# Letters

## Biology and Metaphysics

I read Fred E. Hahn's letter in *Science* [**141**, 1240 (1963)] referring to my advocacy of a Society for Holistic Biology [*ibid.* **140**, 1362 (1963)], and I am now willing to champion a Society for Accurate Interpretation as well.

I am not now, nor have I ever been, a vitalist, if vitalism is taken to mean that there is a "directive force . . . which is metaphysical in nature and thus evades scientific comprehension," to quote from Hahn's letter. I made this clear in paragraph 3 of my original letter.

I do believe that various disciplines study matter at different levels and types of complexity of interaction, that each level has its own laws of development which are, however, consistent with the laws of development at other levels. Thus, the laws of biology (which are, of course, tentative and approximate) deal with phenomena at the supramolecular, supraorganellular, supracellular, and even supraorganismic levels. Those of physics deal with matter at the simplest level of complexity of interaction. Even in this instance the atomistic prejudice is dominant, and the thought, for example, that radioactive decay is, in part, a supranuclear phenomenon is not even widely considered, although there is good reason to present it.

To assert, as Hahn does, that "the disputation between 'traditional biology' and 'atomistic prejudice' . . . can be regarded as the contemporary form of the dispute between vitalistic and mechanistic biology" is to invite biology to bend and break itself on the edge of a false dichotomy. An aspect of this is the sharp distinction Hahn makes between physical and metaphysical. Since he has occasion to refer to history, I am sure that he is aware of the "metaphysical" nature of radio waves—at least they seemed "metaphysical" to many of that day, who felt that we had plumbed all the dimensions of phenomena. This is the thinking that makes so much of the historical ideological antagonism between science and religion, as opposed to the transcendental approach, which sees one as emphasizing the allegorical and symbolic and the other as emphasizing the phenomenological.

As for Hahn's oblique references to style in scientific disputes, I do, it is true, believe in the polemical method in science. There is an odd, mid-20th-century notion that scientific advocacy and feeling are contradictory. This is a reflection of the dehumanization and depersonalization of contemporary life in general, including scholarship. The passive voice in reports is a strong indication of this. This is why I also advocate neoromanticism in art and science.

I share Hahn's faith in the self-corrective character of science. However, unlike Hahn, I believe that this is "guaranteed" only over a long period [see *Science* **141**, 1010 (1963)], and that it consists only of a leap to another stage of irresolution. Nothing is ever settled once and for all.

EUGENE KAELLIS
*Roosevelt, New Jersey*

The thesis of Eugene Kaellis's first letter in *Science*, on which I commented, is the proposition that there exists in contemporary biology an "atomistic prejudice" nurtured especially by biochemists and molecular biologists. This prejudice is thought to be inimical to traditional biology, and the establishment of a Society of Holistic Biology is proposed, which, with the support of "the still more complex social disciplines," may serve as an organization to which biologists may "rush" in "the defense of their science."

Kaellis has now further specified his views by stating that (i) physics, also, is dominated by the atomistic prejudice, and (ii) "the laws of biology (which are, *of course* [my italics] tentative and approximate) deal with phenomena at the . . . supraorganismic [level]." This second statement exempts almost the entire content of biology from being subject to "the laws of biology," with the exception of phenomena exhibited by a plurality of organisms. Even traditional biologists may consider such limitations of their science to be no less prejudicial than the mechanistic views opposed by Kaellis.

I hold the proposition of an "atomistic prejudice" in biology to be scientifically untenable. In terms of the genealogy of ideas, it is significant that Kaellis professes disenchantment with contemporary life as well as scholarship. He lauds transcendentalism, which relegates science to mere phenomenology, and advocates "neoromanticism in art and science" as a remedy for the, to him, undesirable cultural condition of the mid-20th century.

I am not aware of tendencies in romantic thought which could be envisaged as points of departure into a neoromantic school of science. *Transcendental Idealism* (Schelling, 1800), R. W. Emerson's *Nature* (1836), the ideas of the Transcendental Club (founded in Boston, 1836), and especially Schelling's *Naturphilosophie* (1806–08) which he expounded in the *Jahrbücher der Medicin als Wissenschaft* (Nature not comprehensible through observation and scientific theory but, rather, an object of speculative and intuitive interpretation and constituting one "universal organism" endowed with a "world soul") are the metaphysical antithesis of Cartesian and Newtonian mechanistic theories of the universe and hostile to the rationalistic heritage of the Enlightenment.

The contribution of the romantic school to man's ideas about the universe are irrationality and mystification rather than a noteworthy advancement of science. During the same period, however, Wöhler [*Poggendorff's Ann. Phys.* **12**, 253 (1828)] synthesized the first organic substance of biological origin—urea—from ammonium cyanate and thus established one experimental basis of the "atomistic prejudice" in biology. The schism between philosophy and science, originating in the romantic decades of the 19th century, has contributed materially to the emergence of "the two cultures" (C. P. Snow, 1959). Does Kaellis propose to advance biology by sacrificing the one to the other?

FRED E. HAHN
*8309 Westmont Terrace, Bethesda 34, Maryland*

[Hahn's earlier letter in *Science*] states explicitly in print a fallacy that has become an annoying commonplace —that is, that "molecular" biology alone is "mechanistic," while "traditional" biology is "vitalistic" and requires the missionary activity of molecular biologists in order to be converted to scientific respectability.

In an etymological sense it is a tautology to say that biology is vitalistic, since "vital" and "biology" are grounded upon Greek and Latin roots, respectively, for the same word, "life." Therefore if a biologist maintains that any problems are uniquely biological, and not physical or chemical, he can be said to be "vitalistic" if it is understood that by "vitalistic" you merely mean "biological" and are saying that biology concerns itself with problems appropriate to biology.

Philosophers, however, as Hahn correctly states, mean no such thing. The doctrine of vitalism in its pejorative sense implies the operation of a suprascientific force, an *élan vital* or entelechy, which would give to biological phenomena an aspect beyond investigation. This doctrine has had no standing in biology for many years. The biologist says, "Merely analyzing a class of phenomena into a more elementary level does not, in itself, explain the original phenomena. There may be logical correlations evident only at the original level of complexity to which analysis into more elements is irrelevant."

"Aha," says the biochemist (always remember that he is a chemist and not a biologist), "you are saying that vital phenomena are not investigatible by nonvital means. Therefore, you are a vitalist." What has happened is a switch from the first to the second meaning of vitalism. The biologist has intended to say only that biological phenomena have a logic and theoretical framework of their own, and the chemist has converted him into a teleologist.

The fallacy is obvious enough if removed from biology. The organic chemist pursues his research by means of perceptual three-dimensional models which are, from the standpoint of a nuclear physicist, extremely crude, not to say naive. Should the organic chemist, then, drop all current investigation and go haring after quantum mechanics, because the ultimate particles making up the atoms in his models can only be appreciated and described by mathematical formulae, and cannot be visualized by models at all? If he stubbornly maintains that the phenomena he is investigating still require the use of the crude old models, is he then guilty of postulating an entelechy? Obviously not.

The majority of molecular biologists, who claim to be in the forefront of advancing biological research, unfortunately are ignorant of that subject. This is not surprising. Most university departments offering a Ph.D. in biochemistry require only freshman biology as a prerequisite to admission and no training in any biological subject outside of biochemistry during the program. The situation in biophysics is just as bad [F. W. Ness, *A Guide to Graduate Study* (American Council on Education, Washington, D.C., ed. 2, 1960)]. The result of this one-sided training is exactly the sort of ignorance reflected in Hahn's statement that "Traditional biology has not developed great generalized theories . . . with the notable exception of the theory of evolution."

Here is a list of comprehensive theories, each forming a different kind of conceptual framework into which the whole science of biology can be fitted:

1) The cell theory—the oldest of them all, dating from the 1830's. All living material is divided into morphological-functional units having essentially the same structure in all organisms. This subdivision is the mechanical result of the limits of the efficiency of diffusion in conveying metabolites. Increase in size of organisms is thus neatly related to increase in complexity. Investigations of the cell thus become investigations of phenomena common to all living things. It is strange that Hahn should miss this, as it is this generalization, not evolution, which led biology in the direction of chemistry.

2) Biological cycles. All organisms are regarded as forming a part of the cycling of various elements (carbon, nitrogen) and as occupying positions in food chains. Essentially, biological phenomena are regarded as a particularly elegant manifestation of the law of conservation of energy. I am not sure from what period this should be dated—I would say the early 1920's, if Sir Charles Elton is recognized as the author.

3) Feedback mechanisms—encompassing both internal equilibria like homeostasis (Walter Cannon, 1929) and concepts like "the balance of nature" (much earlier) which are related to ideas like "food chains" and Malthusianism, and of course the whole of endocrinology.

4) Communities. All organisms are regarded as members of associations of organisms, which in turn have some of the characteristics of organisms themselves. The function of the organism in the community is the central idea. Dates from about 1890.

The list is certainly not exhaustive, but if we add evolution, and molecular biology, we have six different ways of looking at biology, all of them fruitful and stimulating in their own way, and all of them stimulating current research. None of them can be dismissed as being on the same level as the phlogiston theory or the geocentric universe.

STUART O. LANDRY
*Department of Biology, Harpur College, Binghamton, New York*

. . . Innumerable quantitative relationships remain to be established between the morphological, anatomical, and cytological levels before the basic units on the molecular level can be built into believable objects or responses. If these relationships are not established, each biologist—traditional and molecular alike—will stand accused of having developed lonely, sterile, descriptive disciplines. . . .

RALPH MARSHALL DAVIS, JR.
*Agricultural Experiment Station, College of Agriculture, University of California, Davis*

## The Use of "I"

There are circumstances under which we, as authors, must refer to ourselves as individuals. Yet the custom of writing objectively has been so thoroughly ingrained in us that we dislike making personal references. To avoid this problem, we have used such terms as *the author* or *we*. Clemence [*Science* **141**, 1131 (1963)] has now rightly suggested that we say "I" when we mean "I."

To permit us to follow this suggestion and yet not lay ourselves open to the charge of immodesty, I propose that we adopt the convention that an author should use the personal pronoun *I* whenever necessary (i) to avoid illogical constructions, such as dangling participles, gerunds, and infinitives, and (ii) to avoid ambiguous expressions, such as may occur when an author is referring to his own work or ideas.

F. BRUCE SANFORD
*U.S. Bureau of Commercial Fisheries, Seattle, Washington*