

letters

ralism for support sources might concentrate too much power in a single large department, and the political visibility of such a large DST might become a liability.

Such problems must be weighed against the need to amalgamate the federal R&D effort to solve the problems of administrative inefficiency. But perhaps the plethora of the federal R&D effort has not resulted in duplication, waste and administrative inefficiency—and the creation of a super science agency in the name of administrative efficiency would only create a plethora of problems resulting in a deterioration of American science and technology. The burden of proof that the present system is best—though not without its faults—must rest with the science and technology community.

A. MICHAEL NOLL
Stirling, New Jersey

7/12/77

* * *

The author was a staff member of the Office of Science and Technology from 1971 to 1973 and personally witnessed the disbandment of the White House science-advisory mechanism by the Nixon administration. This letter is based upon testimony submitted by the author to the Committee on Science and Technology, US House of Representatives, for consideration during its deliberations on re-establishing the mechanism.

Philippine repression

I have just recently returned from Manila where I visited a Filipino priest friend of mine incarcerated in the military stockade at Camp Crame for almost a year now without any formal charge against him. His cellmate was Roger Posudas, a physics professor at the University of the Philippines (PhD, University of Pennsylvania, 1970).

Posudas has been in prison for a year and a half as a political prisoner, although he told me that he is accused of being a demolition expert for the New People's Army. His field is general relativity. Some of the reprints he receives are rather formidable looking and have been held up for quite a while, while his jailers scrutinized the equations for hidden messages.

I do not know him well. I only know him from some pleasant conversations I had with him during my visits to the stockade. But I do know very many innocent people here who have suffered imprisonment and much worse for the sole crime of trying to defend human rights. So I promised him I'd write this letter.

The physics community can be proud of itself for the many letters from all over the world sent to President Ferdinand Marcos on the behalf of Posudas. I saw

a stack of these letters, but he is still in the stockade. It is very frustrating when both courts and private appeal are of hardly any help. However, public outcry and letters do have their effect. The jailers will sometimes release their victims, not so much out of a respect for human rights or due process of law but simply out of embarrassment.

RALPH S. KROES

Maryknoll Fathers
Davao City, Philippines

10/18/77

Misapplications of uncertainty

Although careless applications of the quantum-mechanical uncertainty principle are not rare, the recent article on ultracold neutrons (June 1977, page 42) contains one that is too gross to ignore. The argument begins with the relation $\Delta E \Delta t \approx \hbar$. It is then asserted that $\Delta E = D\epsilon$, where D is the neutron's electric dipole moment and ϵ is the external electric field. Taking $\Delta t = T$ (the neutron storage time) then yields the result that the smallest electric dipole moment detectable in an experiment on a single neutron is $D \approx \hbar/(\epsilon T)$.

But the uncertainty in the neutron energy is not $D\epsilon$, but rather $D\Delta\epsilon$ where $\Delta\epsilon$ is the rms fluctuation in the electric field (due to spatial or temporal variations). One can regard $D\epsilon$ as the uncertainty in E only in the purely subjective sense that D is a totally unknown magnitude. But purely subjective uncertainties are irrelevant. For example, my own subjective ΔE for a cold neutron is very large at this moment because I do not remember its rest mass, but one would be foolish to hope that one could use this large ΔE to reduce Δt .

In fact, the above estimate of the minimum detectable electric moment is correct, but the derivation quoted is unsound. The experiment consists of measuring the shift of the free-neutron nuclear magnetic resonance frequency that is produced by the application of an electric field. The uncertainty in any frequency measurement is of the order of the reciprocal of the duration of the measurement. Thus $\delta\omega \approx 1/T$, where T is the neutron storage time. The frequency shift due to the electric field ϵ is $2D\epsilon/\hbar$. Omitting numerical factors, we obtain the uncertainty in the electric dipole moment to be $\delta D \approx \hbar/(\epsilon T)$, which is also the minimum detectable dipole moment.

We can now see that $\hbar$ does not enter the answer through the quantum-mechanical uncertainty principle, which plays no role in the argument. The uncertainty in measuring a frequency during a finite time interval is entirely classical, so my conclusion does not depend upon any particular (or potentially controversial) interpretation of quantum mechanics. Finally, I note that F. L. Shapiro,

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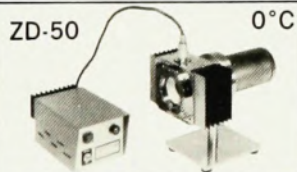


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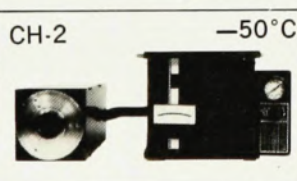


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who is credited with proposing this experiment, did not use the unsound argument criticized above.

LESLIE E. BALLENTINE
Simon Fraser University
Burnaby, B.C.
Canada

7/25/77

Leslie Ballentine's criticism of Lushkov's article is absolutely correct. The crux of the matter is that the purely classical relation $\Delta\nu\Delta t \approx 1$ is often combined with the Einstein relation $\Delta E = h\Delta\nu$ and the resulting $\Delta E\Delta t \approx h$ is then mistakenly called a Heisenberg uncertainty relation. A careful distinction between the Heisenberg uncertainty relations and the relationship between energy and time is drawn by M. Bunge in *Foundations of Physics*, Springer-Verlag (1967).

P. D. MILLER
W. B. DRESS
Oak Ridge National Laboratory
Oak Ridge, Tennessee

10/25/77

Scientists out of the closet

To Frank von Hippel's essay on scientific freedom: Bravo! The time has come for scientists to come out of their closet-like laboratories. For centuries science has been at the beck and call of the slavish desires of self-seeking elements of society. Science, like other professional fields, must control its own destiny. The world must know the true humanity of science, as the soporific seventies become the eager eighties.

RICHARD A. RODMAN
Falls Church, Virginia

10/20/77

Referee standards

In view of the dedication of The American Physical Society to the advancement and diffusion of the knowledge of physics, I wish to remind members of the APS and other interested readers of two ongoing and pervasive situations that threaten these goals. It has been my misfortune in recent months to have come face-to-face with these situations in two separate instances.

The first point I wish to raise again concerns the standards applied by referees to judge the suitability of papers for publication. The variability of these standards among referees has been a subject for discussion (and lament) on many occasions. The issue of its desirability, and the separate issue of whether or not the physics community can change this variation, depend to some extent on the degree of variation under discussion. Small-scale variations are probably desirable, and almost certain to be found.

On the other hand, large-scale variations are both undesirable and destructive, and no less certain to exist.

By large-scale variations, I am referring to referees whose standards are either extremely exacting or very lax. For example, the referee who will not approve a paper unless it represents a truly major step forward, and the referee who approves papers with elementary mathematical errors, represent large-scale variations. Both are destructive, especially the latter. The former referee excludes material useful to workers in the field. The latter, however, permits diffusion of work that represents a bad example, and serves to diminish the quality of work of the journal's readership. The argument that bad work in the literature sharpens the wits of the readership is, I believe, false. Bad work is generally believed as readily as good work by many readers, except in extreme cases. The referee who approves work containing logical, mathematical or conceptual errors of an elementary nature does the community a great disservice, and I would urge all potential referees to be more vigilant than the community as a whole has been in the past.

Some papers in this category contain an error of the type where one or two results are quoted to support an idea or method, whereas several others to which the idea or method applies, and which tend to discredit it, are ignored. These papers exhibit especially poor research, and I hope that we can minimize their appearance, or better still, eliminate them altogether.

The second point I wish to make concerns a style of politics that has recently appeared. In recent years, the emphasis in funding has moved towards "relevance." By this I mean that the proposed work must be shown to assist the efforts of the agency to complete its mission. Not all agencies have missions, but more and more those that do are requiring proofs of relevance. Consequently, we see efforts by some to show that work that is either wrong or controversial or just irrelevant is in fact the Crux of the Matter. Unfortunately, sometimes these claims are sufficiently well orchestrated to require a careful and thorough response. From my own experience in recent months answering such groundless claims has taken time away from other work that I feel is important. Again, referees can help here. Also, the community as a whole, through contract monitors and reviewers, can help to minimize or eliminate this problem.

On the other hand, the suppression of good work, whether it disagrees with one's point of view or not, is highly undesirable. The basic point I wish to make is that time, effort and frustration can be saved by eliminating from circulation work that contains elementary mathematical, logi-

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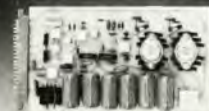
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