of no better example than this to illustrate Abelson's plea, "We must find better mechanisms for allocating our investments in the future *if we are to have a future.*"

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Multiple Authorship: Other Interpretations

In analyzing multiple authorship, B. L. Clarke (*Science*, 21 Feb., p. 822) speculates that the difference in authorship habits between members of the Federation of American Societies for Experimental Biology and members of the American Chemical Society "may lie in the much higher qualifications for membership in the Federation. Perhaps the more mature and seasoned scientists who make up the Federation find less need for multiple research collaboration than do the chemical writers who are, on the average, less well established as independent investigators."

Since, unlike Clarke, I am not an employee of the Federation, and since I have experience with research and research workers in both areas in question, I feel free to propose an interpretation which I believe is more realistic, if less flattering to those who publish solo in the *Federation Proceedings.*

In the first instance, the comparative statements about Federation authors and American Chemical Society authors are based on a comparison of unlikes. Membership in the Federation is, to a good approximation, limited to those who are active in research and therefore presumably publish. The comparably selected membership of the American Chemical Society, particularly if one makes the criterion two or more publications, is a quite small fraction of the total membership of the ACS, which is no less "mature and seasoned" in science than the Federation membership (aside from the fact that a number belong to both groups). The apparently statistically significant difference in practice, therefore, must be based differently.

From personal experience, I believe that this different basis rests in a traditionally, shall we say, "autocratic" mode of operation in biology, with plentiful use of technicians who are regarded as essentially so many hands and therefore not to be considered in assigning scientific authorship credit; and a more "democratic" mode of operation in chemistry, with a minor use of technicians who do not contribute otherwise and greater collaboration as between equals. The one results sometimes in less authorship credit than is merited, the other perhaps sometimes in more. If one is to apply a psychological sort of interpretation, as Clarke has done, I would think it defensible at least to suggest, as an alternative, that the chemical researchers are less afraid that their share of the credit will be diminished by allowing authorship status to others.

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Clarke's study is based on the surprising and, I believe, unwarranted assumption that abstracts of papers presented at meetings and journal papers (which furnish most of the material in *Chemical Abstracts*) are comparable. Criteria for authorship in these two cases are certainly different, both in theory (the function of these two types of communication is different) and in practice (space and sponsorship considerations in the case of the abstracts, for example). The only valid comparison reported is the one-shot examination of 1963 abstracts for the American Chemical Society meeting and the Federation meeting. Looking at these by themselves, I see no significant difference in authorship distribution.

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Rebuttal

The vitriolic review by R. N. Kreidler (13 Mar., p. 1156) of my book *America's New Policymakers: The Scientists' Rise to Power* calls for a rebuttal.

Although Kreidler agrees that I have pinpointed the major problems plaguing present-day scientists in their relations to our changing society, he becomes emotionally incensed at my conclusions and states that they "[cry] for a rebuttal." Fine. I would welcome a scholarly retort, but I resent the insinuations that, as a member of what C. P. Snow has dubbed the "third culture," who is trying to bridge the gap between the scientists and the nonscientists in our world, I am not qualified to undertake such a book. According to Kreidler, only a scientist or a "safe" political scientist or historian—like Price, Dupree, or Gilpin, who have previously passed muster as being "kind" to the hard scientists—have the right to write such a tome. Kreidler berates me for being an educator, but if he cared to look a little deeper into my background, he would also have discovered that I possess two additional academic college degrees in pure history and political science. I suppose the 12 years spent working closely with scientists and engineers in universities, industry, and the government also disqualifies me from undertaking an analysis of the problems which "Big Science" faces today.

As for his fuming about my failure to use footnotes, if he had taken the trouble to look in the back of the book at the 8-page comprehensive bibliography, 14-page index, and additional appendices, he might not have made such a slurring, below-the-belt remark. I welcome objective criticism of my work. But the unscientific attack made by your reviewer may have served one purpose. It proves that scientists can be human after all and can toss logic out the window in the heat of stress just as often as the social scientists whom they accuse of using unscientific methods.

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Terminology

There is some confusion in the use of the terms "multidisciplinary" and "interdisciplinary." I would like to suggest that "interdisciplinary" be used when one is referring to the combined or team approach to problem solution. The term "multidisciplinary" could then be reserved to indicate that the thinking of a single individual is broadly based in the sense of having characteristics of more than one discipline. The connection between the two terms can be stated in the form that "multidisciplinary thinking" is an essential requirement for participation in interdisciplinary work, and that interdisciplinary work is an essential requirement for the development of multidisciplinary thinking.

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