Letters

Private Spending for Science

I hold a view that differs from that of P. W. Hutson [*Science* **129**, 1369 (1959)]. In my opinion private expenditures for scientific or educational institutions for public welfare have, on the whole, done more good than comparable expenditures by the Federal Government. In short, I believe a man expending money that he himself has earned will usually do a better job than a politician expending someone else's money; there is more objective consideration of real human welfare, less influence of human selfishness.

That "disaster could overtake us" is more likely to follow from unwise government expenditure than from unwise private expenditure. In Russia at present the "primacy of the larger society" is nearly absolute. I think it should be lessened if not "undermined."

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Teaching and Research

A point of fundamental importance was raised by Edmund M. Spieker [*Science* **129**, 1324 (1959)] in his answer to Fourman's earlier letter on the undervaluation of teaching ability in comparison with research performance, as measured by publications, in the evaluation of college professors. The point he makes, and seems to accept as an established fact, is that teaching ability cannot exist without the stimulus of research. Furthermore, he specifically applies this thesis to undergraduate college teaching.

It is not my intention to argue this point, but I do wonder whether it has ever been proved. Whether or not one can recall excellent but "unproductive" professors from his college days is probably not pertinent, but it is strange that scientists will make a statement like Spieker's without presenting a scrap of supporting evidence. He says that in all the literature that he has read "on this troublesome subject" there is little or no mention of this matter. As a matter of fact, I have heard the "troublesome subject" resolved in Spieker's way many times; perhaps it is all right to say such things without documenting them.

Many years ago a project (participated in by the AAAS) seemed to demonstrate that the undergraduate background most likely to lead to the doctorate in science is to be had in small colleges. Is it possible that the professors in such institutions are more active in research than those in large universities, and is the impression some of us hold that the professors in small schools are not very active "publishers" wholly incorrect?

Until a relationship has actually been demonstrated it is not safe to assume on philosophical grounds alone that good teaching necessarily depends on whether the teacher is actively engaged in research. This is an important point to resolve because many potentially excellent teachers *may* be doing less than their best teaching in an effort to satisfy the university administration by doing research—research that may very well needlessly add to the volume of scientific publications. In some fields, at least, we could profit by fewer but more significant publications.

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"Personal Liberties" Threatened?

In the News of Science section [*Science* **129**, 625 (1959)], headed "Loyalty Provisions of National Defense Education Act Meet Opposition from Educators and Congressmen," were published quotations from a letter signed by the president and general secretary of the American Association of University Professors. The part that especially irked me was, " 'the Act seems to say to members of the educational community: ". . . you are a particularly suspect part of the population and will have to pass a special test that other citizens need not take." ' "

I do not see why *they* should feel picked on. After all, there are a few million citizens in military and civil service who take oaths of allegiance and every few months are asked to check the attorney general's list of subversive organizations to make sure they have not inadvertently fallen into the clever trap of the communists, who organize or infiltrate organizations with the most innocent and patriotic sounding names. One may, of course, think that those in the U.S. military or civil service take loyalty oaths because they receive money from the government. The National Defense Education Act will also give government money to those successful applicants who are asked to take the oath and sign the disclaimer affidavit. Not that I believe for one moment that the requirement of taking an oath is going to deter a real communist from doing *anything*, but I do believe signing the disclaimer affidavit may alert an unsuspecting youth to communist psychological warfare tactics. It may even prevent him from innocently becoming so involved in suspect organi-

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zations, like some college professors, that he is afraid to declare his loyalty.

The usual argument of those who so emotionally oppose the oaths is that their personal liberties are being threatened. Just what do they think is going to happen to their precious "personal liberties" if the communists continue to win the cold war? Name calling, such as speaking of "the stale aroma of McCarthyism," is not going to help our youth face the facts. When they fulfill their military obligations they will be taking the oaths and signing the affidavits. When they do the scientific work for the government for which they are to be trained at government expense—the purpose of this act—they will be taking the oaths and signing the affidavits! If they are unwilling to do so, it is best to find out now and not waste the taxpayers' money on them.

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Science and Public Education

There appeared in the 21 November 1958 issue of *Science* [**128**, 1290 (1958)] an advance notice, as it were, of a paper on Ice Age History, which was to be presented at the December meeting of the AAAS, by Richard J. Lougee, of Clark University, Worcester, Mass. The paper was obviously fantastic in its purported new concepts of ice age history and should not have found its way into the columns of *Science* without being referred to a geological authority. The claim that was made in the first paragraph, "that abundant new data have been collected at the department of geography of Clark University during the present International Geophysical Year" should have been enough to "flag" attention.

It did attract the attention of public-relations people, for press bureaus were waiting for the presentation of the paper to broadcast what they seem to have regarded as new vibrant scientific thoughts. Early the following week I arrived on the Pacific Coast and, in due time, friends handed me a clipping of a news article which had been taken from the Los Angeles *Times* of 27 December 1958, bearing a Washington date line of 26 December and prominently headed, "New Theory of Ice Age Presented." It contained the following lead: "A revolutionary new concept of the Ice Age today depicted one great glacier that temporarily buckled the upper United States to great depths."

Papers all over the country carried write-ups. The Washington *Post* of 27 December listed the paper on its front page as the number one feature of "yesterday's events in the sessions of the American Association for the Advancement of Science." The paper also carried on its first page an account written by a research professor of physics and a consultant to the National Science Foundation. In that, the physicist reported, "The theory demands a change in glaciological thinking as radical as that caused by Copernicus' theory that the earth is not the center of the universe."

Surely the prodigious labors of Chamberlin, Geikie, Leverett, and other pioneers of glacial geology and of the scores of workers today who have helped to build a formidable literature of monographs, professional papers, bulletins, and journal articles, confirming over and over again the verity of the doctrine of multiple glaciation, cannot be set aside by a writer's flight of imagination or by venturesome commentators.

It is evident that there is dire need for public education in science, but to be worth while, science must make its guidance effective. Specialization in geology has developed to such a degree that the untenability of the views entertained by Lougee would not necessarily be sensed by all geologists, let alone other scientists, and this emphasizes the need for correct handling.

It is my feeling that a disservice has been rendered to education and that the time has come for careful consideration of these matters.

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Leighton rightly says "the time has come for careful consideration of these matters." They attracted my attention in 1927 when, with Stuart Weller and L. C. Conant, I first observed iceberg-dropped Canadian granite erratics at Ozora, Mo.; last summer state geologist Sigurd Hansen of Denmark and I traced them northward into the Dakotas to elevations a thousand feet higher, at Pierre and Bismarck.

In 1954, Hansen and I mutually confided our belief that there is some basic misinterpretation of Ice Age history which is preventing normal correlations across the Atlantic, or even over short distances. Antevs estimates, from his studies of varved clay, that twice as much time has elapsed since the Ice Age as others have claimed on the basis of carbon-14 dates on supposed tills in the Middle West. To a few American geomorphologists, the glacial history of the Middle West and its "formidable literature" have long been suspect.

The trouble started in the days of Chamberlin, Geikie, and Leverett, in failure to recognize that certain widespread stony clays—which looked like bedded tills and sometimes contained perfectly preserved shells and black layers of organic matter—were deposits dropped by icebergs floating in open water, in the manner claimed by Sir Charles Lyell. Marine shelly drift of this sort reaches high elevations in British Columbia, as described and illustrated by J. E. Armstrong, and I have observed it blanketing portions of coastal Denmark, Sweden, Norway, and the British Isles, far above younger Yoldia shorelines.

On 29 April 1959, at the Naturalists' Forum of the Philadelphia Academy of Natural Sciences, I proposed the name "lyell" for these water-laid deposits, to distinguish them from till, which is spread by land ice. My type example of lyell is the Kansan-Illinoian complex, overspreading a true till, "Nebraskan," known to the north as "Wisconsin." It is apparent that many so-called tills on this continent—such as "Jersian," "Iowan," "Valders," "Toronto," "Cochrane", and "Vashon"—are lyells.

Twenty-five years ago I was instrumental in preventing a mass movement in geological thought toward a concept that claimed no unwarping in New England and retreat of thin stagnant ice from *north* to *south*! Using the clay-measuring and surveying techniques of DeGeer and J. W. Goldthwait, I found that the elevations of delta deposits, rising northward, show a history of immense crustal depression followed by intermittent upwarping as thick live ice melted away from *south* to *north*.

In the course of many years' association and field work with Leverett, Taylor, Upham, Hubbard, and D. H. Chapman, I found in the East that the earliest and greatest upwarping movement, "Hubbard uplift," commenced after 2000 years' crustal stability of Lake Hackensack and was followed in turn by 4000 years' stability of Lake Hitchcock, the latter coinciding with Maumee-Whittlesey stability in Great Lakes history. The discovery localities for Hubbard uplift are in Connecticut, but this uplift has now been impressively demonstrated in the Middle West, where the widespread submergence that preceded it has been mistaken for "multiple glaciations."

It is not my intention to combat all claims of multiple glaciations regardless of the region under consideration, but I hope to show that such claims are completely groundless in the classic localities in the Mississippi basin.

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