with Swanson about conference proceedings. Indeed, they should be abolished, and proceedings of summer schools and the like should be published only to the extent that genuinely new pedagogical materials are presented. Organizers or supporters of conferences and schools should rid themselves of the compulsion to commemorate their efforts by publishing expensive collections of mildly revised versions of already existing work.

Robert Lynch and H. F. M. Goenner do not wish to reduce the number of journals because to do so might threaten the freedom to publish new ideas. But the present scheme of uncontrolled publication in greatly excessive numbers of journals jeopardizes the possibility of getting anybody to pay any attention to those new ideas. On Lynch's specific point, I was not complaining about *Physics Letters* (it may be easy to guess, but guessing right is another matter), which performs an important role among publications, and which is much less expensive than the journal that continues to arouse my ire. To maintain *that* journal's existence for the sake of avoiding page charges would have to be bad economics in Europe, the third world *or* the United States—the country I actually had in mind, where research budgets are getting to be every bit as straitened as anywhere else. But I think my chief difference with Lynch and Goenner is that I believe the present situation has become intolerable, and they do not. Surely it is no more in the interest of Europeans and third world physicists than Americans to have worthwhile articles so diffused and diluted that nobody can afford easy access to all the journals in which they might appear.

James L. Lauer, B. D. Silverman and Roger Carr all propose technological fixes. Since nobody has ever succeeded in explaining to me what money really is, I'm not surprised that I don't understand why permitting the copying of books for direct educational use should enable publishers to bring down the price of the originals, but if it works, I'm for it. The Computopias of Silverman and Carr will certainly help to bring the right papers rapidly to the right readers, but again I fail to understand the economics that will motivate those who presumably will put together and organize that enormous bulk of literature to which we will all have delightfully instant (free?) access. Can the final manufacturing process, the paper and the ink be that big a part of editorial costs? Unless we abandon existing efforts (such as they are) to control what goes into the data base, I still don't see how we can arrive at a system anybody can afford without reducing the number of publications.

I also fear that waiting for Computopia to solve this pressing problem (assuming I'm just dumb about the economics) is rather like waiting for high-temperature superconductors to bring down the cost of the SSC.

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4/89

Feynman: A Model for Mechanics

Reading your February issue on Richard Feynman meant a lot to me. I met Feynman through his book *Surely You're Joking . . .* a few years ago, and although I never met him in person, he has been my hero ever since.

In my profession (automotive technician) it is important to look at a problem with an eye whose mind is not made up (Mrs. Malaprop would love that one!) and to question the ideas and diagnoses of people who you think know more than you. For his example in these areas, Richard Feynman was my hero, as well as for his enthusiasm for problems.

I was moved to read how many others felt him to be their hero. Grieving is easier knowing that the feeling is shared by so many. You in the physics community are fortunate indeed to have known Feynman in person.

Sally Mills
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3/89

Lilienfeld Radiation Brought to Light

I wish to commend William Sweet on the excellent news story he wrote on Julius E. Lilienfeld for the May 1988 issue (page 87). However, work for which Lilienfeld was very well known was overlooked. He was the first to discover the effect now known as Lilienfeld transition radiation.[1]

In 1919 Lilienfeld found that in addition to x rays, radiation ranging from visible light through the ultraviolet is emitted when electrons approach a metal electrode. This radiation has a characteristic polarization, spectrum and intensity.

Lilienfeld transition radiation can be considered to originate from the time rate of change of the virtual dipole between charged particles and their image charges that forms as the charged particles move near a conducting surface. In Lilienfeld's original experiment, the charged particles were low-energy electrons moving toward a metallic anode. In a variation of this, the charged particles move roughly parallel to a conducting serrated surface, producing an oscillating virtual dipole whose frequency is related to the particle velocity and the serration spacing. The term "transition radiation" has now taken on a broader meaning to include the radiation emanating when charged particles go from one medium to another. When the particle has a high energy, so that its velocity is greater than the velocity of light in the medium, Čerenkov radiation is emitted, as first predicted by Arnold Sommerfeld.

As a graduate student I observed Lilienfeld transition radiation in 1961 in connection with my doctoral thesis research, before I had ever heard of it. A literature search led quickly to Lilienfeld's work. The visible light is easily seen at the anode of a high-voltage vacuum tube.

In some ways Lilienfeld's work anticipated the 1953 experiment of S. J. Smith and Edward M. Purcell,[2] and possibly even John M. J. Madey's invention of the free-electron laser.

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