letters

matter..." are unconscionable.

We technical people are experiencing a period of deep and painful readjustment, for sure. The sharp decline of spending on space, the nucleus, and defense, and its shift to people and environment, will result in undiminished demand for technical effort, even though this will take time to formulate and come into focus. The problems calling for technical solutions (aside from those, equally urgent, calling for social solutions) have apparently never been greater or more pressing, and the physicist's capability to contribute to these solutions will in the long run continue to be recognized.

It will help if the physicist recognizes, himself, the breadth of his capacity. Specialization is only habit of thought. Changing fields can always be sharply beneficial both to the individual physicist and his sense of well-being, and to his useful output; fear of venture into totally new problem areas is—to paraphase the old saw—understandable, natural, almost universal, wrong.

It will be wise not to sit back and wait for those overly swayed by philosophical fluctuations to develop the market for our talents for us. The time to wade out into these shifting currents, to identify some of the myriad technical problems and opportunities, to rank these as closely as possible with real human needs—whether short range or long range, local or universal, to take the responsibility for initiating work toward their solution (and incidentally to support ourselves in the process), is now. The one to do it is he who is best prepared to do it. Have a look in the mirror.

W. A. Thornton
Cranford, N. J.

Handicapped groups

In response to your editorial in the April issue (page 84) you may be interested to learn that several minority-group scientists have organized a national committee [Committee for Minority Participation in Physics, W. E. Henry, chairman (Howard University), F. R. Norwood, co-chairman and secretary (Sandia Laboratories)] to augment the development of scientists and technicians among the minority societies. One of the principal problems faced by this committee will be finding ways to bring this effort to the attention of members of minority groups who may not realize that a small number of their members *have* survived the obstacle course. Several means come to mind, and some avenues will be explored with firm determination, but it is to be noted that placement of minority-group persons may test the extent to which the present-day scientist's objectivity has been conditioned by his social environment.

It seems clear that the long periods during which the occupations open to the minority members have denied them a prestige education—short of living a Spartan routine—have taken their toll. It is no longer a question of working at peak efficiency just to keep up; rather, the question will be in what manner and how soon minorities may reach the starting gate. It is also clear as a result of our recent enlightenment on the plight of the minority-group member that he should not be flatly denied should he ask for more than the average assistance to help begin his career.

J. V. Martinez
St. John Fisher College
Rochester, New York

X-raying biomolecules

In his article "Mossbauer spectroscopy and biophysics" (February, page 35) Charles Johnson states that "because biological molecules have high molecular weights (between 10^4 and 10^6 g/mole), they are not amenable to structural analysis by x-ray diffraction." Max Perutz was awarded the Nobel prize for the structure of hemoglobin (molecular weight 6.6×10^4) determined by x-ray diffraction analysis. Since that pioneering work, many other molecules with molecular weights in the range 10^4–10^5 g/mole have also been successfully studied. No structures with molecular weights in the range 10^5–10^6 have yet been determined, but there are no insuperable obstacles to success here. Further, Johnson suggests that knowledge about the state of the iron in iron-proteins is sufficient to understand how the molecule works. This idea grotesquely oversimplifies the complexity of the problem. For example, cooperative behavior among the subunits of hemoglobin can only be understood with the full, three-dimensional structure of the molecule in hand.

Eaton Lattman
The Johns Hopkins University
Baltimore, Maryland

The author comments: Lattman is quite right in pointing out that the beginning of the article oversimplifies the problem of understanding biological molecules. The article was an edited version of a paper presented to the Magnetism and Magnetic Materials conference in Miami. The offending distortions were introduced in the editing process and were not caught by me when I approved the edited version forwarded

Continued on page 81

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letters

continued from page 15

to me by *physics today*. In my original version (which will appear in the *Journal of Applied Physics*) I was careful to say "it is a long and difficult process to determine their (biological molecules) structure by x-ray diffraction," which is not the same as "not amenable to structural analysis by x-ray diffraction." Also I did not write that knowledge of the state of the iron is all that is needed to understand how the molecule works—I was careful to use phrases like "one might hope to understand. . ." which are quite different in meaning. Later in the article there are several references to the fact that the structure of hemoglobin is known, which Lattman may have failed to see. It is unfortunate, however, if readers got the impression that we are unaware of Perutz's contributions. Perutz has in fact given us great encouragement in our Mössbauer work.

Incidentally Lattman is not right in supposing that there are no insuperable obstacles to determining the x-ray structures of other biological molecules, and is mistaken in believing that the difficulty is proportional to their molecular weight. For instance some of the iron-sulfur proteins mentioned have molecular weights of only 6000, and the x-ray patterns still defy interpretation. The success with determining the structure of hemoglobin (which is ten times bigger) resulted from Perutz's skill (a) in growing good single crystals of hemoglobin and (b) in devising a method to perform isomorphous replacement by heavy atoms to get the phase of the diffracted x-rays. What other molecules will yield to this treatment depends upon how easy it is to achieve these two things, and has nothing to do with their molecular weight.

C. E. Johnson
The University of Liverpool

Solid-state biology

Freeman Cope presented an optimistic viewpoint of solid-state biology in his April letter (page 13) addressed to solid-state physicists. The letter reviews semiconductor behavior in biological systems and concludes that "the field of solid-state biology is wide open to solid-state physicists, but it is extremely difficult because of experimental problems."

I submit that in addition, a class of theoretical problems exist in the field of solid-state biology that are socially relevant and belong, as a proper subset, to the field of solid-state physics. One such problem is the proposed model of the Parkinson psychosis and extrapyramidal diseases. G. C. Cotzias observed that animals without melanin in the *substantia nigra* are immune to natural and induced extrapyramidal disorders.[1] Cotzias also noted that drugs such as phenothiazine tranquilizers can both induce and relieve these disorders, as a function of concentration.[1] P. Proctor and I have noted that the common property of these drugs is their ability to transfer electrons.[2] It is proposed that brain melanin may exist with the energy levels filled close to the band edge.[2,3] An increase in the concentration of electron donor compounds can fill the band producing an insulator. Higher concentrations may fill the next band restoring conductivity. This model would explain the paradoxical behavior of these drugs. The semiconductor behavior of melanin and attempts to determine the band structure are available in the biochemical literature.[4] The final resolution to the problem will probably rest on photoconductivity and band-structure measurements.

Biological journals are beginning to contain sufficient data to choose among various models, and problems are being defined in terms that form a serious challenge to solid-state physicists of both experimental and theoretical inclinations. In particular, Proctor has been able to extend the electron-donor model to include an explanation of the loss of uricase in the primate line.[5] Sufficient data were available to justify his model and no immediate experiments were necessary.

Solid-state biology is, in fact, "wide open." It is in great need of theoretical unification, which in the examples given above at least appear to stem directly from the established body of information in solid-state physics. The existence of problems in model building should interest physicists who have been reluctant to join the experimental efforts in solid-state biology.

References

1. G. C. Cotzias, P. S. Papvasiliou, M. H. Van Voert, A. Sakamota, Fed. Proc. **23**, 713.
2. P. Proctor, J. E. McGinness, Lancet, Dec. 26, 1970.
3. J. E. McGinness, P. Proctor, in submission.
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5. P. Proctor, Nature **228**, Nov. 28, 1970.

John E. McGinness
Youngstown State University
Youngstown, Ohio

Correction

June, pages 30–31—The caption for the center photograph should read: "Picnicking on Barton's farm are Barton, George B. Pegram, John Tate and Madeline Mitchell . . ." We interchanged the first two names. □

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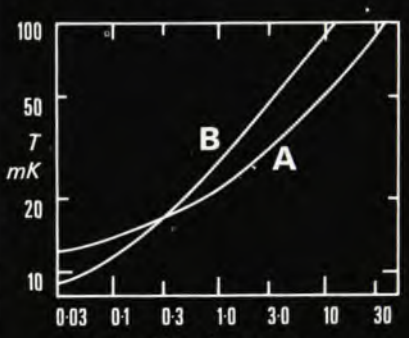

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