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.

RAYMOND K. APPLEYARD
Biology Division, European Atomic Energy Community, Brussels

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 is no unique theory that describes a given phenomenon, as is easily proved by making a trivial modification of any given theory which does not change its description of the set of phenomena that are on hand. The theory that endures is the one that describes a large set of phenomena compactly. This involves the minimization of a subjective quantity, human effort, which, like many quantities in engineering, is not capable of exact measurement. A theory is modified or abandoned when it fails to describe a phenomenon that it should describe. A theory does not necessarily have predictive power, and it may not necessarily be capable of predicting numerical values in a useful way. For example, consider the problem of designing a pulse circuit using diodes and transistors. Many have felt the desirability of applying digital computers to this task, but only a few have achieved any significant degree of success. Fundamental philosophic difficulties have impeded progress. To see this, first note that if the transient performance (response to arbitrary time-varying waveforms) of the circuit can be computed, then by known procedures a circuit can be designed (component parameter values obtained) given a circuit configuration. The determination of a circuit configuration is a creative act. To compute the transient response to a given waveform by known methods requires that differential, integral, and other equations be available for each component. A semiconductor diode is a physical device whose operation is described by solid-state theory. However, the equations describing the response of a real, physical diode to arbitrary stimuli have not been obtained. In principle, quantum mechanics applied to the physical structure of the diode could give quantitative answers, but no computer smaller than the universe could solve the problem in a reasonable time. The band approximation and the diffusion theory serve only to describe but do not give quantitative, useful results except for nearly ideal cases. The circuit designer has to design his circuit to use real devices obtainable from a manufacturer. He is limited to only those tests that can be made at the diode terminals. For linear black boxes, systematic procedures exist for obtaining accurate describing equations, but none exist for nonlinear systems. This problem is very similar to that faced by the physical scientist in constructing a mathematical theory on the basis of experiment or experience. That is, if a general synthesis procedure for nonlinear systems based on terminal properties could be found, the pulse-circuit-by-computer-design-problem could be done mechanically, and the procedure would be of inestimable value to the physical scientist.

H. J. GRAY
*412 Colonial Park Drive,
Springfield, Pennsylvania*

For Complexity

Szent-Györgyi's suggested resolution of the problems of teaching associated with expanding scientific knowledge (4 Dec., p. 1278) would have the undesirable effect of perpetuating an underlying assumption of simplicity or parsimony in the "laws of nature" that is directly contradicted by the increasing complexity evident in the very "explosion" of scientific knowledge to which Szent-Györgyi addresses himself.

As I have attempted to document in detail elsewhere ["Parsimony in psychology," *Psychol. Rept.* **11**, 555 (1962)], an inappropriate adherence to simplicity of scientific investigation and explanation has been a major deterrent to progress in experimental psychology, and probably in other fields of science as well. It is most unfortunate that current scientific methodology offers no satisfactory guarantee that an excessively simple principle or technique will be rejected merely because it is too simple to cope with the empirical facts. Consequently, it becomes exceedingly difficult in practice to rid science of oversimplified formulations, especially when so many scientists, like Szent-Györgyi, appear dedicated to the proposition that the many presently unresolved "riddles of nature" will ultimately yield to a single simple and general explanation. . . .

Our teaching will be far more helpful and effective if we attempt to convey the great importance and excitement attached to the complexities of science and the investigation thereof, rather than misleading the student by emphasizing an underlying simplicity which he probably will never experience.

WILLIAM F. BATTIG
Department of Psychology, University of Maryland, College Park 20742