

Letters

Physics in the evening

I was very much interested in Leon Lederman's letter "Saturday morning physics" (September, page 11) and in the program for high-school students offered by Fermilab. Your readers might like to hear about a somewhat different program for high-school students which we are now offering for the 11th year, our "High-School Physics laboratory Project." Each year about 50 final-year high-school students from Windsor and the surrounding counties, recommended by their physics teachers, spend one evening a week in our general undergraduate laboratory, doing experiments they could not possibly do at school, under the supervision of our teaching assistants. These young people (and their teachers!) have been displaying extraordinary enthusiasm for almost any kind of experimentation and their performance has been noticeably better than that of our regular first-year students. We are immensely impressed and rather touched when we see a full laboratory in a winter evening during a snowstorm, knowing that some of the students live up to 50 miles away. It is obvious that there is a lot of talent and enthusiasm for science among high-school students, if only we could bring it out early enough!

L. KRAUSE

University of Windsor
Windsor, Ontario, Canada

Refereeing procedures

I would like to comment on Patricia Dehmer's Guest Comment on "APS reviews refereeing procedures" (February, page 9). Dehmer views the problem from the position of a privileged insider, a member of the Publications Committee, while I, on the other hand, view the problem from the underprivileged position of an APS member who has been unable to publish in his society's PR and PRL journals. Of the more than a dozen complaints received by her PCAPS, I was responsible for two of them, and much of the information in this letter is the same as that contained in my 27 May 1980 letter to

Dehmer in response to her BAPS appeal.¹

Concerning Dehmer's comment "In choosing appropriate persons to review the numerous manuscripts, the journal editors use various methods that reflect their own style and areas of expertise," I would like to present the following example of how this has worked for me. On 3 June 1969, I submitted a paper, "An Analysis of Inconsistencies in Published Interplanetary Radar Data," to PRL. The last paragraph of the referee report sent back August 15 states "It is suitable for *Physical Review Letters*, if revised, and deserves immediate publication if the radar data can be compared directly to geocentric distances derived from optical directions and celestial mechanics." I revised the paper as the referee recommended and resubmitted it 21 August. The editor, S. A. Goudsmit, sent me a reply 11 September, in which he stated that the paper had been sent to another referee and rejected. I sent a letter 13 September, complaining about the use of the second referee. I received a reply from Goudsmit on 23 September, in which he then stated that he had made a mistake in saying the paper had been sent to a second referee and that it had actually been sent back to the first one. He did this, in spite of the fact that there was absolutely no correspondence between the two reports. They were obviously typed on different typewriters, the first was completely positive, while the second was strongly negative and made no mention of the first report! I eventually published a revised version "Radar Testing of the Relative Velocity of Light in Space" in a less prestigious journal.² At the December 1974 AAS Dynamical Astronomy Meeting, E. M. Standish Jr of JPL reported that significant unexplained systematic variations existed in all the interplanetary data, and that they are forced to use empirical correction factors that have no theoretical foundation. In Galileo's time it was heresy to claim there was evidence that the Earth went around the Sun, in our time it is heresy to claim there is evidence that the speed of light in space is not constant. A

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letters

major reason for my recent loss of a position at Eckerd College was due to ridicule that came from publication of a letter on this question.³ The heresies change, but human nature remains the same!

On 24 March 1976, I had an associate, Richard Rhodes, submit a research paper "Crossed Beam Electron-Electron Scattering at 90° and 300 eV" to PRA. While I had been the principal investigator on the project, I felt that considering the controversial nature of the previous paper, it would be wise to have Rhodes handle the correspondence. The following are some of the statements made by the first referee in rejecting the paper: "Surely work on helium structure provides a much more critical test of electron-electron interaction at low energy." "Aside from the question of the usefulness of the measurement, it was not carried out with the sophistication befitting so fundamental a problem." "Perhaps a report of these measurements would be suitable for publication in the *American Journal of Physics*." The second referee knew of the opinion of the first referee, and essentially restated it. We eventually published the paper in a different journal,⁴ one that considered it important to publish the first report of a primary isolation-type experiment such as crossed beam electron-electron scattering in a new energy range!

Werner Heisenberg has argued that we need to develop a unified theory of the mass dynamics in elementary particles.⁵ He states, "In the theory one should try to make precise assumptions concerning the dynamics of matter, without any philosophical prejudices." I've followed Heisenberg's lead and done extensive work in this area,⁶ and found that the major problem with this approach is the philosophical prejudices of other physicists. In trying to publish papers on this question, I've gone through the PR author's appeal procedure three times, two of these appeals went all the way up to the final step, review by the PCAPS. In answer to the first appeal's argument "It is my contention that by using arbitrary biased opinions to deny an APS member the right to publish a controversial paper your journal is violating his human rights as set forth in the statement of principles for The American Physical Society activities with regard to human rights,"⁷ the PCAPS chairman Hans Frauenfelder wrote "I should like to point out that publication in *Physical Review* or *Physical Review Letters* is not a right, but a privilege. Any member of the American Physical Society has a right to express his ideas in the *Bulletin*." Considering the impossibility of eliminating the philosophical

prejudices of physicists, I personally believe the best solution to this problem is to follow the enlightened lead of the *Bulletin* and make APS members responsible for the scientific integrity of the material they publish in PR and PRL. This would encourage membership in the APS, eliminate the expense of the current bureaucratic editorial nightmare, and make the APS statements on rights something more than pretentious empty rhetoric!

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BRYAN G. WALLACE
St. Petersburg, Florida

3/82

The first computer

In May (page 116) the letter by Yale Jay Lubkin states several "facts" concerning the history of the earliest electronic digital computers. First, the EDVAC was designed by John W. Mauchly and J. Presper Eckert. This design began late in the year 1944, continued through the following year in parallel with the completion of the ENIAC, and was put into practice gradually so that the transition from the completion of the ENIAC to full-time on the EDVAC was smooth. Eckert and Mauchly not only carried the design through its major creation, but their work was ably forwarded after April 1946 by persons such as Kite Sharpless, who succeeded Eckert. Samuel Lubkin was not and had not been on the Moore School staff up to this time. Incidentally, Eckert and Mauchly wrote a relatively complete outline of the design of the EDVAC, which was published (classified) in 1946.

Second, the EDVAC was entirely a Moore School machine and had nothing to do with the computer then to be, or being, built at the Institute for Advanced Study. The latter computer was completed much after the EDVAC.

Third, Moore School participation in computer development was hardly terminated with the EDVAC. To give an example, the Moore School's UDOPT (Universal Digital Operational Flight Trainer) was essentially a very large computer that for the first time enabled existing flight trainers to be re-

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