ties in a university, externally sponsored or not; and this reality should be directly felt by every actor on the university stage, whether externally sponsored or not.

What is needed is an "overhead for all" policy, in which every expenditure of funds on a university campus either *bears* overhead or is *part of* the overhead. Each academic department, for example, should receive a departmental budget enlarged over its present budget by approximately the current indirect cost rate. It should then pay overhead at the appropriate rate as it expends its funds for faculty salaries, teaching assistants, office supplies or whatever, exactly as we sponsored researchers now do. When I pay a research assistant from contract funds, I pay overhead; when the History Department pays a graduate assistant from a foundation grant, it should explicitly pay overhead on those expenditures also. (It's of course irrelevant to argue that "foundations won't pay overhead." Somebody pays those indirect costs, somehow.) Note that I call for no necessary change in allocation of resources within the university, only for a change in how this is accomplished.

Perhaps the most important virtue of this scheme would be to make the reality of indirect costs apparent to all, and to give all of us an equal motive for reducing them. The motivation of faculty members to keep university costs down becomes far stronger when they realize that overhead cuts directly into their spendable funds than when their indirect costs are simply covered by the university without their explicitly knowing about it.

A second advantage would be to increase the preceived fairness of the system. Those of us in heavily outside-sponsored engineering and science, and those in the nonsponsored humanities, would visibly all be in the same boat together. Universities frequently claim that government contracts do not in fact pay all their real indirect costs. The university might thus have a "university overhead rate" even higher than the federally allowed rate. Those of us in science and engineering, and in humanities, would equally have to negotiate for university funds or make other arrangements to cover the excess indirect costs.

Finally, it should be emphasized that this proposal would not involve extensive new accounting and record-keeping costs. To justify their indirect cost rates on government-sponsored projects now, universities must carefully record all indirect costs, and then make a complex allocation of these between sponsored and nonsponsored activities. "Overhead for all" is already being calculated. What we need to do is make it explicitly visible, and visibly fair—and then all set about reducing it.

ANTHONY E. SIEGMAN
*Stanford University*
*Stanford, California*
*1/81*

## Hampered participation

The American Physical Society Committee on Opportunities in Physics would like to determine the dimensions of a problem that has come to its attention—a problem which can make it very difficult for physicists employed in certain government agencies and laboratories to participate in professional activities of the Society. The Committee has learned of several instances in which local management has imposed complex, limiting and time-consuming regulations in situations where the employee would normally be reimbursed in whole or in part by the American Physical Society for travel and living expenses while on Society business. Taking part in Society affairs on personal time does not appear to ameliorate the situation. It is even necessary for an officer of the Society to write a letter of invitation and justification for each instance of participation.

If the problem is sufficiently widespread, the Committee would prefer to appeal more generally for relief to an appropriate federal agency or official, rather than to deal with individual cases. To this end, the Committee asks that those physicists whose participation in Society affairs has been hampered seriously by such regulations write to the undersigned at 2159 Orchard Park Drive, Schenectady, New York 12309. Replies will be kept confidential if those writing so desire.

RALPH A. ALPHER
*Chairman APS Committee on Opportunities in Physics*
*1/81*

## Anonymous refereeing

Having been an author and referee for American Physical Society journals for more than 30 years, having served as a member of the Publication Committee of the Society, having served as Associate Editor of the *Physical Review*, and now in my third year as Editor of *Physical Review Letters*, I found a certain antic charm in Christopher Sherman's description (January, page 15) of the editorial judgment of papers in APS journals as roughly parallel to the judgment of heretics by the Spanish Inquisition and, especially, in Sherman's assurance that all of the refereeing problems of the journals can be solved with simple procedural changes which Sherman will explain to us—if we will

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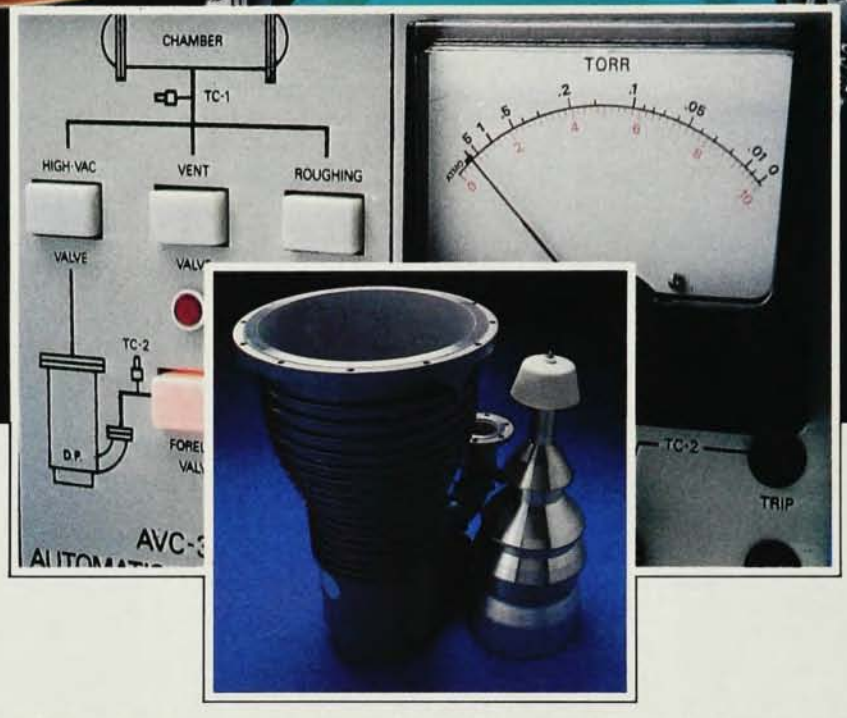


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only listen. Of these changes, Sherman seems most enchanted by proposals for withholding from referees the identity of the authors, a change which he says is "widely opposed by those in position to make changes." I note that in the 10 November issue of *Physical Review Letters*, APS Editor-in-Chief, David Lazarus, announced that the APS journals would honor requests by authors to have their identity withheld from referees. The statement was actually an affirmation of a policy that this editor had previously followed.

Perhaps Sherman and others believe that the editors—who select the papers to be published—should be required to withhold the identity of authors from *all* papers submitted to the referees they choose to advise them. Aside from the dubious merits of such a procedure, the anonymity could not be enforced. Referees would recognize the authors of most papers. In fields like experimental particle physics (where I tend a few vines) and in experimental nuclear physics, the recognition would be near 100%. While the recognition factor will be lower in some areas, the results of some rather rough experiments suggest to us that about 80% of the 2500 papers submitted annually to *Physical Review Letters* are such that referees (chosen, of course, such that they work in the specific field covered in the paper) will be able to determine the authors of the paper from its contents.

ROBERT K. ADAIR
*Brookhaven National Laboratory*
*Upton, New York*
*2/81*

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While I agree with many of the sentiments expressed by Christopher Sherman in his letter "Refereeing justice," it also is true that (1) editors would have a hard time finding referees without offering them anonymity, and (2) anonymity often permits the referee to suggest constructive changes in a manuscript that any objective author would admit improved his paper, sometimes greatly. Certainly, inequalities do arise from the system. As the author of nearly 40 papers, I can recall many situations when the referee took advantage of his anonymity to make scurrilous comments about me designed to prejudice the editor, and others when two referees took exactly opposite views on the same subjects. But in the final analysis it is the editor, not the referees, who can be held accountable for the justice of the refereeing process. A good editor looks for referees who make constructive comments, not scurrilous ones. The latter type he simply discards from future consideration. Thus although frequently I have been angered by unfair anonymous comments, I ultimately have had to decide whether it was the editor, not the referees, who was doing his job well.

There are exceptions to the use of anonymous referees by journals. For example, at least one British journal identifies the referee(s) making comments, and the editor replies to the author with his own comments as to why he has accepted or rejected the submission. While this takes a lot of work by the editor and his staff, it has other benefits. One such rejection led me into correspondence with an editor/academician that has resulted in planned collaboration and exchange of scientists between our laboratories involved in problems of mutual interest, with expected benefits to both.

Finally I must comment that Sherman may have hit the nail on the head regarding prejudging of authors by referees. I have been involved in a controversial research area (infrared absorption by molecular clusters in water vapor) for several years, during which time my contributions have progressed from disreputable as seen by most referees, to avant-garde, to accepted at the present. The acceptance rate of my manuscripts has increased proportionately. But to complicate matters, I received a PhD in 1979 from a nonprestigious university. Acceptance of my manuscripts seemed to increase step-wise after my receipt of the PhD and use of the title in journal correspondence, leading me to wonder if the appearance of "Dr." on a manuscript makes it easier for the referees to decide that a given such paper represents less of a threat or risk than one from an untitled author. Apparently PhDs are considered less likely by referees to make serious and embarrassing errors than are simpler folk. But isn't this also an admission that some referees don't really understand what they are reading?

HUGH R. CARLON
*Chemical Systems Laboratory*
*Aberdeen Proving Grounds, Maryland*
*2/81*

THE AUTHOR COMMENTS: On rereading my letter, I do not find any claim that "all of the refereeing problems of the journals can be solved with simple procedural changes." My criticism is clearly quite specific in its intent. There is one point, however, on which Robert Adair and I do seem to be in agreement. He implies that he does not wish to listen—and this is precisely what I contend. A reluctance to listen lies very much at the heart of the matter.

Concerning David Lazarus' editorial in the Nov. 10, 1980 *Physical Review Letters*, I have the following to say. Although my letter was dated as hav-

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## letters

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ing been received by PHYSICS TODAY on Oct. 9, 1980, the letter was originally submitted to PHYSICS TODAY on April 21, 1978, and in a revised version on Feb. 24, 1979. Its publication was delayed for over two years in spite of persistent efforts on my part to convince the editor to publish it. These efforts included hearings before two committees, the Advisory Committee on PHYSICS TODAY and the Governing Board of the American Institute of Physics. Both of these committees unanimously backed the decision not to publish the letter. On August 22, 1980, in a continuation of the effort to get my letter published, I wrote to Lazarus, soliciting his aid as Editor-In-Chief of APS publications. This correspondence contained a copy of the revised letter to PHYSICS TODAY. Thus, not only did Lazarus receive a copy of my letter three months prior to the announcement of his policy, but the content of my letter had been circulated among quite a few officials of the APS and AIP considerably before this. I can only conjecture why the editor of PHYSICS TODAY finally, after such a long delay and with only a minor deletion agreed to publish the letter. Nor do I know whether Lazarus was motivated in part to initiate the changes he did as a result of reading my letter.

Adair objects to the institution of reciprocal anonymity on the grounds that "Referees would recognize the authors of most papers." This objection has already been commented on in my letter, but I wish to add a further comment. The fact is that in cases where there is recognition the anonymity is less likely to be necessary, since the author is already an accepted publisher in a specific field. Where anonymity is most needed (that is, for authors entering and as yet not known in a new field) it is most likely to be maintained.

The reason for opposition to the institution of reciprocal anonymity is stated in my original letter, and I repeat it here; those in control of the refereeing system are more interested in maintaining the status quo than they are in trying to develop a more just system of refereeing. I am glad that Lazarus has finally broken the tradition, and only hope that other editors will follow his example. The institution of this as well as other procedural changes will be welcome.

Although Hugh Carlon seems to agree with me on the unfairness of the present refereeing system, there seems to be some misunderstanding of my suggested remedy. I do not advocate revoking the rights of referees to anonymity, but rather extending this anonymity to authors. His experience in

breaking into a new field is quite common, and it is just this sort of treatment which can be mitigated by procedural changes. These should include, but not be limited to, the institution of reciprocal anonymity.

CHRISTOPHER SHERMAN
*Andover, Massachusetts*

*2/81*

## Save the ads

Despite the contrary views voiced by J. H. Mauldin in his letter in February (page 110), I believe that for the majority of the Americans the day after election was a happy, not sad, day! This, however, is irrelevant, for PHYSICS TODAY is not a political and sociological forum.

The purpose of my writing this is my one-man campaign against the practice of "stripping" advertising material from scientific journals before binding or storing them. This material has considerable potential use as a ready source of information concerning suppliers of equipment and services, and for identification of inherited apparatus. In the long run, though, there is something of considerably greater importance. Think, for example, how much concerning the state of physics and other sciences could be learned from a study of unstripped issues of a scientific journal of even fifty years ago. For the history of physics this material is of much importance and deserves preservation.

In some libraries it is customary to bind only pages bearing consecutive roman numbers, and this not only results in the loss of advertising material but also valuable cover illustrations. I have always instructed my binder to "bind all," and I would strongly urge others to do the same, so as to provide historians of physics in the future with important and interesting material.

I think that the new format is fine, and I particularly like the new spine with its clear identification of the issue when on the shelf.

E. SCOTT BARR
*Tuscaloosa, Alabama*

*3/81*

## Mid-career women

In response to Michele Kaufman's letter in February (page 13), her complaint regarding the Kennedy bill's lack of support for mid-career women scientists is without basis. To quote the bill, which is now Public Law 96-516, Section 33, "Women in Science": "The Foundation is authorized to ... make grants, to be known as the National Research Opportunity Grants, to women scientists who (A) have received their doctorates within five years prior to the date of the award