Letters

Biology Is Not a Totem Pole

A visit by an eminent American ecologist, Eugene P. Odum, rekindled an old fire, and stimulated the uncovering of an outline that had been gathering dust for nearly 2 years. The outline was constructed originally after I heard an important speaker at the AIBS meetings at Purdue describing ecology as being near the bottom of the totem pole of biology. I was incensed, not so much because it was my first love, ecology, that lay near the bottom, but because there was a totem pole at all in biology.

The development of biology has been similar to that of science in general. Any science begins with observation of nature and classification of its characteristics. Later comes experimentation and finally the development of generalizations and of theory. Physics and chemistry evolved as exact sciences much earlier than biology did; the latter remained largely in its descriptive and taxonomic stage until quite recently (with the important exceptions of the concepts of evolution and the basic generalizations in genetics). Similarly, within biology, descriptive biology (including descriptive ecology), genetics, and molecular biology have evolved farther and more rapidly than experimental and physiological ecology.

It is natural for a person to think of his own specialty as being the most important. On the other hand, many objective investigators see their own studies as merely important queens, bishops, and pawns on the chessboard of science, whose movements both are determined by the positions of the other chessmen, and at the same time are determiners of the possibilities of movement of the latter. A very few Lamarcks and Darwins can see the game as a whole and generalize broadly from the positions of all the pieces. Science differs from chess, however, in two basic ways: (i) no master-mind directs the game; this is directed by the decisions of the chessmen themselves, either individually or collectively, and (ii) the object of the game is to aid rather than to prevent the other chessmen from moving in the direction most favorable for them.

The conviction that one's own field of work is the most important is never more than partially correct, and the belief that the work of others in different fields is unimportant is usually wrong. Of course, particular subjects (at least for the time being) may have been milked dry of most of their possible information; others have been proved to be based upon false premises. For example, it would be a mistake today to set a graduate student to work to ascertain whether blow-flies originate spontaneously from rotting meat, and the existence of a homunculus within the human sperm has been shown to be the figment of overactive imaginations. But who in 1870 would have dreamed that the hypothesis of spontaneous generation that had just "finally" been laid in its grave by Pasteur would again become accepted and promulgated in the 1960's to explain the geochemical origin of life? And who would recognize the homunculus dressed up in its new genes?

As in many other areas of human endeavor, biology has developed unevenly. Partly this has been from necessity, for description and classification must come first, and the initial things studied are the simpler ones. It has also been a matter of popularity (even at times of fad) or of tradition. The proponents of each movement in biology have tended to think of themselves as doing the most basic work ever done. Jacques Loeb and his contemporaries, in their discovery and study of parthenogenesis, thought they were investigating the very essence of life itself. The students of the biological effects of heavy water in the 1930's were certain that they had discovered one of the most intimate secrets of old age and death in organisms. Similarly, for a time, problems of mitogenetic radiation captured the imagination of biologists, and many were convinced that they were approaching a complete understanding of problems of growth. Today some DNA molecular biologists may be certain that when their task is completed there will be nothing of importance remaining to study in biology. A number of ecologists (who *also* are humans) may feel that the threads of knowledge they are gathering into their hands today will allow them tomorrow to explain clearly, for all to see, how living things utilize their anatomy, genetics, physiology, DNA code, and messenger RNA as integrated beings in an environment that includes many and diverse other beings, similarly integrated.

Along with many other areas of biological study, the fields mentioned above have contributed, or continue to contribute greatly to our knowledge and understanding of living things, even when the ideas themselves may have been wrong. Biological knowledge, however, has not been and will not be advanced indefinitely by any one field of study alone, but only by all fields together. It is natural and correct to exploit major breakthroughs in research wherever these may occur, but advances will be limited if any area gets too far ahead of other fields, including those apparently most foreign to the viewpoints of one investigator or another. It is also important to remember that breakthroughs never occur in a vacuum, but depend upon years of hard, unspectacular work by many relatively obscure investigators, often working in unpopular and even unpromising fields.

There should be no intellectual "peck-order" in biology, yet one exists. Today, in the minds of some people, molecular biology and genetics are the alpha hens; ecology ("the wave of the future") and taxonomy and anatomy ("the waves of the past") are near the bottom of the list, so that students, promotions, working space, salary increases, and grants tend to go one way more than the other. To a considerable degree taxonomists and anatomists have perhaps brought disrepute upon themselves by a certain narrow and static approach to biology, and by a disdain for function. Many ecologists likewise have not been free of static viewpoints in spite of the fact that

their field of study is potentially the most dynamic of all. Too often they have been ignorant or even contemptuous of process, and to all appearances, involved mainly in the invention of complex and rather meaningless terminologies. But these taxonomists, anatomists, and ecologists do not express the essence of their specialties any more than does the molecular biologist who thinks all plants and animals are more or less identical except that they contain slightly different DNA's, or than the physiologist who has no knowledge of or interest in the functioning of the parts of his organism in any environment other than the laboratory incubator or the test tube.

For the society we live in to function and advance we need every encouragement, not only to biologists and other scientists, but also to musicians, artists, writers, sociologists, engineers, plain every-day working people, and others, with no peck-order intended. Similarly, all pertinent fields in biology must be encouraged if we are to maintain a healthy science and continue on really to understand and control the living world around us.

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Western Europe: Research and Development

I agree wih your general observations on the rapidly advancing state of research and development in Western Europe [*Science* **140**, 773 (17 May 1963)]. However, some of your comparisons regarding the present situation are probably not valid.

Commerce alone does not determine the relative technological position of nations, particularly when one of those nations has vastly different standards of external and internal modes of commerce. Your values for research costs in Europe are much too low. Perhaps the figure of one-fourth represents the situation as it was quite a number of years ago.

With respect to most of the parameters—size of the effort and generation of new ideas—the United States can certainly be said to be ahead of any nation in Europe. However, the Europeans have the advantage that they are seriously examining the question of the proper level of research and development expenditure in relation to eco-