modified to provide an inducement for the recipient to teach.) In order to enhance the prestige of these fellowships, and to compete with the regular postdoctoral fellowships, their monetary rewards must be greater than those of the research fellowships (which *permit* the holders to devote up to 10 percent of their time to teaching). The awards should be flexible enough to permit the recipient either to do research entirely on his own (he may need funds to buy equipment) or to do research with a more established investigator. The teaching requirements would be subject to negotiation between the individual and the institution to which he is going and should not require him to spend more than 10 percent of his time teaching. The kind of teaching I have in mind is giving lectures on agreed-upon subjects, helping to prepare and grade examinations, helping to organize and oversee laboratory exercises, and acting as an adviser to students. With a little foresight and initiative on the part of the universities and colleges, the teaching load of the staff and graduate students (who are sometimes unfairly overburdened) could be reduced. It might be argued that the threat of government control over education makes this suggestion unworkable; however, it is my opinion that the present control government is exerting over education (by supporting research exclusive of teaching) is a much more real threat.

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Sap Pressure in Plants

In their article "Sap pressure in vascular plants" (16 Apr., p. 339), Scholander, Hammel, Bradstreet, and Hemmingsen present some interesting data on the water relations of plants. The experimental procedure that they employed does not, however, demonstrate the existence of negative sap pressures as they stated. This is not to disclaim the existence of negative pressures, but rather to point out that their procedure measures only the difference in free energy per unit volume between water in the plant and the same water outside the plant. Their pressure chamber operates on the same principle as the pressure-membrane apparatus developed by Richards [*Agr. Eng.* **28**, 451 (1947)] for measurement of the potential energy of water in soils. When air pressure is applied to the sample chamber, the free energy of the water is raised. If this pressure increase is carried out isothermally, the free energy of the water would be raised by approximately $V\Delta P$, where V is the volume of water in the sample and ΔP is the pressure increase necessary to establish equilibrium between water in the system and that outside. It is common practice to express this energy difference in terms of energy per unit volume (the water potential), which, of course, is dimensionally the same as pressure. In the experiment of Scholander *et al.*, the plant itself provides the membrane which is permeable to water but not to air.

Richards emphasized that the pressure-membrane apparatus measures an "equivalent" or "apparent" negative pressure within the soil. Nevertheless, many soil scientists have assumed that the pressure necessary to force water out of the soil gave a measure of the actual pressure of the water in the soil. It now seems apparent that the adsorptive force field around soil particles acts throughout a sufficiently long range to account for a significant proportion of the potential-energy lowering in soils. Thus, the pressure membrane measures the free-energy difference but not necessarily the pressure difference. If sufficiently long-range adsorption forces do exist in plants, as seems entirely reasonable, particularly within the cell wall, the same would hold true for the experiments of Scholander *et al.* One could argue that the data presented prove the existence of adsorption forces just as readily as the existence of negative pressures. This is a challenging problem, and it is to be hoped that a means can be found to distinguish between the various components of the potential energy in plants in a conclusive way.

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Gardner and Rawlins argue that our measurements do not demonstrate negative sap pressure in plants, let alone measure it. This criticism appears to stem from a misplaced analogue between their soil and our plant experiments. It is true that the balancing bomb pressure does not differentiate between hydrostatic and osmotic forces

in the soil; neither does it do so *inside* the cells. On the *outside* of the membrane (extracellular), however, it measures the negative hydrostatic pressure in the xylem ducts as a straightforward null measurement. It does not matter what causes the negative pressure balance in the xylem sap, nor what adsorptive forces may reside in the vessel walls; nor does it matter whether the ducts run plain water or sap solution. In plants, nevertheless, a close approximation to the turgor pressure (intracellular) can be derived from the pressure-volume curve.

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Mass Extinctions at the End of the Cretaceous Period

The environmental factors that influence communities of living organisms are so complex that they are rarely well understood. Consequently, the problems that confront the paleoecologist may seem insuperable. Yet, recurring regularities in the fossil record of past life tempt paleontologists to venture explanations of real events, now millions of years past, in the history of life. Such an explanation is M. N. Bramlette's stimulating new hypothesis (25 June, p. 1696) to account for revolutionary changes in marine life at the close of the Mesozoic era. His conclusions are based on an exceptionally well-documented case history, and once and for all remove any doubt that marine life has undergone catastrophic changes over wide expanses of the oceans. This is a matter that has long been suspected and much debated. Now that the facts seem well established, the problem is to explain them.

I have cited evidence [(*Sci. Amer.* **208**, 76 (Feb. 1963)] that extinctions of the past display a spectrum of patterns ranging from apparently catastrophic revolutions in the faunas of the world to very slow selective and evolutionary replacement. For the former, I have sought general, rather than specific, causes because some of these mass extinctions have involved both terrestrial and marine animals and have recurring rhythms that would seem to eliminate unique causes. Bramlette considers the apparent synchroneity of extinction of land and sea organisms as unproven and possibly a result of circularity in the method of stratigraphic paleontology, in which rocks are dated by fossils and fossils are dated by the rocks that contain them. However, there cannot now be any reasonable doubt that these and comparable changes in world faunas were compressed into time intervals that were very short as measured on the scale of geologic time. It seems to me reasonable to conclude that worldwide vicissitudes among organisms were the result of recurring general ecological disturbances that destroyed the most fragile populations of both land and marine animals. Extinctions on a massive scale clearly were highly selective among animals, affecting some groups while sparing others, but it is one of the unsolved problems of paleontology that plants were not simultaneously and equally affected by crises in the animal kingdom.

Bramlette's interesting hypothesis explains mass extinctions in marine fauna at the close of the Mesozoic era by gradual failure of supply of nutrient salts and sudden collapse of the worldwide ecosystem at a threshold point in a gradual diminution of supply of land-derived sediments carried to the sea by streams. This hypothesis is intriguing and highly promising, but in the present form it seems vulnerable on two counts. In the first place, terrestrial animals did indeed undergo important mass extinctions at approximately the same time as marine animals near or at the close of the Permian, Triassic, and Cretaceous periods. It is incredible that the supply of nutrient salts to the sea would directly affect land animals.

It is also probable that fluctuations of the rate of supply of land-derived nutrients are not nearly as important as variations in oceanic circulation, particularly upwelling, in determining the concentration of nutrients in the euphotic zone. Marine ecologists believe that by far the greatest part of immediately available nutrient salts are contained in the deep basins of the oceans. These salts are continuously recycled by oceanic circulation. Compared with this reservoir, the annual increment from the lands must be infinitesimal. In areas of rapid sedimentation, of course, much nutrient matter is