to raise their level of historical awareness and philosophical sophistication will find this study of Metchnikoff extremely valuable.

Thomas Söderqvist
Program in the History of Science, Stanford University, Stanford, CA 94305, and Department of Biology, University of Roskilde, DK-4000 Roskilde, Denmark
Craig Stillwell
Program in the History of Science, Stanford University, Stanford, CA 94305

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Pioneers of Embryology

A Conceptual History of Modern Embryology. SCOTT F. GILBERT, Ed. Plenum, New York, 1991. xiv, 266 pp., illus. $69.50. *Developmental Biology: A Comprehensive Synthesis*, vol. 7.

When we think of embryology around the beginning of this century, we might imagine that in those days progress was made slowly by investigators who worked in isolation and, unlike those of the present day, had time to think. Perhaps we even assume that recent advances, particularly in cell and molecular biology, have answered most, if not all, of the questions of those days. This book should dispel any such assumptions. It shows the pioneers of the discipline energetically competing for publication, disputing each other's findings, and traveling around the world to collaborate and disconcertingly reminds us of just how many of our current views about developmental control mechanisms are 100 or more years old.

Before about 1880, embryologists were addressing questions about developmental mechanisms almost exclusively by means of direct observation and drawing conclusions by extrapolation and imagination. One might describe their work as anecdotal, rather than mechanistic. Then Wilhelm Roux and contemporaries (1870–1910) such as Eduard Pflüger, Oskar Hertwig, Curt Herbst, and Hans Driesch began to use experimental manipulations to address specific questions about the rules governing cell behavior in the embryo, which we are now starting to rediscover. Some of the insights on which modern ideas are based came directly from their work. The first few chapters of the book concentrate on these early efforts (1800–1910), tracing in a concise and readable way the origins of the approach that became known as experimental embryology or developmental mechanics (*Entwicklungsmechanik*).

Johannes Holtfreter's autobiographical chapter, in which he reminisces about his life and career, is of a rather different nature from the rest of the book. It is in fact a letter, written in 1981, sent as an apology for Holtfreter's inability to accept an invitation to attend a meeting in Japan. For this reason, it is not exactly scholarly, but it is a real gem, particularly because Holtfreter recollects his feelings more than his science and records his perception of other contemporary scientists as people rather than just of their ideas.

The book is eclectic, but all the chapters are important and make interesting reading. Two themes recur throughout the book and give it a coherence uncharacteristic of collective volumes. One is the development of the concept of embryonic induction, by which one group of cells emits a signal that changes the direction of differentiation of another group of cells. Holtfreter's statement that "today there is barely anybody around who is still active in this once so exciting area of research" sounds ridiculous in 1992. But through the book one can trace the origins of this concept that is central to the way in which we now view development. It was not the famous experiment of Spemann and Mangold (1924) that led to the idea, nor even Spemann's earlier work on lens induction (1901–1906). These experiments were the culmination of a long-standing dispute over whether differentiation of the lens was a process arising entirely from within the cell

"The worries of an embryologist." [Johannes Holtfreter, in *A Conceptual History of Modern Embryology*]

or was an induced phenomenon; they were done 30 years after Pfeffer's first use of the term "induction" and at the same time as the publication of Herbst's monograph on developmental mechanisms in 1901. Herbst is the subject of a stimulating chapter by Jane Oppenheimer.

The second recurring theme is the dialectic between neo-epigenesis and neo-preformation, arising out of two centuries of debate about whether the embryo forms *de novo* or whether a germ (homunculus) is contained within the egg or sperm. The "neo" implies that the debate has not yet been resolved. Indeed, it continues today as a dialectic between genetics and epigenetics: the genetic explanation proposes that all the information required to build an embryo is contained within the genome; neo-epigeneticists, on the other hand, believe that even a complete knowledge of the genome and of the interactions between gene products will not suffice to understand development: we must also understand the rules that are imposed on the genome by the cellular environment. In the book, this debate is followed up to the 1980s in an interesting chapter by Jan Sapp.

Hans Spemann and some of his associates and Thomas Hunt Morgan were responsible for the introduction of genetics as a tool complementary to experimental embryology, although Morgan's contribution did not earn him a chapter in this book. His role is reviewed briefly in Gilbert's own chapter, which does a great job by filling some gaps and bringing together genetics and embryonic induction. Here one catches a few glimpses of Morgan and a good perspective on Waddington's contribution, as well as a timely survey of the role of Salome Gluecksohn-Schoenheimer (now Salome Gluecksohn-Waelsch) in research on the *T* (*brachyury*) gene, which as early as the 1930s was suspected to play an important role in induction and axial specification. New work on this gene, which has recently been cloned, is beginning to lead to the same conclusion.

Leon Browder's insight in including this volume in his "comprehensive synthesis" of developmental biology is laudable. Equally worthy of praise is Gilbert's choice of authors, although a brief reading list of other books on the history of developmental biology would have been nice. But this is a quibble. This book, together with Viktor Hamburger's *The Heritage of Experimental Embryology: Hans Spemann and the Organizer* (Oxford University Press, 1988), should be on the shelf, if not at the bedside, of every developmental biologist and should be read by everyone new to the field. It will be a source of real inspiration.

Claudio D. Stern
Department of Human Anatomy,
Oxford University,
Oxford OX1 3QX, United Kingdom

Drosophila Unfolded

The Making of a Fly. The Genetics of Animal Design. PETER A. LAWRENCE. Blackwell Scientific, Cambridge, MA, 1992. xiv, 228 pp., illus., + plates. Paper, $29.95.

The 1980s were years of extraordinary excitement in developmental biology because of the explosive coming together of genetics and molecular biology: for the first time mechanisms that direct the unfolding of the egg into a complex multicellular animal, the fruit fly, became clear. We now understand, at a satisfying level, quite a bit about the development of this animal.

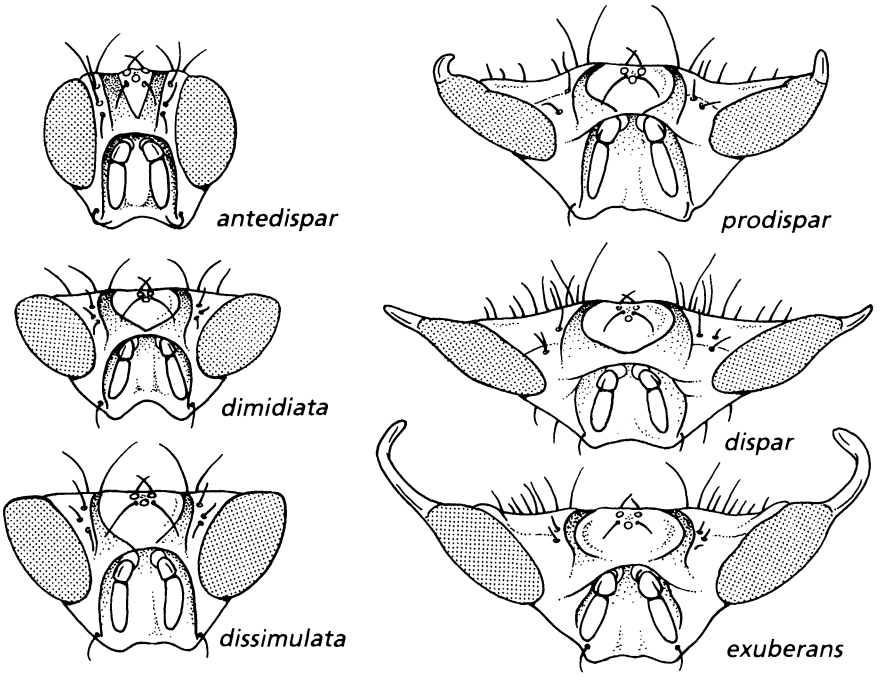
Peter Lawrence has put together much of what we know in his new book. From a deliberately personal and biased viewpoint, he describes what is known about early axis determination, segmentation, segment identity, bristle formation, and eye development.

Lawrence does not attempt to cover everything that is known about fly development. He has selected topics that he is particularly interested in, and in most cases these are areas in which he has made a personal contribution. It is a tribute to Lawrence's scientific good taste that his career has covered a broad range of fundamental issues in development. Segmentation, compartments, segment identity, muscle development, intrasegmental patterning, bristle spacing, and eye development are all areas that Lawrence knows intimately through his own work. He covers the facts in each of these areas, spicing them with his individual perspective.

Because of the range of topics covered and the chatty, easy-to-read style, this book will be of particular value to those with some background in developmental biology who would like an overview of the current understanding of *Drosophila* development. Physically it is a pleasure to read: it is printed on high-quality paper; the print is not cramped on the page; there are lots of figures.

Given the scope of the book, Lawrence makes amazingly few mistakes of fact. However, the facts are very dense in places. For instance in the segmentation section, I was concerned that the non-drosophilist who didn't already understand the relationship between *nanos* and *hunchback* and *knirps* would throw up his or her hands and complain again about all those silly gene names.

At some points I think Lawrence's informal style makes concepts murkier than they need to be. The analysis of genetic pathways is always a complicated business, and in his attempt to be easy to read Lawrence is sometimes imprecise to the point of being misleading. For instance, in several places he says that "downstream" genes serve "subordinate" functions. This is confusing because "subordinate" inevitably connotes being of lesser importance; surely he does not mean that the Bithorax Complex is less important than *Polycomb*, but I fear he could



"Heads of related species of *Drosophila*. These can be arranged in a series stretching from the mundane to the fantastical." [From *The Making of a Fly*]