Letters

Social Responsibility of Scientists

The editorial "Between two extremes" [Science **131**, 1013 (8 Apr. 1960)] finds the two extreme attitudes toward the social responsibility of American scientists to be: (i) delegation by the scientists of responsibility for social consequences of his research to the state, and (ii) refusal by the scientist to do the work which his evolving conscience deems a threat to the "country or humanity at large."

The editorial chooses the middle way, the way of free expression by the scientist of his knowledge and considered opinion, while he remains loyal and faithful to the policies of his government. In making this choice, the editorial consigns the second "extreme" to the limbo of "anarchy."

It is my view that the second "extreme" position is in order wherever mass death is planned and carried out, whether under Hitler, Nehru, Eisenhower, Khrushchev, or whomever. Linus Pauling, in his book *No More War!*, states that "testing of one great superbomb, with 10 megatons of fission, requires the sacrifice of . . . 1500 children, . . . 150,000 children or even more." In the light of this, the second "extreme" position makes sense to a great many of us in the United States, England, Russia, France, and elsewhere.

Statements made in the editorial are related to the following quotation from G. B. Kistiakowsky's article, "Science and foreign affairs," in the same issue: "We, as scientists, must do all we can to help keep the tools of our diplomacy and the tools of our force in efficient readiness." The phrase "tools of our force" needs special consideration in the age of weapons of mass annihilation. The editorial mentions the lunch-counter demonstrations in the current moral struggle for human rights in this country. The tools of force used in this struggle are successful insofar as they are not tools of violence. In other words, there appear to be two kinds of ultimate force to be used when diplomacy "fails"—namely, tools of violence and tools of nonviolent force.

I am persuaded that putting our tools of nonviolent force into readiness is more important than readying our tools of violence. The former can eliminate the necessity for the latter. Is there a government program for the development of nonviolent force, in the sense that Martin Luther King or M. K. Gandhi have used the term?

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I do not think that a scientist can shift the moral responsibility for the work he does to someone else. There was a jeweler who made a timing device to explode a bomb in an aeroplane for a man who wished to kill his wife, and necessarily also the other occupants of the plane. The jeweler was adjudged equally guilty with the man who attached the device to the bomb and concealed it in the plane. The scientist who contributes his skill to the development of nuclear or biological agents whose sole use is for the killing of thousands or millions of his fellow human beings would seem to be just as morally responsible as the jeweler, and perhaps several orders of magnitude more so. Neither a morally sensitive jeweler nor a morally sensitive scientist would accept such work.

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Elementary-School Science

The editorial of 13 May [*Science* **131**, 1405 (1960)], which reviews an article of mine on teaching elementary-school science, contains several points with which I now agree. (The article was written several years ago, although it was not published as part of a book until early 1959.) Obscure and flowery language is a fit subject for criticism. However, I do want to make a strong statement on one substantive issue raised in the editorial and to clarify my present position on a second.

Children can exhibit more than factual recall in elementary-school science and should be encouraged to do so. To begin to view science as a stimulating intellectual endeavor, a child must realize that speculation plays a crucial role in scientific discovery. One way to foster this realization is to create situations in which the youngster himself can (yes) hypothesize, where he can also suggest methods for testing hypotheses. A second-grade youngster certainly can speculate about conditions that speed evaporation. There is no doubt he can suggest rational, even rigorous, methods of putting his speculations to further test. Alerting the teacher to the fact that children can "formulate and suggest tests for hypotheses" in science, as I did in the article, may add a dimension to teaching that places it more in consonance with a modern view of science than is usually true of science instruction at any level.

However, it is a second point that I most wish to clarify. The editorial states the general argument of my chapter as follows: "Even young children should be taught 'to apply the scientific method in solving their everyday academic and personal problems.' " Whether or not this theme is indeed the general argument of the chapter is open to question. Certainly the sentence quoted, as the editorial acknowledges, came not from anything I wrote but from an introductory remark by the editor some 357 pages removed.

More significant is the fact, which I chiefly wish to stress in this letter, that my efforts for the past 2 years have been directed primarily toward demonstrating the merits of a viewpoint directly opposite to that ascribed to me in the editorial. An elementary-school science program wherein content is selected primarily on the basis of personal and social utility is weak on several grounds. In present programs reflecting this bias, elementary-school children often study details of municipal sewage systems because such knowledge helps them better to understand how man makes use of science. For the same reason, they study design of space suits and construction of automobile engines. Criteria for content selection that emphasize the theme of utility lead to patchwork curricula. At their best, such programs lack foundation in basic scientific principles; at their worst, they stress solely the rapidly changing gadgetry confused in the public mind with science.

Some of us at the University of Illinois have been pursuing a program for 2 years in which the choice of content for elementary-school children is made by the professional scientist entirely on the basis of the significance of the content within the discipline. We have been working in astronomy so far. My role in this effort has been to try the topics so selected with youngsters in regular elementary-school classrooms. For example, we have been working with some classes to see whether children can acquire a concept of a geocentric solar system based on evidence that is relatively simple to obtain. As far as I know, the social or personal utility of this idea is nil. Yet the problem thus posed does give children an insight into a significant astronomical problem, albeit an old one. Children begin to understand by such study what astronomers do, what types of evidence they must collect. Similarly, we have been teaching children how distances are measured in space (instead of just telling them the distances that are thus measured, as is usually the case at present).

There are many topics we plan to try. The work so far has been episodic. But on the basic of limited trial we can say that content selected solely because of its crucial role in a scientific discipline is intellectually exciting for children, and the comprehension of such content by children in the normal intelligence range is truly surprising, particularly in the light of our current expectations of children.

I have written at greater length elsewhere on this theme [see my article in *The Science Teacher* (March 1960)], and I expect to spend the next several years assessing the feasibility of elementary-school science programs based on content selection by professional scientists.

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Weather Forecasting

In his recent article "The atmosphere in motion" [*Science* **131**, 1287 (1960)], Robert R. Long has presented an interesting summary of his well-known work on the channel flow of stratified fluids, and the comparisons between theory and experiment which he presents are impressive. I think most dynamic meteorologists would certainly agree with him in stressing the need for a great deal more basic hydrodynamical research in order to strengthen the foundations of dynamical weather prediction. I also feel that he would find few who would quarrel with the statement that forecasting accuracy has improved little in the past 40 years or so, although more variables are now predicted over greater regions of the atmosphere. Long's introductory remarks on the role of numerical weather forecasting in the past decade, however, may be misleading to the general scientific reader and deserve some comment.

The numerical (or dynamic) forecasts now used subjectively by the forecasting meteorologist differ from his other sources of information in at least two important and fundamental respects. In the first place, the numerical forecasts represent the result of a systematic application of dynamical equations to the problem of large-scale atmospheric flow and are in this sense objective and reproducible. Secondly, the numerical forecasts may be (and have been) systematically improved by the introduction of more realistic models and previously neglected physical effects, as well as by improvement of the numerical procedures employed in the solutions. From a practical viewpoint the test of a forecast is, of course, its accuracy, and in this respect the present numerical predictions are disappointing in some ways. The low-level forecasts issued, for example, by the Joint Numerical Weather Prediction Unit in Suitland, Md., are not superior to those produced by the usual synoptic means; the higher-level (500 millibar) numerical forecasts, on the other hand, are now more accurate for periods up to 3 days than other comparable forecasts. This recent improvement has resulted from the systematic error reduction noted above. In view of the many physical and mathematical approximations incorporated in present operational models, I feel that their performance is more surprising than disappointing; relatively simple dynamical methods are here effectively competing with all of the synoptic calculations and intuitive skill of the forecaster.

From a broader viewpoint, the numerical integrations represent an attempt to verify the same set of basic dynamical equations with which Long is concerned, although for larger-scale phenomena, in which different physical effects are important. While the comparison of theory and observation is here poorer than in the more restricted experiments of Long, I feel there is good reason to entertain more optimism than he suggests is in order. The small but systematic improvement in the prediction of the large-scale flow is here, I believe, a significant improvement. As this scale of motion is progressively better understood, the results of research on small-scale phenomena—of which Long's studies of tornado-like circulations is an excellent example—may then be incorporated into the overall dynamical picture and should result in further systematic forecast improvement, especially for the smaller-scale motions which are closely associated with our subjective impressions of "weather."

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Binocular Fusion of Colors

In an article entitled "Colors of all hues from binocular mixing of two colors" that appeared in *Science* [**131**, 608 (1960)], the following statement was made by Geschwind and Segal. "The problem of binocular fusion of colors has interested investigators since Hecht's demonstration in 1928 that presenting red to one eye and green to the other led to a subjective sensation of yellow. . . . Hurvich and Jameson . . . confirmed these results; it is today generally accepted that such fusion is readily obtainable in most subjects."

A major finding of the article by Hurvich and Jameson cited by Geschwind and Segal was the following: "The fact that does clearly emerge from these results is that, unless there is a yellow