Thoughts on Living Systems

Towards a Theoretical Biology. 1, Prolegomena. Proceedings of an International Union of Biological Sciences symposium, Bellagio, Italy, Aug.–Sept. 1966. C. H. WADDINGTON, Ed. Aldine, Chicago, 1968. viii + 234 pp., illus. $8.95.

It would be easy to condemn this book out of hand. Even as "prolegomena" it seems, at first glance, to be a pretentious hodgepodge of original notes, essays, correspondence, and reprints of uneven merit and level of difficulty. Biologists will find many of the papers extremely heavy going—such as that by Paul Lieber on "Constants of nature"—and other papers quite trivial—such as Waddington's "Does evolution depend on random search?," which is a competent attempt at elementary education of the Neo-du-Noüyian school of mathematicians but not a contribution to theoretical biology. For two reasons, however, I'm not going to condemn the book. First, attempts at making theoretical progress in biology are all too often impeded because of a widespread distaste for theory among biologists ("Don't bother me with theory, I've got ten students chained to my lab bench digging out *the facts*"). In my view virtually any progress toward a theoretical biology, no matter how halting, is to be encouraged. The second reason for not condemning the book is more important—it contains some very interesting papers.

Biologists will find Ernst Mayr's article "Cause and effect in biology" and Waddington's comments on it (both reprinted from *Science*) straightforward, clearly written, and interesting. Similarly, Waddington's introductory article, "The basic ideas of biology," also contains considerable meat, including an interesting discussion of the limited applicability of information theory in biology. I am surprised, however, that he seems to feel (p. 8) that the only information which a developing organism gets from its environment comes in the form of food molecules. Waddington must also be in contact with a rather strange orthodoxy in modern Neo-Darwinianism when he states that it "usually tacitly assumes that randomness of genetic mutation implies randomness of phenotypic variation . . ." (p. 18). His own work, as well as that of Rendel and many others (to say nothing of dominance and epistasis), is well known to evolutionists, and they could hardly function if they made this "tacit assumption." In spite of these and other quibbles I finished reading Waddington's paper with my usual admiration for him, and a slight feeling of disappointment that other biologists often do not seem very interested in his ideas.

Howard Pattee's article on the physical basis of coding will be of interest to most biologists, as it deals with the reducing of current molecular "explanations" of life to physical terms, and with related problems of the origin of life. It seems characteristic of natural sciences that quite consistent explanations may be achieved at various levels of reduction, but some molecular biologists have gotten into the habit of claiming that biological phenomena cannot really be understood until they are understood at the molecular level. It gives a population biologist a certain perverse pleasure to see Pattee claim, in essence, that molecular biology must be reduced to quantum mechanics before life will be understood.

After wading through some of the more mind-bending contributions, the reader can always relax with Michie and Longuet-Higgins' "A party game model of biological replication" (reprinted from *Nature*)—a diversion which makes a good point about the evolutionary value of genome-phenome separation. Another short, interesting paper is that of Maynard Smith on "The counting problem." He asks, for instance, how a developmental system, when stimulated, manages to do something N times, such as produce a given series of vertebrae. Even shorter is Waddington and Lewontin's "Note on evolution and changes in the quantity of genetic information," which is of such interest that it should be reprinted in a journal such as *Evolution*.

Towards a Theoretical Biology is clearly a volume which should be examined before purchase. Its contents are too diverse for a general recommendation to biologists. Perhaps its greatest drawback is the lack of really general discussion of whether or not a "theoretical biology" analogous to theoretical physics *should* exist. That it should—a view which I share intuitively, but would like to see justified—has been taken for granted by the participants. Above all, this volume makes it clear that if a theoretical biology can be achieved, the moment of creation lies well in the future.

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Superconductors: Possibilities and Actualities

Superconductivity in Science and Technology. Proceedings of a conference, Chicago, May 1966. MORREL H. COHEN, Ed. University of Chicago Press, Chicago, 1968. viii + 164 pp., illus. $5.95.

This is a fascinating book because it gives a very good description of the state of work on superconductivity in 1966. Its contributions range from the best in the field to the mediocre. Some review the past, and others occasionally give an exciting look into the future. It ends with a very intriguing article, by Schmitt and Morrison, on the economic aspects of superconductivity, research and, more important, applications. This concluding article poses the question, Where has all the money gone—the several hundred million dollars spent—and how worthwhile has the expenditure really been?

One soon realizes that the answers have already been given in the preceding pages of the book. Bardeen reviews certain aspects of the BCS theory, the theory most widely accepted today. Josephson describes the Josephson effect predicted by him almost six years ago. Also, his article is, from an esthetic point of view, one of the highlights in this collection.

The applications, such as superconducting magnets and engineering uses of quantum effects, are reported by Hulm and Mercereau. These authors give a good picture of the state of technology in 1966. By contrast, in their article Garwin and Matisoo deal with one of the most exciting projects of the future, transmission of electric power through superconducting cables. In an exceptionally well-thought-out and excellently documented study, they consider everything from Maxwell's stresses to shipping coal on barges. They come to the conclusion that for the transmission of high power over long distances, the superconducting cable is technically feasible and economically competitive.

Thus one comes back to the question of whether all the work on superconductivity and superconductors has been