provosts "publish or perish" has pretty much been replaced by "get funded or get lost."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.

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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.

References

1. J. E. Lilienfeld, Phys. Z. **20**, 280 (1919).
2. S. J. Smith, E. M. Purcell, Phys. Rev. **93**, 1069 (1953).

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