membership in a professional society. As such, the inclusion of specific persons does depend upon the information supplied by laboratory directors and department chairs. With the wider distribution of the 1984–85 edition, we hope that individual staff members will urge their laboratory directors and department chairs to be more responsive to our requests for information for the 1986–87 edition. Individuals whose names are missing from the last edition should complete the form on the last page of the *Directory* and return it to the AIP Education Division; we will inform the appropriate laboratory director or department chair that that person feels that he or she should be included in the staff list for the next edition. If a copy of the *Directory* is not available, the AIP Education Division would be happy to supply a copy of the form.

Because about half of those listed in the *Directory* are not members of any AIP Member Societies, the *Directory* provides information about physicists, astronomers and scientists in physics-related fields that is not available elsewhere. It also provides information for those interested in contacting academic departments and research laboratories rather than specific individuals. The *Directory*, which is published by AIP rather than the The American Physical Society, certainly is not a replacement for the *American Physical Society Membership Directory*.

Responding to an earlier suggestion, AIP is studying the feasibility of combining in a future edition of the *Directory* the membership lists of the nine AIP Member Societies with the list of staff members provided by academic departments and research and development organizations. The lists of academic departments and research and development organizations would continue to include only the information supplied by the listed organizations. If this were done, the number of individuals listed would increase from 30 000 to about 80 000.

DION W. J. SHEA
*4/85* *AIP Education Division*

## Lateness

I find myself morbidly fascinated by the growth of polemical rhetoric contained in your Letters department. It seems to span a diversity of items ranging from libel to ideological leanings and personal criticism, interspersed with evidently harmless matters of technical and historical merit, and now and then an equation or erratum or two. The November issue of PHYSICS TODAY seemed to reflect a more lively collection of communications than any I can recall recently.

With these comments I hesitate to bring up a mundane matter regarding timeliness of and in PHYSICS TODAY. Today is the 4th of December, and on this day I received my November issue of PHYSICS TODAY in the post. Indeed, I have consistently received PHYSICS TODAY a month late even after having written to your offices a few years ago to this effect. As far as I know, the mail services at our laboratory are not usually more than two or three days late in delivery, which leads me to inquire if (a) your facilities mail out issues long past the first of the month for that issue, or (b) there is something anomalous about domestic delivery to the West coast. I have at times been disconcerted by reading announcements of meetings and similar events a day or two *after* such have taken place. And, as if that were not enough, I find in the September 1984 issue announcement of the 1983 E. O. Lawrence Award winners—which, I assured myself after identifying the recipients, was indeed correct. We got our own news of the 1984 winners shortly prior to that.

In any case, I am concerned about not receiving PHYSICS TODAY in a timely manner, and I would like to ask if anything can be done about it. If PHYSICS TODAY is intended to function as a news magazine for the physics community, which I am sure is part of your policy, then I suggest that timeliness of reception be looked into closely. I may not be the only APS (or ASA) member with this problem.

STEPHEN I. WARSHAW
*12/84* *Lawrence Livermore Laboratory*

*The November 1984 issue was mailed on 20 November. Now that I've become editor of* PHYSICS TODAY, *one of our primary goals will be to mail each month's issue during the first few days of that month.*

*After a staff change at* PHYSICS TODAY, *the 1983 Lawrence Award was overlooked. When the omission was discovered, we carried the story late rather than not at all.*

GLORIA B. LUBKIN
*5/85* *Editor*

## Med school: A bitter pill

I absolutely disagree with Robert J. Yaes's assessment (August, page 13) of the value of a physics education to a practicing physician. I too decided to study medicine after having worked in elementary-particle physics. In contrast to his reaction, I find that my physics orientation to problems in medicine is very productive. It enables me to analyze clinical problems in a systematic way that my "rote memory"

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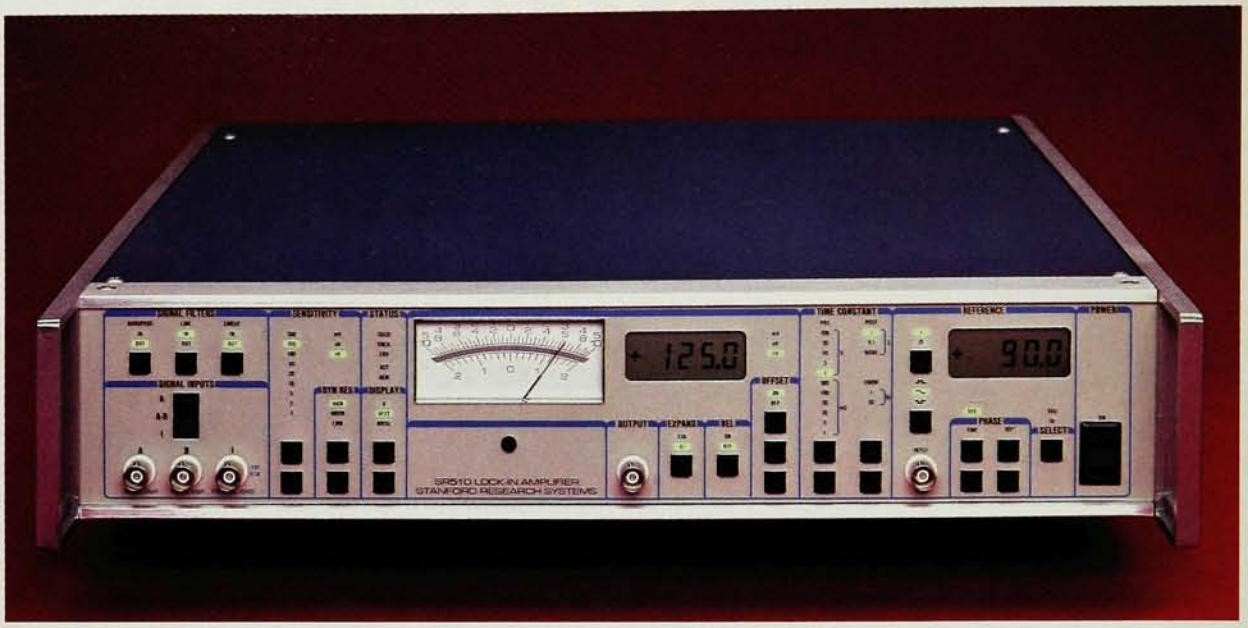


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colleagues are unable to. I believe that I can organize the voluminous quantity of material that is important in medicine much more effectively by utilizing "understanding" to the extent to which it exists, and in many areas that is quite considerable. My principal objection to the way in which medicine is taught in the US is the emphasis on memory alone rather than on fundamentals. I believe that medical science, physicians and patients would be much better served if the opposite were true.

Like every well-established discipline, medicine has its strong traditions and biases, and one cannot expect a viable effort for change to arise spontaneously with that profession. I had expected that the infusion of hard scientists, physicists in particular, into medicine would accelerate this process. I am confident, however, that Yaes's viewpoint represents a minority opinion in the community of scientists retrained in medicine.

MARK MANDELKERN
*University of California*
*Irvine, California*
*9/84*

## Molecular spectroscopy

In Search and Discovery (July, page 17), PHYSICS TODAY presented an interesting discussion of a fascinating topic in molecular spectroscopy. The theory of clustering in levels of high angular momentum in $SF_6$ and the resolution of the resulting extremely small splittings combine to form a very nice piece of research in this field. It is hoped that the editors will continue to expand their coverage of molecular spectroscopy so as to include some of the many other advances being made in the subject.

There are in the article two particular points to which I would like to draw your attention. First, the essence of the clustering phenomenon is contained in the article by A. J. Dorney and J. K. G. Watson, *J. Mol. Spectrosc.* **42**, 135 (1972). Since this article presented the original discussion on the subject well before the cited "first indication" of clustering, it would have been most appropriate to include this paper in the list of references.

Second, there seems to be some confusion regarding "the violation of a hitherto sacrosanct selection rule of molecular spectroscopy; different symmetry states will become so mixed that the symmetry species designation is no longer a valid quantum-state label." Contrary to the implications of the term "hitherto sacrosanct," the breaking of this selection rule has been observed many times over the years in a variety of molecules including $CH_4$, $OPF_3$ and $CH_3SiH_3$, using a variety of techniques including molecular-beam magnetic resonance, molecular-beam electric resonance and nuclear spin relaxation. The first mention (to my knowledge) in the literature of such symmetry-breaking is to be found in C. H. Anderson and N. F. Ramsey, *Phys. Rev.* **149**, 14 (1966). The first direct observation of such symmetry-breaking transitions was reported in I. Ozier, P. N. Yi, A. Khosla and N. F. Ramsey in *Phys. Rev. Lett.* **24**, 642 (1970). Although the experimental circumstances are somewhat different than those in the $SF_6$ work, the quantum mechanical principles are identical.

It is recognized that in the context of your recent article this point regarding the symmetry-breaking in molecules other than $SF_6$ does not require in-depth discussion. However it is also felt that your readers should not be left with the impression that the symmetry selection rule has been "hitherto sacrosanct," or has not been previously discussed or observed.

IRVING OZIER
*University of British Columbia*
*Vancouver, BC, Canada*
*10/84*

## Redshift debate

I found your Search and Discovery article on the redshift (December, page 17) very interesting. Grote Reber (the radioastronomy pioneer) and I have been advocating the Compton effect interpretation of the redshift for years, for different reasons. If you think such professional astronomers as Geoffrey Burbidge and Halton Arp have problems getting their papers published, you can imagine how difficult it is for people who are not in the club! However, Reber and I both managed to get published about 17 years ago—Reber in the *Journal of the Franklin Institute*,[1] and I in *Solar Physics*.[2] Reber concluded[3] that the Compton effect caused the redshift, to explain the very bright night sky at very long wavelengths (hectometric). I used this explanation for the solar redshift and the quasar redshift.

I've come to the conclusion that quasars are among the most intrinsically dim nearby stars instead of being the most intrinsically bright distant objects, as generally thought. They have a redshift on the order of a million times as large as the solar redshift, and therefore are surrounded by an electron cloud about a million times as large as the solar corona. The redshifts alone reduce their energy by a factor of about three, which shifts their luminos-

*continued on page 102*