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OBJECTIVITY IN BIOLOGY

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When I was an undergraduate I once had a conversation with a graduate student who was experimenting on rats. During the course of our discussion I pointed out the evidence that had been accumulating that the orientation of Hymenoptera depends upon visual memory. "Well", said my companion with some scorn, "I have always considered such explanations anthropomorphic". When I asked him to supply an explanation of his own, he said, "it is probably kinesthetic". I thought at the time that, had the young man been more familiar with wasps and less exclusively concerned with rats, he might have reached a different conclusion. And besides, his statement did not, from his standpoint, eliminate what he called "anthropomorphism", since even in falling back upon kinesthesis the explanation would be incomplete without the postulation of some kind of memory.

The history of biology is very largely a history of attempts to eliminate superstitition and folklore. Even so good a naturalist as Romanes was a romanticist. We are probably in agreement that romanticism, anecdotalism, and metaphysical teleology are out of place in the laboratory. The extreme mechanists cannot be blamed for taking exception to some of the sentimental trash that has come out in print concerning biological processes. Efforts to purge biology of humanistic and even supernatural impedimenta have resulted in many casualties on both sides. Some of us can still remember the "unnatural history" we read in our childhood.

Today we have exchanged those traveler's anecdotes for what is called "ecology", and the ecologists are so afraid of being thought unscientific that they have invented their own barbarous jargon which no one can understand. And for the past few years a battle has been waged in education between the advocates of "nature study" and the "objectivists". The former claim that, considering the nature of child psychology, it is impossible to teach elementary pupils science impersonally and objectively; the latter that unless science is taught objectively it is not science.

It seems to me that the question of objectivity is relative. No experimental result, even in chemistry or physics, is absolute; and certainly the arguments between students concerning the significance of results indicate that interpretation is not.

Nor dare we conclude that, because we can apply physical and chemical measurements to biological phenomena, vital processes can be explained only on the basis of physical and chemical concepts. Physical concepts are themselves in a state of flux. Twenty-five years ago all physical phenomena were referred to the concepts of conservation of matter and of energy; today, if electronic physics has demonstrated anything, it is that we have no absolute definitions for either matter or energy, and that causality is comprehensible only on a statistical basis. Even the "laws" of Boyle and Arrhenius are still undergoing modification.

For the so-called "laws" of science are not edicts from the Creator. They are generalizations from experience - an experience ever changing but never perfect. Objectivity, even in physics and chemistry is a goal never quite achieved.

"Ah, but a man's reach should exceed his grasp,
"Or what's a Heaven for?"

In writing that, Browning came close to feeling truly the scientific spirit. Indeed it would be a dull world for all of us were the universe so ordered that the predictability of all phenomena in all their phases was completely within our grasp. And it is just as well, perhaps, that no science is ever likely to be a finished product.

We are so used to generalizations in science, and so familiar with reliable predictions based upon such generalizations, that we often overlook the fact that generalizations are merely approximations. If we are not careful this is likely to lead to oversimplification of problems. Now oversimplification is a dangerous technique in biology. The Law of Parsimony is admissable only if and when it does not lead us astray. Indeed the history of biology is littered with the derelicts which ran upon this Charybdis in an effort to escape the Scylla of confusion. One need only recall Haekel's "Biogenetic Law", Loeb's "Forced Movements", and Watson's interpretation of thought as speech movements; or, to go further back, spontaneous generation and epigenesis.

We must also avoid the conviction that we have explained a process by inventing a name for it. Ecology is filled with meaningless jargon. So is physiology. Who dares attempt giving a satisfactory explanation of "instinct", "chemotropism", or "differential sensitiveness"? Physiologists are now inclined to regard reflexes with suspicion. Is there any real need to substitute the word "chemotropism" for "olfaction" or "photo-sensitivity" for "vision"? Does not the employment of such peculiar terms lead to confusion?

The Mechanists, especially Jaques Loeb, introduced such terms to avoid what they considered metaphysical interpretations of natural phenomena. But in so doing they committed an error and an oversight: the error was to suppose that a term applied to the behavior of a few species of organisms necessarily applies to that of all others; and the oversight was that no matter how careful one may be to define a term, others are bound to use it differently. Hence the multiplication of terms often confuses rather than clarifies the issue.

Behavior is too elusive to yield itself to simple generalizations for a variety of phenomena exhibited by a wide range of organisms in various circumstances. Does one suppose, for example, that the behavior of a hungry man entering a restaurant duplicates that of phagocytes attracted to an area of inflamation? Apparently some do, for as late as 1925, one psychologist referred to the attraction between the sexes in human beings as "erotropism"--on the assumption, presumably, that the reactions of adult human beings duplicate the behavior of the kind of germ cells they contain.

To be sure, all living processes are based upon certain common chemical and physical principles. But to admit this is very different from insisting that by substituting a few chemical equations, mathematical formulae, and physical constants for the organism we have adequately described it. We cannot even explain the simplest physiological process by such juggling. It is not to be wondered at that not long ago a biochemist prefaced his book on blood chemistry with the challanging assertion: "This work is teleological". Nor did one find in the book any speculative discussion on the nature of the Deity or the immortality of the soul. Apparently all the author intended to imply is that the chemistry of the blood is self-regulatory.

It is this self-regulation that distinguishes the living organism from non-living systems; making it impossible to adequately describe the former on a basis of chemistry and physics alone. To refer to man, as Herrick has done, as "The Thinking Machine", is, biologically speaking, a contradiction of terms. A machine does not think - that is why, by definition, it is a machine. No organism is "automatic" in the sense that it exhibits invariable behavior. Indeed, Goldstein, in his recent book "The Organism" insists that a living system never reacts except as a whole. Even Sherrington is careful to point out that the scratch reflex of dogs is not a single reflex, nor does its initiation always depend upon a single stimulus.

Even the mechanists occasionally admit that one cannot define an organism by its parts, nor its behavior as a mere system of reflexes. Bethe, for example, states that the adjustment of the simplest organism after injury is an adjustment of the whole organism and proportional to the injury. It is true that he attempts to define this mechanistically; although in that he is not entirely successful.

Not only does every organism differ from every other; variation in adaptive behavior may be observed even in organs. Recently I have been observing the pulsation of immature ovaries in **Lucilia sericata** Meig. The rate may vary from one pulsation in thirty seconds to one per second. Sometimes, when a fly is opened, the ovaries pulsate feebly for a few seconds and then stop; yet often, under what appears to be identical conditions, they pulsate for hours, and even do so when kept in vitro in Ringer's Solution. The same kind of stimulus may, in one case accelerate pulsation; at another time it may entirely inhibit it. Or it may stop one ovary and accelerate the motion in the other. Although eserine generally accelerates the action, it may inhibit it in one or both organs. It may stop the organ in either a contracted or relaxed position. Similar variations are observed when acetylcholine is used, either alone or in conjunction with eserine. Nicotine may stimulate the ovary to pulsate rapidly for several minutes and then stop, but quite as often it slows the rate of pulsation,which then continues several hours. Heat and concentrated salt solutions generally increase the rate of pulsation, but they may inhibit it entirely or in part.

Similar results are obtained in the case of the digestive system. Seldom, in two consecutive specimens, are corresponding sections of the gut active. The addition of warm water may initiate pulsation in the rectal sack, mid-intestine, or crop - or it may have no effect whatever.

No doubt the activities of the whole organism are, in general, adaptive, but this adaptiveness varies qualitatively as well as quantitatively. A fly recovering from the toxic effects of eserine may recover in a variety of ways: the wings may become active first, or the legs; or both may become active at the same moment. Poisoning from strychnine sometimes has a symptomology resembling that of eserine; or the symptomology may differ markedly in the two cases. And this is as true of the frogs as of flies.

One of the most difficult of scientific techniques is to determine whether one is dealing with a result or a conclusion. It is, strange as it may seem, difficult to draw a distinct line between observation and meditation. All one can do is to apply cautious logic to the problem. The best interpretations often come, not by a slavish regard for results, but through a careful selection of the most significant results. We must be careful, of course, not to draw conclusions from insufficient data. Neither can we afford to ignore statistical probability. We cannot, as one worker is said to have done, draw conclusions from four cases in a hundred; that is if all of the cases were carried on under essentially similar conditions. We can reduce both labor and the margin of error by making certain that our experiments are as carefully controlled as conditions will permit.

The most efficient experimentalist is the one who observes with an open mind; who, without bias, experiments, not to substantiate a prejudice, but to establish a fact. If one must generalize, let him generalize as if he were the first and last observer in existence, uninfluenced by preconceptions. It is difficult to do this, but it is the only way to avoid the mistake the mechanist himself makes when he accuses the Medieval Scholastic of doing the same thing: that of trying to demonstrate only what one wishes to prove. Deterministic mechanism may be quite as much of a religion as Calvinism; in spite of their contrary conclusions, both are based upon the same system of metaphysics. And metaphysics, though valuable elsewhere, is out of place in a laboratory. The advice which Francis Bacon applied to readers, applies equally to the laboratory experimentation. "Read", he advised, "not to accept, nor to refute; but to weigh and consider."

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