## Reductionism and Real Biology

Few things in life are more enraging than being told publicly that one's work "may well be of value," especially if that statement is based on untidy and, in places, self-contradictory arguments. The tone of the little gem proffering all this is, moreover, so insufferably pedantic that my esteem for *Science's* editorial discretion has fallen to a new low (K. F. Schaffner, "Antireductionism and molecular biology," 11 Aug., p. 644).

Schaffner's logical troubles start at the beginning of his paper with the well-known error of the excluded middle. To him, the only alternative to absolute reductionism is antireductionism, just as in the early days of Christianity all non-Christians were heathens. Thus he places Glass, Commoner, and Elsasser in the antireductionist camp *by definition*, and proceeds to dress them down. As I understand these men (taking into account the tenor of their entire statements, not just of one less felicitous passage), they are anything but *anti*reductionist. They acknowledge the tremendous strides made by the molecular biologists in reducing the mysteries of genetics to a chemical mechanism, but caution against the overgeneralization that *everything* biological can now be reduced to chemistry. Pure reductionism is a vast generalization based on a not-so-vast amount of evidence. It is a canon of faith and, as such religious assertions go, can neither be proved nor disproved. Thus, some of the arguments intended to demonstrate that there can be no such thing as complete reductionism are indeed a bit quixotic, since they assail a windmill that has not even been demonstrated to be there. The spectacle of Schaffner's counterattack is no less than comic.

Observe some of the slapstick: In order to "clarify the issue" and show where Glass misses the mark, Schaffner pulls John von Neumann out of his holster and lets fly with the big bang of the latter's demonstration that quantum mechanics cannot contain any "hidden parameters" that would turn the stochastic theory into a deterministic one. This profound observation demonstrates then (says Schaffner) that Bentley Glass did not do his homework; he should have presented "an appropriate axiomatization of a true probabilistic theory in biology and demonstrate that the identification of biological entities with physicochemical entities and explanation of the biological entities' behavior on the basis of either causal or statistical laws involving physicochemical terms would entail a contradiction." I wonder if it has occurred to Schaffner that, in order to pull off this feat of reasoning by analogy, it would have behooved him to present "an appropriate axiomatization . . . etc.," so that *he* can at least demonstrate that the two systems being analogized here (quantum mechanics and biology) are indeed isomorphic with respect to the question at issue. If they are not, the whole argument is vacuous. The punch line to this joke comes on the next page, when Schaffner invokes Ashby's notion of homomorphism to propose that there may exist an operator $\phi$ which would enable one to transform chemical systems to biological ones, and vice versa. If this is not a "hidden parameter," I'd like to be shown one.

And all this time Schaffner, true to form, keeps missing the glaringly obvious point that especially Elsasser is making: biological phenomena may sometimes (and eventually, perhaps always) be explained a posteriori in terms appropriate to lower levels of organization, such as molecular systems; the lower system, however, does not *predict* even the *existence* of the higher one, leave alone its organization.

Schaffner almost puts his foot into it when, again through the medium of Ashby, he points out that an engineer in building a bridge is working with a homomorphism of an "atomic" system (through the magic of operator $\phi$). The parable of the engineer has exactly the opposite moral of what Schaffner tries to imply, and is commonly used by adherents of less extreme viewpoints. The engineering fact that an I-beam of given weight will support more load than a square rod of the same weight is not predictable from thermodynamics, because this discipline does not consider the form or *morphe* which a substance can be made to assume. If such considerations are introduced into thermodynamics after the fact, some clever fellow may quite well demonstrate that the engineers have been right all along, which fact is comforting to know, but is wholly irrelevant to either thermodynamics or engineering since neither discipline is enriched by this "discovery." In this context I wonder how much genetic engineering (that is, selective breeding) will benefit from molecular genetics, notwithstanding the bright prophesies of the late H. J. Muller.

Schaffner continues: " . . . no evidence has been unearthed in our inquiries . . . that would argue positively and persuasively for the inherent autonomy of biology." What about necessary and sufficient conditions? Life is the necessary *and* sufficient condition for natural macromolecular systems, but macromolecules, though necessary, are not sufficient for life (at least not until someone actually synthesizes life). Similarly, biology implies biochemistry, but not the other way around.

There has been an interesting progression in the titles on the doors of the people who study fractionated life in test tubes. They used to call their work "organic chemistry," but soon found that "physiological chemistry" would be more appropriate, and "biochemistry" even better (more general and easier on the tongue). "Molecular biology," though not quite so euphonic, sounds much more impressive and gives one the illusion of being a biologist instead of a chemist. In the wake of the great successes in genetics, it appeared a simple matter to declare that genetics is central to all of biology, and thus the "molecular" may as well be dropped from the title, leaving Biology, pure and simple. All that remains to be done now is to dispose of what used to be called biology and relegate it to Siberia by insinuating that it has no "real autonomy" (that is, is really illegitimate), and thus has no right to the throne. In fairness it must be said that this is more genteel than outright assassination, but who is going to mind the store in anatomy, embryology, phylogeny, ecology, taxonomy and all the other "classical" fields of biology? Reductionist philosophers maybe? After all, research in these fields may well be of value (heuristically speaking, of course), so *somebody* ought to do it.

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. . . Schaffner points up my contention that the arguments centering around reductionism and quantum mechanical theory involve mainly physical theoreticians—not practicing biologists. . . . Still, we are all *natural* scientists living in the same universe, and hopefully the borders of our sciences will become less and less distinct as we gain a more comprehensive view of our surroundings. Surely, Bentley Glass should have no quarrel with anyone for attempting to provide physical explanations for biological phenomena; this approach is quite commonplace and often successful. But, it is a relatively heavy reliance on the inductive method, coupled with a deeply ingrained appreciation for the comparative and evolutionary approaches to understanding organisms, which largely set the biologist apart from the physical scientists and psychologists. However, one would not be correct in equating differences in ways of looking at natural phenomena with any fundamental autonomy of the various sciences. More effective *two-way* exchanges of data and viewpoints seem desirable. In this regard, academic arguments, such as Schaffner's, have their heuristic value; but it seems to be he, who would attach the title "antireductionist" to others, who gives this term an aura of connotation which would be, I am sure, quite alien to the would-be "attachees." Perhaps those working in the so-called classical areas of biology might be called "nonreductionists" (if any title is really necessary), but I seriously doubt that this labeling would delineate any significant areas of ideological opposition among present-day biologists.

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It seems to me that Schaffner's argument entirely misses the point of the problem. Consider, the following well-known case:

The pituitary gland in vertebrates controls, via adrenocorticotropin, the hormone production of the adrenal glands; the steroid hormones produced by these glands control the activities of enzymes that in their turn control a number of important metabolic reactions of various types of cells. Moreover, when the concentration of the steroid hormones circulating in the blood exceeds a critical level they inhibit the hormone production of the pituitary gland by a characteristic feed-back mechanism. In this way a functional complex is built up that obeys a specific set of rules.

Now there can be no doubt that each single step in this functional complex can be "explained" in molecular terms, but I do not think one is warranted to call this set of molecular explanations a reduction of biological phenomena to the level of physicochemical data. The important point is that, no matter how complete our biochemical knowledge, the specific organization of the hormonal-enzymatic system just mentioned could not be deduced from molecular data, just as the grammar of a language cannot be deduced from a knowledge of its alphabet, and a poem by e.e. cummings cannot be deduced from a knowledge of English grammar. To state "research in classical biology may well be of value" is not even an understatement, it is a complete misstatement of the problem, because if this so-called "classical biology," that is, the study of biological systems rather than that of biological elements, did not exist, molecular biology would have no pegs to hang its data on.

Biology is not really concerned with the "reduction" of one set of data to another set of data, but with the study of relationships on different levels of organization. The discovery of relationships on, say, the organismic level may be just as significant as the discovery of relationships on the molecular level, and the fascination of modern molecular biology has nothing to do with the claim sometimes made that all biological phenomena can now be reduced to physicochemical data. Rather it is due to the fact that out of a certain biological stalemate we have become explorers again, that we have learned to make generalizations on a level of organization that practically did not exist just 25 years ago.

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Instead of trying to reduce biological phenomena to the laws of physics and chemistry, perhaps we should reverse our point of view—similar to what the Copernicans did; that is, instead of looking backward in terms of reductionism, look forward in the direction of time's arrow and contemplate the problem from where the story of evolution began, from the interstellar cloud of hydrogen. From this point of view, it becomes immediately clear that every element of the periodic table represents a new *quality* which emerged as a result of changes in *quantity*—of atomic particles, energy levels, or whatever units you wish to use. We are dealing with a principle which states that changes in quantity transforms into changes in quality. Furthermore, if we define quality as the sum of all the properties of an object at whatever levels they are studied, it becomes evident that by choosing the units which have undergone quantitative changes properly, we may associate with the change the emergence of a new quality. In this manner we can keep going up the scale of levels in the whole process of evolution: chemical, biological, and even social.

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My position can be stated in three points: (i) Current advances in molecular biology have provided strong evidence for the thesis that biological organisms are explicable in terms of chemistry. The evidence is not, however, conclusive. To provide conclusive evidence for the reduction, all biological phenomena (and most likely, biological theories) would have to be obtainable; that is, derivable from the theories of chemistry supplemented by what I have termed elsewhere (*1*) as appropriate "reduction functions." These functions are formally and functionally equivalent to the part which a "transformation" or "operator" plays in effecting reductions. They identify entities of the reduced science with entities or groups of entities of the reducing science (*2*). The recent investigations of C. Yanofsky and his co-workers (*3*) can be seen as attempts to determine some of these reduction functions. (ii) The complexity of organic molecules, especially as regards tertiary structure and its chemical consequences, may never be fully explicable in chemical terms without adding sentences which describe this structure and which function as initial conditions, to the chemical theories appearing in the explanans. This thesis about the role of initial conditions admits of a natural extension to organ systems interconnected by hormonal messengers. Here, as in the previous case, a change in the arrangement would affect the system's behavior, as an interchange of a capacitor and of a resistor in a radio would alter that device's performance. Few, however, would contend that a radio is not explicable in terms of electrical theory. [See Polanyi (*4*) for a contrary view.] (iii) Arguments put forward to demonstrate that chemistry (or physics) cannot ever reduce biology are without foundation. Nevertheless it would be silly to conclude from this assertion that biology (in the classical sense) ought to be given up. In connection with points (i) and (ii) it is clear that "classical" biology has much to do, though I do not think that one can build up a philosophy of "emergent vitalism" or some type of system of "levels of emergence" out of this admission.

With regard to van Bergeijk's letter, it seems to me that we may legitimately take "antireductionist" to mean a person who does *not* believe that physics and chemistry can ultimately explain *all* of biology. There are no biologists who do not acknowledge the utility of *some* chemistry. In line with points (i) and (ii) above, I cannot and need not show an "isomorphism" of the type van Bergeijk asserts I must. It is sufficient on my position to show that the antireductionist has not shown that an "isomorphism" cannot be established. [Actually a simple isomorphism will not do (*1*).] It also seems that a claim in favor of the reductionist position in biology is no more a "canon of faith" than the quantum chemists' claim that quantum mechanics will ultimately be able to account for all of chemistry. I think I have indicated sufficiently well what the role of an operator like $\phi$ is under point (i) above; van Bergeijk has apparently misunderstood its function—it is certainly not a "hidden parameter." He notes that though "biological phenomena may sometimes (and eventually, perhaps, always) be explained a posteriori in terms appropriate to lower levels of organization . . . the lower system, however, does not *predict* even the *existence* of the higher one, leave alone its organization." This point is essentially the same as the thesis I present in the section of my article under the subhead "Organization and Emergence." Since the point can easily be misconstrued, however, let me refer to point (ii) above, and also note that I stated in my article that the "chemistry of biological evolution . . . [is] a significant exception to this point." For in this area of inquiry, we do wish to provide, at least plausible arguments, as to how the organization of the more complex chemical and biological systems came about. Experiments suggested by the Oparin hypothesis have been fruitful enough to indicate that the argument van Bergeijk is making is not a liability to the reductionist position. In regard to his I-beam example, the actual case is that quantum mechanics accounts for the strength constants of metals, and the addition of a sentence describing the shape of the beam as an initial condition to the quantum physics will entail the correct engineering claim which van Bergeijk refers to. Why he brings in thermodynamics is not clear. Finally, it would seem according to all current theories, and without admitting arguments based on an appeal to ignorance, that the macromolecule(s) of proper organization will be "living"—there is no clear evidence to the contrary.

In Chernetski's letter, I note that there are important scientific and philosophical problems about reductionism that will eventually have unambiguous answers; namely, whether chemistry (and physics) is in fact fully adequate to explain all of biology. Classical biologists might currently be called nonreductionists if it is kept in mind that ultimately their work will be viewed as having assisted in establishing either the reductionist or antireductionist thesis.

With respect to Wieser's comments, I believe that my discussion under point (ii) indicates what my position is on the explanation of the hormonal-enzymatic system to which he refers.

Finally, to turn to De Leon's suggestions, permit me to note that the word "quality," from both scientific and philosophical points of view, is notoriously obscure. Nevertheless I have no doubt, if we assume the "big bang" theory without cyclical reoccurrence of the fireball, that novel combinations and new "qualities" have appeared. The question at issue is whether a physical theory—which is essentially a timeless entity somewhat like a number—could account for these novelties, such as nuclear physics plus quantum mechanics accounts for the periodic chart *and* for the density, hardness, melting points, color, and acidity of many elements and compounds. To date, I do not think that the word "level" has ever been given a precise operational definition. At this point I am inclined to simply reassert the theses made partly under the subhead "Organization and Emergence" in my article, and partly under point (iii) above.

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**References**

1. K. F. Schaffner, *Phil. Sci.* **34**, 137 (1967).
2. Also see E. Nagel's relevant discussion on this point in his *Structure of Science* (Harcourt, Brace & World, New York, 1961), chaps. 11 and 12, especially pp. 353–58 and 433–35.
3. C. Yanofsky, B. C. Carlton, J. R. Guest, D. R. Helsinki, U. Henning, *Proc. Nat. Acad. Sci. U.S.* **51**, 266 (1964).
4. M. Polanyi, *Chem. Eng. News* **45**, 54 (1967).