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Letters

Open to misinterpretation

Your recent report on superconductivity in organic compounds ("Superconductivity: Experts disagree," SN: 4/2/83, p. 212) was interesting and, for the most part, accurate. I think that some of the remarks of Dr. Jerome that you selected to print could be misinterpreted by readers who are not familiar with the background of research on organic superconductors. As you stated, in 1980 Jerome and co-workers found the first example of superconductivity in an organic compound. The transition temperature was 0.9 kelvin, but Jerome et al. claimed that "precursor" effects could be seen as high as 30 kelvin. After I received a preprint of Jerome's work, Dr. E. M. Engler of IBM Research, San Jose, Calif., synthesized the superconducting compound and we carried out further experiments. Our results [Phy. Rev. Lett. 45:1587 (1980)] showed that "precursor" effects were probably not present in these organic compounds. Our subsequent work in collaboration with Professor P.M. Chaikin, of UCLA, further confirmed this view. The experimental data obtained by Jerome's group and my collaborators are *not* different. Jerome has interpreted the data in a highly speculative and, as yet, unsubstantiated manner. I have interpreted

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the data with the conventional BCS theory. Clearly, the burden of proof is on Jerome to convince me and other superconductivity experts that a new mechanism or theory is needed for organic superconductors. So far, he has failed to do this.

Your article quotes Jerome as saying that I missed the opportunity to interpret his original data as due to a precursor of superconductivity. In reality, I missed only the opportunity to make some unfounded speculations. I chose to make what now appears to be a more reasonable explanation of the data. Certainly, there are still some interesting and unexplained phenomena which are present in the organic superconductors and these are at the focus of the current research in this exciting field. It would have been better if your article had mentioned these instead of quoting the rather disparaging remarks of Dr. Jerome.

Richard L. Greene
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San Jose, Calif.

A subtle point

Thank you for the report on our new study, *Acid Rain in Europe and North America: National Responses to an International Problem* ("Study criticizes U.S. acid rain policies," SN:

4/9/83, p. 231). I would like to make one subtle but important point concerning your generally excellent treatment. We do not, as you suggest, conclude that the White House Office of Science and Technology Policy has impeded U.S.-Canada cooperation. Rather we take issue with the White House decision to force separate national reviews of joint U.S.-Canada scientific efforts, a precedent likely to lead to increasingly serious disputes over acid rain and other international pollution problems. This is not to say, however, that the scientific committee convened under the auspices of the White House Office of Science and Technology Policy to review the joint work has itself been culpable of obstructing progress. That committee, chaired by Dr. William Nierenberg of the Scripps Institution of Oceanography, is still in the process of deliberations.

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