NEW BIOLOGICAL BOOKS

The aim of this section is to give brief indications of the character, content, and cost of new books in the various fields of biology. More books are received by The Quarterly *than can be reviewed critically. All submitted books, however, are carefully considered for originality, timeliness, and reader interest, and we make every effort to find a competent and conscientious reviewer for each book selected for review.*

Of those books that are selected for consideration, some are merely listed, others are given brief notice, most receive critical reviews, and a few are featured in lead reviews. Listings*, without comments, are mainly to inform the reader that the books have appeared; examples are books whose titles are self-explanatory, such as dictionaries and taxonomic revisions, or that are reprints of earlier publications, or are new editions of well-established works.* Unsigned brief notices*, written by one of the editors, may be given to such works as anthologies or symposium volumes that are organized in a fashion that makes it possible to comment meaningfully on them.* Regular reviews *are more extensive evaluations and are signed by the reviewers. The longer* lead reviews *consider books of special significance. Each volume reviewed becomes the property of the reviewer. Most books not reviewed are donated to libraries at Stony Brook University or other appropriate recipients.*

The price in each case represents the publisher's suggested list price at the time the book is received for review, and is for purchase directly from the publisher.

Authors and publishers of biological books should bear in mind that The Quarterly *can consider for notice only those books that are sent to The Editors,* The Quarterly Review of Biology, *E-5340 Frank Melville, Jr. Memorial Library, Stony Brook University, Stony Brook, NY 11794-3349 USA. We welcome prepublication copies as an aid to early preparation of reviews.*

SEEKING COMMON GROUND BETWEEN PHILOSOPHY AND SCIENCE

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A review of

GENETICS AND PHILOSOPHY: AN INTRODUCTION. *Cambridge Introductions to Philosophy and Biology.*

By Paul Griffiths and Karola Stotz. Cambridge and New York: Cambridge University Press. $29.99 (paper). viii + 270 p.; ill.; index. ISBN: 978-0-521-17390-2. 2013.

Although there is no wall of separation between philosophy and science, there is often an uneasy feeling between those identified as scientists and those identified as philosophers. I did not know this as an undergraduate and enjoyed reading books such as *What is Life?* (Schrödinger 1945) or *Time's Arrow and Evolution* (Blum 1951). We remember the codescript and aperiodic crystal models in Schrödinger's book, but few scientists dwell on "negative entropy" as a source of life and the ultimate mysteries of the universe that Schrödinger also favored. When I entered graduate work at Indiana University in 1953 I was assigned to Tracy Sonneborn as my advisor because Hermann J. Muller was on sabbatical leave. Each student was required during the first year to give a departmental seminar on a topic of one's own choice. I decided to talk about "time's arrow," the popular term for thermodynamics that Blum used to discuss why evolution does not go backward. I prepared an outline and went to

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Sonneborn to discuss my talk about a month before I was due to give it. He looked at it, frowned, and said, "Elof, the last person to give a talk on this topic is cutting meat in Chicago." I went into panic mode, and spent every available hour while I was awake, and not otherwise obligated, in the biology library. I read about irreversibility in evolution by Muller and other faculty in the department and eventually gave that talk with one of the worst cases of stage fright I experienced. I was talking to Cleland, Sonneborn, Kinsey, Heiser, and other top biologists and my graduate student peers and superiors. Fortunately, it was a success and Sonneborn was the first to congratulate me.

When Muller returned and I got to know him better, he told me how much his mentor, Thomas Hunt Morgan, despised philosophy. Muller said he attended a public lecture at Columbia University as an undergraduate. The speaker was Jan Christian Smuts and he spoke on holism and emergent evolution. Muller described Morgan as so upset that when he stalked out of the lecture, his face was livid. At issue in these attitudes was the embrace by many scientists of a view we call reductionism. Scientists believe there is a material reality that they try to describe, analyze, and test. Some philosophers have objected that reality is more complex and living cells cannot be reduced into components and interpreted through those dismembered components. There is a siren song of holism that has appeared under many names over the past two centuries. It may be called vitalism, élan vital, mneme, entelechy, or general systems biology. Many of those proposing such a status for the dynamic living cell have argued that reductionist approaches destroy the integrity of the living system and thus they negate the claims of understanding how genes work, how developmental processes occur, or how a life cycle adapts to its circumstances. Holists often claim that interpretations of a dynamic state cannot be achieved by tearing things apart. At the same time there is a siren song of reductionism. Each new success in the life sciences encourages a belief that the latest model explains some aspect of biology such as the term gene, chromosome, natural selection, or mutation. Within a generation each of these concepts undergoes significant changes. The old definitions fall apart or become inadequate. Those who think their new work has supplanted the old sometimes express doubts that there is such a thing as a gene or natural selection.

Griffiths and Stotz have written an important book. It is a bridge between the way scientists traditionally approach their work and the way philosophers do theirs. When I read scientific articles or review articles and monographs, they are heavily saturated in findings, methods, technologies, the organisms involved, and the way concepts emerge from the abundant data and generate possible tests and applications. That is largely absent in *Genetics and Philosophy*. There is more emphasis on how scientists think, how they interpret their data, what their assumptions are, and what they fail to state. There are nine chapters. An introduction gives an overview of the book. The ensuing chapters discuss the Mendelian gene, the material gene, the reactive genome, outside the genome, the gene as information, the behavioral gene, and the evolving genome. The book concludes with a chapter presenting four conclusions.

The authors identify the conflict between the Mendelians and the statistical model of heredity based on ancestry used by Galton and Pearson. The authors dismiss the earlier Darwinian model of pangenesis and Wiesmannian-inferred units as speculative. Griffiths and Stotz describe Mendel's inferred units as tools. They skim the debates between Bateson and other geneticists in the first two decades of the 20th century on the names and implied composition or function of these units. Johannsen tried to clear this confusion by introducing the term gene as a unit lacking any attributes that might be taken as chemical or physiological. The Mendelian gene for the authors is thus a gene associated with transmission. They call this an instrumental function of the gene. Soon, of course, Morgan's school tied genes to the chromosomes through his analysis of the white-eyed mutation and his discovery of additional "sex-limited" traits, which made sense when he tried to interpret the odd ratios from crossing these sex-limited traits to one another. He concluded they were X-linked and he had found another

way in which transmission of traits occurred between the sexes. Many geneticists then expected a material basis for the gene would eventually be found. As the authors point out, this led to a molecular gene, but it was not, they argue, a paradigm shift of moving from classical to molecular genetics because there was no dissatisfaction involved in the corpus of classical genetics. It was the finding of what was expected ever since Johannsen named the gene in 1909.

The material gene turned out to be DNA. Speculation as early as 1896 in E. B. Wilson's *The Cell in Development and Inheritance* thought of Miescher's nuclein as a possible basis for the genetic component provided by the sperm to the egg. A long delay occurred after Phoebus Levene's tetranucleotide model demoted DNA, in many a geneticist's mind, to that of scaffolding for proteins. Olby (1974) and others have followed that long history. The mid-1930s saw a revival of interest in nucleic acids as hereditary material, especially through the use of ultraviolet light as a mutagen and its correlation to the UV absorption by DNA. Griffiths and Stotz argue that reductionism was indeed the basis for the transformation of the classical gene to the material (molecular) gene. They also bring out the important role of integration as numerous experiments demonstrated how those genes were organized and acted in viruses. The molecular and biochemical approaches yielded new findings of genetic roles in biochemical pathways, regulation by operons, and components of genes associated with transcription and translation. Most of this was worked out in bacteriophage, bacteria, and fungi. Very little, however, was known about multicellular eukaryotic organisms. These experimental approaches to gene action were running parallel to the physical approaches using X-ray diffraction and molecular model building that Watson and Crick and their competitors were using to resolve gene structure as a double helix of DNA. The avalanches of new knowledge from 1950–1980 created the glory days of molecular biology.

Things began to change as the 20th century came to a close. What Griffiths and Stotz call "the reactive genome" shifted genetic work from laboratory work on Petri dishes to complex machinery and computers to work out the sequences of genes and genomes. Genes turned out to be diverse in their organization. Bacteria and viruses were usually collinear in Crick's sense that the sequence of DNA determined the sequence of RNA and the latter determined the sequence of amino acids in the proteins coded by the genes. This was not true for eukaryotic genes with their organization into introns and exons and immense amounts of repetitive DNA. The cytoplasm of the cell turned out to be more complex and feedback relations from the cell surface to the nucleus and back to components of the cytoplasm made it harder to describe a gene as simply the maker of a product. Genes could be temporarily altered by methylation and their association with histones. The production of sperm or eggs differed in which genes were temporarily switched off. There were more proteins than genes to make them and this forced biochemists to look for mechanisms in which domains of proteins could join. This is still an ongoing field. As Griffiths and Stotz point out, each new component or process by which information flows from gene to product or from the cellular response to an array of environmental signals back to the genes creates a more complicated model of what is going on in the eukaryotic cytoplasm. Sonneborn rejected the lower status of the cytoplasm as the "playground of the genes," but all of his efforts to find "plasmagenes" failed. They turned out to be symbionts like kappa for his killer cytoplasm. I took all three of Sonneborn's courses on the genetics of microorganisms (algae, fungi, and protozoa) and when we asked him why cytoplasmic mutations were so rare compared to nuclear genes (even when the microorganisms were exposed to mutagens) he gave, it seemed to us, an unconvincing reply that the processes were so fundamental that they led to cell death if mutated.

Griffiths and Stotz are correct that the genomics diversity across the phyla, the attempts to figure out from proteomics how functional proteins are assembled, and higher levels of analyzing the role of genes through ecological or developmental studies

makes it more difficult to have a "one gene fits all" type of model. This is also true for their discussion of evolution and how genes are connected to adaptation, to the multiple ways of gene expression throughout the life cycle, and how new body plans emerge. The four important lessons in their concluding chapter are useful. They argue that the gene has multiple identities especially as units of transmission and information. Their second conclusion is that molecular epigenesis makes it difficult to reduce the gene to a single entity. They stress the importance of plasticity in phenotype diversity and intensity in response to changing environments. They favor networks rather than single causes for these complex expressions of the cell or organism. Their third conclusion is that reductionism involves two phases—it is mechanistic and analytical in determining the existence and function of cell components or processes but it is also integrative when using those components that are more likely to involve networks of associations than isolated causes. Their final conclusion is more difficult to assess. They believe the nature versus nurture argument requires an acceptance of the interplay of the environment with what they call the "Crick information" of the genes. But they see it is as a creative role for the environment with possible information of its own leading to the rich number of epigenetic events in the cell.

Some 60 years have passed since I began my graduate work in genetics. I, too, have learned a thing or two about science. Neither reductionists nor holists (or systems biologists as they like to call themselves today) feel it necessary to remind their readers or themselves that we can only interpret what we have access to. We do not know the future findings on the organization and dynamic activities of the cytoplasm. We do not know the cell-to-cell interactions that make a complete organism or that identify the changes taking place in the life cycle just through Crick information in the genome. We do not know the interactions of hundreds of species in an ecological natural setting. In not yet two centuries we have gone from an ignorance of genetics and biochemistry to a substantial insight into how things work, especially in the wisely chosen simpler forms of life such as viruses and bacteria. Those without a deep sense of the history of their field and necessarily with no knowledge of the future of their field, may conclude that the gene is dead or natural selection is antiquated or plays a minor role, but they should not be surprised if, two or three generations from now, these concepts are very much alive. Griffiths and Stotz's book will make such quick judgments of science less likely and give scientists and philosophers a much-needed respect for each other's contributions.

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