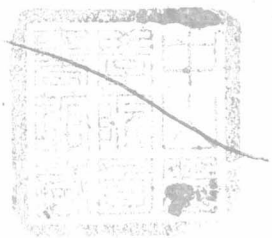
perspectives in marine biology

edited by A. A. Buzzati-Traverso

PERSPECTIVES IN MARINE BIOLOGY

EDITED BY

A. A. BUZZATI-TRAVERSO



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PREFACE

The present volume consists of the papers presented at the symposium on "Perspectives in Marine Biology" held at the Scripps Institution of Oceanography of the University of California, March 24-April 2, 1956. The symposium, which was conducted under the auspices of the International Union of Biological Sciences and sponsored jointly by the Office of Naval Research and the Scripps Institution of Oceanography, was prompted by the expansion in biological research at the La Jolla campus of the University of California made possible by a large grant received from the Rockefeller Foundation.

As a result of discussions among members of the staff of the Scripps Institution as well as with other colleagues, the conclusion was reached that at the present stage of development of marine biology it is worthwhile to discuss areas of investigation that could be most profitably attacked at the experimental level. We thought that the sweeping advances that other fields of biology have made, and the ensuing great progress we have witnessed in medicine and agriculture, are primarily a consequence of the experimental approach that has distinguished most branches of terrestrial biology. We felt confident that the time was ripe for a broad, frontal attack on the problems of marine biology thanks to our greater familiarity with the sea, to the development of new tools and theoretical approaches, and to the deeper insight into general biological problems obtained by biochemists, biophysicists, geneticists, and microbiologists. The symposium was held, therefore, in order to permit a discussion focused on possible forthcoming fields of development in marine biology rather than on a survey of past accomplishments. We, accordingly, invited to La Jolla a number of marine and nonmarine biologists to explore problems within the limits of marine life that are ready for experimental attack and to outline definite plans for research.

The discussions following the presentation of the papers are here recorded according to the written contributions of the participants in the symposium. At the end of the volume a summary is presented of the most significant ideas and plans for research that were discussed during the conference.

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PART I

ECOLOGY

PARAMETERS OF THE MARINE ENVIRONMENT

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We are asked to suggest what might be done "to reach a better understanding of the relationships between organisms and between them and their physical environments" and how we may bring some of the newer and more exciting experimental techniques to bear on these problems.

It seems to me that the first essential step is largely a technological one and I fear therefore that my suggestion is both mundane and naïve. It calls for what we might term a "school of plankton husbandry," or for a concerted effort by a team of workers to establish in our laboratories a series of oceanic animals which are amenable to experiment. We require, as it were, the marine equivalents of the guinea pig, the mouse, and the fruit fly, and I believe that without them our progress will remain slow and insecure. Our studies will continue to pose problems rather than provide solutions.

There are two main reasons for this: (1) until we can take a closer look at the animals and learn more of their intimate biology our field work will lack objectivity; (2) unless our laboratory work can become more realistic, it will not necessarily be relevant to the behavior of the animals in their normal habitats.

It is trite to reiterate the inevitable difficulties that face the ecologist who seeks to explain from his field observations alone why the animals in the sea are distributed as they appear to be or behave as they seem to do. He cannot observe the movements and behavior of his animals while they are taking place. He must first catch and kill his subjects and then deduce their activities in retrospect. He has no control over the variables and must extract his data from the normal sequence of events. Indeed, one of the few courses open to him is the arduous one of extending his observations over a long period in the hope that he may see cycles or trends in the quantitative or qualitative distributions. This achieved, he may hope to find also some parallel variation in an environmental factor which can be collated. If he is so fortunate, he can then postulate interrelationship between the two and await another long series of observations to support or contradict his hypothesis. But regardless

of the ultimate significance of his correlations he is still far from establishing causal relationship. By the nature of his experiment, his conclusions are not amenable to rigorous proof. Unless he can control the environmental parameters in repeatable experiments, he can but argue the plausibility of the hypothesis by analogy with the accepted tenets of terrestrial ecology. He is further handicapped because many of the physical parameters of the natural environment are themselves interrelated; rarely can they be separated in any given locality or in any time series. If he attempts to overcome this limitation by comparing the behavior of animals in a variety of areas, then he is equally frustrated; he has at present no means of telling whether the animals in the different localities are comparable ecologically.

We are often reminded that, in the more exact sciences, the approach to a problem is in three phases: (1) the observation of the natural phenomena; (2) the formulation of a hypothesis to account for them; and (3) the experiment to test the hypothesis. In such an idealistic process our problem starts in the field with the collector and ends in the laboratory with the experimentalist. But we may well founder early in the sequence. We cannot hope to postulate useful hypotheses about the causes behind the distributions of animals unless we happen to measure also the relevant parameters of the environment. And, as I will try to explain below, there is some evidence that we at present are failing to do this. Or, as an extension of the argument, we may conclude that, until we can learn more about the environmental requirements of the marine fauna, the extensive and costly programs of systematic collection will fail to achieve their potential value.

I would not imply that programs of field sampling should be stopped or reduced in scope. On the contrary, they will ultimately provide the framework on which the mosaic of knowledge can be laid. We should, therefore, ensure that our records of long- and short-term fluctuations in natural populations are as comprehensive as possible. Rather I would suggest something of a reversal of the classic approach. We require, first, more realistic experiments in the laboratory designed to test the relative importance of the various environmental factors in order to ensure that we are measuring those which are significant in the sea.

In the past, there has been the tendency to presume that the gross physical and chemical parameters are the all-important ones. Temperature and the availability of inorganic nutrients have been singled out for particular attention possibly because of following closely the analogy with earlier conclusions in terrestrial ecology, or because of the limited selection of parameters available to the ecologist, who is usually forced to work with observations taken by the oceanographer for his own very different objectives. However, there is now a growing weight of opinion that other less obvious factors play the prime role in determining many of the variations of marine populations both in space and time. It seems that, although there is an over-all pattern to geographical distributions and seasonal productivity which is dictated by these gross physical and chemical conditions, superimposed upon this pattern are highly significant variations stemming from other causes. Whereas in the open ocean temperature may set the limits of distribution of a species and may restrict its breeding season to a certain part of the year at a given place, it does not necessarily influence directly the local success or failure of that species from year to year. This point was nicely made by Loosanoff and his colleagues (1955) when they showed that there was no consistent agreement during eighteen seasons between the number of larvae