schools. . . ." Teachers who are doing "solid work" have kept up to date through the years by reading and by attending institutes and conferences. In this country it has been particularly easy to do so, thanks to the generous financial support given by the National Science Foundation. One of us completed his formal study of biology 30 years ago, the other 3 years ago. There is no noticeable difference in the success of our pupils as measured by standardized tests or as reported by our graduates. We would go so far as to say that the BSCS materials have come upon those teachers who are doing solid work as a quite logical and expected development, prophesied by the advances in the discipline itself. Teachers who reject BSCS, or any new curriculum, on the basis of their own insecurity may be encouraged by our faith that they have nothing to lose but their trepidation.

Fourth, we read with perplexity Brierley's statement, "I am not suggesting that it is wrong to teach about DNA coding, but it should not be taught as a proven fact." We cannot find any indication in the BSCS materials that we teach DNA as a fact any more than we teach the steps in photosynthesis, or digestion, or genetic continuity as facts. Indeed, to teach any concept in science as an unalterable fact is foreign to our understanding of the methods in science, as is clearly demonstrated in the following passage from the Green Version (p. 556):

> Biochemists have found that chromosomes contain large amounts of DNA. And all the biochemical evidence indicates that each gene is a DNA molecule or a part of one. Mutations, then, probably result from a disturbance in the structure of the DNA molecules.

This passage appears along with the explanation of replication under the proper heading "A Theory of Gene Mutations."

Fifth and last, we wish to comment on Brierley's assumption that the school teachers in the project have been forced by research scientists into accepting material in their texts. The BSCS materials grew out of writing sessions held on the campus of the University of Colorado; in these sessions, research and teaching scientists, professors of education, and high school teachers prepared the texts and laboratory manuals. Of course the research and teaching scientists were looked to for technical information when it was needed. In many cases, these scientists took over whole writing assignments, but by no means all of them. There was, no doubt, considerable argument over whether or not a certain statement or diagram should be included, but to assume that the research scientist got his way every time his opinion differed from that of the rest of the writing team is unfounded. There were, and are still, several checks set up to assure that unwarranted detail or personal predilection on the part of any individual cannot be forced into the texts.

One such check was the BSCS Evaluation Center meeting. Weekly during the evaluation years 1960–61 and 1961–62, teachers met in groups across the country to discuss the teachability of the materials. Too complex ideas and wording were weeded out, and further explanation was called for where it was needed. Written reports of the success or failure of the materials in the classroom were sent by each participating teacher to the curriculum headquarters. Final revision was made on the basis of these reports. We have been happy to see several of our own recommendations appearing in the textbook and manual as printed by Rand McNally.

The periodic testing of the students' achievement served as another check against the inclusion of unsuitable material. Results of the tests and teachers' comments on the test questions were part of the reports. Addison E. Lee, chairman of the BSCS Committee on Innovations in Laboratory Instruction, through his "project associates," themselves high school teachers, and by use in the high school laboratory, has measured the practicability of each laboratory block.

As a third check, high school teachers served as writers and center leaders. One was editor of the Green Version. We knew many of the high school teachers who were in positions of responsibility; none of them could be cowed in the way Brierley implies.

We have selected these five criticisms of Brierley's report because they are, we feel, most likely to reflect a widespread misunderstanding of our endeavor. We have not commented on certain other of his statements, such as his stand against dissections by high school students, his doubts concerning the transfer from pupil experience to theory, or his opinion that the text outruns the pupils' laboratory experiences, because we think that these are personal opinions and not those of most science educators. We think examination of certain other BSCS materials would lead Brierley to revise some of his opinions.

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. . . Brierley's major criticism seems to be that the materials are generally pitched too high for the comprehension of the 15-year-old age group. He ignores the extensive testing and feedback programs which were employed to insure that the materials would be compatible with the abilities of high school sophomores. A great deal of objective evidence has been compiled in a well-designed series of tests which would not support Brierley's opinion. I would like to call his attention to *BSCS Newsletter No. 19*, which treats this matter. . . . Being a Yellow-Version teacher on the firing line daily for the past four years, I can state with confidence that these materials have been within the ability range of my students. This includes the passage on RNA from the Yellow Version quoted by the reviewer.

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Basic Question in Chemistry

This letter is in wholehearted support of your editorial "Chemistry in the universities" (17 Apr., p. 251), in which the main thesis is, "Adequate financial support for basic research in chemistry in universities should enjoy a very high priority among the federal granting agencies. Chemistry is crucial to both science and technology."

One basic question in inorganic chemistry which this nation has never attempted to answer in a manner commensurate with its importance is: to how high a temperature can we heat substances and still contain them for substantial periods of time and thus make engineering use of them? For gases the answer is simple; they can be heated to over 50,000°K and still be contained, because of the very small energy density. The answer in regard to liquids, however, is wide open. And mastery of high temperatures is essential for our whole effort in rockets and missiles for national defense or for the peaceful conquest of space. I can think

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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