# Letters

## Atoms for Peace

I would like to comment on Greenberg's report headed "Atoms for Peace: Concern growing that program is spreading means for more nations to build weapons" (19 Feb., p. 843). I firmly believe that now is the time, not to "drag our heels a bit" as regards the Atoms for Peace Program, but to commit ourselves more deeply to it.

From firsthand experience (I was formerly technical adviser in health physics from the International Atomic Energy Agency to the governments of Turkey and Nationalist China), I can say that the program has revitalized many areas of the societies in which it has been allowed to function: medicine, the physical sciences, the life sciences—especially those dealing with agriculture-engineering, and even the law and administration. By enabling training and research centers to be started in developing countries, it has provided employment for science graduates who would otherwise have no professional future in their own countries. The Atoms for Peace program has removed some of the stigma resulting from our original introduction of atomic energy. It is a means of demonstrating our superiority in applying hardware and techniques to peaceful purposes and of centering the attention and efforts of these countries on the constructive rather than the destructive aspects of atomic energy.

To back down now would be to provide fuel for those in other countries who say that we have no long-term objectives but begin and discard programs according to the whim of the moment. What we should do now is assess what has been accomplished —where we have failed and where we have succeeded. Then we should plan another 10-year program, and this new Atoms for Peace should be not on a curtailed but an expanded scale. Now is the time when some of the test-tube work can move out of the laboratories and into the fields where a greater portion of the people will benefit from them.

That much of this effort should be channeled through the International Atomic Energy Agency is obvious. Nationalism would certainly raise its head if one nation attempted to impose its rules on another; but if consensus is obtained from a group of nations, then even control will become more palatable.

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## The College Boards in Biology

The controversy between the Biological Sciences Curriculum Study and the College Entrance Examination Board over the adequacy of tests (Grobman, Letters, 13 Nov., p. 866) exposes some knotty problems related to test-making generally. Every institution that seriously attempts to devise objective tests is continually striving for perfection and admittedly never achieves it. The well-known critics of objective testing have demonstrated that one can find enough of the bad in any test to hang it, especially if one is looking for the bad; and no test provides adequate opportunity to anyone to demonstrate all that he has learned in a course.

The CEEB Biology Test Committee (of which I am currently chairman) makes every effort to prepare good tests and has available the resources of the Educational Testing Service to help it do so. Each item that eventually appears on a Board test has a lengthy history of survival through a process of item preparation, preliminary selection, pretesting, final selection, and assembling. Items making the grade have statistical histories demonstrating that they have good chances of making a contribution to the objectives of the College Board examinations, objectives which relate to the ranking of students as one criterion for college admissions.

The committee attempts to keep abreast of the trends and sees to it that these are reflected in its tests. The tests are not designed to be final exams for BSCS or any other given curriculum in biology, but are intended "for all candidates who have studied the subject regardless of curricular variations across schools and teachers" (H. Dyer, *College Board Review*, Fall 1964). The fact that students who have had BSCS biology do as well on the College Boards as students who have had conventional courses seems to support the notion that the exams are fulfilling this intention rather well.

When the committee examined the early experimental BSCS materials, some of its members thought that a special test might be needed for BSCS students, and the committee was willing to prepare such tests. But analyses of College Board scores revealed that the first BSCS students taking CEEB examinations did as well statistically on those tests as students from conventional courses. Analyses of scores from subsequent tests indicated the same. During the early period of the BSCS, panels of judges reported that the tests seemed to convey the basic BSCS spirit and philosophy and that about 85 percent of the items were based on content in the BSCS courses. All this, coupled with the expectation of a rapid convergence of curriculums, encouraged the committee to conclude that for the purposes of college entrance separate tests might not be needed. The committee's conclusion was given early support by BSCS spokesmen.

The day of convergence seems to be here. The 1965 editions of the so-called conventional texts have incorporated much of the new. Even the laboratory activities seem to have taken on a BSCS flavor. It is estimated that next year 50 percent of the nation's biology students will be using one of the BSCS versions. Thus these versions are now among the "conventional," and they are among the references on which the committee relies heavily for test-item content.

I offer two reasons why BSCS students do as well on the College Boards as other biology students. (i) Where the three BSCS versions claim 70 percent of content in common, the conventional might be considered a fourth version, with the four versions having 70 percent in common. (ii)

Above-average students appear for the College Boards; we cannot be sure where such students acquire all their knowledge of a subject, especially as there is a lapse for many of them of one or more years between completing a course and taking the Board exam.

Grobman showed some concern over the influence on curriculum of widely used tests such as the College Boards. The committee does not try to dictate through its tests what should be taught, but is aware of the influence of examinations on curriculum. For this reason the committee attempts to encourage change by keeping the tests abreast of the trends. The extent to which College Board examinations influence curriculum is moot and undeterminable. Less than 3 percent of the nation's 2 million biology students take the tests. The tests are highly secure. New forms are prepared for each sitting. Teachers do not have access to them, even the outdated forms. Examinees may report a few details back to their teachers, but as nearly as the committee can ascertain, the feedback is a report not so much of the familiar as of the new and unfamiliar. An examinee, for example, who encounters DNA or light-and-dark reactions for the first time on a CEEB examination would be prone to report the fact to his teacher. It would seem reasonable, therefore, that student feedback would tend to encourage rather than obstruct changes in curriculum.

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## Tax Deductions

In my earlier letter ("Beware the tax collector," 26 Feb., p. 986) I did not intend to imply that professional expenditures were being routinely disallowed by the Internal Revenue Service as tax deductions. I have had such deductions allowed on several occasions, as have many others. That there is a problem is evidenced by cases of disallowance, such as the one I described, in other parts of the country, to which my attention has been called since my letter was published.

My intention was to point out that each tax return is subject to interpretation by the local IRS agent. One should be prepared for the occasion if one happens to deal with an overzealous agent who regards professional affairs as a hobby. Obtaining a just settlement in these cases can be time-consuming and expensive.

The process will probably continue on a local basis until a national ruling has been made which can be cited to IRS agents if necessary. I suggest that the most effective remedy is to be had through our professional societies rather than on an individual basis.

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## The Primate Centers and Taxonomy

Hershkovitz's complaint about the neglect of taxonomy and zoogeography in primate research (5 Mar., p. 1156) is justified, but it is directed against the protagonists of his own cause. He says of the Primate Research Center program: "It seems incredible that the government would spend millions of dollars for the establishment and operation of seven huge primate centers and millions more for their respective research programs but not one cent for . . . authoritative determination of the kinds of animals used in research." This statement is based solely on a preliminary planning report, the *Fact Sheet* to which he refers, written when only one of the seven centers was operational and the others were still in the blueprint stage. Also, at that time as now, the program could be planned only to support interested scientists; no government funds can create the unavailable workers in morphological taxonomy and zoogeography of primate animals. The fact is that the scarcity of authoritative texts is due to the rarity of competent scientists rather than to lack of support in this field of research.

It is expected that the publication of Hershkovitz's own work will fill many of the existing gaps. However, it is the support by the Primate Centers program which brings about a rapid accumulation of new data in those fields related to primate taxonomy where interested workers are available. Morphological taxonomy is represented by the monumental work by W. C. Osman Hill, *Primates: Comparative Anatomy and Taxonomy* (Wiley, New York), now in its sixth volume, begun in 1953 in England and since 1962 carried out in this Primate Center. Most of the other workers, however, are interested primarily in differentiation on the molecular level of blood and tissue components, in differential parasite susceptibility, and in neuroanatomy as delineated by MacLean's answer (5 Mar., p. 1157) to Hershkovitz's letter. In some of these areas the available information has been more than doubled in the last 2 years because of support by the Primate Centers, even though most of them are not fully operational.

Certainly much work still remains to be done, and I should like to join MacLean in his statement that "It is to be hoped that Hershkovitz's letter will stimulate further interest in the important problem of the taxonomy and zoogeography pertaining to animals used in research." This appeal, however, is a plea to research workers and teachers rather than to funding agencies.

Finally, I am certain that it should not be assumed a priori that the planned program of Japanese-American studies of primate biology, reported in *Science* by L. Carmichael and A. J. Riopelle (20 Nov. 1964, p. 1078), will be carried out with taxonomic parameters excluded, as Hershkovitz presumes. Already much of the work on molecular differentiation and on differential parasite susceptibility, as mentioned above, is being carried out by Riopelle and coworkers, at the Delta Regional Primate Research Center of Tulane University.

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## Simplicity and Clarity

. . . The writers of physical papers now use such an abbreviated jargon that, though a Master of Science in Physics from M.I.T., it is foolish for me to try to read them. When I came to the Smithsonian in 1895, Secretary S. P. Langley asked me to translate and abridge Röntgen's x-ray work for an article in the *Smithsonian Report.* He impressed on me that he desired articles clearly and simply written so "the country schoolmaster would understand and appreciate them."

I've always tried to follow that good advice.

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