letters

However, even if the necessary broad technical competence could be assembled, the actual social decisions involve assessment of the social and other values associated with different choices of technologies. In the present case this would require evaluation of the public consequences and relative values attached to radiation vs. toxicants from fossil fuels.

As Devaney suggests, there is much to be done in evaluating risks from coal, and one suspects that these risks are considerable. The Panel would be interested in a proposal that focused on the physics issues involved in this question. Indeed, some members of the Society have suggested that a study of the physical role of submicron aerosols in pollution could be useful, not only in evaluating the present problem, but also in its technical and legislative amelioration. It has also been suggested that a study be made of the relationship between sources of pollutants and ambient air quality.

Suggestions or proposals for studies that might be sponsored by The American Physical Society would be welcomed by the Panel. Guidelines for APS-sponsored studies will be published in the September *Bulletin*.

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Surface theory

The atomic structures of many molecules and solids are today well understood, but we are only beginning to explore the fundamental structure of surfaces. The articles in the April issue of surface physics did an excellent job of outlining the transformation that is taking place today in this subject. By describing the many new ideas and techniques that have appeared in the last few years, the authors have acquainted the general reader with many recent developments.

A key feature in analyzing atomic structure is the close interplay between theory and experiment, an interplay which in the past has proved essential to working out the basic interactions in atoms, molecules and solids. It is therefore a pity that, in their otherwise excellent article on theory, Robert Schrieffer and Paul Soven in their concluding paragraph (April, page 28) may have left their readers with the impression that all theorists have been working only on "highly simplified models," and that it is a task for the future to do theoretical work on "more realistic systems."

While theoretical work at the University of Pennsylvania has concentrated mainly on simplified models (such as *s*-band cubium), there has been, as Schrieffer and Soven indeed note, theoretical work by J. A. Appelbaum and D. R. Hamann[1] on semiconductor surfaces that has identified new surface states (back-bonding states, as distinguished from Shockley's dangling-bond states). What they did not mention was that these new surface states have been observed experimentally on Si and Ge (III) surfaces by J. E. Rowe and H. Ibach.[2] By examining differently reconstructed (III) surfaces, Rowe and Ibach were able to show that the back-bonding surface states are a characteristic primary feature of the relaxed semiconductor surface, independent of the lateral reconstruction (which involves secondary interactions). Thus Appelbaum and Hamann's derivation for a relaxed but unreconstructed model of the surface is relevant to experiment. In my opinion, it is general distinctions of this kind, rather than extensive computer studies of highly simplified unrealistic models, that constitute the major contribution that theory will make in this field.

Let me add that valuable work on surfaces is carried out by chemists as well as by physicists, and that many of the most useful new ideas are of a chemical rather than a physical nature. (This conclusion is implicit in the excellent article on photoelectron spectroscopy by Dean Eastman and Marshall Nathan (page 44).) The theorist who wishes to make a significant contribution to the field of surface science does well to regard himself as a scientist who combines theoretical ideas from *both* physics and chemistry.

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High-field superconductor

The news article, "Superconductor operates in magnetic fields above 500 kG" (January, page 17) reports a comment by Myron Stongin in which he points out that $H_{c2} = (2)^{1/2}\, \kappa\, H_c$ (bulk) where κ is the Ginzburg-Landau parameter, H_{c2} is the upper critical field and H_c (bulk) is the bulk critical field which increases with transition temperature, T_c. He notes that "a very large κ usually indicates lower ultimate values of the flux pinning forces, which could imply a small useful current density." He thus argues that the κ for the lead molybdenum sulfides with T_c = 14.4 K should be much larger than κ for Nb_3Sn with T_c = 18 K. Strongin's argument applies equally well to V_3Ga as to the lead molybdenum sulfide materials.

The fact that V_3Ga does carry high current densities at high magnetic fields suggests that the future of the ternary molybdenum sulfides may be much brighter than Strongin's statement implies.

The comparison of $H_{c2}(T)$ for $V_{2.95}Ga$ and $Pb_{1.0}Mo_{5.1}S_6$ in the figure shows the essential features. The $V_{2.95}Ga$ data appeared in 1962 in a paper[1] entitled "Evidence for a Critical Magnetic Field in Excess of 500 Kilogauss in the Superconducting V-Ga System," and the $Pb_{1.0}Mo_{5.1}S_6$ results appeared recently.[2] For both curves the solid lines correspond to measurements in dc magnetic fields, the dashed lines are the theory for a dirty type 2 superconductor with no paramagnetic limiting, and the experimental point (the arrow) at 4.2 K involves pulsed fields. (Data for $V_{2.95}Ga$ yield H_{C2} (4.2 K) $\approx$ 180 kG, i.e., 18 T.) T_c is 14.4 K for the Pb compound versus 14.5 K for $V_{2.95}Ga$, and $(dH_{C2}/dT)_{T=T_c}$ is $\approx$60 kG/K for the Pb compounds and $\approx$50 kG/K for $V_{2.95}Ga$. We thus expect κ to be approximately the same for both materials. If V_3Ga were not strongly paramagnetically limited, the upper critical fields at low temperatures would be very similar for both materials. Because $(dH_{C2}/dT)_{T=T_c}$ and T_c are nearly the same for both materials we can expect similar useful current densities at high fields (based on Strongin's discussion). It should be noted that even though T_c for V_3Ga is less than that for Nb_3Sn ($T_c \approx$ 18 K), commercial grade V_3Ga now exists (a decade after reference 1 appeared). Above approximately 150 kG the useful current densities of V_3Ga are much higher than for Nb_3Sn,[3] and magnets above 150 kG are being made with V_3Ga inner sections.

The question then is what limits Strongin's discussion? Possible explanations are: (1) that the ultimate current densities (determined by depairing) are not achieved in practical materials; (2) that current densities well below the ultimate are more than adequate for high-field applications of superconductors, and (3) at best, Strongin's argument that compares Nb_3Sn ($T_c \approx$ 18 K) with the lead compounds leads to a reduction of only 2 to 3 in current density of the lead compound, whereas metallurgical processes (working, adding impurities, etc.) are known to change useful current-carrying properties in superconductors by orders of magnitude. The more important parameters for attaining useful current densities in practical superconductors appear to involve materials processing effects rather than the theoretical limits imposed by κ and T_c.

Unless some unusual metallurgical problems develop we would assume that the ternary molybdenum sulfides will carry useful current densities at high fields. Several groups are now attempting to make wire materials and measurements of technical properties of these new materials, and their results should be available soon. Rather than be discouraged by theoretical estimates of ultimate current densities (which may not be relevant), I favor encouraging materials development and measurement.

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THE AUTHOR COMMENTS: In my opinion there is no unusual physics per se in finding critical fields near 500 kOe or above, since many materials with high and high T_c's, or thin disordered films in parallel fields, can be expected to have such high fields in the presence of spin-orbit scattering. Of course, the main interest in these materials is whether they would be useful for high-field applications, and critical current and metallurgical stability are crucial factors that should be discussed, even in the present format. All other things being equal, higher T_c materials with high critical fields are on the average better choices for magnet materials than lower T_c materials with high critical fields. Foner is correct when he argues that at present metallurgical factors limit the critical currents of magnet materials, and some lower T_c materials with higher pinning forces can carry more current than higher T_c materials. This need not always be the case, and it is certainly one of the aims of materials research to understand and increase the critical currents of practical materials to levels approaching the fundamental limits.

I was not trying to discourage people by what can hardly be considered a theoretical estimate, but was mainly injecting a note of caution that critical field alone is not the whole story. I also favor materials development, and this constitutes a large fraction of our program at Brookhaven. Work on increasing the current carrying capacity of lead ternary sulfides has been underway

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The first Conference on Lasers and Electro-Optical Systems (CLEOS) will be held at the Town and Country Hotel in San Diego, May 25–27, 1976, under the joint sponsorship of the Quantum Electronics Council of the Institute of Electrical and Electronic Engineers (IEEE) and the Technical Council of the Optical Society of America (OSA).

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here for some time, which I hope will make Foner happy. Finally, while we shouldn't be discouraged by theoretical estimates, we insist that there is some value in thinking about them.

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Saturn absorption

It was a pleasure to be able to read Carl Sagan's discussion of "The past and future of American astronomy" (December 1974, page 23) after having heard his address to the American Astronomical Society during the special session devoted to its 75th anniversary. There is, however, a small point that bothers me.

In the section on spectroscopy, Sagan mentions "an absorption band at 6183 Å in the body of Saturn . . . now known to be at 6190 Å and is 6 ν_3 of methane." Recent planetary and laboratory spectra of CH_4 (H. Spinrad and L. M. Trafton, *Icarus* **2**, 19, 1963; T. Owen, *Science* **167**, 1675, 1970) show considerable structure, which makes it difficult to assign a band origin or center until a theoretical analysis is made. Furthermore, it appears unlikely that the attribution 6 ν_3 is definitive on the basis of what we presently know from high resolution infrared spectra of CH_4. It seems that an early unproved quantum mechanical assignment has "hardened" over the years into a "scientific fact." The cautionary remarks of G. Herzberg (*Infrared and Raman Spectra of Polyatomic Molecules*, Van Nostrand, New York, N.Y., pages 308–309) are particularly relevant here.

I hope that my comment amounts to more than just nit-picking. If the CH_4 band at 6190 Å were to be studied by multiple-photon laser techniques, it might be essential to know whether the spectral features of interest belong to 6 ν_3 or some other overtone or combination band.

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Laser corrections

The news story published in the April issue (page 18), entitled "Excitation-Transfer Nitrogen Lasers," contains in its last section one error and several omissions. These deficiencies leave the reader with the false impression that the N_2/SF_6 pin discharge 2nd Positive laser was developed in Los Alamos and subcontracted to The Aerospace Corporation.

The 2nd Positive N_2 bands at 3371 and 3576 A were first made to lase in resistively loaded pin discharges by Steven Suchard, David Sutton and Luis Galvan at Aerospace.[1] The indispensable ingredient to achieve threshold in these relatively slow rise time discharges is SF_6. Ultra-violet laser pulses from this device exceed in duration the N_2C state lifetime of 40 nsec. Independently, George Arnold and Robert Wenzel, of LASL, had observed energy enhancement of a factor of 10 and pulse lengths of 15 nsec with SF_6 addition to a fast rise-time device.[2] Similar effects have recently been reported by other groups operating fast-discharge lasers.[3,4]

Subsequent to the Aerospace discovery, kinetic and parametric studies based on preliminary findings were proposed to Los Alamos. Funding, however, was forthcoming through ERDA headquarters in Washington, D.C. Aerospace is not present "a subcontractor to Los Alamos on this project." This unfortunate phrase and the lack of proper acknowledgments leave the impression cited earlier.

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Trivial error/disastrous goof

Albert Claus (May, page 11) called for examples where trivial errors in calculations lead to disastrous goofs. Here is a true story my father enjoys telling on himself.

It all happened one Spring day at the Oak Ridge National Laboratory. And what better place to test a premise with scientific fact? Setting out for a nearby reactor site, Sam Hurst, then a brand-new physicist, was given directions on where to find the company car that was to provide his transportation. He was given a description of the car, its license plate number, and was told that the keys would be beneath the floor mat. Soon, our intrepid sojourner came upon the vehicle in question, and every bit of information was confirmed except that the final digit of the license plate number was off by one. "That's close enough for me," thought Hurst, "my wonderfully logical and scientific mind assures me that only one chance in ten thousand exists for this to be the wrong car." So away he drove, feeling certain that this black Ford was the one intended for his use.

Not long after his arrival at the destination, Sam was relentlessly accosted by the company guards. It seems that

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