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BOOK REVIEWS

Neurohormones in Invertebrates. M. C. THORNDYKE AND G. J. GOLDSWORTHY, eds. Cambridge University Press, New York, 1988, ix + 318 pp., illustr., subject index, $59.50.

This book is the thirty-third volume that has been generated by the seminar series of the Society for Experimental Biology, and is the product of talks given at a meeting held during 1986 in Bordeaux, France. Joosse has a prefatory chapter entitled, "What is special about peptides as neuronal messengers?" He included an interesting discussion of the selective advantages imparted by the evolution of peptides as chemical messengers. There are 15 additional chapters that fit into four major categories: Immunochemistry and Ultrastructure; Arthropod Neurohormones; Neurohormones in Coelenterates, Annelids and Protochordates; and Neurohormones in Molluscs.

The section on immunochemistry and ultrastructure consists of four chapters. The first chapter in this section deals with peptide release at the ultrastructural level from neurons of members of several invertebrate phyla (Golding and Pow). This chapter is followed by three that are concerned largely with immunocytochemical localization of various invertebrate-type and vertebrate-type neuropeptides in molluscs (Boer and van Minnen), insects (Rémy and Vieillemaringe) and crustaceans (Martin). The next section consists of five chapters that mostly discuss the isolation, characterization and roles of arthropod neuropeptides. Four of these chapters treat insects (Mordue and Siegert; Duve and Thorpe; Wheeler, Gäde and Goldsworthy; O'Shea, Hekimi, Witten and Worden) and one concerns crustaceans (Webster and Keller). The following section is especially welcome because it deals with the neuropeptides in coelenterates (Grimmelikhuijzen, Graff and Spencer), annelids (Porchet and Dhainaut-Courtois) and protochordates (Thorndyke and Georges), three groups that have not been receiving as much attention lately in reviews as have molluscs, crustaceans and insects. The final section consists of three chapters that are devoted to molluscs. One of these chapters is concerned mainly with the chemistry and physiology of the molluscan neurohormones that regulate such physiological processes as egg-laying and growth (Geraerts, Vreugdenhil and Ebberink). Another chapter treats the actions and roles of the FMRFamide family of neuropeptides in *Helix* (Cottrell, Davies, Turner and Oates). The concluding chapter consists mostly of a discussion of studies done with the *Aplysia* FMRFamide precursor protein and a CRF-like neuropeptide from this mollusc (Taussig, Nambu and Scheller).

This volume shows the advantages of using invertebrates to gain a clearer understanding of the evolution and functioning of the neuroendocrine systems in more complex species. Several of the authors made an important point, which is worth emphasizing here; that is, molecular biology, with such technology as the recombinant DNA techniques, holds great promise for major advances in our understanding of these neuropeptides in invertebrates. This book is well worth having in one's personal library in spite of the fact that the conference from which the chapters were drawn was held in 1986. The volume will be of particular interest to students and investigators who are interested in and involved with endocrinology and/or neurobiology whether of invertebrates or vertebrates.

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Prospects in Systematics, D. L. HAWKSWORTH, ed. Oxford University Press, Oxford, 1988, xx + 454 pp., index, $92.00.

This volume is the result of a three-day symposium that celebrated the 50th anniversary of the Systematics Association of Great Britain. The symposium had various aims, including a review of the achievements of the last 50 years, as well as an assessment of the role of systematics in the future of biology. On these subjects different observers are sure to have very different opinions. The editor, a botanist, was also the organizer of the conference, and the contents of the 25 chapters suggests that in shaping these proceedings he placed more emphasis on the practical aspects of systematics than he did on the conceptual. This seems to reflect his perception of the role of systematics (p. v): "the main challenge for systematics in the coming decades emerges as the need to re-establish its central unifying position in biology; this can only be realized by fulfilling the demands of its consumers." If one agrees with the proposition that the central role of systematics has been lost, why is this the case? One answer, it seems to me, is that for many years systematics de-emphasized historical pattern analysis in deference to microevolutionary "process analysis." Examine the pages of *Evolution, Systematic Zoology* during the 1950s and 60s, along with the major treatises of the so-called "evolutionary synthesis," and you will see a dearth of systematics applied to historical pattern analysis. The last decade or so, in contrast, has seen a reversal in this attitude: investigators in all fields—even including the "process" disciplines of behavior, genetics, and ecology—have realized that historical interpretation makes sense only within the context of an explicit phylogenetic hypothesis. Systematics has reasserted itself, not because of some ill-defined demand by a consumer, but because it has fought tooth and nail to interject historical pattern analysis into evolutionary biology. This is not to say that systematists are still not viewed by many as being on the bottom rung of the academic ladder, but the situation has clearly improved and will continue to do so.

This book barely reflects the conceptual revolu-

tions that have taken place in systematics over the last two decades. Instead of intellectual vigor, we are given academic reviews, often of minor subdisciplines within systematics. Interesting though this may be to some people, it will not intellectually challenge or inspire young people to take up systematics as a career, nor is it likely to convince nonsystematists of the importance of systematics. This volume speaks more to the past than to the future.

Many chapters deserve comment, but space is available to discuss only a few. Mayr's contribution, for example, is sure to be viewed as provocative. Thus, many biologists who reject the biological species concept (for very good reasons, by the way) will not appreciate having their views dismissed as "typological." Colin Patterson has some interesting things to say about the relationship between systematics and evolution, but he unfortunately proposes a method that purports to be able to root a tree unambiguously, not by polarizing characters but simply by looking at patterns of similarity. Any most parsimonious tree, however, can be rooted along any of its branches, hence something other than a phenetic assessment of character distributions is needed to find that root.

Given that cladistics has had an enormous impact on systematics, it is disheartening that this subject was given only passing mention in a chapter by P. H. A. Sneath that was devoted primarily to phenetics. Not only does he misunderstand the basic principles of cladistics, his discussion consists only of ill-informed criticisms.

In summary, this volume does little to convey the dynamic intellectual content of modern systematic biology: Systematic theory and methodology is barely mentioned; the major revolution that has taken place in biogeographic analysis is ignored altogether; classification theory and methodology is given perfunctory treatment; the roles that cladistic hypotheses play in speciation analysis, the study of biological diversification, the historical analysis of behavior, ecology, or host-parasite evolution, to name only a few, are likewise deemed unimportant. The Systematics Association missed an opportunity to make a bold statement about the necessity of systematics in contemporary comparative biology as well as to define the critical conceptual problems that will be directing future research. Instead, this symposium focused on the nuts and bolts of taxonomic (*sensu stricto*) and nomenclatoral practice, aspects of systematics that must assume a supportive role to considerations of theory and methodology if systematics is to occupy its rightful place in biology, namely, the cornerstone of comparative biology.

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The Age of Lamarck: Evolutionary theories in France—1790–1830. PIETRO CORSI, Revised and updated edition, Translated by JONATHAN MANDELBAUM. University of California Press, Berkeley, 1988, xiii + 360 pp., notes, bibliographical references, author and subject index, $42.00.

This scholarly work is concerned primarily with the debate on species and the evolution of life between the late 18th century and Darwin's era. It covers the history of science in France during the Revolution, the Consulate and the Empire—from the 1780s to the 1830s. The text is well documented with footnotes and an extensive bibliography. In preparing for this book, Corsi researched the various manuscripts of Lamarck and the other scientists between the years of 1780s and 1830s.

Jean-Baptiste-Pierre-Antoine de Monet de Lamarck, a collector of shells of the Paris basin, began his scientific career with an appointment by Buffon to the Jardin des Plantes in 1782, and in 1793, secured the chair in zoology for the study of insects, worms and microscopic animals in the Museum d'Histoire Naturelle. Prior to the 1800s, Lamarck was an opponent of the new chemistry, the Linnean system of taxonomy, a believer in the fixity of species, and that minerals were the result of the degeneration of living beings. He also ruled out spontaneous generation since he felt that the distinction between organic beings and nature was immense. Lamarck's post-1800 publications presented a total difference in his concepts of nature and the real world: he hinted at the possibility of spontaneous generation, and the prospect of a transformation of living beings. He may have been the first to introduce the distinction between invertebrates and vertebrates, and the first to introduce the term *Biology* into the literature. He now denied the fixity of species. As with a number of scientist-authors, Lamarck did not bother to mention the authors from whom he may have taken some of his doctrines.

In his *Recherches,* Lamarck suggested that the simplest forms of living beings could have been produced by nature, but this could not be proved with any certainty. He also felt that the diversity of animals may be due to the organism's nervous system, be it simple or complex. An individual's adaptation to a changing environment could have resulted from an organ's (system) use and disuse, and the individual's continuous need for that organ. A change in need triggered a mechanism that gradually changed that organ or sometimes created it. He insisted on the immense amount of time required for the production of complex living beings.

Lamarck's works were so extensively circulated that the transformist hypothesis had scientific credence in Europe. Three of his colleagues published some of the following ideas concerning life and nature. Jean-Baptist-Julien d'Omalius d'Halloy suggested that the agents responsible for the past changes on the earth's surface were largely identical to those now present on the earth's surface. Chiaverni postulated the existence of nerve cells too small to be seen by the human eye in organisms in which a nervous system could not be observed. According to Bory d'Saint-Vincent, man's physical, intellectual and moral qualities could be explained by natural laws.