That is to say, Dr. Mahin did not make it clear that from the moment they enter college most students are under drastic coercion tending to destroy their initiative, break their spirit and bring them into dull submission to destructive interests.

In the second place Dr. Mahin gave no attention to the bad condition in schools below university or college grade. Matters are bad enough in higher institutions, but, in truth, the mischief is often done before a youngster reaches college. He is discouraged by the outspoken contempt of the parasite for the "grind." He is impressed by the success of the bluffer or shirker supported by the sporting group. He is intimidated by the bluster or actual physical and social injury of the special interests in his school. He is made suspicious of the motives of other people. He is deprived of the poise and self-confidence so necessary in a good scientist. He is disgusted at the constant demand for money to support those creatures whom he knows to have no interest in either the school or in education.

I charge that the American Association for the Advancement of Science is derelict in its duty and false to its aims so long as it shuns active opposition to such evils. I also consider the association unscientific in its procedure when it tries to add to a superstructure while neglecting to repair its decaying foundation.

Dr. Mahin has clearly stated the ways in which *commercialized* athletics makes bad environment for scientific work. Most of us know cases in which science has been deprived of a competent scientific worker because of its evil influence. Every one of us can find evidence immediately at hand showing that *commercialized* athletics is conducted in open and contemptuous disregard of physical and mental hygiene. It is also true that *commercialized* athletics hinders accumulation of resources for scientific work and particularly diminishes the rewards of the scientific worker.

So much for the relationship of unhealthy amusement to "Advancement of Science." Personally I am intensely interested in the fact that the boy or girl who wishes to get a maximum of scientific training on limited resources is *coerced* into paying varying amounts of money and time and energy to the support of the socially and educationally and scientifically destructive activities of the sporting fraternity.

Individually or collectively 10,000 scientists ought to be able to exert a wholesome influence in these matters, not only for the good of science but for the welfare of our country. In fact it is disgraceful that such appeals as that of Dr. Mahin do not bring *action* as well as silent approval.

W. E. ALLEN

THE SCRIPPS INSTITUTION
FOR BIOLOGICAL RESEARCH

METHODS OF THE CARNEGIE INSTITUTION

TO THE EDITOR OF SCIENCE: President Pritchett's lamentations regarding the woes of the administrators of great benefactions are perhaps the natural reaction of a kindly, just and generous man who, in the nature of things, has to say "no" more often than he can say "yes." When the Carnegie Institution was first founded I wrote the following to SCIENCE (1902, xvi, 484):

> The scholarships should be allotted to laboratories the heads of which have shown themselves competent to do research work. It is a mistake to compel men, who are presumably competent, to reveal an outline of the subject to be investigated. The greatest discoveries are often accidental observations made by trained minds. The former product of their laboratories or of their personal work should be the criterion. In this way, if one line of investigation seems fruitless, the scholar can at will be turned in another course. Thus, the Carnegie Institute may endow but not control the course of science in San Francisco. There must be no limitation to the *akademische Freiheit*.

Consider one example in which this plan was followed, the endowment of the work of Osborne and Mendel, which resulted, among many other discoveries, in our knowledge of the production of xerophthalmia when butter fat is eliminated from a diet otherwise complete, and of its cure by administration of cod-liver oil or of butter fat itself. The two workers were individual scientists, one a university professor, the other a chemist in a state agricultural station. The money was conferred because it could be productive. The men were trusted absolutely. There were no conditions, no red tape, no general uplift organization with strict rules and regulations for conduct, no publicity department, no puffing, no visiting detectives, no superior intelligence to tell them

what they ought to do. They were simply left alone to do as they thought best, and they did so. "By their fruits ye shall know them."

It seems to me that the Carnegie Institution is to be greatly congratulated on the methods of its work.

GRAHAM LUSK

TINGIIDÆ

MR. PARSHLEY (SCIENCE, Vol. LVI, p. 754) credits me with too much. I can not lay claim to any "novel idea." And I wish here to state only three facts.

First: Words like Aphiidæ have been used for a long time. See Aphiidæ, "Traité d'Entomologie Forestière," Barbey, 1913.

Second: We are here concerned not with Latin usage and with professors' opinions but with the International Rules of Zoological Nomenclature.

Third: Article 4 of these rules simply states that *idæ* is to be added to the stem. No latitude is given us. It is the writer's humble opinion that any desired modification or interpretation of this article should be made by the International Commission and not by an individual.

A. C. BAKER

BUREAU OF ENTOMOLOGY

SCIENTIFIC BOOKS

The Cactaceæ: Descriptions and illustrations of plants of the Cactus family. By NATHANIEL L. BRITTON and J. N. ROSE. Vol. III. Carnegie Institution Publication No. 248. 1922.

What Professor Wheeler irreverently calls silo and saleratus botanists, and doubtless others, often sniff in private over "the futility of spending fortunes in monographing the Cactaceæ," or any other group of plants. Whiffs of such sedition occasionally reach the outside world, but scarcely penetrate the costly shrines wherein such deeds are accomplished. It is not the purpose of this review to make the appearance of the third sumptuous volume of this greatest of modern monographic ventures either the occasion, or the excuse, to fan into a breeze the undeniable zephyr of discontent that comes from botanists who feel that a great deal too much money is being spent on them. And they are unquestionably costly, as rumors of fourteen thousand dollars spent for illustrations alone on this third volume amply testify—not to speak of the still greater cost of exploration, cultivation of specimens and years of study. So that each of these four volumes, judged by a botanical gauge of wealth, costs a fortune, and by any gauge the four of them are perhaps the most expensive of any recent botanical publication.

The completion of this volume, however, with its twenty-four gorgeously colored plates and two hundred and fifty half-tones, does make a good occasion to reiterate that the enterprise is one that only modern conditions could have produced. For in the hurly-burly of the modern educational and scientific world, the three things that can produce such a work are hard to find, and to find them together is all but a miracle. They are knowledge and the opportunity to increase it, time and money. The authors supplied the first, bringing to their work long experience, and having, in the equipment of the New York Botanical Garden, unexampled opportunity to increase it. Freedom from the rush to produce "research" as a manufacturer might produce a foundry was made possible by the far-sighted policy of the Carnegie Institution in providing sufficient money over a long period of years. The whole enterprise is one where cooperation between great institutions and individuals, willing to sink institutional or personal aims for the sake of the work, has been a conspicuous success.

As to the botanical merit of the volumes, specialized journals will no doubt report upon that in due season. All the botanical world knows that the authors are the greatest living students of the Cactaceæ, and their studies have led them into every part of North and South America, to which the group is practically confined. As something over half a million square miles of North America is a desert country, the necessity of knowing pretty accurately the cactus constituents of this flora is obvious. These volumes are, therefore, the foundation upon which all ecological, phytogeographical and physiological work on desert cacti must be based. And in spite of gentle zephyrs of doubt, such as were noted above, the logic of their preparation and the excellence of the product must be as great a satisfaction to their collaborators as the volumes