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Hypothesis testing and rigorous statistics as criteria for marine research proposals*

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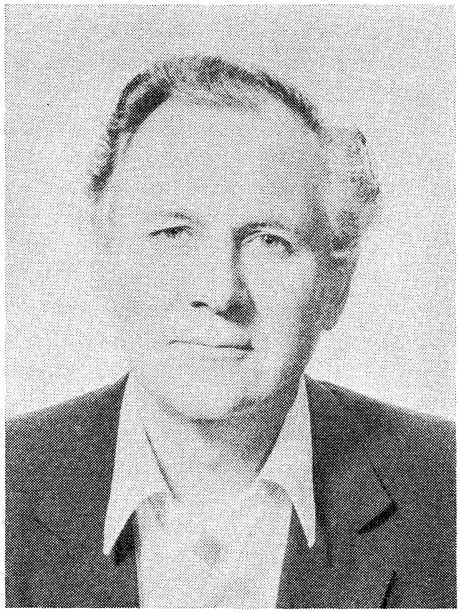

Oceanography is a young science, too young to become bureaucratized in its approach towards a better understanding of the world's oceans. In spite of this, there has recently crept into the minds of many funding agencies the need for scientists to define a testable hypothesis and to accompany applications for money with proposals which will yield statistically valid results. For the administrator, the submission of both a testable hypothesis and the proposed use of rigorous statistics gives the application a ring of scientific authenticity and veracity which can be readily defended to those who are concerned with the taxpayers' dollars. Unfortunately the approach may not yield *new* discoveries about the oceans.

I do not want to suggest the elimination of grant proposals which outline scientists' intentions. Rather my plea is to assure that researchers may probe the ocean depths without necessarily having to formulate their plans into some preconceived idea (the hypothesis) of what they expect to find. While fisheries data are collected for many purposes, it has perhaps been their endless use in order to verify the hypothesis of a "maximum sustainable yield" that has been one of the most oversold chronic forms of hypothesis testing (LARKIN, 1977). It has resulted in very little being known about long term changes in fish populations relative to the large amount of money expended. In contrast, I believe that the recent flurry of papers (e.g., HARBISON *et al.*, 1978) on the massive occurrence of gelatinous zooplankton in the sea has been largely the result of developing a new way to look at the ocean (i.e., open ocean SCUBA diving). This was not the result of any testtable hypothesis and it did not require rigorous statistics for verification. The importance of this discovery may in fact have far reaching effects on fisheries science since in many cases the "jellies" are competing for the same food resources as commercial fish. The description of populations of large deep sea fishes and scavengers (e.g., ISAACS and SCHWARTZLOSE, 1975) and the discovery of the hydrothermal vent communities (e.g., EDMOND, 1982) are additional recent examples of hitherto unknown phenomena.

Many theoreticians and administrators in marine science have long abandoned the essential element of field observations. What we really need to know about fish populations is

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unlikely to be revealed by more theoretical models on catch statistics, nor is it likely that nature can be contained inside a computer. The aim of all *basic* research should not be to produce statistically valid results but to study what is new and previously undiscovered. One fallacy in the "statistically valid" approach is that it emphasizes the reliability of a result which occurs 19 times out of 20, or whatever other odds you choose. Nature, on the other hand, seems to succeed, *against all odds*. The evolution of new species and life itself are now being described as improbable events. The scientist who finds himself looking at a previously unrecognized phenomenon in the ocean and describing it for his colleagues is analagous to the chance occurrence of a new species in evolutionary time. In other words, it is the anomaly that has been the true driving force for new science and not hypothesis testing and rigorous statistics. The latter should be thought of more in the role of technology—an area of science dedicated to improving the agreement between facts and currently held dogma. In this area, a workable hypothesis and rigorous statistical validity are necessary for the solution of practical problems. It is, however, very certain that this process did not lead us from the age of Leonardo de Vinci to lasers. Rather, a few keen observers of nature pointed out unique phenomena which had hitherto gone unnoticed by the rest of mankind. The contrast between preconceived notions about nature and natural phenomena is succinctly given by KUHN (1970) in the statement

> "Unanticipated novelty, the new discovery, can emerge only to the extent that his (the scientist's) anticipations about nature and his instruments prove wrong".

It is a common observation that children ask innocent and revealing questions. It is no coincidence that their eyes, seeing the world for the first time, see it differently than adults. In marine science, much encouragement needs to be given to the new approach, the original idea, the astute observer, the novel question and pioneering instrumentation—these are the 'eyes' through which science advances. The successful progress of marine science will be accomplished through seeing differently the complex interactions of nature afloat, rather than simply through the application of statistics to hypothesis testing. The latter fulfills an important role in science but it is more akin to the role of an engineer in our society than to one who discovers (*sensu stricto*—one who reveals something not previously seen).

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