

letters

Program at the National Institute of Neurological and Communicative Disorders and Stroke at NIH. My PhD in general has been a basic requirement for all of the positions I have held—and the fact that it is in physics has, at least, impressed potential employers—but the real point is that my graduate education, potentially valuable for the national defense, has been wasted. Fortunately, my career has been relatively exciting and rewarding.

Despite the anecdotal nature of this report, I think it is likely to be a general rule that women who have the qualifications necessary to assume a professorship in physics have already attained positions of responsibility in other fields, or in places outside the university. For the most part, they will be unwilling or unable to become professors of physics. In addition, it seems to me that it is certainly not effective to make up for past neglect by appointing people to senior positions for which they are not superbly qualified. There are perhaps senior research associates at one or two major universities who do meet the necessary criteria. The real point, however, is that it is quite ludicrous to suggest substantial increase in the percent of high-level, tenured women faculty at a time when the number of positions is, at best, not increasing and retirements are not expected to be effective in opening up slots until the 1990s.

In general, to make a plea to accomplish an important deed in a way and a time that it cannot be done is a travesty as well as a disservice and discouragement to those women presently entering the field. It is not possible to correct decades of discrimination in a single fell swoop by placing non-existent senior women in non-existent positions. Instead, the few women recently and currently entering the field must be nurtured and allowed to develop to their full potential and then to achieve the positions for which they are well qualified.

JANETT TRUBATCH

National Institute of Neurological
and Communicative Disorders and Stroke
4/82 Bethesda, Maryland

More on junk mail

Henry Blosser (April 1981, page 74) and John Vossen (August 1981, page 71) have both made pleas for help in getting rid of junk mail. Neither seems to have had any effect on this nuisance avalanche. (I've been saving it all religiously for 2½ years for statistical purposes and my bedroom is now crowded with bags and old suitcases of

the stuff, which drives my wife to distraction. I sent about 25 pounds of it to my senator once but never heard from him again.)

The truth is not generally recognized that the US Postal Service is in business principally to deliver junk mail; the first-class material is only a side issue. Instead of stuffing it all in the postage-paid envelopes and sending it back (which doesn't do any good if you have to go broke putting stamps on those that don't have the "No Postage Necessary" on the right) or sending it to the postmaster general, I think a better answer would be for us all to save it up for a month at a time and then mail the bundles of the stuff to our respective congressman. Their offices would soon be filled to the ceiling with these bundles, and the business of the US government would come to a screeching halt until this massive abuse was stopped.

WILLIAM B. ELMER

6/82

Thornton, New Hampshire

Complaint about authors

I am writing to draw attention to a bizarre grammatic practice, endemic among contributors to AIP journals, that seems to be spreading to physicists throughout the world.

It would be peculiar to pick up a paper with the section subheadings "Introductory," "Theoretical," "Resulting," "Discursive," "Concluding" and "Acknowledgemental," so why is "Experimental" considered acceptable?

J. M. D. COEY

6/82

Trinity College, Dublin, Ireland

Microfiche journals

Please give very serious consideration to the letter by Dinh Ton-That (February, page 104) concerning microfiche editions of AIP journals for AIP members.

I am a member of both the AAPT and the IEEE. As a member of the IEEE's Computer Society, I receive the microfiche edition in lieu of the paper edition of *Computer* at no additional cost. If this alternative were not available, I would not subscribe to the paper edition—I don't have the room to store back issues in paper form. With my microfiche copies, I can store more than ten years of back issues of microfiche in less than the space of a single issue; I can copy any pages I need in hardcopy on the microfiche reader-copier at the local library.

I would definitely subscribe to more scientific journals (and popular magazines, for that matter) if the storage

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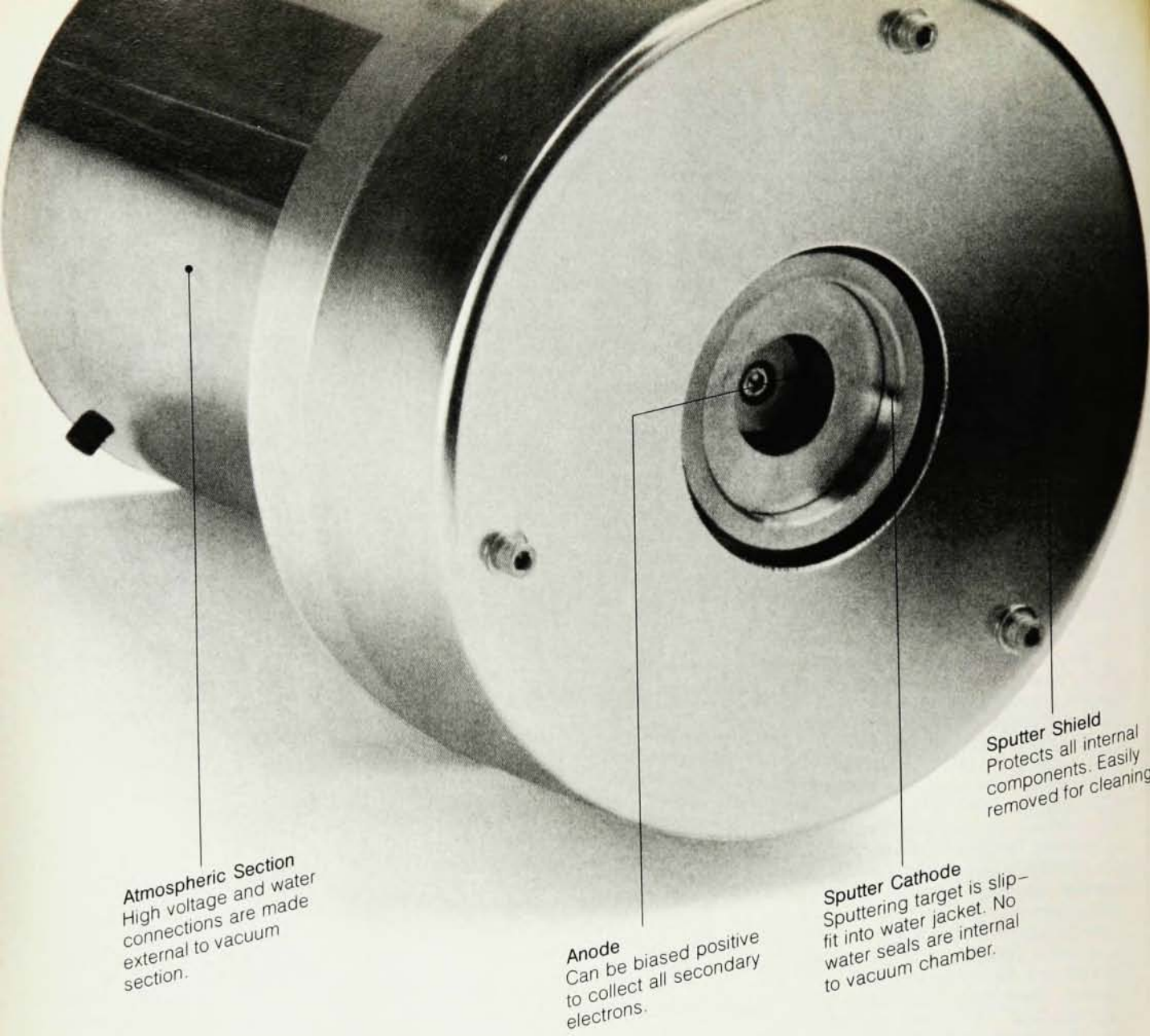


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letters

problem were solved by providing the option of receiving them on microfiche.

RODMAN E. DOLL
Schoolcraft College
Livonia, Michigan

7/82

Differences between physicists

The contrasting emphases on show-and-tell, computer-assisted, and pen-and-paper approaches to learning physics, so well portrayed in July (page 11) by the letters of Julius Miller, and of W. P. Allis, A. Bers and L. P. Harten, brought to my mind an anecdote that seems pertinent concerning William Rowan Hamilton, the great Irish mathematical physicist.

Hamilton's mathematical analysis of the propagation of light in a birefringent medium led him to predict that "conical refraction" should be observable. Not being an experimenter, he asked his friend Humphrey Lloyd to look for the effect. After about six months, Lloyd succeeded in observing it. Their separate papers, published in the same issue of the *Proceedings of the Royal Irish Academy* in 1833, attracted great interest. Many experimenters, however, were unable to observe the phenomenon, and one distinguished optical worker—either Brewster or Airy, I think—told Hamilton that he would not have believed the effect existed had he not seen it with his own eyes. Hamilton is reported to have said "How different we are!" Because his eyes had deceived him so often, he believed that it existed only because he had proved it must.

To me this seems to say that the advance of physics today, as then, is dependent upon the complimentary coordination of men and women with widely differing gifts. Not to take advantage of what each approach can give is to the detriment of physics instruction.

E. SCOTT BARR
Tuscaloosa, Alabama

8/82

Communicating with the public

I read with great interest the editorial in July (page 96) concerning the Commission on Pre-College Education in Science, Mathematics and Technology. Although I agree totally that something must be done about the dismal failure of scientists to communicate with the general public, I am skeptical about the efficacy of yet another national commission to study the problem.

Nonetheless, I would like to suggest that the Commission include in its study not only established academic

programs, but also the fine example set by the "Exploratorium," a "hands-on" science and technology museum in the Palace of Fine Arts in San Francisco. This museum, operated by Frank Oppenheimer and an enthusiastic cadre of students and technicians, is a marvelous example of what is lacking in most science courses. The story of the museum was recently featured on PBS television. Clearly one important ingredient in the success of the Exploratorium is the high priority placed on interacting with the "students," namely the public. Exhibits that don't communicate well are quickly replaced by others that do. This feedback process is probably the reason that microcomputers (and video games) also are so popular today. Perhaps the Commission on Pre-College Education could do worse than recommend that other cities be encouraged (and possibly funded?) to set up their own exploratoria.

I have no connection with the Exploratorium except that of an enthusiastic fan.

K. MORIYASU
University of Washington
Seattle, Washington

8/82

Fletcher and the oil drop

Regarding Harvey Fletcher's "My Work with Millikan on the Oil-Drop Experiment" (June, page 43), I want to share a few thoughts.

When I was Ernest Kempton Adams Precision Laboratory Assistant at Columbia University, 1935-1938, graduate students did the "Millikan Oil-Drop Experiment." After reading Harvey Fletcher's posthumous autobiographical excerpt, I wondered if the experiment might not better be called the "Millikan-Fletcher Oil-Drop Experiment."

I think of a misnomer I would not have known about except for Erich Hausmann and F. P. Slack, who in their *Physics*, third edition, page 388, say: "The Wheatstone Bridge... method devised in 1833 by S. Hunter Christie, was brought to public attention by the English physicist, Sir Charles Wheatstone (1802-1875) and has remained associated with his name."

It is commendable that PHYSICS TODAY has published Fletcher's account of this remarkable experiment on the determination of the electron charge.

DON KIRKHAM
Iowa State University
Ames, Iowa

8/82

I was delighted to see the most interesting article—an historical gem!—by Harvey Fletcher on his work with

continued on page 104



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