India, in some parts of South Africa, and in Hawaii, and in South Australia.

Apart from all this, the thing which I chiefly strove to emphasize is the fact that a marine laboratory can hardly hope to justify itself as an investment if it relies solely upon the sporadic visits of biologists recuperating from a siege of instructional duty. It seems to me that there is most urgent need for a station, suitably located, which may hope to do the sort of thing which Naples did and which Plymouth is now doing, and in an environment of the most advantageous character. Convenient accessibility is highly desirable. One can hardly speak of Pacific Coast laboratories as "accessible" for the majority of American biologists; it is cheaper, or as cheap, to go to the nearer parts of Europe.

But in my letter I spoke of "accessibility" in another sense, with reference to a varied fauna. I have in mind especially the conditions at Bermuda, but I doubt not that other locations equally favorable can be found. Within less than an hour's run in a small boat from the Bermuda laboratory and in most cases closer than this, one could reach exposed or sheltered shores of sandy, muddy, or rocky type, caves, mangrove creeks, lagoons, and the locations of four or more distinct types of coral associations, each with its characteristic fauna, and all of them free from pollution; and this at all periods of the year. I doubt very much if those who have not tasted the experience of continuous biological work (not merely collecting of specimens) under such conditions have a proper realization of its possibilities. The abundance of this fauna can be appreciated only by living with it. It seems to me stupid that the opportunity to further the advancement of biology in locations of this sort has not been seized.

W. J. CROZIER

ZOOLOGICAL LABORATORY,
RUTGERS COLLEGE

A PROGRAM OF OCEANIC INVESTIGATIONS

TO THE EDITOR OF SCIENCE: Like Dr. Fisher (SCIENCE, February 23, 1923, p. 233) I was much astonished at some of the assertions made by Dr. Crozier in SCIENCE for December 29, 1922. I can not hope to add to the admirable lucidity of Dr. Fisher's comment, but it does seem desirable to add special protest against Dr. Crozier's scornful dismissal of the idea of a "program" of oceanic investigation.

I have recently read Raphael Pumpelly's "Reminiscences" and I am now reading Jordan's "The Days of a Man." From these and other sources I get the overwhelming impression that when great problems of science have been successfully attacked by these men and their fellows there was in mind a definite program (or plan) of operations. As I understand it, the failure of the brilliant Rafinesque was primarily due to lack of plan.

The general difficulties of oceanic investigation seem to me to be somewhat similar to those of astronomical investigation. I can not imagine any one seriously suggesting that the Harvard, or the Yerkes, or the Lick, or the Mount Wilson Observatory should work without a program. Furthermore, what less than a program of some sort could justify the installation of a "stationary staff," or, indeed, make it possible to intelligently select such a staff?

Those of us who work day by day and year after year in the La Jolla laboratory are frequently and vividly impressed by the constant intercrossing of lines of influence between our respective fields and between indefinite numbers of minor points in each field. A major stimulus to continued investigation is the hope that we shall some time get enough accurate information to enable us to trace many of these lines of influence through the intricate web. This hope would be denied to us if we ignored the idea of a general program for all of our oceanic research.

Indeed, I know of nothing more satisfying to the scientific staff of the Scripps Institution than the fact that a program of some sort has been followed from the beginning and that a *permanent* program now seems to be definitely recognizable. All along one of the most prominent elements in our program has been the study of natural (in nature) activities of marine organisms as distinguished from (or at least compared with) circumscribed activities of such organisms in laboratories. Several years ago both Michael and Esterly showed through a series of statistical examinations of the marine population that laboratory deduc-

tions are not safe guides to interpretation of the intricacies of life in nature. McEwen's work in hydrography has even more strikingly shown the inadequacy of laboratory deductions. The success of all such studies has been largely due to following a program of operation.

Recently, Dr. O. T. Wilson, of the University of Cincinnati, arrived here with the statement that he was not committed to any special study and that he would like to do something to fit in with our work. As a result he is making some studies of floral successions on various kinds of surfaces in shallow waters with excellent results and with pleasure surely not to be surpassed by ignoring a general idea of plan in oceanic work.

W. E. ALLEN

SCRIPPS INSTITUTION,
LA JOLLA, CALIFORNIA

DEFINITIONS IN TEXT-BOOKS OF PHYSICS

I WISH to enter a protest, through your columns, against the continued use of two common expressions in text-books of physics, both of them true if properly interpreted, each of them capable of various interpretations and therefore inexact, unscientific and pedagogically wrong.

Year after year students tell us on the authority of text-books that "efficiency equals output over input," and year after year some of those students apply the idea to forces and confuse efficiency with mechanical advantage. "Output/input" is a form which lends itself to use in concise summaries of chapters on mechanics, but it is a stumbling-block in the path of just those careless or undiscriminating students who will most readily grasp that form of words in lieu of an idea. Such an expression as "output/input" means nothing in itself, because it might refer to anything, whether work, force, mass, volume or time; it means nothing unless it is defined, and if you must define it, why not use the definition directly? To be sure, it affords joy for a moment to the heart of a certain type of student, who, after struggling through a discussion of work as applied to machines till his brain is surrounded by a semi-luminous fog, turns over a page and suddenly sees a magic formula which he follows as a bright and shining light, a will-o'-the-wisp which lures him on to worse confusion than before.

The other practice to which I object is that of putting Newton's third law of motion into the form "Action = Reaction." What does it mean standing by itself on the page of an elementary text-book? It might refer to force, or to work, or to momentum, or even to that "action" whose leastness troubles the students of more advanced texts. Even the authors of the first book in which I ever saw a clear exposition of the third law of motion took pains to place before their own clear statement of the law the statement that Newton had put it in the form "Action is equal to reaction." Apparently even they, eminent physicists and understanding teachers as they are, felt obliged to pour out a libation to the shades of the original translators of Newton in the traditional way. My own experience in trying to teach with that book as a text has been that invariably some of the men who stood in greatest need of the clear statement were sure to learn the shorter and catchier statement which meant nothing to them. In some books the old form is the only formal statement of the law given, and, in spite of good illustrative explanations when they are given, it serves fairly effectually to prevent an understanding of the law as distinguished from an ability to parrot off a formula.

I am aware of the usefulness of very concise or semi-algebraic forms for gathering up and emphasizing the important points in a chapter, but there is also a grave danger of increasing the tendency of some students to fail to discriminate between a form of words as such and the real physical idea associated with it. This tendency is present even in the case of formulas and other very concise forms of statement which are perfectly accurate and definite, and is greatly aggravated in the presence of forms whose intended meaning does not necessarily follow from the words used. Two of the worst of the latter are those two which I have mentioned, and I should like to see them relegated by common consent to outer darkness, not afforded even the meager hospitality of a foot-note.

A. B. MESERVEY

DARTMOUTH COLLEGE