

letters

This announcement has been requested by seminar leaders, who include M. Azbel and A. Voronel. They are urgently in need of contacts with visiting scientists, having been denied normal routes of information exchange as well as professional employment.

GREGG DASH
University of Washington
Seattle, Washington

EARL CALLEN
Committee of Concerned Scientists
New York, N.Y.

More on Sherwood

I enjoyed reading about the origin of "Project Sherwood" in your October issue (page 78) and wanted to share with your readers an "alternative explanation" given to me by Charles Nomand. Nomand worked with E. O. Lawrence at Berkeley and later developed the vacuum systems for the calutron magnetic separators at Oak Ridge. When I arrived at Oak Ridge in 1954, I asked Nomand about the meaning of "Project Sherwood." He explained to me that it was a project to control thermonuclear fusion and told me all about Robin Hood and Friar Tuck. But then, he added, "... Project Sherwood really means that it 'sure would' be nice if it would work!" We certainly have a lot of reason for increased optimism for this possibility in 1974 than we did in 1954. However, in view of Charlie Nomand's interpretation, I vote that "Project Sherwood" be retained as a relevant and fitting title.

JOHN H. GIBBONS
University of Tennessee
Knoxville, Tennessee

Birth of radioastronomy

I feel that I must correct an inaccurate statement, made by Victor Weisskopf, which you have quoted on page 28 of the November issue. Radioastronomy was not born in Holland, although his story about Oort is correct as regards the first radio observations of a spectral line. While several unsuccessful attempts to measure radio emission from the Sun were indeed first made in Europe at the turn of the century, the first actual detection of extraterrestrial radio radiation was made by Karl Jansky in 1931-32 at the Bell Telephone Laboratories in Holmdel, N.J. The only other significant pre-war observations were done by another American, Grote Reber. During and shortly after World War II, however, the Europeans, in particular the English, and the Australians very soon took the initiative away from this country and dominated the field for over a decade.

As Weisskopf also mentioned, the US

is today once again being outclassed by the new radiotelescopes in England, Holland, and Germany. Unless the funding situation dramatically changes in the near future, I fear that American radioastronomy will remain "second fiddle" for a long time to come.

WOODRUFF T. SULLIVAN, III
University of Washington
Seattle, Washington

THE AUTHOR REPLIES: I agree very much with Woodruff Sullivan's statement and I regret that I have not been accurate enough in my interview about European science. Surely the credit for having made the first detection of radio emissions from outer space goes to Karl Jansky. It would have been perhaps more accurate if I said radioastronomy was not born, but was "raised" in Holland.

VICTOR F. WEISSKOPF
MIT
Cambridge, Massachusetts

No crystals in biology?

The impact of quantum concepts on biological systems is currently being felt. In particular, your May issue (page 9) contains a quantum-mechanical formulation by Charles Lumsden and Melvin Silverman of membrane processes. In the majority of biological systems, it is questionable if there is sufficient regularity to carry out quantum-mechanical calculations of electron states based on crystalline-lattice derived methods. Crystallinity is a precise mathematical concept, and although there is a great deal of organization in the molecular sense and some biological materials can be crystallized, the incidence of *in-vivo* crystallinity (as opposed to order) is probably rare. A review of the literature will turn up very few diffraction patterns, or other evidence, showing what is accepted to be crystallinity.¹ Thus, the quantum-mechanical formulation of most biological systems requires the application of quantum mechanics for disordered systems. Until recently, this has presented a virtually intractable problem.

The discovery of amorphous semiconductor switching in melanins, by J. E. McGinness, P. Corry, and P. Proctor² represents data that identify the melanins as amorphous semiconductors. This prototype system makes it clear that the direction of formulation is away from crystallinity and into the concepts of conductivity through disordered systems. As a further example, G. Kemeny and R. Rosenberg³ have shown that conduction through proteins and many biological materials may be of a polaron nature. This discovery is interesting because, in amorphous ma-

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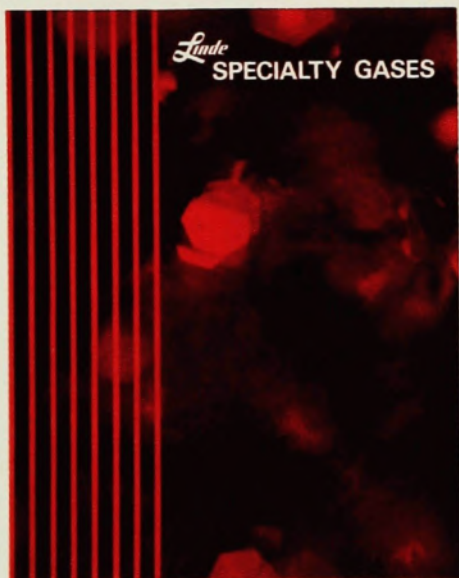
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materials, the localization of states is expected to be enhanced because of the random fluctuations in the potential.

In the case of membranes there is a hydrophobic region surrounded by a hydrophilic surface. There is a further complication by the large number of proteins and enzymes associated with the membrane. This includes specialized proteins, which are associated only with the surface, as well as proteins that pass through the membrane. All of this makes for a complex situation. The conductivity of these materials can be changed in many cases over at least five orders of magnitude by the extent of hydration. It may be possible to fit some aspects of behavior of membranes with the superlattice-crystalline approach. However, it appears that efforts based on crystallinity or order will be of limited efficacy. On the other hand, the application of concepts from quantum mechanics of disordered systems may have some application in charge-transfer interactions with the membrane or in other specialized cases. It is to be hoped that there will be enough growth in the understanding of amorphous substances to tackle the more complex problems in biological systems

References

1. A. S. A. Spiegler, G. C. Henney, X-Ray Diffraction Studies in Biology and Medicine. Grune (1947).
2. J. E. McGinness, P. Corry, P. Proctor, Science 183, 853 (1974).
3. G. Kemeny, B. Rosenberg, J. Chem. Phys. 52, 4151 (1970).

G. J. FILATOV

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More on math

I was pleased to see Robert Hermann's letter (December 1973, page 9), which regards as inadequate the current mathematical training of physicists. In rebuttal, Joel Spira feels (June, page 15) that the practical problems he runs into, as an executive of Lutron Electronics, require less mathematics than training given to him and his subordinates. This may well be a case of one man's poison being another's fancy, but as Head of the Mathematics Research Center of the Naval Research Laboratory under the scrutiny of a very practical and conservative authority, we have been given the problem of surveying areas of fundamental physics to examine if the mathematics being used is not responsible for stagnation, and if new techniques would not lead to new insights. There were eight positions open for people with clear analytic and creative minds, and in this review of candi-

dates I am forced to agree with Hermann. Electrical engineers receive a better and more fundamental training in the mathematics of modern physics.

In my opinion physics owed much to the classical Göttingen school, but now may well be encumbered by its rigid influence. The powerful techniques of the French *Bourbakiests* have been restrained by physicists who many times as an example try to study discontinuous behavior with techniques suited to the continuum. Distributions, exterior algebra and meromorphic functions are mathematical areas that are suited for study of surface physics, condensation, phase changes, to name the phenomena basic to the field Spira represents—the electronics industry. Too often the technique dictates the problem, as people try and connect continua across an interface, when the interface itself should be studied. Nonlinear equations are the only ones that implicitly carry discontinuities and can describe particles as well as unite the ensemble of different physical regimes. Yet each nonlinear equation has been solved as a single entity in itself, and it is the theory of differential forms that is indicating a basis for establishing a systematic method of approaching these problems, as an alternative to perturbation theory.

In my opinion, no matter how bad the market is, intelligence is rare and precious, and the mind that can distill the essence of a system with simplicity and clarity will always be welcomed in this laboratory. I for one will endeavor to share whatever limited resources we have to give temporary shelter to talented people who want to pursue new mathematics in physics away from the indifference and morass of social economic pressure, which seeks only quick answers to be marketed in a hurry. I ask that we not make the problem any worse for these dedicated minorities—there is much work to be done in physics.

ANGELO J. SKALAFURIS
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Though I speak only for myself, seventeen years in Government, Industry and University persuade me that, while some physicists may need to apply better the fancy mathematics they already know, many more physicists need to know much more and more relevant mathematics. What then is the problem and where does it lie?

The problem is one of attitude. It lies partly in the attitude of many professors who appear to inculcate in their students the fallacy that areas of physics that have redeeming social value are by definition not scholarly and are to be avoided. It lies partly in the attitude of

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