Letters

The Wooldridge Report

I should like to add two comments to Cooper's article ("Onward the management of science: The Wooldridge report," 11 June, p. 1433). One has to do with the recommendation of the Wooldridge Committee that indirect costs on each grant project be itemized and added to direct costs, and that grants be made to cover the combined total, or a standard percentage of it. The National Foundation tried this, with one minor difference, 15 years ago. It may be useful now to recall how it turned out.

In brief, an elaborate formula covering the various items of indirect costs was worked out by Harry M. Weaver, then our director of research. He was aided by accounting officials of two universities. For 2 years our practice was to make grants covering the investigators' budgets in full, supplemented by 90 percent of the indirect costs. When a grant was approved, a copy of the indirect-cost formula was sent to the grantee institution. It made its own estimate of indirect costs, following the formula, and an appropriate supplement was automatically added to the basic grant. After 2 years we abandoned the formula, and simply added indirect costs figured as a percentage of the basic grant. The grantee institutions' accounting offices had been unhappy at having to fill out the formula.

During the 2 years the formula was in operation the indirect costs, expressed as a percentage of the basic grant, varied from 13 to 155 percent (91 grants, 33 institutions). Seeking a reasonable fixed percentage to allow, we found that indirect costs had averaged 46 percent on grants of $10,000 or less, but that the ratio of indirect to direct costs became smaller as the direct costs rose. We therefore adopted a sliding scale that we have applied ever since. Our indirect-cost policy is now under review. In my judgment, the sliding scale was appropriate when adopted but is now too high. In the original formula, the largest element entering into indirect costs was the working time of personnel on the established institutional staff. We did not then admit salary payments to such people as part of the direct costs. The only salaries budgeted under direct costs were those of temporary employees. In theory we still make that distinction, but in practice it has become almost impossible to draw it clearly.

My second comment is that neither the Kistiakowsky report nor that of Wooldridge mentions the main source of trouble in accounting for project grants. The Kistiakowsky report does point out that an investigator, in proposing his budget, cannot possibly foresee exactly what his needs will be, either in total amount or in detail. It does not, however, mention that for this very reason budgets are usually estimated well on the high side. Granting agencies know and even encourage this, to provide reasonable insurance against a deficit. They also allow the investigator some freedom to reallocate parts of his budget. It follows (i) that in the majority of grants there is a prospective surplus as the date of expiration approaches and (ii) that the surplus will be spent nevertheless if the investigator has leeway enough to get away with it. He then proposes a larger budget for his renewal grant, giving him a larger residue at its end, and this goes on indefinitely until the granting agency calls a halt. Not all grantees behave in this way, but the pattern is familiar to anyone with experience in grant administration. The Kistiakowsky and Wooldridge reports both recommend that surpluses left at the end of a grant be carried over into a renewal. But the budget of the renewal grant will already have been made up and approved before accounting of the previous one is rendered. The renewal budget will have been liberally estimated. I do not see the logic of adding a bonus to it.

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. . . Cooper's conclusions left four questions unanswered.

1) It is very difficult to get competent people to serve on committees and panels. Most competent men in the life sciences have received or do receive support from NIH. Does Cooper really believe that the Wooldridge Committee could have avoided "built-in biases"? Or does he just wish that things were otherwise?

2) Does Cooper know of any useful "criteria [for making] assessments of research performance," other than the judgment of competent men? If he does, I think it only fair that he should state them.

3) Why should a committee "propose alternative actions"?

4) The committee and its panels doubless saw and heard a great deal which formed the basis for its findings. Other than a complete audio-visual transcript, what documentation would be "adequate either to support its own findings or to enable readers to make independent assessments" (of any value)?

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In response to Pierce's comments, I would say that getting competent people to serve on committees and panels is, indeed, most difficult. Some scientists, however, are available for multiple service. To use an insurance term, is there an element of adverse selection at work among these? This could stand inquiry.

I did not think the Wooldridge Committee could have avoided built-in biases when it used panels whose members were parties at interest—that is, were financial and advisory participants in a system whose products they were evaluating. Perhaps the assignment was one they should not have been given. In the law courts, judges disqualify themselves if they have had prior connections with matters before them. Are scientists less human? I did question whether, using panels with conflicts of interest, biases could have been minimized through use of measures that would enhance objectivity. If these were used, they were not disclosed.

Modern science has been built on objectivization and quantification. In the evaluation of human activity, subjective judgment remains predominant and is likely to be so for a long time. Social scientists have found it possible, nevertheless, to minimize the subjective content of evaluation through use of rating scales, assignment of weights, definition and quantification of outputs, and so on. The discipline of designing these contributes to rendering judgments more objective. If the Wooldridge panels used any instruments or measures of evaluation, they did not reveal them. Specific percentages of excellence were reported, however, which were used politically.

A committee that proposes organizational and procedural solutions should emphasize principles in preference to specific arrangements. The principles are more enduring. The methods are more likely to be inapplicable as situations change. Since the Wooldridge report was issued, an executive decision has been made (subject to congressional enactment) to put NIH in charge of the regional medical-complex program. This would enlarge NIH's responsibility for community services, possibly at the expense of its concern with research, and would entail changes in its advisory structures. The Wooldridge Committee might have offered alternatives of specific action under different conditions and executive assumptions, all in support of principles.

The documentation of the committee and its panels should have included any rating scales, schedules of questions, and measures of output, together with summaries of the data used in making judgments. The committee would thereby have rendered itself accountable to the science community and the public, which otherwise was asked to take its findings on faith.

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In his excellent critique of the Wooldridge Report, Cooper concludes,

> The Committee did not avoid built-in biases of interested parties in selecting its advisory panels. It did not publish the criteria it used in making assessments of research performance. It did not provide adequate documentation either to support its own findings and recommendations or to make independent assessments.

Though I cannot say how valid such conclusions may be for all of the panels, it is worth noting that the report of the behavioral sciences panel illustrates his point.

In its report (pp. 130–131 of the Wooldridge Report), the panel attributes an alleged change in the leadership of American psychiatry to the research-grant program of NIH.

> Ten years ago the leadership resided in psychoanalytic institutes organized by practitioners for the training of practitioners, with an essentially ideological rather than a scientific base, and strongly influenced by authoritarian considerations. In very large part because of differential success in competing for NIH research grant support, the leadership of American psychiatry has moved to the universities.

Since there were only two psychiatrists on the panel, the viewpoint expressed can scarely be regarded as representative. Many other distinguished scientists disagree with this judgment and regard it as a one-sided pronouncement. In my opinion, it is inaccurate and gratuitous, conveying a serious and erroneous implication concerning the scientific position of psychoanalysis.

The use of the terms "practitioner" and "ideology" is, of course, a well-worn method of denigrating a point of view or a profession by association and pejorative implication. While condemning psychoanalytic institutes as "organized by practitioners for the training of practitioners" and justifying discrimination on that basis, the panel at the same time inconsistently states with regard to the social sciences that

> . . . separation of the training of practitioners from the scientific discipline is thought to have contributed to the relatively poor quality of practice and often of research in these professions, and to much sociological research in which applied considerations are neglected.

Although psychoanalytic practice is necessarily confined to a relatively small group of mental disorders, psychoanalysis as a body of knowledge is concerned with the continuing formulation of a comprehensive developmental psychology which has already provided a sound theoretical basis for diverse forms of psychotherapy.

There has been no shift in the leadership of American psychiatry from the psychoanalytic institutes to the universities, since psychoanalytic institutes have never been directly involved in the teaching of psychiatry. Individually, however, these "practitioners" of psychoanalysis have given generously of their time to research and teaching within university departments of psychiatry, hospitals, and social agencies. Twenty-four out of 84 (29 percent) chairmen of departments of psychiatry are psychoanalysts, four times as many leaders as would be expected on a statistical basis, considering the relative number of psychiatrists and psychoanalysts. A high percentage of other analysts have major responsibilities for teaching residents in psychiatry. In disregard of these facts, the statement of the panel conveys the impression that the theoretical basis of psychiatry in universities is distinct and antithetical to that taught within psychoanalytic institutes.

The role of psychoanalytic institutes in providing teachers of psychiatry is recognised by the National Institute of Mental Health in grants to each of the analytic institutes, and psychoanalysts in university settings have been the recipients of career research awards and research grants. Recent positive statements about the contributions of psychoanalysis to community and social psychiatry by Stanley F. Yolles, the new director of the NIMH, and Leonard Duhl, chief of its Office of Planning, do not support the conclusion that NIMH officials share the opinion of the panel [*Communication and the Community*, vol. 8 of *Science and Psychoanalysis*, J. H. Masserman, Ed. (Grune and Stratton, New York, 1965), pp. 147–149, 171–184]. Such evidence suggests that the panel's report may be as inaccurate and unrepresentative with respect to the policies of the NIH as it is with regard to psychoanalysis. If changes in the character of American psychiatry were "due to the research grant program of the NIH," it would represent an attempt to manipulate the status of a science by a selective allocation of federal funds in favor of one point of view and to the detriment of another with the excuse that the first is scientific while the second is ideological. Whatever the merits of the two points of view, such action would be an example of thought control by an arm of the federal government inconsistent with the American tradition and antithetical to the development of science and to the cause of good government.

I am not in a position to know whether any discriminatory practices have taken place within NIH, but, through its Panel on Behavioral Sciences, the Wooldridge Report appears to be commending what may be an unwarranted pressure for such action on an important government agency.

At a time when all the resources of

the nation need to be mobilized to cope with the major social problem of mental illness, it is essential that responsible committees have a broad viewpoint which encompasses diverse and varied approaches. Evaluation reports should be factual, supported by documentation, and not the medium for expression of sectarian opinion.

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Recording the Data

Taking exception to the point of view expressed in P. M. Newberne's letter (9 July, p. 137), I should like to voice my feelings in defense of shutterbugs at scientific meetings. The tendency at these gatherings seems to be to present as much data as possible in the brief time allotted. This necessitates the use of a number of slides full of information. The listener who desires to study the presented data in detail or at his leisure must either be an extremely rapid transcriber (and draftsman, in the case of graphs and diagrams), have a photographic memory, or be able to operate a camera. In the printed material (such as abstracts), one is indeed fortunate to be provided with one or two equations, much less with tables and diagrams.

As for the author's not wanting his data used and quoted, presentation at a scientific meeting is hardly the way to keep them confidential.

Banning of cameras at scientific meetings would achieve little except inconvenience for those who are most interested in the presented material, as it would require them to copy the data longhand while missing much of the oral discussion or to wait, possibly for many months, until the report is published in full elsewhere.

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Thoreau and "Ecology": Correction

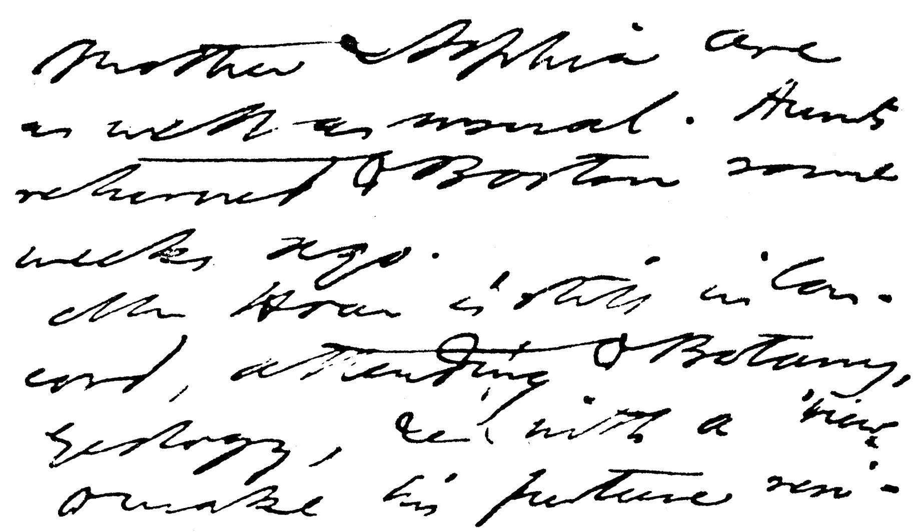
Mother & Sophia are
as well as usual. Aunt
returned to Boston some
weeks ago.
Mr Hoar is still in Con-
cord, attending to Botany,
Geology, &c with a view
to make his future res-

Fig. 1. Part of a letter dated 1 January 1858 by Thoreau to his cousin George Thatcher. The word in question occurs at the beginning of the next-to-the-last line.

Since I was inadvertently responsible for confusing the record on the history of the word *ecology*, I would like now to try to set it straight once more. In 1958, with Carl Bode, I edited *The Correspondence of Henry David Thoreau* (New York Univ. Press), and in it I transcribed a sentence from a newly found letter of 1 January 1858 as reading, "Mr. Hoar is still in Concord, attending to Botany, Ecology, &c with a view to making his future residence in foreign parts more truly profitable to him" (see Fig. 1). In the issue of *Science* of 17 April 1959 (**129**, 992), Paul H. Oehser, quoting from the volume, pointed out that this use of the word *ecology* preceded the generally accepted coinage of the word by Ernst Haeckel by eight years.

Recently, Richard Eaton of Harvard University called my attention to the fact that Haeckel's word was *oecology*, and that American botanists did not adopt the simpler spelling until the Madison Botanical Congress of 23 August 1893. In the light of this new information, I reexamined photostats of the letter (the manuscript is in the Berg Collection of the New York Public Library) and realized for the first time that, while at first glance the word seems obviously to be *Ecology*, it can without too much imagination be read as *Geology*. I also noted that several times in his *Journal* that winter Thoreau mentioned Hoar's interest in rocks and quarries. Under these circumstances I think I must assume that, since *geology* makes as much sense in the context as *ecology* does, *geology* must have been the word that Thoreau intended. I think we may once more assume that it was Haeckel who originated the word, in 1866—although, as students of Thoreau will realize, even if Thoreau did not coin the word, he was unquestionably a pioneer in the science of ecology.

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Women—in Science or Out

I should like to add one item to the list of "tasks ahead" in Alice S. Rossi's article ("Women in science: Why so few?" 28 May, p. 1197): High school guidance teachers should be persuaded that a career in science or engineering will not ruin a girl's future. Several years ago, when I expressed a desire to enter M.I.T., I received from my high school adviser—a warm and friendly woman—a stunned and slightly horrified look. She wished to save me from my "immature desires" by getting me admitted to Bryn Mawr or Smith, where I could write poetry and avoid those brutal, masculine calculus courses if I wished. Well, I have an "unusually supportive" father such as Rossi mentions. I entered M.I.T., and in 1963 left it with a Bachelor of Science degree. But at last report my high school adviser was still doing her best to dissuade girls—even if they were excellent in high school mathematics, physics, and chemistry—from applying to schools oriented toward science.

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