FUNCTION, ORGANIZATION, AND SELECTION

by William C. Wimsatt

One might believe, from the language of many of the symposiasts, that the concepts of "order" and "organization" have provided a unifying theme for the different perspectives presented here. In my opinion, this would be a mistake—at least if this belief is taken to imply that the kinds of order that were primarily discussed, thermodynamic order and the spatio-temporal organization produced by simple dissipative structures, are likely to shed much direct light on the nature of biological or social order, or upon valuational or normative concerns arising from them.

Csikszentmihalyi[1] has already raised doubts concerning the usefulness of such results in the characterization of social organization. But in the light of the remarks by Katchalsky[2] and Edelstein[3] it might seem that the study of a theory of dissipative structures would yield a direct analysis of the nature of biological organization. In spite of substantial agreement with their perspectives, I wish to disagree on this point and to try to indicate what I think is missing.

Adaptive Organization

Dissipative structures have a number of features which suggest biologically significant theoretical connections. They involve an energy flow through the system (unlike the closed systems of classical equilibrium thermodynamics) and nonlinear reactions operating far from equilibrium (unlike the systems of classical nonequilibrium thermodynamics). The study of dissipative structures has afforded the first thermodynamic explanations for ordered spatial and temporal heterogeneity in open systems. Theoretical biologists have been intrigued by predictions and demonstrations that relatively simple dissipative structures could exhibit periodic oscillations, hysteresis, "developmental switching," and other "morphogenetic" effects. It seems reasonable to predict that further study of such systems will have numerous biologi-

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cal applications and will permit a simplified treatment of some heretofore intractably complex biological phenomena.

But a study of dissipative structures is not likely, by itself, to lead to a penetrating analysis of biological organization—at least as most biologists would understand that term. Since Aristotle, biological organization has been characterized in terms of teleology and adaptiveness. This is no less true, now that Paley's "argument from design" and Panglossian Harmonies of Nature have been replaced by Darwin's mechanism of natural selection and variegated interpretations of "survival of the fittest."[4] There is no more pervasive quality of living systems than their adaptation, in some degree and at all levels, to surrounding conditions.

Since coadaptation (selection for the reduction of mutually disfunctional interactions) of parts of a unit of selection is a primary mode of adaptation of that unit to its environment, and units of selection (as Lewontin has pointed out)[5] can themselves exist simultaneously at a number of different levels, we should expect to see the production of adaptations and coadaptations at virtually all levels of organization. That which increases fitness (roughly) is selected for, anything which promotes the first is derivatively selected for, and so on, through the transitivity of the causal relation. These chains of causal relations must themselves be coadapted, and the result is a hierarchy of causal relationships arranged in a manner formally similar to a means-end hierarchy in human decision making. I have elsewhere described some of the formal properties of these causal networks.[6]

Selection is essential here to explain how this adaptive organization could have arisen. Nothing more is postulated in modern evolutionary theory to explain this adaptation than the directive effects of differential selection, and nothing less will do. Somewhat paradoxically, differential selection is a product of different degrees of adaptation; so adaptation is what Ashby might call a "self-amplifying" property.

To say that something is a dissipative structure, however, is to say nothing per se about adaptive organization, or about the selection necessary to produce it, and thus nothing about the most striking and central feature of biological organization. I therefore cannot believe that the study of dissipative structures provides a powerful and direct approach to the problem of the nature of biological organization.

Phenotypic Complexity

This is not to say that dissipative structures have nothing to do with biological organization. Both Katchalsky and I agree that the interaction of reproduction with variation and differential selection is itself a dissipative structure. This is illuminating, but not too helpful because it does not tell us what to do next. It is at this stage slightly more helpful than being told that organisms do not fail to obey the second law of thermodynamics, but probably less helpful than being told that natural selection is an optimizing process. Katchalsky's lament over Bohr's principle of complementarity applies here as well: "The distance between the principle[s] and the operational requirements [is] too great."[7]

Abstraction and oversimplification have caused problems in biology before. A particular case in point will illuminate the problem of biological organization. Because I have said the magic words "selection" and "adaptation," it should not be thought that I have given an analysis of biological organization. I have at most indicated a direction in which such an analysis should go. Can adaptive organization be analyzed in terms of a means-end hierarchy to optimize some parameter? Then why cannot all interacting systems be considered parts of a means-end hierarchy to maximize entropy? Is selection crucial? What prevents us from saying that a chemical component which is increasing in a mixture is being selected for? Self-reproduction is surely paradigmatically biological—but what distinguishes it from autocatalysis? The problem is that almost every supposedly distinctively biological property has counterparts in very simple physical systems. Thus, a mixture of simple dimeric, inorganic autocatalysts competing for a common substrate could display an astounding repertoire of "biological" phenomena—from logistic growth curves and a Lotka-Volterra type of ecology (complete with competition coefficients, population oscillations, and extinctions) to balanced (or unbalanced) polymorphisms, genetic loads, Mendelian ratios, and Hardy-Weinberg equilibria.

It should not be surprising that, as idealized oversimplifications, most mathematical theories in biology also apply without modification (and frequently with a great deal more accuracy) to much simpler systems than those the models were originally constructed to explain. These simplifications may be irrelevant or even desirable in some respects, but not when it comes to attempting to characterize biological organization, for it is just this which has been left out. It is this simplification which allows Williams to write: "In its ultimate essence, the theory of natural selection deals with a cybernetic abstraction, the gene, and a statistical abstraction, mean phenotypic fitness,"[8] just as if nothing were going on in between those two levels and selection operated directly on "naked genes." This denigration of intermediaries goes in spirit back at least to Butler's comment that a hen is just an egg's way of making another egg. But in biology, the means are as definitive of life as the ends. Waddington recently put it this way: "I do not believe that if we discovered systems that were nothing more than mechanisms for the hereditary transmission of mutable information we should . . . consider them to be living. . . . To be worthy of being called alive they must . . . exhibit some sort of 'physiological activity.' . . . Life involves not only the genotype but also the production of something of the kind geneticists speak of as the phenotype . . . something which was developed out of the genotype and which interacts with the surrounding non-living environment."[9]

Waddington goes on to call for the development of a "theory of phenotypes," and he and Williams[10] both clearly regard the problem of adaptation as requiring substantial further study and analytical treatment.

Biological organization, then, is the organization of the phenotype, and it is adaptive organization. No theory can claim to be a theory of biological organization unless it deals both with the complexity of the phenotype and its optimal or quasi-optimal organization in terms of means and ends. The theory of dissipative structures will undoubtably play a *part* in such a theory when it is constructed, but this is not to say that it *is* that theory.

And Beyond?

A "generalized theory of phenotypes" may be what biology lacks, but the fruitfulness of such a theory need not stop there. This, I take it, was the germ of truth in the organicist analogies of the functionalists in the social sciences. The works of Simon,[11] Campbell,[12] Levins,[13] and others[14] suggest that what is sauce for the goose may be good for General Motors—that common principles may apply to adaptive systems at any level—from biological systems of varying degrees of complexity, through the variety of cognitive systems generally studied by psychologists as learning systems, to whatever at the social and cultural levels meets the conditions required to speak of evolution through differential selection. Adaptive systems, as well, have close ties with human purposiveness and teleology—at least in the possession of a common conceptual structure[15]—and this appears to promise ties with problems of valuation which are somewhat less metaphorical than those afforded by thermodynamics.

Nonetheless, just as biological systems, though dissipative structures, are a great deal more as well, so psyches and societies, for all their fame and failures as adaptive systems, both present a few new conceptual niceties of their own. "Adaptive organization" should not be a panacea. Experience should have taught us by now that it is usually an expensive conceptual economy to attempt to cram all the phenomena at one level too quickly into the straitjacket of a set of categories designed for use at a lower level. But the continuing high enthusiasm of many scientists and philosophers in the service of reduction cannot help making some of their colleagues wish that William of Ockham had invented a safety razor.

NOTES

1. Mihaly Csikszentmihalyi, "From Thermodynamics to Values: A Transition Yet to Be Accomplished," *Zygon* 6 (1971): 163–67.
2. A. Katchalsky, "Thermodynamics of Flow and Biological Organization," *Zygon* 6 (1971): 99–125.
3. Barry B. Edelstein, "Thermodynamics, Kinetics, and Biology," *Zygon* 6 (1971): 160–62.
4. A recent and influential work on evolutionary theory, George C. Williams's *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought* (Princeton, N.J.: Princeton University Press, 1966), provides a representative sample of biological opinion on these topics: On organization—"When a biologist says that a system is organized, he should mean organized for genetic survival or for some subordinate goal that ultimately contributes to successful reproduction. . . . Each part of the animal is organized for some function tributary to the ultimate goal of the survival of its own genes" (pp. 255–56). On teleology—"I have stressed the importance of the use of such concepts as biological means and ends because I want it clearly understood that I think such a conceptual framework is the essence of the science of biology" (p. 11).
5. Richard C. Lewontin, "The Units of Selection," *Annual Review of Ecology and Systematics* 1 (1970): 1–18.
6. William C. Wimsatt, "Modern Science and the New Teleology I. The Conceptual Foundations of Functional Analysis" (Ph.D. diss., University of Pittsburgh, 1971); "Teleology and the Logical Structure of Function Statements," *Studies in History and Philosophy of Science*, vol. 2 (1971), in press; "Functional Organization" (paper read at the Boston Colloquia in Philosophy of Science, January 26, 1971) in *Boston Studies in the Philosophy of Science*, ed. R. S. Cohen and M. W. Wartofsky (New York: Humanities Press), forthcoming.
7. Katchalsky, p. 100.
8. Williams, p. 33.
9. C. H. Waddington, "The Basic Ideas of Biology," in *Towards a Theoretical Biology*, ed. C. H. Waddington (Edinburgh: Edinburgh University Press, 1968), 1:4–5.
10. Williams, p. 258.
11. A recent and important synthesis of the views of H. A. Simon is his *The Sciences of the Artificial* (Cambridge, Mass.: M.I.T. Press, 1969). His modern (1962) classic, "The Architecture of Complexity," is reprinted here as chap. 4. The argument of that article—that evolved systems will tend to be hierarchically organized in terms of stable subassemblies—is an interesting anticipation of Bronowski's notions of "stratified stability" and actually uses this concept to derive further properties of the system. (The notion of "stratified stability" was expounded in J. Bronowski, "New Concepts in the Evolution of Complexity: Stratified Stability and Unbounded Plans," *Zygon* 5 [1970]: 18–35.) Simon's ideas on "near-decomposeability" are also very close to some of the ideas expressed by Levins in his "Complex Systems" (see n. 13 below). Other suggestive works of Simon include his articles, "Thinking by Computers" and "Scientific Discovery and the Psychology of Problem Solving," in *Mind and Cosmos,*

ed. R. G. Colodny, Pittsburgh Studies in the Philosophy of Science, vol. 3 (Pittsburgh: University of Pittsburgh Press, 1966), pp. 3–21, 22–40.

12. Since 1956 Donald Campbell has been publishing a series of studies exploring in great detail applications of the "natural selection model" in psychology, the social sciences, philosophy of science, and general epistemology. Probably the most recently published survey of his work is his "Natural Selection as an Epistemological Model," in *A Handbook of Method in Cultural Anthropology*, ed. R. Naroll and R. Cohen (Garden City, N.Y.: Natural History Press, 1970), pp. 51–85. Most of his other important articles on this topic are cited in this work.

13. Richard Levins has been writing some of the most exciting new work in the study of complex systems. Most noteworthy are his "Complex Systems," in *Towards a Theoretical Biology*, ed. C. H. Waddington (1970), 3:73–88; *Evolution in Changing Environments* (Princeton, N.J.: Princeton University Press, 1968); and "The Limits of Complexity" (paper prepared for a symposium at the Smithsonian Institution, 1969).

14. Some of these other works are discussed in chap. 8 of my dissertation (see n. 6 above).

15. See esp. Wimsatt (n. 6 above) "Functional Organization" and "Teleology and the Logical Structure of Function Statements."