

letters

judgment states that the government libraries have the right to copy and distribute articles and journals in photocopy form to government scientists. Since almost any grantee institution or researcher can claim to be included under this definition, this means that the government is, in practice, free of any and all copyright restrictions. In short, it has accorded itself the right to steal.

The scientific community apparently does not understand the significance of this problem. For authors, it means that specialized journals that would usually publish their papers will soon cease to exist unless the authors themselves are willing to pay all the costs of publishing their own materials. This would remove the invaluable process of technical editing and peer review from the papers that would be published. Another result is already seen in the continuing rise in price of such journals as are now being published.

We would like to suggest to all interested readers of the physics literature that they write to Senator John McClellan's office, Washington, D.C., for a copy of Bill S. 1361, the proposed revision of the copyright law. Readers as well as authors must get involved in this matter, not only to protect their right to obtain information, but their right to publish and maintain ownership and responsibility for the use and distribution of their own works.

WILLIAM WHITE JR

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Teleneural physics?

Those who are favorably impressed by Wilbur Franklin's appeal (August, page 11) for an open mind toward the seemingly direct transmission of information from one brain to another should in all fairness be told that what he is describing has for the last 40 years been better known as "extrasensory perception."

The foremost entirely experimental journal in this field is the *Journal of Parapsychology*, which has published without interruption approximately 11 000 pages since 1937. Since 1950, more than 1200 articles on parapsychology have been indexed in *Psychological Abstracts*. A professional organization, the Parapsychological Association, was founded in 1957 and accepted as an affiliated society by the AAAS in 1969. While the phenomena of parapsychology, if real, constitute a challenge to physics, it is probably too late to substitute Franklin's new name, "teleneural physics."

As a "rightwing" parapsychologist, I am pleased that Franklin has brought

this field to the attention of physicists. My concern is that he has failed to include a warning that most of what passes into public knowledge under the name "parapsychology" will be seen as anti-science by most physicists, myself included.

After years of silence on this matter, I have recently mounted a frontal attack against the left wing of parapsychology. Those who like a good battle should enjoy my opening salvo, "Parapsychology and the Occult," in the July, 1973, issue of the *Journal of the American Society for Psychical Research*.

R. A. McCONNELL

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Physics establishment

I wish to point out the crime of the "haves" of the current physics establishment against the unemployed "have nots." For years "have nots" such as myself have been educated to believe that a good graduate training in physics would allow one to be adaptable to many possible positions in physics with a minimum of on-the-job retraining and reeducation.

Does this in fact occur? The answer from one who has recently been seeking a change in employment is a resounding *no!* I have recently been interviewed by a variety of high-technology companies over the past six months usually seeing some moderately well known "have" physicists. Despite a broad and varied background (which I use currently to be an industrial metallurgist-ceramist to keep my wife and two children eating while I search for that will-o'-the-whisp elusive position as a physicist) the answer is always the same: "Without experience on NMR studies of green, #5, left-handed smoke shifters, the job will go to a more experienced applicant" or "if one has had such experience on NMR studies of green, #5, left-handed smoke shifters," one should have preferably worked primarily "on the 1963-B model," and with preferably twenty years of experience on the 1963-B model (only possible by 1983 if I am diligent).

This same trend, which unfortunately industry subscribes to quite completely, is even more shocking when heard coming from a physics-department chairman interviewing for faculty positions, as was in great evidence at the New York meeting in January. I have even heard this same sort of infinitely specific requirement from two physics departments from which I obtained my advanced degrees—and these were the very same people who urged me as a student to be broad and let the future take care of itself. Well, I've arrived in the future and feel

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letters

greatly betrayed by these "haves" in the physics community. One well-known university had originally advertised in *PHYSICS TODAY* for "a junior faculty condensed-matter physicist with experience or interests in biophysics," yet when contacted they indicated that interests in biophysics wasn't the same as five years experience in the field—exit all unemployed, solid-state physicists with a naive interest in becoming biophysicists.

Of course, it's a buyers' market, as anyone who has been seeking a position can readily agree. The AIP Placement Register this last year in New York was an utter fiasco (made a fiasco by the specificity and small numbers of interviewers—not by the AIP, which does a bang-up job as always), but, coming from a "have" generation who sold us this line of "general physics education, applicable to all problems and positions," it is particularly galling.

My advice to all who have not had this experience is as follows. A rigorous, broad training in physics is *not* enough—a good knowledge of materials science (metallurgy, ceramics, polymers, and so on) will help you get some sort of a job until your training, in scattering studies of charged raindrops off green bullfrogs, offers a position, say, sometime before 1997. However, experience on the smoke shifter should also be gotten!

EDWARD SIEGEL
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Superconductive tunneling

Friends of Ivar Giaever, Brian Josephson and Leo Esaki, and I also, were delighted with the choice of recipients for the 1973 Nobel Prize awards in physics, and believe that the recipients were truly deserving of this high award.

In writing a history of the development of the field of electron tunneling through barriers between superconductors, it seems that the earlier, pioneering investigations by Hans Meissner (Stevens Institute of Technology) of superconductive tunneling through normal metal films between superconductors ought not be overlooked. See, for example the article in *PHYSICS TODAY* for December 1973, page 73–75, on the Nobel awards.

In this connection, a comment by P. G. De Gennes in his paper on "Boundary Effects in Superconductors" in the *Reviews of Modern Physics* 26, 226 (1964) is apropos:

"(c) a thin ($\sim 1000\text{\AA}$) normal slab N separating two superconductors S and S^1 is able to carry a finite supercurrent from S to S^1 . These SNS¹ junctions have been studied first in the pioneer

work of Meissner (*H. Meissner, Phys. Rev.* 117, 672 (1960) (my italics). Their interest is twofold: (1) from the dependence of the critical current on the thickness of the N slab one may obtain an estimate of V_N , the electron-electron interaction in N . (2) the SNS junctions have a wide range of critical currents and critical fields: this could be useful for some low temperature devices . . ."

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Eckart's contributions

I was much saddened to read recently (January, page 87) of the death of Carl Eckart, whom I had known, and who had served as my counselor and inspiration when I was a graduate student at the University of Chicago before World War II.

While I was aware of Eckart's outstanding success in oceanography following his moving to California after the war, I was surprised to find no mention of his prior contributions to theoretical physics. What I recall, without searching through the literature, are his papers on the variational method, and his showing of the equivalence of the Schrödinger and the Heisenberg formulations of quantum mechanics. During my time at the University of Chicago, he was generally considered to be by far the most competent physicist around. He was both loved and admired by the students, whom he was always ready to guide and assist in their research, whether it be in theoretical or experimental physics, geology, or as in my case, biophysics.

FRANKLIN F. OFFNER
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NSF referees

I agree with the basic argument given by Robert L. Chaplin in his letter (January, page 121) that a change is needed in the method used by NSF to evaluate proposals.

The fact that NSF assumes that referees are infallible and expects them all to be "enthusiastic" in favor of a new proposal is an extremely unfair obstacle. And in a situation where the writer of the new proposal has challenged the orthodoxy of some branch of science, the present NSF policy represents an essentially insurmountable obstacle. If, in addition, the writer happens not to be in an academic position, present NSF policy is *de facto* discrimination, pure and simple. This is because there is "no way" that all the reviewers will be "enthusiastic" to fund a proposal, by someone not in

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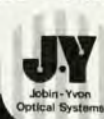
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