

Full-Time Researchers in Universities

Far from creating "parasites" in universities as H. W. Davenport alleges (Letters, 21 May, p. 1040), any arrangement [such as the recently terminated NIH Career Awards] which exempts a faculty member from teaching and administrative duties for a time obviously enables him to pursue his chosen research activities with a vigor and to a depth not possible otherwise. It would appear to be a strange and limited field of science indeed, and a limited university, which could not profit from such pursuits. . . .

Man's social evolution is closely related to his development of specialized skills and the ability of his community to organize in such a manner as to take advantage of these. The *raison d'être* of any organization is to nourish all activities in its domain by directing to specific functions the individuals best able to perform them. An administrator who is unable to perceive any better division of responsibilities than a sharing of all activities by all in the community is no administrator at all but a primitive computing device.

Let us not deny the increased difficulties in communication and administration which arise as the members of a community extend their skills over a broader spectrum. Let us also recognize this problem for what it is, however, and proceed to solve it by developing the necessary communicative and administrative skills and organizations rather than by decreasing the breadth of the spectrum (to make it more manageable) by arbitrary elimination of its brightest components.

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. . . Davenport's credentials are impressive indeed, and his scientific output is remarkable in view of his teaching and administrative load. But most departmental chairmen with past histories of productivity in the laboratory do not find it possible to continue major scientific work—a fact which, as Curt Stern indicates in "Thoughts on research" (*Science*, 7 May, p. 772), is painfully obvious to most of us. . . .

If it is agreed that excellence in research is necessary to a university department, the question is, How is it best nurtured? If an excellent scientist, with a valuable research contribution to make, came bearing gifts of frankincense and myrrh from NIH, would Davenport refuse him admission to the

academic community unless as a precondition of employment he accepted unwanted teaching and administrative assignments? Or take as an example a renal-physiologist undertaking basic research in a clinical setting, such as a department of medicine. Would it be appropriate to insist that he reduce his productive hours in the laboratory in order to assume some of the burden of the outpatient clinic? The imposition of teaching, service, and administrative duties on a reluctant scientist seems as unwarranted as it is wasteful of talent.

Federal support of research has come about because of an unfilled need. The primary purpose of NIH grants has been the support not of teaching, service, or administration but of research. When an agency is willing to provide substantial—sometimes complete—financial support for full-time research scientists, it seems unfortunate that the program should be in jeopardy because the idea of full-time research is inconsistent with the traditional concept of academic life. It is unfortunate indeed that a scientist who regards research as a way of life should be considered a "parasite." If this is the prevailing attitude among departmental chairmen, then the scientist should indeed, as Davenport suggests, seek his home in the research institute.

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. . . As an undergraduate in a technological institute that had a large and famous research staff, I had the misfortune of having some eminent researchers as lecturers. They were not good teachers; they treated their teaching duties as a necessary evil and had little personal communication with students. Davenport's policy implies that anyone can teach. I do not think that is true. The qualities that make a good researcher are not necessarily those of a good teacher, although there may be a few individuals with both kinds of gifts. Teaching really *is* a noble profession. It should be considered an opportunity and a challenge; people should not be shanghaied into it. . . .

I have two suggestions regarding the teaching-research dilemma:

1) The financial rewards and the prestige of the teaching profession should be increased. A representative group, such as the American Association of University Professors, should

prepare a program (acceptable to itself with regard to government non-intervention in education) and submit it to HEW. The plan should include increased stipends for teachers, and travel and training grants for increasing their skill; there might be "career" teaching professorships.

2) Additional independent research institutes should be set up. Apparently the Europeans are far ahead of us in this respect [R. P. Grant, C. P. Hutter, C. G. Metzner, *Science* **146**, 493 (1964)]. Such institutes would provide a favorable climate for full-time research and, as Davenport suggests, would be a good place for those not interested in teaching except through research training.

I am certainly not advocating the abandonment of research in universities; these steps would, however, increase the attractiveness of teaching and would provide more space for research throughout the country. In any case it does not seem that Davenport has much to worry about, since before long the career-award holder will apparently be an extinct species.

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Old Ties with the Smithsonian

This is a note of appreciation of Greenberg's story about the Smithsonian Institution in the issue of 12 March (p. 1266). It filled in a number of blank spots in my knowledge of the earlier years of the institution, with which Research Corporation has always had unique and close ties.

The long-term ties we have had with the Smithsonian are, I suspect, little known but quite interesting. Cottrell, who founded Research Corporation, had earlier offered his patents to the Smithsonian, and the offer had been accepted; but the board of the Smithsonian reversed itself a month later on the ground that the institution had no means of commercializing patents. Its secretary, Walcott, took Cottrell in hand, however, and introduced him to the distinguished group which, with Walcott, became the incorporators and first directors of Research Corporation. Walcott participated closely in the foundation's activities and aided greatly in its development as a philanthropic organization. Upon his retirement from the Smithsonian, Abbot succeeded him on the board,

and this tradition—that the Smithsonian's secretary serve on the board of the foundation—has carried on through our 50-odd years of existence, Ripley having just accepted membership on the board.

Another little-known tie is described in a letter (12 Feb., p. 680) from Hinkley, president of the Research Corporation, commenting on Wolfe's earlier editorial on Robert H. Goddard. Hinkley points out that the funds provided by the Smithsonian in support of Goddard's work in 1924–25 were given it for that purpose by Research Corporation. Consistently over the years, the foundation has provided funds to the Smithsonian for a variety of special purposes for which the institution was unable to find other funds either at all or in time to serve urgent needs.

In these days of big science, and with the prospects Greenberg foresees for the Smithsonian, there is little likelihood of our playing any major role in its further evolution. Nevertheless, there may well be those occasional times when real gambling money is needed urgently by the institution to test some highly speculative idea, and here we may well have an opportunity to exercise our special interests further.

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Why Bibliography?

In a recent letter (12 Mar., p. 1241) I suggested that the preparation of bibliographies was becoming increasingly difficult and time-consuming and was being made so by such arbitrary rules as inclusive pagination and alphabetizing. From the mail I have received, evidently many agree. . . . A number of correspondents have suggested, however, that perhaps the publication of bibliographies is not important, after all. Some believe that computers will provide them; others think they should be deposited in centers to be retrieved when wanted; still others think more anonymity of authors should be fostered. So the time required in preparation, the expense of publication, and a growing independence from the past all are contributing to breakdown in use and understanding of the need for bibliographies.

Not long ago every good paper had

a carefully selected and, in the opinion of its author, complete list of references. The omission of a particular paper from the list signaled the author's disapproval. Failure to refer properly to others' work was taken as a grave omission. Authors and editors insisted that the rules of the scientific game be followed scrupulously. True, there were abuses. Some authors loaded their bibliographies in order to give them an air of erudition, to avoid even the slightest possibility of omission, or to flatter others and usually themselves.

With the growth of knowledge, bibliographies have become much harder to compile, and there is a temptation to give the job a lick and a swipe by referring to a few original articles and a few reviews. Too often bibliographies are unbelievably sloppy. I found 9 out of 13 references in one article to be wrong, and neither the very intelligent author nor editor recognized the errors. Careless reference to any but the most recent literature has become commonplace.

We must decide whether bibliographies as we have known them have enough immediate and future worth. Have the older rules become outdated? The arguments in favor of proper and complete bibliographies run like this: they keep an orderly, progressive record of the advancing knowledge in a field; they help insure that work is not repeated; they tell much about the knowledge of the author who selects the reference; they keep arguments about priority reasonably clean; and they aid the reader in his search for original sources. The arguments against them are: they take up too much space; they are usually only to gratify the author's vanity; they needlessly distract attention from the factual part of an article; they keep alive the problems of priority; preparation is very time-consuming.

People make science, and this is usually the association by which I remember it. I hope I never come to the point where the name of an investigator means nothing more than a computer number. The man's name to me is the tipoff to the quality and importance of the work. Science will lose this personal aspect, I believe, at its peril. Does the appearance of a man's name mean anything important to the man? Under our current social system it is in part a means to increased salary, recognition, self-esteem, and power. Does anyone honestly think

these are unimportant to people? How many enjoy joining that amorphous group called *et al*?

Few other devices help more to keep us strictly honest than a good bibliography. Most of us tend to take just a little more credit than we are entitled to until we actually see that "little more" in hard print. And, finally, what is more helpful than careful documentation of the facts when one examines critically a tightly spun scientific hypothesis or argument? With the accelerated growth of knowledge this can only become more, not less, important.

I hope that authors and editors alike will help one another in coming to an international agreement on a uniform format of reference, and that rules and good table manners will be insisted upon in the preparation and use of bibliography. I am one of those old-fashioned people who believe that the "older literature" does not begin in 1959. I am deeply concerned with the current trend of downgrading bibliography on the one hand and making its preparation needlessly complex and time-consuming on the other. We all need to reconsider these developing trends before it is too late.

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Reprints of Reprint Requests Requested

Readers of *Science* may be interested in the response to our recent letter concerning reprint-request forms (12 Feb., p. 679). We received in all—directly or forwarded by *Science*—45 letters and requests for reprints of the reprint-application form described in our letter. Eleven of the writers realized our proposal was a joke; 13 took it seriously and were outraged at our arrogance; and the remaining 21 were undecided or noncommittal. Our favorite comment was from a reader who asked if we had any publications on the subject of multiple authorship.

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