Letters

Identifying Great Teachers

The editorial by Dael Wolfle ("The great teachers," 11 Dec. 1964, p. 1421) is timely and should be of great interest to all who teach and all who learn. Many of our great teachers go unrecognized by all save their own students. Especially in the smaller universities and colleges, the better teachers are known only by the students with whom they come in contact.

I would take issue, however, with the way in which Wolfle would evaluate professors. . . . I would suggest that the evaluation be based on surveys of former students, not current students. Current students have no valid criteria for judgment on the overall effectiveness of teaching. An alumnus has had a period of experience in which to compare his training with that of others. He is also removed from present pressures and peeves. He is likely to remember vividly the good teachers and the bad teachers, and the others gradually merge in a gray area in between. It is true that a survey of alumni would be more difficult and expensive than a campus survey, but any school seriously bent on evaluation and identification of the best teachers should be willing to undertake such a survey.

These suggestions are merely a difference in method, not in intent. We agree on the need for recognition of our better teachers.

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Wolfle's argument is roughly . . . that the necessary enhancement of the status of good teachers . . . presupposes the identifiability of such teachers; that the validity of this presupposition is in doubt, and that we must begin to test it; that, finally, the teacher "who wishes for enhanced status" must "cooperate in efforts to see if the ablest teachers can be identified reliably" or else, holding that "good teaching is essentially a private and unmeasurable affair," give up his hope for such enhancement. What is left out here is, obviously, any attempt to characterize "good" teaching. The omission is understandable; in so short a compass, Wolfle could scarcely have addressed this question. It is a nonetheless regrettable omission; for, if we continue to talk to each other in this way, we shall before long forget that we have never discovered what good teaching is. The nation's schools of education long ago made precisely this mistake; the results have been catastrophic, and I should not like to see our scientists repeat the performance. . . .

There are various competitive conceptions, of a more or less unreflected and certainly preoperational sort, of what good teaching is. Wolfle hints at some of these in suggesting lines along which measurement might be attempted: good teaching is that which excites the admiration of one's students, or of one's colleagues, or of one's administrative associates. Notice that the only alternatives he offers rest upon a single *fundamentum divisionis*: a conscious, introspectively identifiable, personal response, or something of the sort. What is likely to happen is that some clever investigator will seize upon one or another of these unsophisticated and intuitive conceptions, learn how to measure in respect of it, and by that very success establish it as *the* conception of good teaching. Once entrenched, the conception will be extirpated only with the greatest difficulty; it will tend to displace competitive conceptions which may be of far greater moment. . . .

Let us bear fixedly in mind how little we really understand of teaching, good and bad. What warrant have we for the belief that even the good student (whom we equally ill understand) recognizes a good teacher as such? Does the latter recognize himself? Are the results of excellence in teaching manifest at all while the student is still in school? Do good students actually have any real need of teachers? I am well aware of the impatience with which most investigators view such questions; they regard them as mere quibbles, which only impede the forward progress of the inquiry. It is true that a penchant for unattainable precision of conception can divert a thinker from constructive theorizing. Some part of the scientist's art consists in his knowing when a conception is well enough worked out to justify employing it, devising means of measurement in respect of it, and so on.

I therefore reject Wolfle's dilemma. It is far too early to begin to devise means of measuring excellence in teaching and of identifying good teachers; it would be equally premature to assert that good teaching is "private and unmeasurable." Let us first do what we can to decide among ourselves what good teaching *is*, what it is *like*, what *sort* of thing it is. Perhaps then we shall be able to determine its susceptibility of measurement.

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. . . Able teachers can be identified, I believe, on the basis of responses from two groups of listeners. The more reliable is comprised of the above-average (serious) students, 3 to 5 years *after* having taken a course with the individual in question. The second group consists of the teacher's colleagues who listen carefully when he presents seminars in his own specialty. Such information is really not difficult to obtain.

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Biomedical Sciences in Europe

You have recently published two highly intelligent—and highly critical—articles about the state of the biomedical sciences in Western Continental Europe (V. K. McElheny, 14 Aug. 1964, p. 690; R. P. Grant, C. P. Huttrer, C. G. Metzner, 23 Oct. 1964, p. 493). By their support of reform and of the diversion of external funds into European biology, these articles may do much good. But they should

not be allowed to obscure or discourage the degree of self-help, of change in attitudes, and of development of new thinking and organization which is being achieved, some of it wider than the purely national and quite a bit of it centered around either the European Molecular Biology Organization (EMBO) or the European Atomic Energy Community (Euratom), which themselves work closely together and in close harmony with national organizations. True, these activities are limited to only a part of biology and medicine. True, much remains to be done. But some advances have been made along the path that may yet lead toward a fully fledged European Scientific Community.

Among other things, the Biology Division of Euratom has:

1) Intervened in a crucial manner in the decision to set up the Naples Institute referred to extensively by McElheny; and Euratom retains a powerful voice in the affairs of that institute.

2) Intervened to re-equip and expand the group constituted at Brussels by Professor Brachet and his colleagues.

3) Set about helping a third group to expand into a powerful unified institute at Leiden University, around Professors Cohen and Sobels.

4) In partnership with the German government and universities set up at Freiburg and Munich a clinical research project of very modern structure and concept to study particular aspects of the hemopoietic system.

5) Developed one completely international biological institute—even if it is as yet small—at Euratom's own nuclear research center at Ispra.

6) Instituted a European-community-wide competitive project-support scheme (see the article by Grant *et al.*) whose existence suffices to defend the parallel national organizations against criticisms or possible abuses of monopoly.

7) Built a truly international staff (which now numbers about 70) devoted purely to research in the biological field, with good salaries, good security, and considerably enhanced freedom of movement, especially between national boundaries.

8) Started the first international cooperative scheme, between a group of five or six leading institutes in five different European countries, designed to help young research workers acquire a second discipline and especially to enable young physicists or chemists to enter the biological sciences. This scheme consists of fellowships and courses, of which the first, on the physical chemistry of macromolecules, was held recently in Brussels. It is integrable later in any wider effort, say by EMBO, for which it constitutes at the same time a suitable pilot study.

Your readers will readily understand my concern that the real risk which these and other useful initiatives run just now through a mixture of political difficulty and discouragement should not be unnecessarily increased from outside.

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Empiricism in Engineering and Science

In his article "Academic organization in physical science" (2 Oct. 1964, p. 35), Henry G. Booker makes some derogatory statements about the inclusion in a university of "any engineering department that still regarded its prime function as the professional training of students in empirical design." It is my impression, gained by experience, that such statements are subject to misinterpretation. To some they mean that all instruction in design and all empiricism should be excluded from a university engineering department.

Webster's Collegiate Dictionary (5th ed.) defines "empirical" as follows:

> 1) Depending on experience or observation alone without *due regard* [my italics] to science and theory.
>
> 2) Pertaining to, or founded upon, experiment or experience.

Judgments about whether "due regard" is given must be based upon particular cases. The theory of engineering practice is that problems must be solved even when a scientific base does not exist. Many people do not realize that this occurs very frequently. For example, the accurate design of digital computer circuits completely by digital computers is theoretically impossible at present; hence empirical (definition 2) methods have been relied upon. Many interpret strictures against "empirical" methods as applying to both definitions. Because of a feeling that "empirical" methods have no place in universities, many university engineering departments have come to resemble second-rate departments of pure mathematics. (This is not intended to be an aspersion on the many good pure-mathematics departments. Good work in pure mathematics requires at least a pencil, paper, the human mind, *and experience*.) There is also a tendency to confuse engineers with applied physicists. It is not completely clear whether or not Booker has done so. One point of view, however, is that the professional engineer's prime concern should be the community, its needs, and the problems it has to have help in solving for the survival and well-being of its members, while the applied physicist's concern with these is necessarily less direct.

Booker believes that "the professionally oriented undergraduate programs in engineering . . . are a mistake" and that the undergraduate education of a potential leader in engineering should not differ from that of a physical scientist. Again, this can be interpreted, perhaps contrary to Booker's intent (he does not specifically exclude professional education from graduate school), to mean that a professional engineering department is not properly a part of a university. But the professionals are the transformers of information into action and require knowledge not only of science, but of empirical methods, the arts, psychology, social science, and so on. Where else should they go for this knowledge if not to the universities, the preservers, disseminators, and augmenters of this knowledge? The early universities were founded to give instruction in the professions of medicine, law, and theology. When interest in subjects not even indirectly affecting the community became widespread, the universities entered a decline which some believe was an effect of the change of interest. Later, their renaissance took place with the appearance of physical science, which grew through the joint efforts of empiricists and theoreticians. Social theories exist to describe this sequence of events.

Science is concerned with the development of theories through observation, induction, and verification where possible through experiment. A scientific theory is a description of a phenomenon which is consistent with observation. Occasionally in the physical sciences, and more often in the social sciences, it is not possible to design experiments to check the theory. There