letters

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just an ordinary citizen who happens to have belonged to The American Physical Society for 33 years, I suspect that promiscuous use of the professional cloak in speaking out on non-physics problems is "where we have failed" in the area of credibility with the public.

GEORGE M. STAMPS
GMS Consulting
8/21/78 *Westport, Connecticut*

On second reading

In my review of *Scientists Confront Velikovsky* (August, page 56), I questioned whether J. Derral Mulholland could have meant to say that the number of days per year could have varied as much as 1 or 2 percent during recorded history. Velikovsky, in fact, had proposed even more drastic changes in the Earth's rotation rate within the past 3000 years, and as I read Mulholland's discussion I assumed he was also speaking to the possibility of such variations during historical times. In fact, however, Mulholland was referring to the possibility of such changes over geological times (say, the past few hundred million years), as is clear from a careful reading of his following paragraph (page 109 of *Scientists Confront Velikovsky*). I had read too quickly and misinterpreted his meaning, and I apologize for the error.

GEORGE O. ABELL
University of California
9/21/78 *Los Angeles*

Image slicers

Nathaniel Carleton and William Hoffmann, in their interesting article on the Multiple-Mirror Telescope (September, page 30), state "However, the practical difficulties of these devices [image slicers] are such that they have seen little actual use." This is perhaps a tongue-in-cheek statement, appearing as it does in an article describing a telescope with "21 electromechanical servos", "51 interacting parameters," and at least 73 optical elements. An image slicer is a simple and practical device. Their rarity is due almost entirely to the conservatism of astronomers, combined with a folklore that leads to statements like the one quoted. Slicers were reviewed[1] a few years ago in a book edited by Carleton himself. Richardson slicers, which can be used in beams of small f-number, are in fairly wide use For slower beams, the elegant Walraven slicer[2] (a solid-state equivalent of the Bowen device) can be highly recommended. I have no doubt that the "telescope slicer"[1,3] proposed for the MMT will also work, but it is more complicated than an image slicer and will have its own practical difficulties. I would not be surprised to see an image slicer on the telescope before long.

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DONALD M. HUNTEN
University of Arizona
10/19/78 *Tucson, Arizona*

Boycott anti-ERA states?

In a letter published in September (page 13), Robert Rubin asks why The American Physical Society hasn't taken a position on the matter of holding meetings in states (presumably not holding meetings there) that have not ratified the Equal Rights Amendment. And he asks why there has been no discussion on this question in PHYSICS TODAY.

Maybe this is because there are after all still a few level heads in the leadership of the APS. But according to a recent newsletter, the Division of Particles and Fields is now considering such a position; so I can imagine that the political activists, and also those who look to Washington to solve all society's problems, have long been itching for the APS to raise its banner in this knightly effort.

I strongly oppose the APS's taking any position on such a matter as the ERA because I don't think that this issue has anything to do with the purposes for which the APS was organized. If an APS member is discriminated against then it is appropriate for the APS to express itself and to act thereon. But this is quite a different matter.

Granted the desirability of the goal of the ERA, individuals certainly differ on the appropriateness of the ERA as the proper means to achieve that goal. This is a social and political question and as such has no connection with the reasons that I, and I think most others, joined the APS. I do not think that the APS should even be commenting on such issues let alone be acting on them.

In fact I resent the zealotry of those who inject a divisive social and political issue into an organization whose purpose—the pursuit of physics—should cut across all such questions. There are ample forums for those concerned with social and political matters to pursue these concerns.

W. MOELLERING
10/17/78 *Cincinnati, Ohio*

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Robert Rubin asks why the APS has not taken a position on the proposed Equal Rights Amendment, yet he answers his own question when he quotes the APS constitution that the "object of the American Physical Society is the advancement and diffusion of the knowledge of physics."

The APS has neither the obligation, the authority, nor the ability to solve all of society's problems. We should stick to our declared objective, and not dilute our efforts on unrelated issues.

JAMES POTZICK
National Bureau of Standards
9/28/78 *Washington, D.C.*

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With reference to the letter of Robert Rubin, I would like to suggest that the function of The American Physical Society is not and has not been to take such positions as suggested by Rubin on such matters as ERA. I believe that this was clearly expressed by the membership of APS with regard to the "Schwartz Amendment" of a decade ago.

It might be of interest to recall that members of APS reside in the states who by legal process have chosen to not ratify the ERA. It may also be that members throughout our country have varying views on the wisdom of this particular amendment. To suggest that the APS consider boycott of any region of the United States because of political views of its citizens is offensive and absurd. Surely we do not wish the APS to take punitive action against members for exercising their rights as US citizens? To demand that candidates for office in the APS declare their personal political views on such matters as the ERA would raise questions about the purpose of the office.

I support the continued attention of APS to professional matters, to quality publication and dissemination of the advances in physics, and to public education on concerns of the physics profession. Let us tend to physics as a Society and leave the emotional, political issues to the individual.

BENNY J. HILL
Southwestern Oklahoma State University
9/28/78 *Weatherford, OK*

Managing research

The series of letters on these pages (February, page 83; September, page 11) discussing management practices for research organizations raises several interesting questions. Management, whether a science or an art, is necessary for the smooth functioning of any organization. Fair and efficient application of the principle of accountability is at the core of any managerial system. This holds true both for the loose application of a traditional academic department as well as for the type of controls necessary in meeting a very specific design requirement.

The problem as I see it is the great difficulty in describing suitable performance measures for evaluating an individual's accomplishments. This is especially true in that portion of our endeavors associated with the advancement of knowledge. In the use of resources supplied by others, one is naturally led to two choices: (1) fund all activities with the expectation that the nature of the research on the average is such that some societal good will result, or (2) design in advance certain areas for research, establish criteria for selecting proposed work and make the necessary funds available to accomplish these goals. To me it appears apparent that only option (2) is viable in the long run, though it might indeed be possible to use option (1) under limited circumstances.

Management by Objective, as briefly described by Thomas M. Tobin (February, page 83) appears to me to be a way of enabling the researcher to describe his activities in terms of the organizational goals and make possible his active participation in the establishment of these goals. Listing of anticipated milestones is necessarily part of the process. However, management that does not recognize that unexpected results might significantly change the time schedule for accomplishing what was promised, or that new directions frequently must be sought, is shortsighted and indeed may be subject to Anthony J. Duben's concern (September, page 11) of stifling creativity.

In summary, some accountability for the expenditure of funds is necessary. Participation by the researcher in establishing the performance measures is desirable. Fair application of the general principles by the supervisor is essential.

DAVID T. GOLDMAN
National Bureau of Standards
Washington, D.C.
10/19/78

Astronomical nomenclature

Astronomers deal in small angles, the expression of which seems to baffle editors and printers of some journals. One way of expressing a second of arc is by the use of two dashes, but some editors interpret this to mean inches and have saddled some of my colleagues with photometer diaphragms more than a foot across. The editor of a most distinguished physics journal returned a paper dealing with the angular diameters of stars with the admonition that in his journal they did not use the English system of units and would we please convert to metric. Stifling the rejoinders that rose to our lips we diplomatically replied that we realized there was room for misapprehension and offered a different form of words to express the unit of our trade—the one thousandth part of an angular second. How should we express this? Système International, the adopted standard, uses the same nomenclature for all units. "m" is a meter, μm is a micrometer (to be measured with a micrometer), a nanometer (arguably a female omnivore who gobbled up Angstrom) is one thousandth of this. Seconds are "s," usually meaning seconds of time, and units go all the way from the stately dance of the Gigasec to the high speeds of the ms, ns and ps. Seconds of arc present a problem. One journal insists we say "milli-arc sec" which, unlike milliamp, is hyphenated, and is illogical since it is the sec which is millied and not the arc. The logical notation is "arc ms" for one thousandth of an angular second, but then some pencil-happy copy editor thinks we mean "arc m" and writes "arc mins" thus expanding our result by 60 000 times and leaving us with large quantities of egg (kegg or even Megg) on our innocent faces. We have stood unswervingly ($\sigma < 10^{-5}$ radians) for many Ms by the notation "arc ms" and trust logic may prevail.

DAVID S. EVANS
The University of Texas
Austin, Texas
10/27/78

Tribute to Infeld

This year represents the tenth anniversary of the death of Leopold Infeld, former collaborator of Albert Einstein and one of the most important scientists of Poland after the Second World War.

Infeld was born in Cracow in 1898. He earned his doctorate degree in 1921 there. As a research scientist Infeld is well known as the coauthor of papers dealing with spin in a curved space–time (with B. L. van der Waerden),[1] with nonlinear electrodynamics (with Max Born),[2] and most importantly with the dynamics of ponderable bodies in the general theory of relativity (with Banesh Hoffmann and Einstein, widely known as the EIH theory).[3] Together with Einstein he wrote a popular book, *The Evolution of Physics*.[4]

As an academic teacher, Infeld gave rise to generations of theoretical physicists, first at Toronto and subsequently at Warsaw. I have had the good fortune of knowing Infeld from 1936 to his death in 1968. He was an outstanding theoretical physicist, a great academic teacher, and a human being deeply committed to the betterment of the human condition. Recently the Polish Academy of Sciences has published a book, edited by his son Eryk Infeld, which commemorates the different aspects of Leopold Infeld's life and work.[5]

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PETER G. BERGMANN
Syracuse University
Syracuse, New York
10/10/78

De Sitter argument

A recent news story (March, page 14) claimed that the work of K. Brecher on x-ray pulses from binary sources was the best confirmation ever of the de Sitter argument for the second Einstein postulate. The reason for this being that while "extinction" effects could have attenuated the velocity dependence of visible light from a binary source, x-ray velocities would not have been so altered.

In the x-ray region, the *normal dispersion* index of refraction, n, of many substances is minutely less than unity. For such materials, the Fox "extinction" length hypothesis of $\lambda/(n-1)$ breaks down; the light wave, anticipating the medium interface, would be "extinguished" before it crossed the boundary. J. G. Fox[1] presented the expression $\lambda/(n-1)$ as "an experimental fact which is well known in physical optics." No reference to the alleged experiments on this *fact* was presented. In his experiment with T. A. Filippas,[1] J. G. Fox[2] observed, in a footnote, that in the absence of experimental data on extinction: "Uncertainty about how to estimate [not measure] the effect for γ rays delayed publication of the experiment for many months." On the very next page Fox[2] declares: "Finally we have fairly good direct experimental verification of the extinction length for x-rays.[12]" The superscript 12 refers to Fox's own experiment[1] with Filippas, which was delayed in publication for many months by *uncertain estimates* of the "extinction" length, and which later, within the space of one page, is regarded as direct experimental verification—given no other experimental references to this alleged *fact*. The specific quantitative hypothesis of "extinction" first wrongly asserted to be "an experimental fact which is well known in physical optics," led to "uncertainties about how to estimate the effect," which uncertain estimates are then quickly declared to be "good direct *experimental* verification of the extinction length."

It is unfortunate that so many physicists have for so long been misled by the de Sitter type of argument alone, and, more recently, in conjunction with an experimentally unsupported or tested