Letters

Nature of Science

Professor Simpson's recent article "Biology and the nature of science" [*Science*, **139**, 81 (11 Jan. 1963)] argues vigorously, if somewhat loosely, for consideration of biology as standing "at the center of all science." Much of what he says is provocative and many important questions are raised. We wish to call attention to one issue which requires clarification.

Simpson contends that "why?" questions are legitimate in biology. However, he does not wish to "imply a dualistic or vitalistic view of nature." He presents the following argument. (i) Living and nonliving systems alike are material and physical. (ii) Living systems are different from nonliving systems because "they have been affected . . . by historical processes that are in themselves completely material but that do not affect nonliving matter, or at least do not affect it in the same way." (iii) The result is that "[living] systems [are] different in kind from any nonliving systems."

First, it must be pointed out that only the dependent clause of the second statement makes this a reasonable argument. If the historic processes did not affect nonliving matter at all, there would be no way in which living matter could arise from nonliving matter, as Simpson maintains that it has.

Simpson affirms that some historical processes do affect nonliving matter. He writes, "We would expect [deviations from the ideal gas laws] because molecules of gas, like flies, are real individuals which, however alike they are in other respects, have different histories." Also, "In fact, a neglected historical component also affects many physical laws as in the example of the histories of the individual molecules in a gas." This position is not shared by most students of kinetic theory.

Simpson does not clearly define the qualitative factors which differentiate living systems from nonliving systems. He tells us that they are different in that we can ask scientifically meaningful "why?" questions of the former. Such a question can be asked "when it elicits an objectively testable answer." He takes adaptation as an instance of a process to which "why?" questions are applicable and asserts as a fact that "the hand of man . . . *is* made for grasping."

But is this a fact? The answer depends on the meaning of the phrase "is made for." (He points out a similar ambiguity in the expression "in order to.") The words can be taken as purely descriptive. Darwin seems simply to describe the conformity of structure and function in monkey hands. In this case, adaptation may be discussed in terms of "how?" questions and answers which differ in no more than complexity from the question-answer sequences of the physical sciences. If the facts of adaptation are the description of adaptation, then there is no difference in kind between an adaptive system and a collection of gas molecules.

Alternatively, "is made for" may refer to purpose. But then the question arises whether a proposition containing this expression is scientific. Simpson agrees with Campbell that science involves "those judgments concerning which universal agreement can be obtained." It is obvious that even among competent observers, no such agreement concerning purpose has been obtained. Behaviorists, other mechanists, and "scientific" vitalists disagree concerning the specific purposes of particular adaptations and even whether adaptation is purposive at all. Purpose must be inferred from the evidence; it is not observed. Since little agreement has been achieved, discussions of purpose, however informative, fail to satisfy the criterion of providing "self-testing relationships" that is required by Simpson's definition of science.

Simpson is in this dilemma because he considers purpose a fact. He says, "the purposeful aspect of organisms is incontrovertible," although it surely has been both controverted and controversial. His defense of purposes together with his discussion of the effects of historical processes, leads him to consider two possibilities: (i) such processes affect both living and nonliving systems, as exemplified by his discussion of the gas law and (ii) they affect only living systems. There are two other logical positions. It is blatantly ridiculous to argue that (iii) historical processes affect only nonliving systems, but it is worth considering that (iv) they affect neither kind of system.

The view that any system could be completely described without reference to purposes, neither leads to difficulties in defining the subject matter of science, nor does it exclude biology as a legitimate field of inquiry. The "incomparably complicated" nature of living systems will still leave biology one of the most exciting and exacting areas of study.

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Simpson's article was most stimulating, but I was disappointed in its complete rejection of the "Greek way of thought" and feel that Simpson has a distorted view of the Greeks.

He quotes only Plato and thereby seems to relegate all Greek thinking to an idealistic wastebin. He also mentions Aristotle once or twice but only in conjunction with the Renaissance thinkers who rejected him. This seems to me the second distortion—a distortion of Aristotle, whom it views second hand through the thinkers who froze and dogmatized his conclusions and then through their successors who in turn rejected the dogmatism of the first group.

My impression is that the original and therefore still important and interesting Aristotle was not only a biologist like Simpson but was, for his time, an astute observer. Contrary to what Simpson seems to be saying, Aristotle was not trying to twist his findings into an a priori system but was attempting to find out the facts on which to construct proper generalizations.

Granted, some of his conclusions are screamingly wrong or funny. The fact remains and I believe that Darwin, for example, recognized that Aristotle made many sound observations. He believed, after all, that knowledge arose from an ascending process of sensation, memory, and experience leading on to science and art.

True, Bacon and Galileo rejected teleology as a scientific concern and the overhasty generalization of many Aristotelians. I think that they were more Aristotelian than they suspected. For that matter, does Simpson's exalting of function differ so very much from the Aristotelian concept of final cause?

Perhaps this is quibbling to the practical scientist, but it seems to me that such denigrating of the Greek thinkers does a disservice to the general culture of the scientist by severing his continuity with the origins.

What we need, rather, is a discriminating appreciation of the ultimate roots of the scientific method and its purpose. I would like to speak up for those who believe that this implies knowledge and use of at least some elements of the "Greek way". To deny any relevance to these ancient innovators is to deny ourselves access to the early confrontation and solution of problems of method and observation which are still with us.

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. . . Simpson develops in biological science a place for teleology based upon the phenomenon of natural selection and its inevitable result, adaptation. This has been done many times in the past but, the "big truth" must be said over and over again before it achieves general acceptance. In discussing this same point, however, Thompson (*1*) warns, "So long and so far as fortuitous variation and the survival of the fittest remain engrained as fundamental and satisfactory hypotheses in the philosophy of biology, so long will these satisfactory and specious causes tend to stay severe and diligent enquiry . . . to the great arrest and prejudice of future discovery."

Had Simpson defined science in simple terms for example—science is an attempt to describe the universe, and the events occurring in it, quantitatively in terms of four parameters: space, time, energy, and mass—recalling Thompson's warning at this time would have little point. Another reason that suggests that a red flag be raised is the recent recognition of a special program in biology entitled "Graduate Studies in Evolutionary Biology" at Harvard. There is little cause to fear that biology at Harvard will go down the primrose path of dualism, but lesser lights who look to Harvard for leadership should be aware of such a possibility.

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Reference

1. D. W. Thompson, *On Growth and Form* (Cambridge University Press, Cambridge, England, 1952).

In his discussion Simpson maintains that biology is basic to all the other sciences, a conclusion with which many biologists will agree. Incidental to a discussion of teleology, however, he states that I have proposed an explanation for it that is "quite outside the legitimate field of science." This criticism has been made by others who have not troubled to read further in what I have written than the inflammatory word "purpose."

The teleology with which Simpson is chiefly concerned is that involved in evolutionary adaptation, whereas my own interest has been in something quite different—the biological basis of mental phenomena. It is well known that the self-regulatory unfolding of characters during organic development tends to move persistently in the direction of a precise end in structure or behavior, even though its normal course may be considerably disturbed experimentally. There is in the egg, I believe, and in the organized system of the embryo, a developmental norm toward the realization of which development proceeds. This is a primitive purposive activity and involves nothing mystical or vitalistic. It is much like the purposive homing of missiles or the purposive activity of programmed calculating machines or other self-regulating mechanisms [see U. Neisser, *Science* **139**, 193 (1963)]. I have suggested that this developmental end-seeking is the basis of psychological end-seeking, and that in man it is felt subjectively as the drawing power of motivation or the consciousness of desire or purpose. This suggestion attempts to provide a biological foundation for a monistic interpretation of the relationship between body and mind that regards them as two aspects of the same basic fact—biological regulation or organization. It obviously does not "explain" mind but it makes a statement of the problem that may be useful in determining the relationships of biology to psychology. Such an attempt to find a biological basis for mind has often been made before. It may be attacked as bad philosophy, and some of the conclusions drawn from it are opposed by many, but these are philosophical rather than biological matters. There certainly is such a thing as the science of mind, and the suggestion that biological self-regulation is an aspect of mind violates no scientific legitimacy.

The ideas proposed here obviously have nothing to do with adaptation, teleological or otherwise. I have stated repeatedly that these developmental and behavioral "purposes" do not imply the fulfilling of any "need" on the part of the organism, or the existence of any inherent tendency to adapt itself to its environment that will lead to the production of structures or behavior favorable for its survival. Many goals when attained are deleterious. Organisms displaying them are eliminated by natural selection, and adaptation ultimately results.

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The preceding comments on my essay usefully amplify some matters and are welcome. Some of the points brought up, however, do require brief clarification.

The Heins' first objection involves a misunderstanding. Historical processes, of one sort or another, certainly affect all matter, both living and nonliving, and I exemplified both. The "that," rather than "which," in the cited clause syntactically indicates that particular processes, not historical processes in general, are meant. Syntax apart, I would have thought this clear in the context. One may or may not agree with my conclusions, but there is no fault in the logic on this point.

It really is incontrovertible that all organisms have purposeful *aspects* in the sense, explicit in my essay, of adaptations serving needs. I did not imply and do not believe that this generally involves plan, will, or even feeling. The belief imputed to me by the Heins that abstract, unqualified "purpose" is a fact is not stated in my essay and is not my opinion. That organic adaptations serving needs and *in that sense* purposive in aspect are universal in organisms is a fact obvious to anyone who ever looked at an animal or plant. The question is not whether they exist but whether they can be explained, as I maintain they can, in a scientific and *nonteleological* way.

"Why?" is indeed an ambiguous question, and I did *not* contend that it is "legitimate in biology." I did not use it at all in this connection. The pertinent question, and the one I did use, is "What for?"—that is, what useful function is related to the characteristics under study? Such functions and their usefulness to the organism can be directly observed and tested. In this context it is thus sensible and fully scientific to say, for example, that green leaves are for photosynthesis, and in this formulation classical teleology is not involved at all.

The comment by Portz helpfully adds to but does not contradict what I wrote. In an attempt to cover so much in one essay, it obviously was not practical to characterize the whole Greek contribution. Singling out Platonic idealism and Aristotelian teleology as having had a major impact on the subsequent history of philosophy and science follows much historical authority superior to my own. Surely we all agree with Portz that not all ancient Greeks were Platonists and that Aristotle had solid accomplishments not directly related to his views on teleology.

I would prefer a somewhat different form of expression, but I find myself in agreement with much in Sinnott's comments. In his present letter, however, he of course has not covered all the ground traversed in long earlier studies, notably in his thoughtful and beautifully written books *Two Roads to Truth* (1953) and *The Biology of the Spirit* (1955). (Incidentally, I have not been frightened away by the word "purpose" and have carefully studied those and other works by Sinnott.) In them he did plainly express the opinion that the apparent purposefulness of organisms has not been adequately explained by science and that another approach, involving religion, is likewise necessary. The statement in his present letter that differences of opinion in this respect are not biological matters reflects just the point I was making when I mentioned him as a scientist who has gone outside the field of science in seeking explanations of some phenomena.

D'Arcy Thompson's forebodings, cited by Wharton, have not proved to be justified. When Thompson wrote *On Growth and Form* (first published in 1917) the explanatory theory of adaptation in its current form did not yet exist. It was just beginning to be clear when he revised that book, but he was completely unfamiliar with it. (The revision was published in 1942, Thompson's 83rd year; the date 1952 cited by Wharton is perhaps that of a reprint.) All of us who knew him hold D'Arcy Thompson's memory dear and enjoy and admire his great book, but he was not a student of, or even interested in, 20th century evolutionary theory. It has certainly neither arrested nor prejudiced discovery—quite the contrary!

Wharton's suggestion that I should have adopted a simple definition of science is puzzling. His own definition is both longer and more technical than the one I gave. Moreover, it virtually excludes biology, as such, from the field of science. It thus illustrates one of my points: the tendency of some biologists to abrogate their own field in favor of physical science.

The relevance of Wharton's remarks on Harvard is still more puzzling. The populous biological community here has special provisions of one sort or another for instruction and research on such diverse subjects as electron microscopy and orchids, to name only two. Yet we do generally manage a fair degree of integration and cooperation. That we can provide some support for graduate study in evolutionary biology, as well as in almost all other aspects of the life sciences, surely should be cause for congratulation rather than alarm. I do not have the slightest idea what "the primrose path of dualism" may be, or what it has to do with any of this, and so cannot speak to that subject.

Finally, I must express regrets that I have not been able to fill all of the requests for reprints of my article.

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Choice of a Cell System for Vaccine Production

The paper on "Continuously cultured tissue cells and viral vaccines" [*Science* **139**, 15 (4 Jan. 1963)] is open to several criticisms. The statement, ". . . continuously cultured cells eventually develop characteristics suggestive of malignant change" is particularly debatable.

This statement recurs frequently in the committee report and may well apply to cultured mouse cells (*1*) but its antithesis has been demonstrated for human cells in these and in other laboratories (*2, 3*). Alterations in vitro to heteroploidy may or may not be associated with malignancy but human cell strains are readily available which have not altered. The use of such unaltered human diploid strains would entirely circumvent this problem.

For human cells grown in vitro to develop ". . . characteristics suggestive of malignant change" (alteration) is a rare and fortuitous event. Almost all cell populations cultivated in vitro from primary tissue terminate within periods of time varying from a few days to about 12 months. Of the perhaps thousands of opportunities to detect alterations in normal human cells in the last 15 years, only 50 successes have been reported (*4*). To our knowledge, no indefinitely cultivable cell population exists which lacks aberrations in chromosome number or form. Until the work on morphologic and karyologic alteration by viruses (*5*) and its recent extension to human cells (*6*), no one had reported conditions under which diploid (unaltered) cell populations could be *reproducibly* altered in vitro to heteroploid cells. For this reason the finding that oncogenic viruses such as polyoma and SV_{40} are capable of reproducibly altering normal diploid cell populations has proved to be a significant development in the field of viral oncogenesis. The aforementioned statement ". . . continuously cultured cells eventually develop characteristics suggestive of malignant change" implies the inevitability of malignant change. If alteration were a certainty, then the demonstration of these viral-induced alterations would be trivial. Thus, the fundamental distinction between the two kinds of in vitro cell populations (unaltered and diploid vs. altered and heteroploid) has been largely ignored (*3*). This is evident in the report when the three types of cell cultures to be considered are defined. Human diploid cell populations which, in our opinion, have the greatest potential for use in human virus vaccine production (*7*) would, by those definitions, be excluded.

When human diploid cell strains *are* considered in the report, the criticisms of their use for human virus vaccine production are, with one exception, without foundation. In referring to the human diploid cells the report states that ". . . the resultant cell populations are heterogeneous, which means that