may note another point, not explicitly stressed in the book. The record shows that, from 1920 to 1933, Germany was the great center of research in this field, as in so many others, with England a close second. The decline of science under the Nazis, and its subsequent growth in the United States—much of it catalyzed by refugees from Nazi Germany—shows how rapidly a great country can lose scientific leadership, along with other even more precious things, when taken over by a political regime that is hostile to all disinterested inquiry.

The discovery in 1939, by W. A. Engelhardt and M. N. Ljubimova, that myosin, the chief structural protein of muscle, is also a catalyst for the hydrolysis of ATP indicated for the first time how the structural elements of the muscle fiber appear to be coupled with the mechanism of energy release. Engelhardt's survey of the field (1942), beginning on page 444 of this volume, shows his farsighted vision of things to come. The later developments concerning muscle that the book presents are too numerous to permit even mention of the names of their authors, great as many of them are. I make an exception for the epoch-making work of H. E. Huxley on the double array of filaments in striated muscle, and his sliding filament theory of contraction. It is regrettable that the reprinting (pp. 552–79) does not do justice to the superb quality of the electron micrographs in the original paper and that the captions for the figures were omitted (a printed slip containing these has been provided by the publishers in later copies of the book).

One major aspect of muscle biochemistry is not included—namely, the role of calcium ions in activating the contractile process, and their removal by the sarcoplasmic reticulum during relaxation. Here too ATP plays its part, and it would be good to have something about this in a future edition.

Part 8 of the book, on precursors of polymers, presents some of the papers that were turning points in our knowledge of the biosynthesis of glycogen, nucleic acids, and proteins. Its length is modest—45 pages, compared to some 300 devoted to muscle. This section could easily be expanded into a large book all by itself, but it is good to have these crucial papers brought together, with Kalckar's thoughtful remarks to introduce them. Part 9 deals with the regulation of energy metabolism, and the introductory section here is particularly interesting. Part 10, on organic chemistry and "bioengineering," consists of a single page of text by Kalckar, with nine references; this could be expanded to advantage in a later edition. There is a thoughtful, philosophical three-page epilogue.

A large number of photographs of those who were involved in the story told here enliven the book; although I happen to be among those included, I believe I can be objective in saying that this adds to the value of the book.

All the papers are in English: Kalckar himself has translated most of those that originally appeared in foreign languages, except for those in Russian, where others have helped. In many cases only the most crucial sections of the original papers are given here, as for instance with Fritz Lipmann's epoch-making review of 1941 on phosphate bond energy. In numerous cases a short preliminary note is reprinted here, rather than the later extensive paper—see, for instance, Fiske and Subbarow on phosphocreatine or Ochoa on oxidative phosphorylation. It would be helpful in such cases to give the reader the reference where the full report can be found and to indicate explicitly in longer papers the points at which sections have been omitted. It would also be very helpful to provide cross references between some of the papers reprinted here; for instance, between the work of Fiske and Subbarow (p. 34) and that of Eggleton and Eggleton (p. 340). The most serious deficiency in the book is the lack of an index; this should certainly be remedied in another printing.

One may hope that other books like this will be forthcoming, to deal with other major aspects of biochemistry; and, furthermore, that efforts will be made, through the preservation and study of letters, unpublished documents, and personal recollections, to supplement and enrich the record of the already published literature. In any case Kalckar's book deserves the warmest welcome from all who care to learn how biochemistry came to be what it is today.

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Modes of Explanation and the Tension of Biology

Towards a Theoretical Biology. Vol. 2. Sketches. An International Union of Biological Sciences Symposium, Aug. 1967. C. H. WADDINGTON, Ed. Aldine, Chicago, 1969. viii + 532 pp., illus. $12.50.

This symposium will thoroughly irritate any biologist who comes across it. At first sight it contains a lot of talk and precious little deductive theory; a closer look reveals essays violently attacking the accepted modes of scientific explanation and espousing a biology reformed along more Aristotelian lines. Worse yet, these essays were written by reputable physicists still practicing their trade, emphatically not the "carpenters blaming their tools" who frequent so many theoretical biology congresses. What happened?

This symposium records the attempts of some very intelligent people to digest and understand the disturbing complexities of biology. Many have read Kuhn on scientific revolution, and realize that current models of scientific explanation are as temporary as their predecessors: they are willing to face the possibility that a general theory like that embodied in Einstein's laws and Maxwell's equations is impossible in biology. How do they respond?

Some speakers settle for autonomous theories covering limited aspects of biology: they provide the symposium's only examples of meaningful mathematics. Maynard Smith discusses population genetics, analyzing the preconditions for natural selection and showing how differential reproduction inevitably alters the genetical composition of a population. Kerner and Goodwin apply the techniques of statistical mechanics to interpret otherwise insoluble equations describing, in Kerner's case, the population fluctuations of different species in a community and, in Goodwin's, oscillations in concentration of different enzymes and messenger RNA's. Statistical mechanics is an appealing subject (indeed, one speaker devoted a whole talk to describing just how appealing it is): it can derive a macroscopic from a microscopic level of generalization and works even if we know little about the microscopic interactions. Moreover, one need not determine initial positions and velocities of all the molecules of a gas to apply these techniques, for a gas of given energy and volume would exhibit the same *statistical* behavior for nearly any set of initial conditions. Kerner's treatment holds great promise for ecology, but the random assumptions inherent in statistical mechanics seem incompatible with the self-organizing properties of biological clocks, for example, which Goodwin seeks to understand.

Many more speakers settle for comforting analogies. Gmitro and Scriven, trying to cope with morphogenesis, give models and concrete examples of pattern arising from uniformity which would have delighted D'Arcy Thompson. Arbib, trying to understand what self-replication entails, describes the theory of self-replicating automata and its implications for biology. A posthumous work of Von Neumann published in 1966 apparently transformed the theory from empty talk to decent mathematics, but his idea of an automaton is difficult to visualize and his idea of self-replication even more so. Yet this analogy holds promise. Iberall seeks to understand an organism's function by reducing it to a bundle of oscillations, as Huxley reduced it to a bundle of adaptations. Iberall's analogy becomes an all-too-comforting substitute for understanding, a tool for replacing a disturbingly immediate reality by something comfortably distant and artificial: witness the following (which he follows by even more incredible wordplay): "Mother, in the mother-child relation, teaches the child various patterns that are fairly adequate to provide the range of needs that will saturate the physiological oscillators over time."

Pattee wishes to reduce biology to physics. He is appalled by the contrast between machines which, be they solar systems or computers, are fated to break down and organisms, whose hereditary mechanisms permit the preservation and perfection of wonderfully delicate organization. How can biologists take evolution for granted when they cannot create life? This question underlies Pattee's subtle worries: he will only be satisfied by showing Maxwell's demon (Schrödinger's equation personified) how to arrange molecules into an organism which will reproduce itself accurately enough for selection to preserve and perfect it. The question seems curiously academic: Pattee is sure no man could "create life."

Several participants reject Pattee's goal outright. Living things are organized for their roles in life: why not capitalize on this and explore the harmonies of biology, the hierarchies of the universe? Bohm and Lieber feel we should change our idea of what constitutes a scientific explanation. Bohm asserts that to understand the function of living things we must refer to some goal outside the field of function (a computer makes no sense unless we know that someone built it to do calculations), and he feels uneasy about the role of natural selection as a mechanism which judges function. Moreover, like Aristotle and Eddington, he wants to know why the universe obeys the laws it does. Marjorie Grene, a philosopher, out-Bohms Bohm, speaking out against a "one-level nature of Democritean atomism" and the supposedly objective pose where "Science becomes computation-for-the-sake-of-prediction-for-the-sake-of-computation-for-the-sake-of-prediction . . . , 'understanding' merely a subjective addendum, and 'truth' a dirty word, dropped in weak moments like words with one less letter, but decently avoided, for the most part, in polite society." Bohm and Grene plump for a mathematics of order and hierarchy for the universe, and Bastin tries to supply some of it.

All this sounds suspiciously like a conflict between teleologists and mechanists. This becomes evident in a curious discussion of group selection, where Bohm, Grene, and Waddington insist that birds sometimes limit their reproduction because the good of their group requires it, while Maynard Smith vainly points out that this hypothesis lacks a mechanism because group selection rarely overcomes selection within groups. Here lies all the strange tension of biology: an Aristotelian philosophy emphasizing perfection and harmony which restores perspective but risks replacing explanation by wordplay, opposed to a mechanist philosophy which is by nature far more honest, but which risks losing all perspective in a welter of necessary detail.

Most biologists invoke natural selection to reconcile this conflict, but this does not satisfy Bohm and Grene. Unfortunately, Maynard Smith's discussion of natural selection does not dispel their doubts. The remark that selection favors the most reproductive genotype doesn't say much by itself: one cannot explain the growing complexity of ecosystems or of some of their occupants, or even make very many interesting predictions, unless one states what makes one genotype more reproductive than another, and this is just what Maynard Smith fails to do. If a (sufficiently simple) population depends on a specific nutrient source (like bacteria in a chemostat), then the most fit genotypes are those that require the least food to maintain their numbers: the "survival of the fittest" then becomes a *predictive* statement. If we ask what morphological changes would reduce food requirements, we rapidly wander into problems we cannot solve: how are we to *describe* the population's options, let alone compare their merit?

The conference has nothing to say about the problems of description: a curious silence, in view of Elsasser's assertion that one cannot measure and describe an organism accurately enough to predict its development. The theorist's central problem is to find meaningful, compelling caricatures: descriptions which capture interesting aspects of biological phenomena, in terms of which one can make interesting predictions. It may be easy enough to show that the complexity of life grows if we can describe what we are talking about in a sufficiently simple yet interesting way. At another level, molecular biologists are acutely embarrassed by the need to decide what is wanted of a "physical representation of development."

In sum, this book gave me a lot to think about. It has two or three articles, especially Bohm's first paper, and Kerner's, which I found quite beautiful. Even the abominable papers, of which there were a number, are abominable in interesting ways and forced me to think about what biology should be. This may be a very personal reaction, however: I doubt if this book will have a very great influence, and doubt if it deserves to. It is simply an unvarnished record of the reactions of intelligent people to the oldest problems in science.

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Reproduction in Wild Animals

Cycles Génitaux Saisonniers de Mammifères Sauvages. Entretiens de Chizé, 1967. R. CANIVENC, Ed. Masson, Paris, 1968. viii + 168 pp., illus. Paper, 46 F. Série Physiologique, No. 1.

This volume is the record of the first of a series of annual conferences held at the Centre for Biological Studies of Wild Animals at Chizé (Deux-Sèvres), France. The meetings attempt to unite field and laboratory studies of wild animals, and, in this case, the disciplines of endocrinology and ethology. The papers summarize the authors' previous work or research in progress—they are not meant to review a field, or to replace detailed research results