Letters

AAAS Meeting and the Press

I have covered AAAS meetings for many years—the first time in Cleveland, in 1930, when Howard Blakeslee and I shared in the coverage for the Associated Press. At that time most of the material came from such leading lights as Thomas Hunt Morgan, Robert Millikan, Harlow Shapley, Aleš Hrdlička, "Ajax" Carlson, George Washington Crile, Austin Clark. Most of the leading scientists at that meeting had a fair knowledge of everything of importance in their own fields that was to be reported. Consequently, good stories were not too hard to develop.

Today, millions of scientific reports later, the situation is bewildering. The programs, releases, press conferences, and individual interviews arranged by the AAAS press-room staff in Cleveland were highly satisfactory. But while I was attending a press conference a half dozen programs were going on that seemed promising—where there might have been not only good papers but also free discussions that might inspire stories or add much-needed information to a science writer's background. During this year's meeting I spent the equivalent of two full days attending programmed sessions. This was in addition to time spent attending press conferences, reading releases and speeches, and interviewing individuals. At about 10 o'clock every night I gave up and started writing my story, using whatever material I had. When, on returning home, I reread everything I had written (which, no doubt, was far too much), I found no mention in my stories of any very startling advancement of science. It was mostly commentary on the current state of science in general or on the relation of science to the public. I wondered how many shining nuggets of achievement I had missed. I may have missed many. This is a disturbing thought. More disturbing, however, is the thought that I may not have missed any.

At one of the sessions I attended, a scientist deplored the overabundance of isolated and fragmentary facts that swell the volume of current scientific literature—facts that are presented with no attempt at correlation. He deplored the eagerness of scientists to have their names in print. He wondered how much the volume would be reduced if all scientific reports had to be published anonymously.

At another session, attended by scientists in one specialty, the chairman said that what was being reported was highly significant and should be brought to the attention of scientists in other specialties and to the public as well. Year after year, he said, the men in this specialty merely "talk to themselves."

Putting these two observations together suggests the following question: Would it not be possible, say every other year, to have fewer individual sessions, with fewer fragmentary reports—sessions that are interdisciplinary so there can be more correlation of information?

Or might it be possible to have a conference some time before each AAAS meeting, similar to the Gordon Research Conferences, with no publicity of any kind, and let the conferees decide what is to be reported at the meeting? Of course this would be expensive. But it should not be overlooked that newspapers and other publications spend fairly large amounts of money in sending their writers to AAAS sessions and printing their material. An estimate of $1000 for each writer is probably not excessive, not to mention the extra time the writer contributes.

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Author(s)! Author(s)!

What is the meaning of multiple authorship in the "by-line" of a scientific paper?

Authorship implies not only participation in the investigations leading to a report, but also active participation in the preparation of the report. It implies knowledge of and agreement with the substance of the report by all those whose names appear as authors.

Coordinate research by two or more workers on different facets of a given problem, followed by conferences at which the report is assembled, is the most obvious implication of multiple authorship. In such a collaboration the contribution of each participant should be identified, but frequently this is not done. Departures from this ideal range to the ludicrous. Among the names that I have seen associaetd in "by-lines" are those of senior professors whose knowledge of a student's work is far from detailed, contributors of analyses or other routine information, and laboratory technicians. Four, five, or more names in the author list have become a commonplace phenomenon.

I submit that coauthorship should be confined to near-equal collaborations. The part of contributors of routine or even critical information, technical or editorial guidance, or laboratory assistance should be properly and graciously acknowledged in the body of the report, or in a suitable footnote.

This situation is part and parcel of the "titles race." Although this race may never be controlled by an International Authorship Control Commission (IACC), it can be kept within bounds by diplomatic multilateral agreements among scientists.

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In Defense of Scientist-Rotarians

Michael Amrine [*Science* **142**, 913 (1963)] pleads for freedom for scientists to live their own lives. But he finds it "ridiculous to imagine [a scientist such as Einstein] at a Rotary meeting wearing a button, 'Call me Al'." Thus he is himself by implication trying to restrict the behaviour of scientists. Scientists should be free to join Rotary or not, just as they please, without being ridiculed.

Einstein may not have been a Rotarian, but another famous "Al," Dr. Schweitzer, is or was a Rotarian. Of course, Schweitzer is a philosopher rather than a scientist, but he is certainly one of the "different ones." Among the "different ones" who call me "Bob" in our Rotary Club are a film producer who founded and organized one of the best educational institutions in this area, and the most widely read person I know; a scientist in the field of design engineering who is now on a technical visit to Russia; an income-tax officer

who is also a poet; a fellow psychiatrist; an American sociologist; and two geologists. A well-known anthropologist was a member until his recent death.

As for myself, I have solid claims to being "different." Many scientists and quasi-scientists are trying to emigrate to England and thence to the United States. But I know of only one other person, and that is J. B. S. Haldane, who has done the reverse and emigrated from England to India. Because I am a psychiatrist my claims to scientific status rest on a less solid foundation. I read *Science* and many other technical and scientific journals. I do a bit of clinical research; I think a lot about the nature of what is called schizophrenia; I sometimes forget to have my hair cut; I occasionally become so engrossed in something that I forget to attend a Rotary meeting even though I have to make a speech; I sometimes dream of attending a Rotary meeting minus pants. But my fellow Rotarians understand these things and make allowances. They sometimes call me a goose, but I have not yet laid my golden eggs. I am sure this lack has nothing to do with being a Rotarian.

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Overdone Overhead

In a letter in your issue of 3 January (*Science* **143**, 7), Scheff asks for objective evidence of an earlier assertion in "News and Comment" that "in terms of morality, competence, and devotion to the public interest [scientists] are no better" than other professional groups. Perhaps his request is answered by Pake's letter in the same issue. One conclusion derivable from Pake's rather startling statements on the problems of administrative overhead for research grants is that scientists and scientific educators are certainly as prone as anybody else to look out for their self-interest. Pake admits that as a scientist he juggled accounts in efforts to circumvent what he evidently considered to be administrative highjacking of his grants; as an administrator he now blandly tries to justify even greater baksheesh.

His statement that "in the absence of overhead allowances . . . a private university would be forced to cut its research activities in science by a large factor, perhaps ten" is either sheer poppycock or evidence that Parkinson's Law has been fulfilled with a vengeance. Does this mean that the university's own contribution to scientific research is being supported to the extent of 90 percent by the "droppings" from research grants? If so, some fundamental questions may be raised about the extent to which universities are preparing to divest themselves of their traditional functions and responsibilities. If the universities cannot support "adequate" research programs on their own, or provide the milieu in housing, facilities, and administration for research programs whose direct costs are subsidized by public money, or if they would have to reduce the output of Ph.D.'s in science by a factor of ten in the absence of these large flat charges, then just what is the function of the university in mid-20th-century science? Could not both research and graduate-training functions be more efficiently taken over by other public- and government-supported research institutions, in the manner of the Rockefeller Institute's graduate program? And might not this provide a remedy for the deleterious effects upon scientific teaching in the universities that are ascribed to "professional research"? Research grants already provide the men that the universities now think they are unable to hire without subsidy; the many fringe benefits of course include enhancement of the institution's prestige and its ability to attract and keep scholars in other fields. But are we now to give up as fiction the assumption that research grants are made primarily for the support of the research for which they are solicited, and to recognize them as an accepted subterfuge for the public support of higher education?

These questions are asked in the full recognition of the financial difficulties facing all institutions of higher learning and with a fairly well-educated estimate of the burdens imposed by top-heavy research programs. But perhaps these burdens are unnecessarily self-imposed. Pake does not address himself to the essential question—which Krombein (*ibid.*, p. 8) states succinctly: Does adequate administration actually cost so much? Krombein's example of the $14,000 administrative cost of a 2-year program is dwarfed by Pake's example, in which as a researcher he "lost" this amount to administration each year over a 14-year period. . . . Surely the funds which can be wisely used on the expenses of research itself by one investigator (even as the principal of a team which uses up, let us say, $75,000 a year) do not require the equivalent of one full-time administrative officer and a stenographer—a staff which in a more rational day could be expected to handle the job for an entire school. A university that needs a quarter of a million dollars to administer a million had better let its financial operations out on contract to some private money-management firm. . . .

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An Aye for "I"

Here's an endorsement of F. Bruce Sanford's proposal [*Science* **142**, 1526 (1963)] augmenting Clemence's suggestion that "I" is not a dirty word.

For 13 years as a technical editor I wrote articles which frequently contained personal impressions. I followed the stupid convention, using "this writer," "this reporter," and every other trite device used by the journalists of this country. One day, some years back, I swore off and used personal pronouns. A daring desk man let the copy go by, thousands of readers read it, and our world stayed in one piece.

There is no valid reason for professional jargon in any of the sciences. If we are ever to reach the broad masses of people, it's going to have to be done with 42nd-Street-English, not with the stilted phrases that too often pass for scientific language.

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It seems strange that the use of "I" should cause guilt complexes and anguish in American scientific writers. Is this possibly due to surreptitious influences derived from Germany, where the royal *Wir* and *der Verfasser* still loom large? No such problem seems to exist in the country where the English language originated, and whose inhabitants have a reputation for understatement and self-effacement, but whose authors say "I" if they mean "I"—at least in the earth-science literature that I (that is, we, the author, the present writer) read(s).

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