Comments and Communications

A Note on "Why Vegetation on Watersheds?"

The writer would like to add a couple of items apparently overlooked in the recent note by Chapman Grant (*Science*, October 29, p. 486). First, watersheds that have reservoir storage for the entire annual precipitation are quite rare. In southern California a large part of the water conserved is put underground by percolation of slowly released impounded stream flow. Complete storage is unnecessary as long as the entire season's precipitation does not come at once and is not turned immediately into stream flow. It is the watershed vegetation that slows down the runoff to make storage less extensive and expensive, and that makes long-continued percolation to underground storage possible. Second, the gunited or tin-roof type of watershed has not proved desirable. Residents of the desert areas of California and along the Wasatch front in Utah have suffered severe floods from denuded watersheds. In many cases the affected communities have gone to great effort and expense to get a cover vegetation re-established. As the cover has come back, flood damage has been reduced.

Research findings show that, though vegetation does take its toll of the water supply in arid regions, the residual water is almost all usable. Where the vegetation is gone, stream runoff often becomes flood flow. Such a flow is usually entirely wasted, except for percolation underground, and, in any event, is contaminated with a heavy load of silt and debris at nearly all stages. Interested Californians might well review the watershed studies carried on by the Forest Service at the San Dimas Experimental Forest near Los Angeles.

W. E. Bullard

Carson, Washington

Antigen Films and Long-Range Forces

In a recent note (*Science*, July 30, 1948, pp. 107–108) Karush and Siegel produce evidence from electron microscope studies of deposited protein monolayers that the monolayers on glass slides are not smooth layers of uniform thickness. The protein layers are apparently irregular in thickness with ridges or peaks which, in extreme cases, may be as high as 200 A. They assume from this that when multilayers of barium stearate are deposited on this irregular monolayer, the ridges or peaks project *through* the barium stearate layers. On the basis of this assumption they challenge the necessity for specific long-range forces as postulated by Rothen (*Science*, November 2, 1945, p. 446; *J. biol. Chem.*, 1947, **168**, 75) to explain the specific interaction of an antibody with the antigen layer, through the intervening layers of barium stearate.

There is no apparent justification for this assumption of Karush and Siegel. On the contrary, it seems unlikely that the peaks of the protein layer will project through any monolayer deposited on it. It is well known that, when monolayers are deposited onto a solid plate from a liquid surface, the deposition ratio is almost exactly unity (cf. Langmuir, *et al. J. Amer. chem. Soc.*, 1937, **59**, 1751). This is true if the "solid plate" is a fine wire gauze so that the monolayer does not even follow the contours of macroscopic irregularities on the plate surface. The film is stretched across the tops of any peaks or ridges.

Karush and Siegel observed ridges which were generally between 50 and 85 A high, and there is therefore no reason to suppose that these would have any effect on a monolayer deposited on the protein film. If the protein film is ridged, it means that the bulk of the protein will be even farther away from the antibody than is indicated by the thickness of the "barrier" layer.

If the explanation of Rothen's results is to be found in some penetration of the barrier by antibody or antigen molecules, then a more probable mechanism could be provided by the crystallization of the barrier layers. Multilayers usually form microcrystals which are continuous through the thickness of the multilayer, and so there will be intercrystalline boundaries extending from top to bottom. It is conceivable that one or more active groups of the antibody could penetrate at one of these boundaries. It does not seem necessary for the initial "hole" in the barrier to be large enough for a complete antibody molecule to get through. If a particularly active group can approach near enough to the antigen, it is possible that the forces brought into play are large enough to extend the "hole" so that a considerable amount of antibody could then penetrate the barrier.

John Iball[1]

University of St. Andrews,
University College, Dundee, Scotland

Precedence of Modern Plant Names Over Names Based on Fossils?

James M. Schopf has proposed an amendment to the International Rules of Botanical Nomenclature to the effect that names based on recent material should always take nomenclatural precedence over names based on fossil or subfossil specimens (*Science*, April 2, 1948, pp. 344–345). "Always," in this connection, obviously means even that the law of priority may thereby be violated. In *Science* (October 29, 1948, p. 483) the author reports a "generally favorable" reception of his proposal.

Both proposal and reception seem deplorable from a strictly nomenclatural point of view. They seem to be based on the "natural but mistaken assumption that types are somehow typical, that is, characteristic of the groups in which they are placed," and on the fact that "types . . . are by many students supposed to be not only name-bearers but also the bases on which group concepts are erected and the standards of comparison for those concepts" (Simpson. *Bull. Amer. Mus. nat. Hist.*, 1945, **85**, 29). The primary and only function of types, how-

[1] Edward A. Deeds Fellow.

ever, is name-bearing; this makes strict adherence to the law of priority imperative. Therefore, substitution of a new type species of a genus for an already established one, as suggested by Schopf, e.g. in the case of *Metasequoia,* is not only not permissible under existing rules[1] —as he agrees—but even if it were to be permitted by an amendment, it would be bound to create confusion.

Unconsciously Schopf, himself, gives an example of such confusion (p. 483): Should the living and the fossil *Metasequoia* prove to be really congeneric—a fact not yet established beyond doubt, according to the author—and should, furthermore, his above proposal be accepted and incorporated in the Rules, then, he suggests, the genus "should be cited for type reference as *Metasequoia* Hu and Cheng, *non* Miki." However, such a way of citing has always implied, and obviously still implies, that Hu and Cheng (the authors of the living *Metasequoia*), on the one hand, and Miki (the creator of the genus *Metasequoia,* based on a fossil species), on the other, applied the same name to two different genera, whereas in the present case the species to which both authors apply this generic name are congeneric, according to Schopf's own premise. Thus, the same name means also the same thing. It would seem that no better *reductio ad absurdum* could be thought of for Dr. Schopf's proposal.

Otto Haas

Department of Geology and Paleontology,
American Museum of Natural History, New York City

The Human Engineering Seminar at New York University

Readers of *Science* are likely to be interested in learning about a pioneering effort in the cross-fertilization of ideas from many scientific fields which is currently being made in the College of Engineering, New York University. The present Seminar in Human Engineering, which is a continuation of a more informal series of sessions held during the spring of 1948, is sponsored jointly by the College of Engineering and the Institute of Industrial Medicine of the College of Medicine.

Human engineering, as conceived by the seminar participants, is a rapidly expanding branch of applied science which is concerned with the general problems of the interactions of men and machines. The emerging science of human engineering, which others have referred to as biomechanics, biotechnology, and psychophysical systems research, draws heavily upon the experimental techniques and data of engineering, the biological sciences, the medical sciences, psychology, and certain of the social sciences, notably anthropology, all of which are concerned with the conditions under which man works and the factors associated with optimal performance with machines.

Sessions of the Human Engineering Seminar have attracted representatives from virtually all of the the professions whose mutual interests find expression in the seminar. Each session, although devoted to a consideration of a limited segment of the field of human engineering, has proved useful to various professionals in attendance in suggesting ways in which the data and principles from another science can be applied to the study and evaluation of problems in their area. Among those attending the meetings there has developed a deepened appreciation for the cross-disciplinary approach which characterizes the papers presented, and this appreciation is grounded in the experience of learning to think within the framework of an often alien point of view.

As a result of a number of seminar sessions, the major problems and issues of human engineering have begun to emerge and to clarify themselves, and there is a growing acceptance among participants of the need to fashion practical working procedures for the team approach to the resolution of pressing research problems from many sciences which find concrete expression in this field.

To indicate the trend of thinking among seminar members, it is useful to glance at the broad areas which have been considered. Arthur Lefford, of the College of Engineering, presented a psychological approach to "The Present Status of Fatigue," in which there was a serious effort to understand problems of fatigue within the context of motivation as a psychological process. "An Over-All View of Personality for the Human Engineering" sought to advance the notion that in human engineering research man has for too long been considered either a machine or machine-like, and that it is time now to concern ourselves with the attitudes, motivations, and other personality characteristics and processes of men in relation to the design and operation of machines.

The session on "Environmental Factors in Human Engineering," led by Norton Nelson, of the College of Medicine, New York University, sought to present facts and principles from physiology which have a direct bearing upon human engineering research inquiries. Although devoted to certain selected problems in the thermodynamics of human behavior, the presentation suggested clearly the broad values of the physiological approach to human engineering. Matthew Luckiesh, of the General Electric Lighting Research Laboratory, in his paper, "The Human Seeing Machine," sought to make clear the enormous number of problems confronting the illumination engineer in a consideration of even the simplest human engineering inquiry in the area of illumination.

Other papers on "The Present Status of Principles of Motion Economy" and "Anthropometric Data in the Design and Operation of Machines and Equipment" highlight other interests of seminar members. These and other papers presented before the Human Engineering Seminar have been informally published as "Contributions to Human Engineering" and are already finding use in the work of those who ally themselves and their research with the human engineering point of view which the Seminar has sought so earnestly to develop.

Lawrence Edwin Abt

College of Engineering, New York University

[1] Analogy with neotypes for species whose original type has been lost or destroyed would not be justified, even if the situation were similar, which it is not; for the types of species are physical specimens, but those of genera are species, which are mental concepts.