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Libbie Henrietta Hyman: Life and Contributions*

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ABSTRACT

This issue of *Novitates* consists of papers presented at a symposium on the life and work of American zoologist Dr. Libbie Henrietta Hyman, 1888–1969, held at the annual meeting of the American Society of Zoologists in Atlanta, Georgia, December 1991. Judith E. Winston provides an introduction to Libbie Hyman's early years. Growing up in Fort Dodge, Iowa, young Libbie demonstrated a love of nature and a drive for learning that eventually led to a scholarship at the University of Chicago, where she majored in zoology. Jane Maienschein covers Libbie Hyman's Chicago period. During that period Libbie gained experience in experimental biology by participation in Charles Manning Child's research program on metabolic gradients, which applied the "Chicago style" of biology.

The lack of good manuals for the comparative anatomy and zoology labs she taught as a graduate student led Libbie to develop her own laboratory manuals, published by the University of Chicago Press. Marvalee Wake discusses Libbie Hyman's interactions with the press about these guides. Hyman's correspondence with press officials revealed her growing frustration as she desired more time to work on invertebrates, but was persuaded to revise vertebrate anatomy texts instead. Despite her protests, her seminal ideas and approaches to learning vetebrate anatomy were profoundly important.

Judith Winston discusses Libbie's productive career at the American Museum of Natural History. In 1930, Dr. Hyman left Chicago to pursue the invertebrate work that interested her most—and found a welcome in G. K. Noble's Department of Experimental Biology at the AMNH. With his help she obtained an unpaid position as a research associate, office space, and use of the AMNH library, vital to her project, a treatise on invertebrate zoology. Her 6-volume treatise, *The Invertebrates*, was published between 1940 and 1967 by McGraw-Hill. In 1943 she transferred to the AMNH Department of Invertebrates. Neil Landman outlines the history of that Department in the Museum, and Libbie's connection with it.

M. Patricia Morse discusses Dr. Hyman's influence on invertebrate zoology in general. Her treatise set the tone for invertebrate zoology courses and the publication of books on the subject. Each volume was eagerly received by zoologists, not only for thorough coverage of the literature (including non-English language literature), but also for uniformity of approach, comprehensive illustrations, and thoughtful synthesis of phylogenetic relationships for each group covered.

Robert Ogren discusses Libbie Hyman's contributions to land planarian taxonomy. Hyman was the first American zoologist recognized as an authority on Turbellaria, Tricladida, and Terricola. Contributions began after her 1937 appointment as research associate at the American Museum of Natural History and continued for 25 years, resulting in 11 taxonomic papers, the last published in 1962.

Seth Tyler discusses Libbie Hyman's overall influence on the systematics of turbellarian flatworms, especially through the comprehensive review of flatworms published in Volume II of *The Invertebrates*. The system of classification she adopted for the phylum Platyhelminthes was that of Bresslau, dating to 1933. Modern systematists have clarified the phylogenetic relationships of flatworm groups, in particular by using characters discerned with electron microscopy; and application of principles of cladistic systematics has been important in grouping turbellarians and the major groups of parasitic flatworms into supraordinal taxa. A number of competing systems for these higher-level groupings have been proposed, and these are being tested with molecular techniques comparing nucleic-acid sequences. Still, the current best-accepted system clearly bears Hyman's stamp; her views of evolution in the phylum and its taxonomy are still relevant.

INTRODUCTION

JUDITH E. WINSTON

The best-selling laboratory manuals she began writing while still a graduate student herself (Hyman, 1919, 1922) made Libbie Henrietta Hyman a familiar name to undergraduate and graduate biology students from the 1920s to the 1960s. Beginning with the publication of volume 1 in 1940, her six volume treatise, *The Invertebrates*, established her as one of the strongest influences on 20th century invertebrate zoology and made her famous among zoologists around the world. The name of Libbie Hyman is also closely linked with that of the American Museum of Natural History (AMNH), her scientific home for the last 37 years of her life. Although never a paid staff member at the AMNH, she was dedicated to the museum, particularly to the Department of Invertebrates, where she spent her final working years.

After her death in 1969, her personal papers, photographs, and other memorabilia came to the Department of Invertebrates. Discovering them in department files when I arrived at the AMNH as an Assistant Curator of Invertebrates renewed my interest in the story of her life and added to my appreciation of her as a person. My favorite portrait (fig. 1) shows her as a bright-eyed school girl of six or seven, dressed in ruffled Victorian style, with her dark hair coaxed into stiff ringlets. Looking at it I am reminded not only of my own young daughter, but also, that, in a scientific sense, I am one of Libbie Hyman's intellectual daughters. Although she had no children herself, her encompassing review of the invertebrates and its influence on the field make all present day invertebrate zoologists her scientific descendants.

I found other zoologists very interested in the story of Libbie Hyman. When I mentioned that I worked at the AMNH, they would question me about her life (even though she had died years before I came to New York). I realized that the scientists who had actually known her were getting older themselves, and I started collecting information about Libbie Hyman from them: correspondence, photographs and oral history in the form of taped interviews. I was joined in this project by Anne Fausto-Sterling, a biologist from Brown University, who is working on a complete biography of Libbie Hyman. A brief biography of Libbie Hyman had been published in 1943, as a chapter in a book for girls called *American Women Scientists* (Yost, 1943) and after her death, Horace Stunkard used material from her unpublished biography in AMNH Department of Invertebrates files to write a memorial preface to a volume on Turbellarian biology published in her honor (Stunkard, 1974). Rachel Fink, a developmental biologist at Mount Holyoke College with a strong interest in the history of biology, had written an article about Libbie Hyman for the *Dictionary of Scientific Biography, Supplement II* (Fink, 1987). Rachel's interests and mine came together as an idea for a symposium: *Libbie Henrietta Hyman: Life and Contributions*, which took place at the December 1991 meeting of the American Society of Zoologists in Atlanta. For this meeting we also created an exhibit of photographs and memorabilia celebrating her life and work and I wrote a short biography for the ASZ Invertebrate Zoology Newsletter (Winston, 1991). The interest expressed by biologists who heard the symposium and saw the exhibit, and their enthusiastic support for making her life and photographs more widely available to students and biologists today, finally led to this publication.

ACKNOWLEDGMENTS. Thanks to all who made this symposium and its publication possible, especially Rachel Fink, who instigated the symposium, to the American Society of Zoologists (now SICB) Divisions of History and Philosophy of Science and Invertebrate Zoology for their sponsorship, those who shared sources of information or their remembrances of Libbie Hyman, including Ralph and Mildred Buchsbaum, Bill Emerson, Howard Evans, Max and Bessie Hecht, Fred Schram, our reviewers, Steve Cairns, Jon Norenburg, and George Shinn, for their helpful comments on the manuscripts, and Neil Landman and the AMNH Scientific Publication Committee for supporting its publication in the *Novitates*.

Fig. 1. LIBBIE HYMAN as a young girl in Fort Dodge, Iowa. She said in her autobiography: "I was conscious from a young age of a strong interest in nature. . . . As a child I roamed the woods that bordered the town, hunting the spring wild flowers. . . ."

STUDY OF INVERTEBRATES AT THE AMERICAN MUSEUM OF NATURAL HISTORY

NEIL H. LANDMAN AND JUDITH E. WINSTON

This chapter chronicles the study of invertebrates at the American Museum of Natural History (AMNH) starting from its establishment up to the present day. As used here, the term *invertebrates* covers about 40 phyla, both extant and extinct. These phyla are mostly, but not exclusively, marine; they do not include terrestrial arthropods and spiders, which are traditionally treated as part of entomology.

Over the years, invertebrate research at the AMNH has been conducted in many different departments, depending on the organizational structure at the time. These departments have included, not necessarily in chronological order, the departments of Geology, Invertebrate Paleontology, Conchology and Mineralogy, Marine Zoology, Fishes and Aquatic Biology, Lower Invertebrates, Invertebrate Zoology, and Invertebrates. These organizational changes reflect fluctuations in the size of the curatorial staff, shifts in scientific emphasis, and/or administrative efforts to streamline the organization of the Museum.

In conjunction with the growth of invertebrate research at the Museum, the invertebrate collections have also grown through a combination of fieldwork, purchases, and donations. The collections began with two major acquisitions in the 19th century: the purchase of the James Hall collection of fossil invertebrates (approximately 100,000 specimens) and the purchase of the John C. Jay collection of Recent molluscs (approximately 50,000 specimens). Today, the collections comprise approximately 8.5 million specimens, half of which are fossil and half of which are Recent invertebrates. Of the latter, approximately 3 million are Recent molluscs. There are approximately 35,000 primary and secondary types.

During the last 125 years, about 30 curators have been actively involved in invertebrate research, most of whom are treated in this chapter (table 1). The dates given in parentheses after each curator's name in the text indicate the years during which the curator was active at the Museum.

The first invertebrate curator was Robert Parr Whitfield (1877–1910). Whitfield was hired as Curator of Geology to care for the recently acquired James Hall fossil invertebrate collection. In addition to his collection responsibilities, Whitfield did research on a wide variety of invertebrate fossils, many of which were collected as part of government surveys. He published papers on Cretaceous molluscs from South Dakota and New Jersey, Devonian brachiopods from Ohio, Miocene crustaceans from New Jersey, Devonian bivalves from New York, Carboniferous fossils from the Arctic, and Ordovician fossils from the Lake Champlain region. He also published descriptions of several Recent species of corals and sponges. Many of his articles appeared as AMNH Bulletins, a series he helped establish.

Whitfield was assisted in his curation of the invertebrate fossils by Louis P. Gratacap (1876–1917) and Edmund Otis Hovey (1894–1924). Gratacap was primarily a mineralogist but helped prepare exhibits on invertebrate fossils and Recent molluscs. Hovey assisted Whitfield in compiling a type catalog of the Museum's fossil invertebrate collection (Whitfield and Hovey, 1898–1901). Hovey later became famous for his eyewitness account of the volcanic activity on Martinique in 1902. He was also in charge of a Museum expedition to Arctic Greenland (the Crocker Land Expedition) where he overwintered for two years (1915–1917).

Roy Waldo Miner (1905–1943) was one of the most influential invertebrate curators in the first half of the 20th century. Miner studied marine invertebrates, with a side interest in myriapods, and did fieldwork in New England and the Caribbean, organizing expeditions to the Lesser Antilles, Puerto Rico, and the Bahamas. He did not publish many scientific papers but his *Field Guide to Seashore Animals* (Miner, 1950) became the standard field guide for northeastern U.S.

TABLE 1
Invertebrate Curators at the AMNH[a]

L. P. Gratacap, Curator (1876–1917): mineralogy, conchology
R. P. Whitfield, Curator (1877–1910): ammonites, invertebrate paleontology
E. O. Hovey, Curator (1894–1924): invertebrate paleontology
R. W. Miner, Curator (1905–1943): invertebrate zoology
B. E. Dahlgren, Assistant Curator (1907–1908): invertebrate zoology
H. E. Crampton, Curator (1909–1921): pulmonate gastropods
C. A. Reeds, Curator (1912–1938): stratigraphy
W. G. Van Name, Associate Curator (1916–1942): isopods, tunicates
H. W. Stunkard, Research Associate (1921–1989): parasitic flat worms
L. H. Hyman, Research Associate ((1937–1969): animal behavior, cnidarians, platyhelminths
H. E. Vokes, Associate Curator (1937–1943): Recent and fossil molluscs
J. C. Armstrong, Assistant Curator (1939–1953): crustaceans
O. H. Haas, Associate Curator (1940–1955): ammonites, fossil nautiloids
B. Ellis, Curator (1942–1967): micropaleontology
A. R. Messina, Curator (1942–1968): micropaleontology
G. H. Childs, Assistant Curator (1943–1959): anatomical models of invertebrates
N. D. Newell, Curator (1945–present): fossil bivalves, extinctions, paleoecology
W. D. Clarke, Curator (1953–1955): crustaceans
W. K. Emerson, Curator (1955–present): Recent and fossil molluscs
D. F. Squires, Curator (1955–1961): Recent and fossil corals
D. E. Bliss, Curator (1956–1987): crustacean physiology
M. L. Jones, Assistant Curator (1960–1963): polychaete worms
R. L. Batten, Curator (1961–1986): fossil gastropods
E. Kirsteuer, Curator (1965–1986): nemertaen worms
N. Eldredge, Curator (1969–present): trilobites, evolutionary theory
B. N. Haugh, Assistant Curator (1979–1981): fossil echinoderms
J. E. Winston, Curator (1980–1992): bryozoans
N. H. Landman, Curator (1982–present): ammonites, nautiloids
W. C. Wheeler, Associate Curator (1989–present): arthropods, molecular biology
A. W. Harvey, Assistant Curator (1994–1998): crustaceans
P. M. Mikkelsen, Assistant Curator (1997–present): molluscs

[a] Dates represent the time during which the individual was active at the Museum, including years following retirement. The position listed, e.g., associate curator, indicates the position held at the time of retirement or at the time this chapter was written. For purposes of brevity, this list does not include research associates with the exception of Horace Stunkard and Libbie Hyman.

marine invertebrates until it was replaced in 1978 by Kenneth Gosner's guidebook.

Miner's greatest contributions were in the area of scientific popularization and exhibition. He described the methods he used to create exhibits in articles written for *Natural History*, its predecessor, the *American Museum Journal*, and for Museum exhibit guidebooks.

One of his first exhibits was "Animals of the Wharf Piles" completed in 1913. It was incorporated into the "Darwin Hall," an area devoted to explaining the classification and evolutionary relationships of animals (Miner, 1913). This hall was located in the space presently occupied by the "Hall of North American Forests." "Animals of the Wharf Piles" showed an abandoned wharf at Vineyard Haven, Mass., above and below the water surface. Development of this exhibit combined fieldwork, photography, and sketches made in the field using glass-bottom boxes to study the sea bottom, as well as drawings and photographs of living animals maintained in seawater aquaria.

Miner also developed exhibits on freshwater invertebrate life (fig. 2). "The Rotifer Group" featured glass models blown by Herman Mueller (Miner, 1928). This exhibit, also located in the Darwin Hall, depicted the microscopic life in a drop of pond water magnified to more than 4 ft across. The front of the exhibit was constructed to look like a huge magnifying glass.

Perhaps Miner's most famous exhibit was a coral group constructed for the Hall of Ocean Life and modeled after a coral reef off Andros Island in the Bahamas. At the site, biologists descended in a "submarine tube" to photograph and draw the living corals (Miner, 1933). Fishes were brought aboard ship, and placed in aquaria, where an artist rapidly sketched them before their colors could fade. A modeler then made plaster molds of each fish, from which wax models could later be cast.

Miner collaborated with several other curators during the time he was at the Museum including Brer Dahlgren, George H. Childs, and John C. Armstrong. Dahlgren (1907–1908) accompanied Miner on two field trips to collect animals from coral reefs. Most of Dahlgren's other activities at the Museum fo-

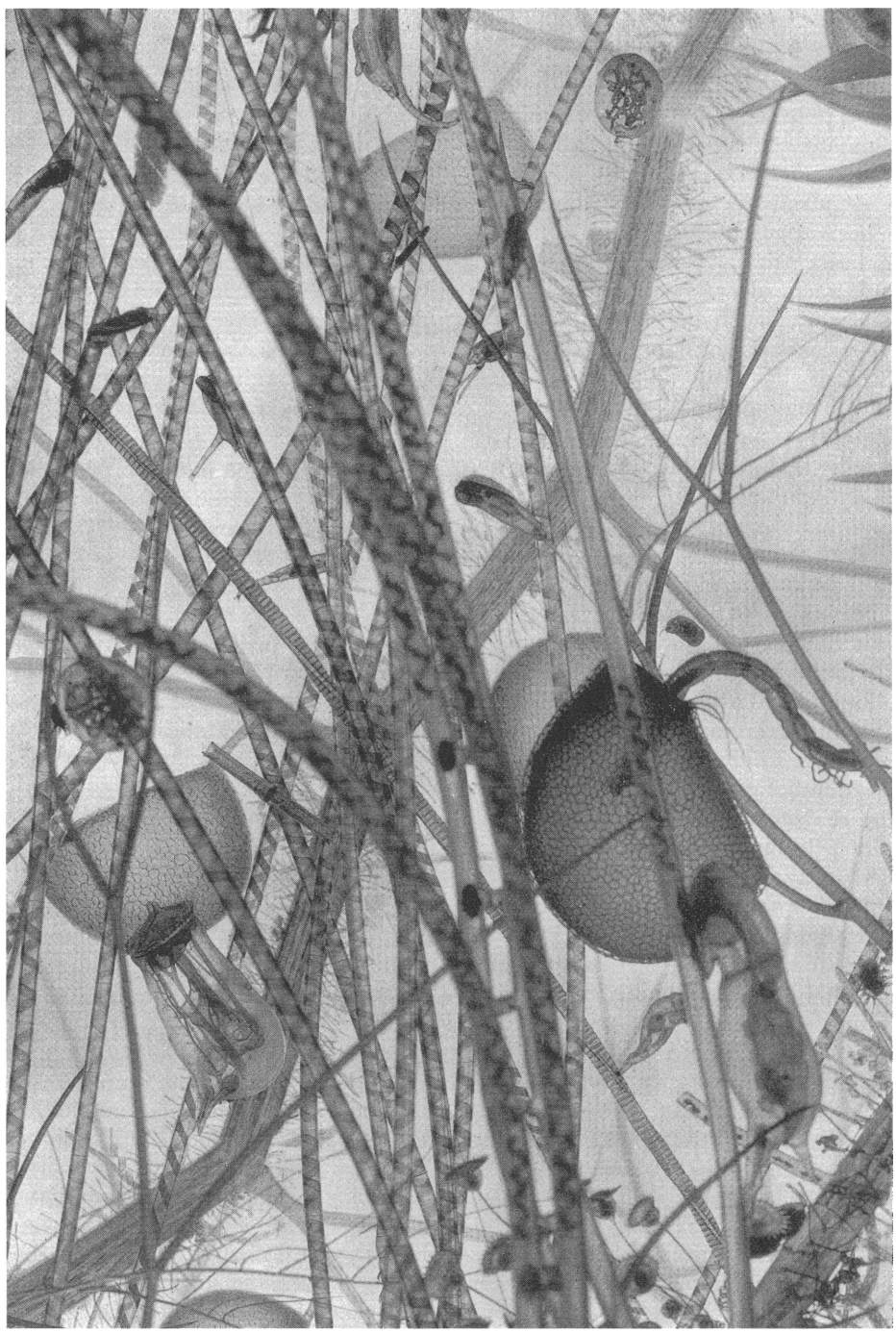

Fig. 2. A detail of the "Rotifer Group," an exhibit designed by Roy Waldo Miner in 1928. It depicts the microscopic life in a drop of pond water and features glass models assembled by Herman Mueller.

cused on the preparation of a botanical exhibit in the 1940s after he had already moved on to the Field Museum in Chicago.

Childs (1943–1959) accompanied Miner on several expeditions in the 1940s to the Bahamas and Bar Harbor, Maine, and helped create the exhibits that grew out of these trips. Most of Childs' activities at the Museum involved the preparation of large-scale anatomical models of invertebrates. He cooperated with Hermann Mueller on constructing glass models of coelenterates, annulates, molluscs, and prochordates. Mueller prepared the glass portion of each model and Childs prepared the wax portion. Many of these models are still on display in the Museum today, for example, in the "Hall of Biodiversity."

Armstrong (1939–1953) joined Miner on an expedition to Tarpon Springs, Florida. Armstrong was an oceanographer with broad interests who published several papers on crustaceans from Bermuda and the eastern Pacific. He also did fieldwork along the eastern coast of the United States, along the coasts of Central and South America (with Robert C. Murphy, Curator in the Department of Ornithology), and at the Museum's Lerner Marine Laboratory on Bimini Island (Bahamas).

One of the major subject areas of curatorial invertebrate research at the AMNH has been the study of Recent and fossil molluscs, including gastropods, bivalves, and cephalopods. The first invertebrate curator, Whitfield, published papers on Cretaceous molluscs from New Jersey and South Dakota. Henry E. Crampton, who joined the Museum in 1909 and was concurrently a professor at Barnard College, studied land snails from the Pacific region, particularly species of the genus *Partula*. To pursue his research, he made many trips for study and collection to the tropical Indo-Pacific including Tahiti, Samoa, the Philippines, Java, Australia, and Hawaii. He also collected in the interior of South America and the West Indies.

Crampton's primary interest was focused on documenting a correlation between the geographic distribution of *Partula* species and their morphological variation. He viewed this as a model system to understand the mechanics of species differentiation and adaptation. In an account of one of his trips, he wrote "The abundant collections in hand give a perfect demonstration of the principles of geographical distribution. Each island possesses its own species, while its different valleys have forms that are usually markedly different" (Crampton, 1910: 126, 127). Crampton prepared an exhibit for the Darwin Hall consisting of a relief map of Tahiti showing the distribution and variation of land snails.

Harold E. Vokes (1937–1943) specialized in Eocene and Pliocene molluscs. He did extensive fieldwork, especially on the western coast of North America, where he spent several months each summer. He also traveled to Oklahoma, New Mexico, and Texas where he collected Permian fossils and to Alabama where he collected Cambrian material. He accompanied Barnum Brown, Curator in the Department of Paleontology, into the field in 1939 to collect molluscan fossils from Montana and southern Alberta. In 1940, he visited Syria to study the Jurassic and Cretaceous molluscs from the Lebanon Mountains.

Most of Vokes' research focused on taxonomic descriptions of new species and documentation of the stratigraphic sequences in which they occurred. He also described the biology of several Recent molluscs including a note on the rate of migration of the gastropod *Crepidula convexa*, and a paper on the gastropod fauna of the intertidal zone at Moss Beach, California.

Norman D. Newell joined the staff in 1945 and was concurrently a professor at Columbia University. Newell specializes in the systematics and evolution of Paleozoic bivalves. In addition, Newell has pursued a broad range of research projects related to the evolution of invertebrates and the interpretation of their fossil record. He did extensive fieldwork in Peru, deciphering the geologic history of the area and describing the local fossil invertebrates. One of Newell's major contributions was the establishment of the field of paleoecology, which integrates the concepts of modern ecology into paleontology. He demonstrated the utility of this approach in his studies of the Permian reefs of the Guadalupe Mountains of Texas, which he interpreted on the basis of his research on modern coral reefs in the Pacific Ocean.

Newell was also among the first to recognize the importance of the effects of mass extinctions on the history of life on earth. He

documented the changes in the bivalve fauna at the Permian-Triassic boundary, the largest extinction event in earth history. Newell also pointed out that we are currently in the grip of another extinction crisis, this one caused by human activities. Newell oversaw the growth of the invertebrate fossil collections and, in the late 1960s, helped design the "Hall of Earth History" (in the space now occupied by the "Orientation Center" on the fourth floor).

William K. Emerson arrived in 1955 and studies marine gastropods, mostly from the Pacific region, with a focus on their systematics and biogeography. He has documented the presence of Indo-Pacific molluscs on the eastern side of the Pacific Ocean. He reasoned that these animals must have dispersed across a deep- water barrier. Emerson argued that during anomalous warm periods (now commonly referred to as El Niño events), larvae of these species must have crossed the Pacific to establish insular populations. Emerson also explored the effects of oceanic upwelling of cold water on local molluscan populations along the western coast of North America. He applied these insights in explaining the distribution of Pleistocene molluscan communities.

In addition to his scientific papers on molluscan taxonomy and biogeography, Emerson published several popular guidebooks to the molluscs of North America. He also helped design the Museum's "Hall of Mollusks and Our World," which opened in 1975 (originally as "Mollusks and Mankind"). This exhibit explains the natural history of molluscs and how they have been used by various cultures.

Roger L. Batten joined the staff in 1962 and was concurrently a professor at Columbia University. He also specialized in gastropods, with an emphasis on the systematics and evolution of Late Paleozoic forms from England and the United States. He did extensive fieldwork in these areas and helped expand the fossil invertebrate collections. He also published a popular textbook on the evolution of the earth and its biota. In the late 1960s, he worked with Newell on the Museum's Hall of Earth History, helping to assemble exhibits that explained the biology of fossil invertebrates and included dioramas reconstructing the invertebrate life on the sea bottom during past geological epochs.

The most recent addition to the list of gastropod curators at the AMNH is Paula Mikkelsen (1997–present). Her studies concentrate on the systematics and evolutionary relationships of marine opisthobranch gastropods. She is also concerned with broader issues of marine molluscan biodiversity in the tropical western Atlantic, especially the Florida Keys.

Two curators, Otto Henry Haas and Neil H. Landman, in addition to Whitfield, have studied fossil cephalopods, mainly ammonites. Otto Henry Haas, who arrived in 1940, was unusual in holding doctorates in both law and science (both from Vienna). He described the Cretaceous ammonites from Angola that had been previously collected on one of the Museum's expeditions (Vernay Expedition, 1925–1927), as well as a variety of other Cretaceous ammonites from Wyoming. In addition, he published monographs on Eocene nautiloids from British Somaliland (presently Somaliland) and Jurassic ammonites from Syria.

Neil H. Landman joined the staff in 1982 with a specialty in Late Cretaceous ammonites from North America. He has expanded the holdings in these areas through collecting trips to South Dakota, Montana, and Wyoming. His studies cover a wide range of subjects on fossil and modern cephalopods including the embryonic development of ammonites, the systematic relationships among scaphitid ammonites, and the life history of the pearly nautilus from the tropical Indo-Pacific.

The second most popular subject of curatorial invertebrate research after molluscs has been arthropods as exemplified by the research of four curators, Dorothy E. Bliss, Niles Eldredge, Ward C. Wheeler, and Alan W. Harvey. Dorothy E. Bliss (1956–1987) studied the neuroendocrinology of crustaceans. Most of her research focused on the hormonal control of growth and water balance in tropical land crabs. In the 1960s, she helped design the "Hall of the Biology of Invertebrates" (in the space now occupied by the Hall of Biodiversity on the first floor).

Niles Eldredge joined the staff in 1969 and is concurrently a professor at Columbia University. He works on the systematics of fossil

arthropods (trilobites). In addition, he has published on many issues in evolutionary theory and biodiversity, most of which emphasize the interplay between ecology and evolution. His early papers, alone or coauthored with Stephen J. Gould (Harvard University, who, like Eldredge, was a student of Newell) described the theory of punctuated equilibria, which critically reexamined the patterns of evolution represented in the fossil record. Eldredge continues to publish articles and books on a wide range of topics. He also recently helped design and organize the Museum's new Hall of Biodiversity. This exhibit portrays the rich variety of life on earth using actual specimens, models, and film footage of animals in their natural habitat. The exhibit reminds us of the alarming loss of biodiversity at the present time due mostly to habitat destruction.

Other curators of arthropods include Ward C. Wheeler (1989–present), who is developing new approaches using molecular techniques and computer technology to investigate phylogenetic relationships, and Alan W. Harvey (1994–1998) who specializes in hermit crabs.

Aside from molluscs and arthropods, several other invertebrate groups have been the subject of curatorial study. Willard Gibbs Van Name (1916–1942) specialized in tunicates, isopods, and ascidians. Van Name increased the size of the invertebrate collections in these areas through field trips to the western coast of the United States, Bermuda, Panama, the Philippines, and the Dutch East Indies.

Donald F. Squires (1955–1961) studied fossil and Recent corals. He worked and published papers on the coral reefs of New Zealand and Anno Atoll in the Marshall Islands. He also did fieldwork on corals at the Museum's Lerner Marine Laboratory on Bimini Island (Bahamas).

Meredith L. Jones (1960–1963) and Ernst Kirsteuer (1965–1986) both studied worms. Jones, a polychaete annelid specialist, investigated the population dynamics of marine benthic invertebrates in San Francisco Bay. Kirsteuer specialized on the systematics and geographic distribution of nemertean worms, especially those from cryptic coral reef habitats. Through his research, he developed new techniques for sampling animals in these habitats.

Judith E. Winston (1980–1992) studies the systematics and ecology of marine bryozoans from the western Atlantic, Caribbean, and Antarctic regions. During her time at the Museum, Winston published several monographs on bryozoan systematics as well as papers on bryozoan life history and behavior. She recently published a book on taxonomic procedures, which showcases the work of many AMNH scientists.

In addition to the curatorial staff, many other scientists have been affiliated with the Museum. These scientists, known officially as research associates, have contributed significantly to the study of invertebrates at the AMNH. Approximately 50 research associates have studied invertebrates at the Museum in the last 100 years. These include A. C. Treadwell (1910–1947), F. J. Myers (1920–1947), A. K. Miller (1943–1952), L. Marcus (1976–present), J. J. Lee (1960–present), S. J. Gould (1974–present), and D. W. Boyd (1968–present). Two of the most famous research associates, Horace W. Stunkard and Libbie H. Hyman, are further discussed below.

Horace W. Stunkard was research associate from 1921 to 1990, the longest recorded association with the Museum. He was Professor in the Department of Biology at New York University from 1916 to 1954, but shortly after his retirement from that institution, was given a laboratory at the Museum and continued to work there until a few years before his death at age 100.

Stunkard was a parasitologist and published nearly 300 papers on the life cycles and descriptions of parasitic flatworms. These include trematodes (flukes) and cestodes (tapeworms). His studies resulted in a greater understanding of the general biology of these worms and the development of approaches for the treatment of diseases caused by them.

One of Stunkard's most famous discoveries was his explanation of how anoplocephaline cestodes are transmitted to sheep and other herbivorous animals. In the late 1920s these tapeworms were causing significant losses to sheep and cattle ranchers. In 1937 Stunkard discovered that oribatid mites liv-

ing on grass are the intermediate host for the tapeworms. Because the mite has a short life cycle, Stunkard suggested that the problem could be controlled by simply removing the sheep from the mite-infested pasture for one year, thereby resolving a major health crisis.

Libbie H. Hyman arrived at the Museum in 1937. She was initially accommodated in the Department of Experimental Biology, the forerunner of the Department of Animal Behavior. Her goal from the start was to compile an English-language treatise on the invertebrates. Most of Hyman's time was spent in the Museum library, making notes from the literature, and in her office, either writing or illustrating the volume on which she was currently working. She spent her days concentrating on her textbooks and her evenings studying her favorite invertebrate animals, planulid worms. Hyman was very self-sufficient in her research and continued working throughout the last year of her life even though she was confined to a wheelchair (W. K. Emerson, personal commun., 1997).

During the summers, Hyman visited marine laboratories to observe and illustrate freshly collected organisms and to learn the latest discoveries from colleagues working on particular groups. Over the years, she worked at the Hopkins Marine Station (Pacific Grove, California), Friday Harbor Laboratories (Friday Harbor, Washington), the Bermuda Biological Station, the Marine Biological Laboratory (Woods Hole, Massachusetts), the Woods Hole Oceanographic Institution, the Lerner Marine Laboratory (Bimini Island), and the University of São Paulo (Brazil).

Hyman described her research as combining a "small amount of original knowledge with a large amount of compilation from the literature" (Hyman, 1951:v). However, a reading of her treatise reveals that the amount of original knowledge was not trivial. Throughout her research, Hyman employed the scientific method: observation leading to questions leading to tentative hypotheses leading to predictions that might be tested by further observation and experimentation.

In conclusion, the study of invertebrates at the AMNH has had a long and rich history. Starting with Whitfield, curators and research associates have studied the systematics and life history of a wide variety of invertebrate animals. Through the design and development of Museum exhibits, scientists have helped to communicate this knowledge to the general public. Museum scientists have also contributed to the training of students through cooperative programs with universities. At the same time, the collection of invertebrates has also expanded so that today it represents one of the largest and finest collections in the world. With the increasing awareness of the enormous contribution of invertebrates to biodiversity, this tradition of invertebrate research at the Museum continues into the third millennium.

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REFERENCES

Crampton, H. E.
1910. A fourth journey to the South Seas. Am. Mus. J. 10(5): 122–132.

Hyman, L. H.
1951. The invertebrates, vol. 3. New York: McGraw-Hill, 572 pp.

Miner, R. W.
1913. Animals of the wharf piles. Am. Mus. J. 13: 86–92.
1928. A drama of the microscope. Nat. Hist. Mag. 28(5): 501–514.
1933. Diving in coral gardens. Nat. Hist. Mag. 33(5): 461–476.
1950. Field book of seashore life. New York: Putnam, 888 pp.

Whitfield, R. P., and E. O. Hovey
1898–1901. Catalogue of the types and figured specimens in the palaeontological collection of the Geological Department, American Museum of Natural History. Bull. Am. Mus. Nat. Hist. 11: 500 pp.

LIBBIE HYMAN AND THE AMERICAN MUSEUM OF NATURAL HISTORY

JUDITH E. WINSTON

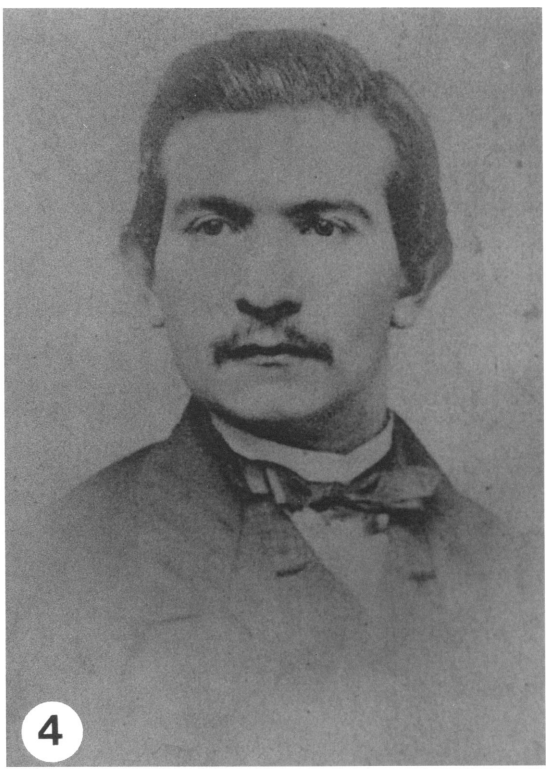

Fig. 3. LIBBIE HYMAN, shown here with her mother SABINA and brothers SAMUEL and ARTHUR, was born in Des Moines, Iowa, on December 6, 1888, the third of the Hymans' four children and the only daughter.

Fig. 4. Libbie Hyman's father, JOSEPH HYMAN, who immigrated to the United States from Konin, Poland. With his friend David Goldman, he moved west to Des Moines, Iowa, where the two men built and operated a successful clothing store. In Des Moines Joseph met and married SABINA NEUMANN, an immigrant from Stettin, Germany. Soon after Libbie's birth, the family moved further west, first to Sioux Falls, South Dakota, then back to Fort Dodge, Iowa, where Libbie grew up.

CHILDHOOD YEARS: 1888–1905

Libbie Hyman was born in Des Moines, Iowa, December 6, 1888, third child and only daughter of immigrant Jewish parents. Libbie's father, Joseph Hyman, had come to the United States from Konin, Poland (then within the Russian Pale), via London, where he had lived and worked as a tailor for several years. With a friend he had met in the United States, David Goldman, he moved west to Des Moines. There the two men formed a partnership and built and operated a successful clothing store. In Des Moines, Joseph met and married Sabina Neumann, some 20 years younger, who had immigrated from Stettin, Germany.

Soon after Libbie's birth, Joseph Hyman decided to move the family further west, first to Sioux Falls, South Dakota, then south to Fort Dodge, Iowa. On his own, he did not do as well in business. Libbie described him as constantly worried about family finances and remote from his children. He was a scholarly man, however, and did love to read,

especially travel and history books. The family home contained a library of books, including classics like Shakespeare and Dickens, over which young Libbie pored.

From childhood on she had a strong interest in natural history, especially botany. She said in her autobiography:

> I was conscious from a young age of a strong interest in nature. This first took the form of a love of flowers. My earliest recollections concern flowers. As a child I roamed the woods that bordered the town, hunting the spring wild flowers. I learned their names from a Gray botany book that my brothers had acquired in high school, but I puzzled over the classification until one memorable day when I suddenly realized that the flowers of a little weed known as cheeses had the same construction as hollyhock flowers. Thus I came to understand the families of flowering plants. (Hyman and Hutchinson, 1991)

But her scientific bent was not encouraged by her parents, who saw her role primarily as assistant to her mother in running the household and taking care of her brothers. Her mother was strict and extremely unaffectionate, according to Libbie. She was also very traditional, catering to her sons, and training her daughter so intensively in household duties that Libbie swore it was the reason that she "violently hated housework all my life" (Hyman and Hutchinson, 1991).

Fig. 5. Teenage LIBBIE HYMAN, circa 1905, the year she graduated from high school. In the fall of 1906, a former high school teacher, Mary Crawford, told her of tuition scholarships available at the University of Chicago. With the help of her teacher, Libbie was able to obtain one of these scholarships and enter the university.

CHICAGO YEARS: 1906–1930

After graduating as valedictorian from Fort Dodge High School in 1905, Libbie returned to the high school the next year to take advanced courses in German. Although she had passed the teacher certification exams at 17, she was still too young to get a teaching job. Lacking any other opportunity, she took a job in a factory, pasting labels on oatmeal boxes. One fall morning in 1906, she happened to meet her former high school German teacher, Mary Crawford, who was shocked to find her working in a factory, and who told her of tuition scholarships available at the University of Chicago for top students of midwestern high schools. Thanks to her teacher's efforts, a few weeks later Libbie was able to move to Chicago and enter the university, where she found part-time jobs to pay her room and board.

She blossomed during this period, taking a variety of science courses and enjoying freedom from her restrictive family. Pursuit of her first love, botany, was discouraged by anti-Semitic harassment by a lab assistant, but Zoology Department staff encouraged her to make a career of zoology.

After finishing her undergraduate studies in 1910, she decided to continue for a graduate degree in zoology under Professor Charles Manning Child, and received her Ph.D. in 1915, presenting a dissertation entitled, "An analysis of the process of regeneration in certain microdrilous oligochaetes." During those graduate years she supported herself with teaching assistantships.

A snapshot taken sometime during those years shows her laughing, arms flung back and hands clasped behind her head, full of joy, beauty, and the energy to take on the challenges of the world. But two events during these years had a profound effect on the rest of her life. The first was the death of her

father in the winter of 1907. Shortly afterward her mother and brothers moved to Chicago. She was once more expected to live at home and be a household servant, receiving nothing but derision for her studies. The second event was an unsuccessful sinus operation in 1916, which impaired her health and over time had an increasingly negative effect on her looks.

She claimed never to have wanted an academic teaching position (at that time very difficult for a woman to find, in any case). Instead, she continued as Child's assistant at the University of Chicago, carrying out experiments on the physiology of planarians and other invertebrates. She also wrote two laboratory manuals for which, as a teaching assistant, she had seen a need: *A Laboratory Manual for Elementary Zoology* (1919) and *A Laboratory Manual for Comparative Vertebrate Anatomy* (1922). She was very much surprised when they sold out as fast as they were printed.

The royalties on the sales of the manuals eventually brought her financial independence. But despite their success, invertebrate zoology remained Libbie's passion. In the mid-1920s, she began to think seriously about what she could do to promote knowledge of that subject. At first she thought merely of writing another laboratory manual; then colleagues convinced her she should produce an advanced text. She later claimed that at the time she began the project she had no idea it would grow to six volumes.

NEW YORK YEARS: 1932–1969

The opportunity to start the project came with her increasing financial independence thanks to the royalties from the lab manuals. In 1931, after her mother's death and with Child's retirement approaching, she decided to leave Chicago and the domestic tyranny of her bachelor brothers and find a place were she could pursue her invertebrate work in peace.

Quitting her job, she toured western Europe for 15 months, then settled in New York City where she could have access to libraries like that of the American Museum of Natural History, one of the best of its kind in the world.

Fig. 6. LIBBIE HYMAN at work at her typewriter, probably at the AMNH. The success of her laboratory manuals brought her financial independence. In 1930, she decided to leave Chicago and find a place were she could pursue her invertebrate work. After a 15-month tour of Europe she settled in the New York area where she contacted G. K. Noble, founder of the Department of Experimental Biology at the American Museum, who offered her use of the AMNH facilities.

In August 1932, she wrote to Gladwyn Kingsley Noble, founder and head of the Museum's new Department of Experimental Biology. That exchange of letters, preserved in the Department of Herpetology archives, records the beginning of a long and productive association. Libbie wrote:

> I've tried to find you two or three times with no success. What I want to know is the possibility of using the library freely and of getting some place in the museum where I could borrow a typewriter and write. I'm writing a book on invertebrate zoology. It is rather a pretentious effort and will take a long time, I suppose. I need a good library and a place to write and the best possibilities in New York seem to be either here in the museum or at Columbia. I've taken a brief look at the library here and it seems suitable for my purpose. I've not yet been to Columbia.
>
> I have resigned at Chicago to be free for this writing and I expect to be in New York until I've finished the job. I'd be obliged if you'd let me know whether there is any possibility of my being a guest, so to speak, in the museum for this purpose.
>
> Libbie Hyman
> 1352 46th Street, Brooklyn

Noble replied cordially:

> Dear Dr. Hyman
> I am glad to learn that you are in New York and planning to spend some time here. Our director is absent from the museum and the question of space cannot be definitely decided at this moment. However, I feel sure that the Museum would be very glad to give you the necessary facilities to carry on your work. I shall try to see what arrangement the director can make before the first of the month. In the meantime, I hope you will use our library and drop in to see us if you have the time.

The Museum to which Libbie came as a guest in 1932 was just emerging from its great age of exploration and was full of larger than life characters. Dr. Frank Chapman had invented the habitat type of exhibition, and according to a biographer, "did more to popularize birds than anyone since Audubon." Theodore Roosevelt, whose father had been one of the Museum's founders, donated many specimens. Carl Akeley made three African expeditions to collect animals (sometimes by hand-to-hand combat) and plants for exhibition (he died on the last expedition). Roy Chapman Andrews had explored the Gobi Desert and brought back dinosaur bones, while the Anthropology Department included the young Margaret Mead. The vertebrate paleontologist Henry Fairfield Osborn was in his last year as the Museum's president. Born to wealth and privilege, Osborn ruled the Museum like an emperor (Hellman, 1968; Preston, 1986). A former controller remembered that,

> Professor Osborn never picked up the telephone to make an interoffice communication. He generally dictated letters, on presidential stationery, to members of the staff, and he sometimes sent them telegrams, which slowed things up. . . . He wouldn't carry anything himself, not even an envelope. He had two offices—one presidential, one paleontological. If he wanted a memorandum taken from his fifth-floor office to his second floor office, one of his secretaries would call a messenger; the two men—Osborne and the messenger—might go down in the same elevator (Hellman 1968: 201).

It is not clear why Libbie chose to approach Noble's Department of Experimental Biology rather than the Department of Invertebrates, which was then under the direction of Roy Waldo Miner. It *is* known that Miner, who did marvelous exhibition work, did little research himself and disliked anyone he suspected might be smarter or more productive scientifically. Horace Stunkard, long time research associate in the Department of Invertebrates, once told me that Miner would never have allowed either a woman or a Jewish person to work in his department. From Libbie's letters to her friend Martin Burkenroad it is clear that Noble was not easy to work with, but as he had lectured at the University of Chicago in 1931, Libbie's last year there, she might have felt Noble would be more approachable than Miner.

The son of the famous publisher Gilbert Clifford Noble, Gladwyn Kingsley fit right in with the wealthy crowd at the AMNH. In 1924, after finishing his studies at Harvard and Columbia, he had been appointed assistant curator in the newly founded Department of Herpetology. Noble had a life-long interest in systematics and evolutionary biology, which had been encouraged by his Columbia adviser, William K. Gregory, chairman of the Department of Comparative Anatomy at the Museum. But he was also trained in the new and exciting area of experimental biology, and he was seeking a way to integrate the two areas. In 1928 he managed to talk Osborn into establishing a fund for experimental research (as well as into giving him a raise). The department was renamed the Department of Herpetology and Experimental Biology. Shortly afterwards the two departments were completely separated, although Noble remained in charge of both (Mitman and Burkhardt, 1991; AMNH Annual Reports for those years).

Once her welcome was established, Libbie moved into Manhattan to be close to the Museum. Her address was 41 West 70th Street, from which it was just a short walk up Columbus Avenue to the 77th Street entrance to the museum. Later (perhaps while saving for a house), she moved again, to a less expensive apartment at 85 West 166th Street in the Bronx.

The New York area also appealed to her for personal reasons. Several of her friends had settled in the city, and as she once said in a letter, "I don't have many friends but those few are dear to me, especially since I have nothing else to take their place" (L.H. letter to M. Burkenroad; Schram, 1993: 136). Other friends lived near the city. In New Haven, Connecticut she had two close friends. The developmental biologist Dorothea Rud-

nick, who had been a graduate student in the Zoology Department at Chicago during Libbie's final years there, was at Yale on a fellowship, and then, after brief stints at Rochester and Wellesley, settled back in New Haven at Albertus Magnus College for the rest of her career. The crustacean biologist, Martin Burkenroad, was then working at the Peabody Museum at Yale. The correspondence between Libbie and Martin, preserved in the archives of the San Diego Society of Natural History, shows how these three friends shared visits and encouraged each other to pursue their scientific and personal goals. Part of this correspondence was published by Frederick Schram (1993).

In addition, her cousin Jack Greenberg, the only member of her family to whom she felt close, was also nearby during much of this time. Jack did his medical internship in New Haven in 1934. He then trained as an ophthalmologist, and later practiced medicine with his brother in New Jersey. Libbie wrote of Jack:

> . . . we cling to each other as two persons wrecked on a rock, surrounded by an ocean of philistinic non-understanding relatives. We are so much of a kind and understand each other so well, and all the others, his family and mine, fail so completely in sensitivity and understanding. (L.H. to M.B., Aug. 2 1938; Schram, 1993)

Libbie was not entirely happy in the Department of Experimental Biology. Noble was an impressive empire builder. During the depression years, while the rest of the Museum was suffering attrition of programs and staff, he built new laboratories, rapidly increased the size of his department, partly by using WPA workers (65 of them worked for him by 1937), and generated outside funding from a number of sources (Mitman and Burkhardt, 1991). However, Libbie complained to Martin Burkenroad about Noble and his pressure on her to carry out experimental work:

> The fact is I like the American Museum very much but I decidedly do not like being in Dr. Noble's department. I doubt that I can put up with it much longer. I do hate a liar and a hypocrite. On the other hand, I can see his point of view, that he is giving me space in an experimental laboratory and I do not produce any experimental work in return. What I really want is to change to Dr. Miner's department but I have never had the courage to tackle Dr. Miner. For one thing it is almost impossible ever to find him free. Then Dr. Noble told me that Miner didn't want me, fearing that I would outshine him. Recently, however, Dr. Noble changed his story, saying Dr. Miner was offended because I had not come to his department instead of to Noble's. Now I do not know what to believe—probably neither story is the truth.
>
> I have never really tried to get what I want at the American Museum. Whatever place and position I have there was given to me by Dr. Noble on his own initiative. He invited me there in the first place with the idea that I would produce research in experimental biology and he had me appointed to a research associateship without me saying anything. I myself am hopelessly and impossibly diffident about approaching anybody for anything and asking for anything directly is beyond me. Not long ago, Noble had quite a talk with me on the question [of] whether I wished to be in his department or Dr. Miner's. I said that I preferred morphological work and that I would

Fig. 7. JACK GREENBERG, Libbie Hyman's cousin and friend. One of the New York area's attractions was the proximity of a number of her friends. Jack interned in New Haven, and practiced ophthalmology in the New York area during part of the 1930s.

like to transfer to Dr. Miner's department and that I hoped he would break the ice for me with Dr. Miner. But nothing further has been heard. (L.H. to M.B. undated letter, Schram, 1993:136)

But, if Libbie wasn't completely satisfied with her situation at the AMNH, at least she was able to get on with her treatise. Being a research associate gave her all the privileges of a curator except salary. She had an office, located off Noble's tank-room laboratory, and she had full library and publication privileges.

During this time she did enjoy fieldwork—both for her book and for the taxonomic papers on flatworms she continued to produce. Some of her fieldwork was local. In a letter to Burkenroad she mentioned visiting Pelham Bay Park one April day with a class from C.C.N.Y., In addition to marine life they

ran into a nudist colony who did not seem at all alarmed at or embarrassed by our intrusion. Most of them had on an extremely brief garment around the essential region but one was entirely naked. There seem to be what are usually called tramps around that region too. The park is so big and so much of it apparently left in a natural state that I suppose men could live there in summer eating clams and fish. But Saturday was rather a cold day to be going around with nothing on but one's skin. (L.H. to M.B., [April] 15 1934; Schram, 1993: 121)

Howard E. Evans, Professor Emeritus at Cornell, recalled to Anne Fausto-Sterling how, as a high school student collecting tadpoles for the New York Nature League, then housed at the Museum, he once ran into Libbie in a wooded area near the Saw Mill River Parkway, a good distance north of the city:

While I was standing in water about two feet deep collecting bullfrog tadpoles in the eddy of a small creek close to a bridge, I saw a disheveled looking figure approaching. When closer I saw a woman in sneakers wearing a tennis hat and carrying a tin box on a strap over her shoulder. . . . I had never seen a vasculum before so did not recognize what this box implied and thus was very suspicious of what she might be up to. . . . I decided not to notice her as she crossed the small bridge close to me, but when I heard the footsteps stop I turned to see her. . . . looking down at me. . . . "Have you seen any flatworms?" she asked me. . . . I told her there were no flatworms in the water and if she wanted to find them she would have to look under rocks on the wet hillside above. . . . at this point she came [down] off the bridge to [the] creekside, tucked her dress into her bloomers all around and waded into the water to show me where they were. (Unpublished transcript of Evans-Fausto-Sterling interview)

But most of Libbie's fieldwork took place at the various marine laboratories to which she traveled during the summers, seeking specimens to illustrate her book, and useful information from other invertebrate zoologists. In July 1935, for instance, she was working at the Bermuda Biological Station, from which she wrote Burkenroad:

It has been very pleasant here but the fauna is certainly disappointing. There is a good variety of sponges but very little else among the minor phyla. I have got a good many drawings that will be useful to me but for the last two weeks I haven't had enough material to keep me busy.

Wheeler [J.F.G. Wheeler, the director of the lab] is very nice but his wife in plain american is a "pain in the neck". She's an insufferable snob. . . . There is hardly anyone here. The scientific visitors consist of Buchsbaum and his wife from Chicago, Fries from C.C.N.Y. and Barnes from Yale. . . . The Buchsbaums are very pleasant company and I tag along with them on various excursions. Barnes is a chronic alcoholic as I suppose is well known at Yale, and is always full of rum. He usually doesn't show any obvious effects but one evening he got thoroughly tanked up and he went around to the room of each of the workers and put a big swastika on the wall. I suppose he thought it was funny, but as three of the four of us are Jews we did not appreciate the humor. However, he put one on his own wall. (Letter of L.H. to M.B. August 8, 1935, part published in Schram, 1993: 126)

In an interview with Anne Fausto-Sterling, Ralph and Mildred Buchsbaum confirmed this incident and added that the director's wife found Barnes very attractive and encouraged him in his unfunny practical jokes, which also included things like throwing the residence dinner bell into the sea. They recalled their field excursions with Libbie, mostly by rowboat, and some diving excursions, using a homemade diving helmet supplied with air by a manual pump on the boat. This setup actually enabled them to stay submerged at a depth up to 30 ft for about half an hour. The Buchsbaums had first met Libbie when they were beginning graduate students at Chicago in 1930, and they became good friends, visiting her in New York from time to time as well as meeting at marine laboratories during the summer (transcript of unpublished interview with Anne Fausto-Sterling). Libbie was a good swimmer, who had once saved a man from drowning, but she was prone to seasickness and waited her turn to use the diving apparatus in the water rather than in the boat, using a glass bottom

Fig. 8. LIBBIE HYMAN in Bermuda, 1935. During the summers Libbie visited marine laboratories to make drawings for her treatise and gather information from invertebrate workers she met there. In the summer of 1935 she worked at the Bermuda Biological Station.

bucket to scan the bottom. She wrote excitedly to Martin about the diving, but found that it caused her severe ear problems.

The summer of 1936 found Libbie at Hopkins Marine Station in Pacific Grove, California. The Buchsbaums were there, photographing marine invertebrates for their forthcoming *Animals Without Backbones*, and they shared a laboratory with Libbie. She introduced them to another friend from her Chicago days, Ed Ricketts, who had done undergraduate work in zoology there between 1919 and 1922. Ricketts' former partner Albert Galigher was another Chicago friend, who at the time had a histological slide business in Berkeley. At the end of the summer, Libbie took a bus up to Berkeley to visit him for a few days. Then she drove back with the Buchsbaums as far as Chicago (Buchsbaum interview and letter L.H. to M.B. July 25 [1937] in San Diego Society of Natural History archives).

The summer of 1937 found her in Maine at the Mount Desert Biological Laboratory in Salisbury Cove. She wrote to Martin:

> This place is really lovely, one of the most beautiful regions I've ever seen. . . . the laboratory consists of two or three wooden buildings but is adequate for ordinary types of work. . . . there are about 25 people here . . . a very nice and friendly group. . . . We eat at a common mess where the food has been satisfactory as far as I am concerned but fussy people would probably not like it. I have a room with one of the villagers. The total cost is $13 a week plus $35 that I'm paying for a room in the laboratory. (July 25 [1937] L.H. to M.B., Letter in San Diego Society of Natural History archives)

And in 1938, she returned to Friday Harbor, which she had first visited 15 or 20 years

Fig. 9. LIBBIE HYMAN (right) and ELIZABETH BUCHSBAUM (left), sister of her friend Ralph Buchsbaum, on the beach at Hopkins Marine Station, in Pacific Grove, California, 1936. Libbie spent the summer of 1936 at Hopkins Marine Station where she shared a lab with Ralph and Mildred Buchsbaum. Photograph by Ralph Buchsbaum.

before, and which she felt had the best invertebrate fauna. On August 2, she wrote Martin that

> Friday Harbor is a lovely place and the laboratory is very satisfactory. It consists of a group of buildings near the shore, three zoology buildings, and one each for botany, chemistry and bacteriology. . . . The fauna is rich in both species and individuals and the equipment is adequate. We live in tents in the woods above the laboratory buildings. . . . I think on the whole I like this place the best of any marine station on our coasts. Unfortunately the seashore climate has had the usual effect on me and I have been ill most of the time I have been here. For this reason I am leaving sooner than I expected and hope to recuperate in the dry climate of Wyoming before returning to N.Y. . . . (L.H. to M.B., San Diego Society of Natural History archives, part in Schram, 1993: 134–5)

Figure 13 shows her on horseback during her Wyoming vacation in August 1938.

Her health was a continual problem. The sinus operations she had undergone as a young woman had almost destroyed her sense of smell and had distorted the shape of her nose and face, causing her to resemble a used-up prize-fighter in front view, surely a severe blow to her self-image. Even worse, the clumsy surgery of the time offered no cure. Ear and sinus problems plagued her the rest of her life. In fact, for a long time, she considered leaving New York for a drier climate, but, after spending some time in Arizona one winter, she discovered that windborne dust found in that area made her sinus problems more severe, and she gave up the idea of moving west.

She also suffered from digestive problems, apparently colitis, and was limited much of the time to a bland diet, lacking in the delicacies, like strawberries and asparagus, that she loved. She wrote Martin in 1938, "Although I apparently give the impression of good health, I am

Fig. 10. LIBBIE HYMAN with protozoologist BILL BALAMUTH, Friday Harbor, 1938.

Fig. 11. L. H. in a laboratory at Friday Harbor Laboratories, San Juan Island, Washington, summer, 1938. The diversity of its fauna made Friday Harbor Libbie's favorite marine lab, but its climate definitely aggravated her sinus problems.

Fig. 12. L. H. in the field with students and researchers, Friday Harbor, 1938.

Fig. 13. L. H. on horseback, August, 1938. On the way back to New York from Friday Harbor Libbie visited Wyoming, where this photograph was taken. She sometimes talked of moving west to a dryer climate that might help her sinus problems, but never did so.

never free from pain . . ." (L.H. to M.B., April 1 1938, Schram,1993: 132)

In 1940 her situation at the Museum took an unexpected turn when Noble died suddenly of a streptococcal infection known as Ludwig's Quinsy; he was only 46. (Mitman and Burkhardt, 1991). Shortly before his death, Libbie was still attempting to transfer to Miner's Department of Invertebrates:

> I at last made a genuine effort to transfer to Miner's department but met a cold shoulder. Dr. Miner said he did not have one inch of space to spare, which of course I do not believe. It also came out that Dr. Noble had spoken to him about my transferring to his department a year ago and received the same reply. Since that time, however, Miner took somebody into his department and gave him the best room. . . . I had my eye on that room. The man's name is Armstrong and . . . he works on Crustacea. I talked to him and learned that he considers himself permanently established there. I haven't made up my mind yet but don't believe I care to stay at the museum much longer under present circumstances. I'm getting pretty tired of the lack of space and of privacy. (L.H. to M.B., Feb. 22, 1940; Schram, 1993: 138–139)

However, Libbie had been working away on her treatise, which she now realized would fill more than one volume (though she still thought it could be completed in three).

Fig. 14. LIBBIE HYMAN'S house in Millwood, Westchester County, New York. Volume I of the *Treatise* appeared in 1940, and Libbie was finally able to buy the house in the country she had always dreamed of owning. Though she later claimed to regret the time spent on her garden and on commuting, it is clear that her time in Millwood was one of the happiest and most productive periods of her life. During her Millwood years she finished Volumes II and III of the *Treatise* (published in 1951).

She signed a contract with McGraw-Hill, and the first volume appeared in print in 1940.

The book was an immediate success, and reaped an almost immediate reward in the form of an honorary Sc.D. degree in 1941 from The University of Chicago. She also was finally able to buy herself a house. Libbie had long wanted a place in the country where she could have a garden. In 1934 she wrote, "I'm afraid I'd rather have a garden than the most exalted scientific reputation." (L.H. to M.B., undated [1934].) But although her lab manuals had given her financial independence, she, like millions of Americans, had lost much of her supposedly safely invested savings in the stock market crash and Great Depression. However, Libbie kept on working and saving toward her goal. She took on as much extra evening work as she could, like abstracting German journals for *Biological Abstracts*, which paid very poorly but eventually helped her achieve her goal (L.H. to M.B. [1934]; Schram, 1993: 123).

By 1939 she was actively house hunting. She thought about purchasing a lot in a development in Yonkers near the Boyce Thompson Plant Institute (the place where the young Howard Evans had encountered her), but eventually she decided to move further from the city, buying a house in Millwood in Westchester County, a two hour commute by train from the Museum.

Until she moved to Millwood she had worked at the Museum from 9:30 a.m. to 5:30 p.m., six days a week. She also worked two or three weekday evenings, and spent

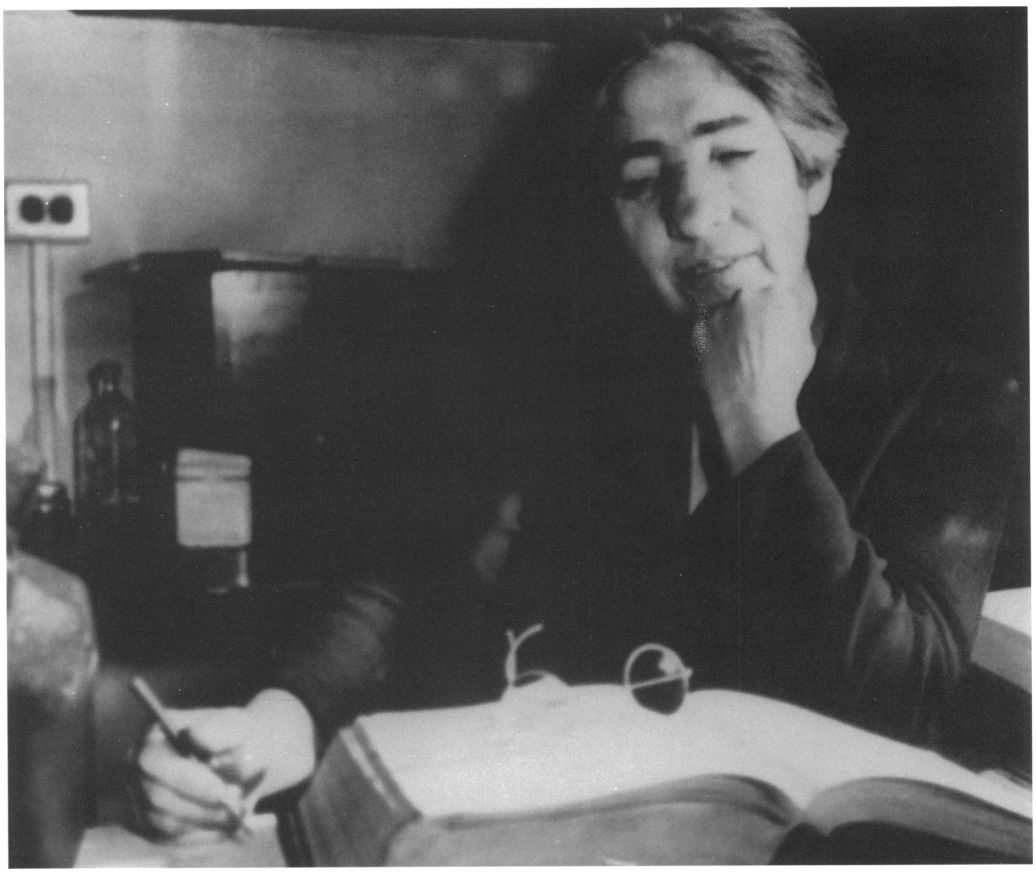

Fig. 15. LIBBIE HYMAN at work in her office at the American Museum of Natural History. After the retirement of Department of Invertebrates Chairman, Roy Waldo Miner in 1943, she transferred to that department, an association she retained (through the department's various administrative transformations) until her death.

one evening a week on correspondence. After moving to Millwood she changed her work schedule, so she could spend time gardening. She stayed in Millwood on Sundays, Mondays, Tuesdays and Thursdays and came in to the Museum on Wednesdays, Fridays, and Saturdays.

Her garden became an important part of her life and she spent time and money freely on it. In fact, her suburban neighbors thought she must be some fabulously wealthy eccentric, because of her plain dress, reclusive habits, and the money she spent on landscaping. But her house and garden brought her great pleasure, never more so than, when she was going at night to remove slugs from her plants and discovered night-feeding land planarians. And her neighbors became more friendly when they realized she could provide them with remedies against their garden pests [JEW, interview with Max and Bessie Hecht].

In 1942, Albert Parr, who had been director of the Peabody Museum at Yale, was appointed director of the American Museum. This was good news for Libbie, as Parr was one of her supporters; during the uncertainties of the Noble years, he had even invited her to work at the Peabody.

In 1943, within months of Miner's retirement, Libbie transferred to the Department of Invertebrates, an affiliation she retained throughout the department's various administrative transformations, until her death in 1969.

After finishing Volume 1 of *The Invertebrates* Libbie had to take time off from her treatise work to revise the vertebrate labora-

tory manual, an unwelcome but necessary task. She wrote to Martin:

> I'm sorry I can't go ahead with the next part of the invertebrate treatise, but that vertebrate manual is my bread and butter, not to say cake also, and it has been in need of a new edition for a very long time. I can't put off the unpleasant job any longer and so am deep in gill slits, etc. already. (L.H. to M.B., April 9 [1941]; Schram, 1993: 139)

Once that job was taken care of, though, she returned to invertebrates for good, refusing to consider a third revision.

Libbie later claimed to regret the time she had spent on her garden and on commuting, but it is clear that her time in Millwood was one of the happiest and most productive periods of her life. During those years she completed Treatise volumes 2 and 3 (published in 1951).

Those who knew her in New York all agree that much of her working time was spent in the library. In fact, Bessie Hecht, who first met Libbie while working at the AMNH in the forties, described her to me as "crepuscular"—a creature of the shadowy library stacks. The head librarians, Hazel Gay and Helen Gunz, had become Libbie's good friends. Libbie attended the teas the library staff organized for those they favored and the librarians celebrated her birthday with a party and cake.

Many people have asked me how she went about her work. According to those who knew her she proceeded this way: in the library she read the relevant literature and made notes on index cards. Once she had organized and digested the facts for a section she sat down at her old Oliver typewriter and typed the first and only draft directly on it, making no intermediate handwritten draft, and very few corrections or changes in the typescript. Her clear, descriptive style was no accident. She once told the Buchsbaums that she had worked for 10 years learning to write in the most lucid scientific style she knew—that of T. H. Huxley.

Most of the illustrations were her own work. At the beginning she despaired of ever being a good enough artist, writing to Martin that "Just now I am taking a drawing lesson once a week in addition to the other occupations but I fear I'm not capable of much improvement. It would be such a tremendous help for me if I could draw decently but I just can't and that's all." (L.H. to M.B. , undated letter, probably autumn of 1934)

But she kept on practicing her drawing and by the time Mildred Buchsbaum visited her lab in the 1950s her skill had greatly improved. Mildred noted that Libbie "worked with great speed, finishing a number of drawings in the day. And as she finished each she threw it into a large paper carton almost as if she were discarding papers into a wastebasket." (Buchsbaum interview with Anne Fausto-Sterling).

Research associates have been a long tradition at the AMNH and have contributed greatly to its scientific reputation. Some have also figured in departmental and institutional politics. But all who knew Libbie agree that she stayed completely away from that aspect of Museum life. She socialized mostly with the librarians and apparently had little to do with the curatorial staff. There are many stories of her being brusque, even rude, to people. She certainly had no patience with those she thought were not serious about science. However, there are just as many anecdotes that show her encouragement of students, even young students. She strongly encouraged graduate students and sometimes even helped them out financially.

In the summers, she kept up her visits to marine labs, gathering material for her books. Her single-minded New York manner contrasts with the stories told by those who knew her in the field. Former students in summer courses describe her as enthusiastic and helpful, someone who could seem to be everywhere in the lab at once, pleased and excited by the living invertebrates under study. Pictures taken by the shore show her relaxed and smiling, in stark contrast to the severe expression of her AMNH studio portraits.

In 1952, at the age of 65, she sold her house in Millwood and returned to New York City to an apartment hotel near the museum. She enjoyed the city's activities, attending concerts with her librarian friends, and acquiring a small, but valuable art collection. But mostly she worked on her invertebrate treatise, obsessed with the immensity of the task she had set for herself. Treatise volume 4 appeared in 1955; volume 5 in 1959.

As the volumes piled up so did the scientific honors, including membership in the

Fig. 16. LIBBIE HYMAN in the Brazilian jungle, 1956. This photo was taken on a visit she made to Ernst and Eveline Marcus, while she was working on Volume V. It was at this time that she first developed symptoms of an illness eventually diagnosed as Parkinson's disease.

National Academy of Sciences, the Elliot Medal (1951), and the Gold Medal of the Linnaean Society of London (1961).

Around 1956 Libbie developed symptoms of what was eventually diagnosed as Parkinson's disease. By the time of her death in 1969 she was wheelchair-bound and dependent on nursing care, but she kept working almost to the last. Volume 6 was published in 1967. Its preface began, "I regret to announce that this will be the last volume of *The Invertebrates* from my hands. What with advanced age (78) and concomitant loss of strength and energy, it has become a physical impossibility for me to continue this difficult task any longer," and ended, "I now retire from the field, satisfied that I have accomplished my original purpose—to stimulate the study of invertebrates."

Libbie Hyman did not see herself as a research scientist (although she published more research papers than many scientists ever do). She claimed her major work to be "essentially a compilation from the literature," and considered her assets to be "some fluency in translating the major European languages and an ability to select and organize material in the literature." (Hyman and Hutchinson, 1991: 108). Although she was President of the Society of Systematic Zoology (1959) and edited its journal *Systematic Zoology* from 1959 through 1963, she apparently had little interest in systematic theory, preferring practical approaches.

Yet invertebrate zoologists have found that she did much more than compile. Her books provided a synthesis of phylogeny that clearly influenced teaching and opinion about the groups she covered. While the discoveries of the last 30 years, in ultrastructure, paleontology, ecology, and molecular biology, have resulted in many changes in our thinking about invertebrate phylogeny, her work still provides a framework against which new ideas can be tested, and sets a standard for excellence that can still inspire us.

REFERENCES

Fink, R. D.
1987. Libbie Henrietta Hyman. Dictionary of scientific biography, suppl. 2. New York: Charles Scribner's Sons.

Hellman, G.
1968. Bankers, bones and beetles. The first century of the American Museum of Natural History. Garden City, N.Y.: The Natural History Press.

Hyman, L. H.
1919. A laboratory manual for elementary zoology. Chicago: University of Chicago Press.
1922. A laboratory manual for comparative vertebrate anatomy. Chicago: University of Chicago Press.
1940. The invertebrates. Vol. 1. Protozoa through Ctenophora. New York: McGraw-Hill.
1951a. The invertebrates. Vol. 2. Plathelminthes and Rhynchocoela. New York: McGraw-Hill.
1951b. The invertebrates. Vol. 3. Acanthocephala, Aschelminthes, and Entoprocta. New York: McGraw-Hill.
1955. The invertebrates. Vol. 4. The Echinodermata. New York: McGraw-Hill.

1959. The invertebrates. Vol. 5. Smaller coelomate groups. New York: McGraw-Hill.
1967. The invertebrates. Vol. 6. Mollusca I. New York: McGraw-Hill.
Hyman, L. H. and G. E. Hutchinson
1991. Libbie Henrietta Hyman, December 6, 1888–August 3, 1969. Biogr. Mem. Nat. Acad. 60: 103–114.
Mitman, G. and R. W. Burkhardt, Jr.
1991. Struggling for identity: the study of animal behavior in America, 1930–1945. *In* K. R. Benson, J. Maienschein, and R. Rainger (eds.), The expansion of American biology: 164–194. New Brunswick, N. J.: Rutgers Univ. Press.
Preston, D. J.
1986. Dinosaurs in the attic. An excursion into the American Museum of Natural History. New York: Ballantine Books.
Schram, F. R.
1993. A correspondence between Martin Burkenroad and Libbie Hyman: Or, whatever did happen to Libbie Hyman's lingerie. *In* F. Truesdale (ed.), History of carcinology: 119–142. Rotterdam: A. A. Balkema.
Stunkard, H. W.
1974. In memoriam, Libbie Henrietta Hyman, 1888–1969. *In* N. W. Riser and M. P. Morse (eds.), Biology of the Turbellaria: iv–xiii. New York: McGraw-Hill.
Yost, E.
1943. American women of science. New York: Frederick A. Stokes Company.
Winston, J. E.
1991. Great invertebrate biologists: Libbie Henrietta Hyman (1888–1969). Am. Soc. Zool. Div. Invertebr. Zool. Newsl. Fall 1991: 3–4.

LIBBIE HYMAN AT THE UNIVERSITY OF CHICAGO

JANE MAIENSCHEIN

Libbie Hyman spent the first third of her career at the University of Chicago, so it is reasonable to ask: "What difference did that choice make in her career and in her research contributions?" That, in turn, leads to the question: "What did she learn at Chicago, and what did she contribute there?" And, even, "why Chicago?" Why did Libbie Hyman go to Chicago to study in 1906?

Unfortunately, I do not know for sure, never having located any private diaries or letters that might reveal much about her decision. After graduating as valedictorian of her high school class, Hyman was gluing labels on cereal boxes in Iowa when "A chance meeting with her high school German teacher (shocked to find her prize student so occupied) led to a scholarship to the University of Chicago" (Fink, 1990: 442).[1] There is no direct evidence about whether she tried other places or had other preferences, but there are clues. It seems that, as the only daughter of Jewish immigrants, her home life was difficult and full of hard work and few rewards. For a girl interested in natural history and good at school work, this must have been frustrating.

From the historical point of view, choosing the University of Chicago made excellent sense. Even though things had improved in education for women by 1906, there were still few places where women could both attend undergraduate school and do graduate work in the biological sciences. Few graduate programs with strength in biology admitted women. This university was the closest and by 1906 the most prestigious research school in the Midwest. In addition, it had a strong scholarship program, which was still rare at most universities and which allowed places like the University of Chicago to attract and keep the best students. The city was attractive as well. A number of American biologists mention in their autobiographies the special attractions of Chicago, with its proximity to Lake Michigan and forests.

Furthermore, there was the "Jewish-woman" factor: even schools that admitted wom-

[1] Besides Rachel Fink's biographical sketch, also see Winston's sketch prepared for the American Society of Zoologists' special session on Hyman in 1991. The standard biographical source is Horace W. Stunkard's preface to N. W. Riser and M. P. Morse (eds.), 1974. Biology of the Turbellaria. New York, vol. 7 of the Invertebrate Zoology series. The reprint collection at the Marine Biological Laboratory and archival materials at the University of Chicago, as well as the standard biographical sources contain lists of Hyman's and Child's publications.

Fig. 17. LIBBIE HYMAN as a young woman. At Chicago Libbie took a variety of science courses and enjoyed temporary freedom from her restrictive family.

Fig. 18. CHARLES MANNING CHILD (1869–1954), a physiologist in the Department of Zoology at the University of Chicago. After finishing her undergraduate studies in 1910, Libbie went on for a graduate degree in zoology under Professor Child, receiving her Ph.D. in 1915.

en did not always afford them equal opportunities or admit them on equal grounds with men; also, many schools did not accept Jews and finding a congenial place for a Jewish woman from a less-than-wealthy background and with serious ambitions would have been particularly challenging. Hyman may well have gone to Chicago less because she knew of its research and scientific strengths than because it was reasonably near and satisfied all the relevant criteria. The scholarship did offer her the chance to escape from home and the factory and begin her studies.

Thus, Hyman went to Chicago on a scholarship, initially intending to study botany since she had long enjoyed wildflower collecting. Right away, however, she switched to zoology, at least partly because of the reported anti-Semitism of a laboratory assistant in the Botany Department. Right away also, her family reappeared; her mother and brothers moved to Chicago after her father died and evidently expected Hyman to help take care of them. She persisted nonetheless, and received her B.S. degree in zoology in 1910, and continued her Ph.D. studies under the supervision of Charles Manning Child. She received that degree in 1915 and remained at Chicago for another 16 years as Child's "assistant"—until Child's impending retirement and her mother's death in 1930 allowed her finally to gain her independence.

To understand Hyman as a scientist in her later career at the American Museum of Natural History, it is worth exploring what she did at Chicago and her activities there as an

Fig. 19. LIBBIE HYMAN in her University of Chicago Office on the fourth floor of the Zoology Building. During her Chicago years Libbie underwent a series of sinus operations which distorted her features, but did little to help the sinus problems which plagued her the rest of her life.

"assistant." During those 16 years, she published over 40 articles of her own, and coauthored a number with Child and others—an average of almost three articles per year. In addition, she produced laboratory manuals for Elementary Zoology (in 1919) and for Comparative Vertebrate Anatomy (in 1922). During the same period, Child published just under four articles per year plus four books that summarized and systematized many of the articles. Child was busy and productive—and so, obviously, was his impressive assistant.

Child traveled to Asia, became department chair, and temporarily slowed his publication rate in 1931. When he also contemplated retirement, Hyman moved on to New York, to make her own way and to support herself on the royalties from her lab manuals and textbooks. Ernst Mayr (in a personal communication) reported that any extra money she had beyond what little she regarded as essential, she used to support graduate students—she did not require or ask for much.

That period in New York forms the basis for other articles in this collection. The question for now is "What did Hyman get from Chicago?" At least two things, I think: (1) participation in an established research program, centered on Child's study of metabolic gradients, and (2) a way of doing science that I have elsewhere called "the Chicago style" of biology.

A look at Libbie's publications shows something about the nature and significance of Child's research program on gradients and how much and in what way Hyman contributed. Her first paper, published just after she received her degree, begins with a reference to Child's work on regeneration: "Under Professor Child's direction, I have been carrying out experiments along similar lines on several species of microdrilous oligochaetes" (Hyman, 1916: 100). After a lengthy, detailed report on the various experiments on axial and other gradients in several species, in which she repeatedly reported using Child's methods or approach, she concluded with another acknowledgment (Hyman, 1916: 160–161): "This work was carried on at the University of Chicago under the direction of Prof. C. M. Child during the years 1911–1914. It is a pleasure to me to acknowledge my indebtedness to Professor Child, and to express my sincere thanks for his continual kind and helpful criticisms and suggestions, and inspiring comments during the progress of the work."

In her next paper (Hyman, 1917: 99), a year later and after her degree, she offered the same credit to Child and insisted that "I wish emphatically to disclaim any originality or priority for the explanation of amoeboid movement which I have presented, although I should perhaps state that it arose independently in my mind as a result of my observations of the axial gradient." Yet she did not hesitate to point out how others had erred in details of their interpretations. This attitude, apparently deferential and obviously respectful of Child while denying her own

originality, belies the significance of her solid, careful observations and experimental contributions.

In all her major papers at Chicago, she explored physiological and metabolic factors in various organisms. Respiration, chemical responses to feeding and starvation, and bioelectric activity provided favorite subjects, with the nature and role of oxygen consumption most frequent. These papers typically asked about the phenomena and considered them as bases for understanding both normal development and pathological deviations from the normal. They studied whole, living organisms, since the focus on gradients and physiological response would be impossible to analyze otherwise. They correlated study of physiological actions and reactions with morphological characteristics. And, whether her specimens came from Chicago or Ann Arbor or Woods Hole or Maine or the other places she visited, she joined Child in asking about the establishment, nature, and functioning of metabolic gradients. Her work beautifully complemented Child's.

As Fink said in her biographical sketch (1990: 442), Hyman did not consider her own contributions as particularly outstanding, certainly not as exceptionally original. Nor did she consider herself the "research type." Yet Child clearly felt her work important enough to warrant publication—and under her own name rather than his, even though she was technically his assistant. This says something about both of them, of course, since not all senior scientists would have encouraged their female "assistants" to publish under their own names.

The period after 1910 (when Hyman arrived to work with him) was formative for Child as well as for Hyman and she may have influenced or at least reinforced the direction of his research. To that point, he had studied morphogenesis, especially looking at regulation of development in embryos and in regeneration. He had concluded first that pressures and tensions direct these processes of formation. Further study of regeneration suggested that "function determines form"—if a part can function physiologically and fill the place of an ignored or missing part, it can redifferentiate or regenerate as that part. Thus regulation and regeneration involve a return to "physiological equilibrium." A series of studies of germ cells and reproduction did not help him to understand the process of regulation much, but, in 1910, just before Hyman began her graduate studies, Child put forth a new theory based on an idea of anteroposterior dominance. For Child, unity of the organism (and maintaining and regulating that unity) is a key problem for living beings. He suggested that such unity depends on "correlation," or "organization" and that the physiological correlation is most crucial. The chief factor is dominance along the anteroposterior axis, so that factors such as distance from the head and efficiency of conduction along the axis are important in guiding development and physiological functioning.

In 1911, as Hyman began her Ph.D. studies, Child extended his study of physiological correlation, looked at physiological isolation of parts along the axis, and began to explore various experimental approaches for studying the physiological control of morphogenesis. Late 1911 and 1912 brought the concept of gradients: there is an anteroposterior or axial gradient that characterizes the organism's fundamental functioning. His research on regeneration showed that the regenerative process, in effect, begins at the head and works back along the tail, along the axis.

In 1913, Child began an experimental study of the susceptibility of different parts of the organism to various lethal agents. He identified another gradient running along the antero-posterior axis: what he called the gradient of susceptibility, where the most susceptible areas were those of greatest physiological activity. In fact, those occurred near the head. Child developed and reinforced a crucial conclusion during this time, which was summarized in his first book of 1915, *Senescence and Rejuvenescence* and his second of 1916, *Individuality of Organisms*. This idea was that the organism is essentially a machine, constructed through its physiological functioning, building on its inheritance, and acting as a "reaction system."

Child continued to explore this view, through various methods, during the years Hyman remained at Chicago. The research program developed along with her career, and Hyman apparently accepted the basic approach as well as Child's interpretation. She

studied different organisms and physiological processes that complemented Child's own studies. In some cases their studies neatly overlapped. She does seem to have presented her results in ways that make the experiments and the results more important than their role in supporting a pet theory, while Child was more inclined toward emphasizing theory—his theory.

Clearly, most of the theory *was* his, and it built on his earlier ideas before Hyman arrived to work with him. Yet the approaches and techniques and details expanded significantly and moved in new directions while Hyman worked with Child, suggesting that a good share of the hard work as well as some of the experimental ideas were hers. Her own disclaimers about her importance notwithstanding, she does seem to have earned the high regard of her colleagues as well. For example, in his history of the Chicago department, H. H. Newman (1948: 232) wrote of Child's role there: "In much of his work he had the able assistance of Dr. Libbie Hyman, generally regarded as the ablest American woman zoologist now living (some say the ablest of either sex)."

Hyman showed, in her various studies of a range of organisms (from worms to planarians to vertebrates), that there is a fundamental axial gradient from head to tail, and that this is also a physiological gradient along which metabolic processes occur with the greatest activity toward the head and with greatest susceptibility toward the head as well. Some organisms have a secondary axis or more than two axes as well, each following the same general rules. Susceptibility may vary in different parts depending on the stage in the life cycle, for example. This meant that the researcher had to be careful if she or he wanted to study susceptibility (relatively easier) and work back to conclusions about metabolism (relatively harder to study directly). Hyman's modifications and careful additions of detail were clearly important in refining Child's theories and interpretations of the data.

This brings us to ask: "What was Hyman's distinctive contribution?" Was she "just" Child's assistant, carrying out work that he outlined and with no input of her own? Obviously not, or he surely would not have accorded her independent status and so much continual support. It is worth looking at her contributions to attempt an assessment.

Her series of five long papers on oxygen consumption revealed Hyman's approach. The first three, in 1919, considered physiological studies of planaria on feeding and starvation, regeneration, age, and size. The fourth, in 1920, addressed starvation; the fifth (in 1923) the length, level, and time after selection for the experiment. This set of five papers added up to over 100 pages and provided clear, detailed descriptions of the effects of oxygen consumption in planaria. A second series looked at the effect of other substances on oxygen consumption, and ran to six articles, also totaling over 100 pages. Later major series examined metabolism gradients in vertebrate embryos, and others focused on taxonomy. This style of presenting a series of coordinated articles, each of which looked at a different aspect of a larger issue, followed Child's approach.

Throughout, Hyman's work was characterized by careful description of precisely what she was studying. She referred to the existing literature and engaged the issues there by explaining how her evidence supported or questioned the existing ideas. Each piece clearly described additional information. Even experimental results were presented essentially as extended observations. While Hyman herself denied the originality, her work was exceptional in its clear focus on the phenomena of life before her. She never became distracted by a pet theory or interpretation of her own but remained attentive to what she saw as facts—facts of morphology, systematics, natural history, and especially physiology.

It would be too easy to attribute Hyman's role as "mere" assistant to her gender and to see her as a would-be research scientist oppressed by her circumstances. That would be to overinterpret. Undoubtedly, the results would have been different if she had had a wider range of alternatives available, but there were virtually no independent positions for female researchers at the time. She remained Child's assistant and seems to have been willing to accept Child's research program and only gradually moved her own work in different directions. Her lasting con-

tributions, that go beyond and in different directions than Child's, are revealed in her lab manuals and in her marvelous study of invertebrates—the latter written after she left Chicago.

Hyman's personal style appeared clearly in the introduction to her *Comparative Vertebrate Anatomy* manual. There she explained that when the University of Chicago changed its approach to teaching lab work in vertebrate zoology from an emphasis on study of types to a comparative approach, it did not immediately provide a new manual. She saw the need and met it. Interestingly—in light of the prevalent trend to encourage discovery by every individual student, which echoed a similar emphasis of the late 19th century represented by Franklin Mall at Johns Hopkins, for example—she noted (Hyman, 1922: viii) that "Our experience with laboratory manuals of the type in which the burden of discovery is left to the student is that the student becomes highly dissatisfied and that the instructors are brought into a state of irritation and fatigue by the continuous demands for assistance with which they are bombarded. Frankly, I believe in the conservation of instructors, and have written this manual with that end in view." Nonetheless, she made clear in other places that she felt that students needed personal hands-on experience with living material.

In introducing the second edition in 1942, still amazingly popular after 2 decades, she noted that in the first edition she had been inclined to follow the standard pattern and stories presented at the time in other textbooks. "The years between, however, have taught me to suspect all standardized accounts copied into a succession of college textbooks." Instead of a static picture where the answers are already largely in place, the current volume presents more of the questions and a sense of "a vast field full of controversial issues and unsolved problems, depending for their solution on future painstaking embryological and anatomical researches. An army of devoted workers is necessary for elucidating these many questions; but nowadays—alas!—all young biologists want to be experimentalists, and hardly anyone can be found interested in the fields of descriptive embryology and anatomy." (Hyman, 1959: ix) Yet many of the basic questions of zoology require careful descriptive work. Hyman seems to have recognized that and to have realized that researchers will not reach any very useful or important conclusions if they rush to do experiments just for the sake of experimenting. We need to keep life, and the living organism, and the relations among living organisms in view as well. She was, in other words, a classical zoologist with its inherent focus on animal life.

That classical work, like the rest of Hyman's publications, reflected an attitude characteristic of the daughter who evidently gave up much out of duty to her family. She had a strong drive, commitment to the purpose at hand, and attention to detail. She was probably also influenced by Child's dedication and singleness of purpose in the face of his own critics, who often saw his study of gradients as unproductive. Both helped to sustain, and both benefitted from, the climate at the University of Chicago—a Chicago style of biology.

This returns us to the second point of the paper, concerning the Chicago way of doing biology, or the Chicago style. We have Chicago styles of architecture, sociology, and pizza, so why not of biology? Embryologist Viktor Hamburger (personal communication) suggested the idea when he commented that it is fairly easy to tell a Chicago product in embryology, at least, because Chicago biology is just done differently.

We could argue about how far the characteristic extends and whether it really represented a "style," for example, or whether it is uniquely Chicago's. What constitutes a "style" in science? Various historians, sociologists, and philosophers of science have taken on such questions in recent years, as evidenced by sessions at national and international meetings, by paper and book titles, and even by a few enlightening and provocative studies such as Jonathan Harwood's look at styles of German genetics earlier this century (Harwood, 1993).

Discussion of whether there is specifically a Chicago style of biology began in a paper of mine published in 1988. A special meeting at the University of Chicago in connection with their centennial celebration, and a publication containing some of those papers con-

tinued that discussion. Time has brought various refinements, of course, but it still seems clear that there is a Chicago style of biology in a meaningful sense, and that both Child and Hyman exemplified it. This style was characterized by a shared set of concerns, specifically to wholism (the study of organization and function of whole organisms and populations rather than to the disembodied parts or molecules alone); physiology (and its relation to structure); and cooperative and comparative study. This does not mean that nobody outside Chicago adopted one or another of these, or even all three, nor does it mean that all biologists at Chicago adopted all three at every moment and for every project of their careers. Rather, there was an unusually high percentage of Chicago biologists who were strongly committed to all three characteristics. Whether those were chosen for Chicago by the first two department chairs because they fit, or whether the Chicago environment and way of working influenced their styles remains a question for further study. The point here is that Child and Hyman accepted all three of these Chicago tenets. The first two affected the content and approach of their work, while the third affected the way they worked—cooperatively and collaboratively as a team. Not many institutions at that time would even have provided for a research assistant who published independently, for example, but Hyman was allowed to remain as such for many years at Chicago.

This was the case because of the particular nature of the University, and also because the first chair of the biological programs, Charles Otis Whitman, and his successor, Frank Rattray Lillie, adopted their particular approaches to biology there. The University was intended initially as a small Baptist college, a "western Yale" (on the history of biology at the university, see Maienschein, 1988). By the time it opened in 1892, however, that mission had changed. The selection of William Rainey Harper as president insured that it would be a research university. Harper saw an opportunity to hire a great faculty by raiding the troubled Clark University in Worcester, Massachusetts. Harper convinced Whitman (Clark's biology chairman) and 11 of the 15 other Clark biology faculty to move to Chicago. In fact, Harper took many others as well, about one-third of the entire faculty at Clark. Since Clark had been set up as a graduate-level and research-oriented school, it had attracted an excellent group of scientists. Harper, therefore, had a great start, and he put Whitman in charge of defining how biology would be organized in this new Midwestern setting.

From the beginning at Chicago, Whitman stressed not only the importance of each person's individual research, but of working together cooperatively as well. He saw this in terms of specialization and organization, through cooperation. Whitman emphasized the importance of studying both physiology and morphology. He stressed that study of individual cells is not enough—there is something about their interaction and regulation that is crucial. Life exists in the whole organism, so the biologist must study the whole organism; and the whole community is needed to carry out the work.

Among others, Whitman hired Child as someone who would adopt these views and carry out a research program based on them. Child had received his Ph.D. from Leipzig in 1894, as Whitman had a decade earlier. Whitman hired him at Chicago in 1896 as an "associate" after which Child moved up the ranks to instructor and on to full professor by 1916 (just after Hyman graduated). Child became department chair in 1931, and then retired in 1934. Whitman had chosen a loyal Chicago researcher, who fulfilled his goals and his ideals.

Not everyone agreed with the choice. By 1920, Child was well known but not universally accepted, especially outside Chicago. Some of the leading researchers elsewhere—notably experimental zoologists Thomas Hunt Morgan and Ross Granville Harrison—thought Child's gradient work did not explain much and was too speculative. They felt he was not a "team player," as we might put it today, perhaps because he did not go to the Marine Biological Laboratory in Woods Hole each summer or even send his students there regularly. It is not clear how well Frank Lillie as second chair at Chicago (from 1910 until Child took over) really liked Child or whether he agreed with Child's most boldly articulated theories about

gradients. But Lillie apparently accepted him as a researcher doing "good science" and contributing to the reputation of the department through his numerous publications and his work with students. Lillie was generally quite supportive and even published with Child on occasion. The internal environment at Chicago was, therefore, quite supportive of the style of work that Child and his excellent assistant Libbie Hyman pursued.

We thus see that not only is the first part of Hyman's career intriguing in itself; it is made even more interesting because the work begun at Chicago benefitted from collaboration with Child and fit into a Chicago style of doing biology. Hyman began by working within Child's research program, developed her own contributions through laboratory manuals, and moved on to greater interest in issues within systematics and invertebrate forms and functions at the American Museum in 1931.

What did Hyman think of Child? Of Lillie? Or of Chicago as a place to work? Did she really believe her work lacked importance or originality? And how would we know? As yet, no documents have been discovered that offer clear answers, but here we see clues. It would be a mistake to see Hyman simply as a misused female, just as it would be a mistake to overlook the significance of her lab manuals and textbooks just because they were texts rather than "original" experimental research reports. Hyman was unique and, as Newman suggested, just perhaps the ablest American zoologist in the first half of this century.

REFERENCES

Child, C. M.
1915a. Senescence and rejuvenescence. Chicago: Univ. Chicago Press.
1915b. Individuality in organisms. Chicago: Univ. Chicago Press.
1924. Physiological foundations of behavior. New York: Henry Holt.

Fink, R.
1990. Libbie Henrietta Hyman. Dictionary of Sci. Biogr. Suppl. 2, 17: 442–443.

Harwood, J.
1993. Styles of scientific thought. The German genetics community 1900–1933. Chicago: Univ. Chicago Press.

Hyman, L. H.
1916. An analysis of the process of regeneration in certain microdrilous oligochaetes. J. Exp. Zool. 20: 99–163.
1917. Metabolic gradients in amoeba and their relation to the mechanism of amoeboid motion. Ibid. 24: 55–99.
1919. Laboratory manual for elementary zoology. Chicago: Univ. Chicago Press.
1922. Comparative vertebrate anatomy. Chicago: Univ. Chicago Press. (1942, 2nd ed. published in 1959, 16th printing)

Maienschein, J.
1988. Whitman at Chicago: establishing a Chicago Style of biology? *In* R. Rainger, et al. (eds.), The American development of biology: 151–182. Philadelphia: Univ. Pennsylvania Press.

Mitman, G., et al. (eds.)
1993. Crossing the borderlands: biology at Chicago (special issue of Perspectives on science, vol.1).

Newman, H. H.
1948. History of the Department of Zoology in the University of Chicago. Bios 19: 214–239.

LIBBIE HYMAN AND COMPARATIVE VERTEBRATE ANATOMY

MARVALEE H. WAKE

Libbie Hyman was not a vertebrate biologist, and was vehement about that point. However, through her laboratory guides, she had a significant influence on teaching comparative vertebrate anatomy. She proceeded with her "vertebrate manuals" with her usual rigor and thoroughness. The history of the first, second, and third editions of *Hyman's Comparative Vertebrate Anatomy* illustrates her personality and her work ethic. Quotes from Dr. Hyman's correspondence with the University of Chicago Press reveal that her attitude about these volumes was significantly less than a "labor of love," but also illustrate the way she went about her writing. I emphasize the significance of her contribution to the field, and especially those aspects of her character that made these texts a remarkable piece of work. Her letters illustrate Libbie Hyman at her most inimitable. Because such correspondence must not be released for 35 years, according to privacy laws, I do not have the correspondence relevant to the third edition, but I share my perspective about it to show that she remained true to her concept.

A mimeographed set of laboratory instructions written by Libbie Hyman was the basis for the 1922 edition of Comparative Vertebrate Anatomy. Hyman was an Instructor at the University of Chicago, in charge of laboratory instruction in vertebrate anatomy, among other duties. Following World War I, at Chicago and at many other universities, the method of teaching the laboratories in vertebrate zoology changed from the "type" plan, wherein students learned the features of a few selected animals, but compared their morphology and development with a view to understanding patterns of similarity and change, or evolution. A suitable manual for the comparative method was not available, so she wrote one to meet the needs of the course, and it was subsequently welcomed by other institutions. She included a section on the general features of typical chordates, and gave for each body system an account of the development and evolution of the system. This integration was the mode for the way most texts, and many lab manuals, have been written ever since. The bulk of the work, the laboratory directions and descriptions of the several animals, was written from her dissections of specimens—all of them repeated at least twice. She also rigorously defined technical terms when they first appeared, commenting in the preface to the first edition that "One is continually surprised and annoyed ... to note the loose and inexact way in which many terms are employed." She considered the manual to be a "plain account of the anatomy of the several animals, which account the student follows." This "verification" sort of approach was criticized at the time, but Hyman defended it as the direct route for learning the anatomy of an animal. Hyman cited the "large number and complexity of the anatomical facts to be acquired, the limited time allowed for the acquisition, the large size of the classes, and the limited number of laboratory assistants available ..." (Any current teacher of morphology recognizes this long-standing roblem.) Hyman believed in clear and precise directions presented in manuals, so that the burden of discovery was not left "to dissatisfied students, and instructors irritated and fatigued by the bombardment of demands for assistance." Hyman stated that she believed in "the conservation of instructors, and ... wrote the manual with that end in view." It is clear that Hyman was indeed a hands-on teacher, and probably a very stimulating one to good students.

The story really began in May 1921, when Libbie Hyman contacted University of Chicago Press about the possibility of publishing her manual—by October 1921, or January 1, 1922. (The Press had a backlog, and demurred about the date.) In July, Hyman wrote the Press that she would not be able to have the 500 page manuscript ready until October, and wished to stay with the Press because she was pleased with the way they had

Fig. 20. LIBBIE HYMAN, taken sometime during her Chicago years. After receiving her Ph.D. she continued as Child's assistant at the University of Chicago, carrying out experiments on the physiology of planarians and other invertebrates.

Fig. 21. L. H. at Chicago, dressed for comparative anatomy lab. She is probably standing in front of the Zoology Building, facing the Botany Pond. Early in her career she wrote two laboratory manuals for which, as a graduate teaching assistant, she had seen a need: *A Laboratory Manual for Elementary Zoology* (1919) and *A Laboratory Manual for Comparative Vertebrate Anatomy* (1922).

handled her Vertebrate Zoology Laboratory Guide. Indeed, the Press put the anatomy manual on their list, and it became available in time for the Fall 1922 teaching. Times have changed in the publishing world—Hyman invested $1000 in the production of the book, in addition to funding the illustrator, etc. Royalties quickly were sufficient to repay her investment, and start a royalty rate of 15%. The book was very successful, for it filled a new niche. The lack of correspondence between Hyman and the Press, except for yearly requests for royalty statements for tax purposes and documents of numbers of sales, suggests that the production of the manual and its success produced a very happy situation for Hyman, and for the Press.

The story changed in 1936, when the Press first suggested to Hyman that a revision of the manual might be appropriate. Several reprintings had taken place, and the Press was receiving suggestions from instructors who used the manual that its content be made more inclusive. Hyman responded 25 February 1936 from New York, where she had moved to the American Museum of Natural History in 1931.

> I regret to report that I cannot find time at present to revise my vertebrate manual. Work on a new book

> occupies all my energies. This book has been accepted by McGraw Hill Company (the first volume of The Invertebrata) and I am trying to finish it as soon as possible. Obviously I cannot interrupt this work to revise the other book. I realize that a new edition of the vertebrate manual is highly desirable from a commercial standpoint but teachers who have used the book for years inform me that the next requires very few changes. However, I feel that the general discussions are in need of alteration.

Hyman promised to do a revision when a new printing was needed, and asked for 6 months' notice. In early 1937, in response to an inquiry from the Press about progress on the revision, Hyman responded that her work on her "extensive treatise on invertebrates" was consuming her entire time, but she hoped to complete the revision by spring of 1938. She also indicated that she wanted to do a more extensive revision than the Press suggested, completely rewriting the general sections, adding new illustrations, and changing the format of the lab instructions. She commented "Since the royalty of this book constitutes the greater part of my income ($1524.85 for 1936) it would be necessary for me . . . to invest again in the book, as I cannot afford to take any reduction in the royalty percent . . . I have no wish to display a commercial attitude, since my writings have always been actuated primarily by a sense of service to zoology, but my living depends entirely on the sale of my books, since I no longer hold nor wish to hold any academic position." A verbal commitment for royalties of 15% of sales and no further investment was made by University of Chicago Press.

Hyman brought most of the manuscript for the "revision," really the second edition, to the Press when she came to Chicago for the University's 50th anniversary celebration, at which she was accorded an honorary Ph.D. In October, she wrote that all of the manuscript except the last chapter was in the mail, and closed her letter by saying, "I guess the end of this job is in sight and nobody could be happier than I to get rid of an unwelcome task. But I believe it will be a success."

In December, Hyman wrote:

> I was not informed that the manuscript was to be read by a critic before any work would be started on it. . . . I am perfectly aware that this book is not exactly what is wanted by teachers of vertebrate anatomy but I do not propose to write a textbook for comparative vertebrate anatomy. As I have repeatedly told members of the Press, vertebrate anatomy is not my field of work at all, and further I dislike the subject violently. Trying to cover advances of the last 20 years or more in a field not my own and one in which I have not the least interest has been a terrific strain on me. I have already exhausted myself in the writing of the revision and therefore I must decline to make any extensive changes in the manuscript. . . . I shall try to finish up the revision as soon as possible but I am already sick of the job so that I don't progress very fast.

Mid-1942 brought a letter with many comments about the compositing of the book, closing with "I am sorry for all the mistakes and omissions on my part. They result from the exhausting nature of the work on this book. I have been bored to death throughout the whole of it and really wonder that I ever was able to complete the job." Shortly thereafter, Hyman commented on the advertising material, finding two paragraphs in the biographical information satisfactory, but two not. "I have never done any research in vertebrate anatomy. It is not my field of work and in fact I have a violent dislike of the subject."

Remarkably, the book appeared at the end of August 1942. Hyman was "very pleased at the splendid acceptance of the book." Then in July, 1943, she received her first royalty statement. She was very unhappy about the compensation for her labors, though she was gratified about the book's commercial success. She said:

> I do not wish to appear mercenary but I often feel that no amount of money can ever compensate me for the agonies I suffered over that book. Not only am I not a student of vertebrates but I hate the subject with the most violent hatred. Only dire necessity (since I have no other income) forced me to undertake the revision and I will never make another one. Hence I have to profit on this one while I can. Of course I could have no possible complaint if the sales continue at anything like the present level. The Press did a wonderful job of pushing the book (although I also hope the book has some merit on its own account) and the sales for the first year were far beyond my most optimistic expectations. . . . It is not so much that the book is itself anything remarkable as that the others are so rottenly bad.

The book sold 8200 copies in its first year; 6700 and 6500 in the next two, but in 1944–45 sales dropped to 4000. Hyman expressed concern about whether this was due to "the

Fig. 22. LIBBIE HYMAN in middle age. Fame for her *Treatise* (she was already well-known for her comparative anatomy manual) had come almost as soon as the first volume was published, with an honorary degree from the University of Chicago in 1941. Other honors soon followed. This studio portrait from the AMNH Department of Invertebrates files was probably made for publicity purposes for one of these awards.

absence of so many young men in war service" or whether the sales would remain at about that level. She needn't have worried—sales in 1946–47 doubled, and peaked in 1948–49 at 11,000, but then dropped precipitously (1950–51; 4900), giving Hyman an income of $2290.

Periodically comments about the content were transmitted to Hyman. Her responses (April 1948) were predictable:

> There has been general complaint about the complexity of the account of the development of the skull. This complexity is not my fault; it is inherent in the subject. It proved impossible to give the simple account that appeared in the first edition, because this was too far removed from the facts. The trouble is, that vertebrate anatomy is too difficult for undergraduate students, anyway.
>
> [Later that year]: This matter (including diagrammatic illustrations in the lab instructions) comes up perennially. There are always popping up well-intentioned teachers who want to do the students' work for them. Thirty years ago at the University of Chicago we tried out the method of handing out drawings of the dissections to the students but soon abandoned it as pedagogically bad. The poor students would not work for themselves and the good ones were confused by the fact that their dissections did not fit too well the diagrammatic drawings. . . . Hence I am unalterably opposed to the inclusion of such drawings in the book. Of course the students would love to have such drawings as those made by the Hildebrands; they would love still more to have a complete set of drawings of all the dissections. This would be in line with the policy of our educational system handing out everything to the students. The results of this system are all too evident in our public life. This country is woefully lacking in leadership; our citizenry knows nothing of history or economics, cannot speak or write good English, and is wanting in responsibility and initiative.

Hyman was concerned as potentially competitive texts came out in 1949 and 1950.

> I notice an announcement of a new comparative anatomy by Adams, and Eddy, put out by Wiley, but can't say until I have seen the book whether it will offer any competition. Adams had a previous book on the subject that was of little worth. [Romer's book] . . . came out this fall and will probably have a wide acceptance. Its text about duplicates the textual part of my book but of course it contains no laboratory directions. I am naturally desirous of seeing how it affects my sales but probably another year will be required to get the full effect. [Romer was very unlike Hyman, in its paleontological and evolutionary perspective.]
>
> There is so much competition nowadays in every field of zoology that one can't expect any longer a book to hold the field as mine did for thirty years in vertebrate anatomy. It had occurred to me a number of times that perhaps the book should be returned to its original status as a laboratory manual. Books like Romer compete only on the textual side. Such reports as have reached me about Romer are to the effect that it is used as a reference rather than a class book.

By late 1951, University of Chicago Press formally discussed the need for a revision of Hyman, for diverse textual treatments were significant competition. They particularly recommended that the format return to the lab instructions alone, without the textual treatment. Hyman exploded at the notion of a revision.

> Your letter was certainly very unwelcome news. Fifteen years ago the Press began pestering me to death to revise my . . . manual. At that time I was engaged on the first volume of my invertebrate series. I refused to do anything until this volume was completed. I then undertook the revision of the vertebrate book. It

was an agonizing and hateful job, because not only is vertebrate anatomy not my field but I detest the subject with the utmost violence. . . . It delayed . . . the appearance of further volumes of my invertebrate series and thereby it was embarrassing to me, embarrassing to my publishers and exasperating to the public that was anxiously awaiting the continuation of that series. And now it appears that it would have been better to have left well enough alone. Now the same situation is up again. I am now engaged on the fourth volume of my invertebrate series. I don't want to be delayed with it. I am very unwilling to do anything about Comparative Vertebrate Anatomy. Fortunately I am not in the position in which I was before. I am not dependent on the royalties of this book for my living. I now have other sources. I am not going to revise this book. I am not going to do anything but cut out the text parts and leave the laboratory directions. The person who wants only the mammal part must be out of his mind. I would never consider any such thing. . . . This is positively the last time I will do anything with that book.

Ilza Veith, then an editor at the Press and later an historian of science, responded in 1952

Dear Miss Hyman: I have not heard from you since December 14, 1951, when you informed me that you would undertake the revision of your Comparative Vertebrate Anatomy, and when you also indicated your somewhat negative attitude towards this revision. In spite of this attitude I hope that you have been able to make some progress and I wish to assure you that we are waiting with impatience for news concerning the expected date that the revision will be completed.

The response on June 2 was

Dear Miss Veith: As you were informed in my last letter I am working on the fourth volume of my invertebrate treatise. I cannot work on two books at once. I am not willing to stop work on this volume to work on Comparative Vertebrate Anatomy. I have not done anything about the latter and do not know when I shall undertake it. . . . I . . . talked to a number of users of Comparative Vertebrate Anatomy. The impression I received from these teachers was quite different from what the Press had led me to believe. They were not particularly inclined to think that the book needed any alteration. None of them were using Romer as a text; they said their students are unable to understand it. They were also not much impressed with other texts available in the last few years. Under these circumstances I cannot feel that there is any pressing necessity of altering Comparative Vertebrate Anatomy. You had better plan to keep on reprinting Comparative Vertebrate Anatomy for some time to come.

Veith responded

Thank you for your letter of June 2. I am somewhat disappointed that you are not yet inclined to undertake the revision . . . I realize the difficulty of your situation but I also hope that you will realize the necessity for us to publish a revision of your book before too long. In thinking about the revision I hope you will take into consideration the fact that your book is a well-established one . . . and that in spite of your new predilection for invertebrate anatomy, some of your reputation still rests on vertebrate anatomy. I am certain that you have been proud of your work and I should feel sorry if this pride had vanished altogether . . . If you are really tired of this book and do not wish to take the time to work on it again, would you prefer to have someone else do the revision for you, and could you possibly think of a suitable person?

Guess what the response was.

June 29th, 1952. Dear Miss Veith: Your letter received. You have a mistaken idea as to my interests. I have always been an invertebrate zoologist. I took my doctor's degree in this field and spent 15 years of research on the physiology of the lower invertebrates. I envisioned the project of a major work in invertebrate zoology at least 25 years ago. I gave up my position at the University of Chicago in 1931 in order to spend my entire time on the writing of a treatise on invertebrate zoology. Since that time I have had no paid position but have lived on the royalties of my books. My project, however, has been constantly interrupted by the U. of C. Press. . . . the press began pestering me to make a new edition of the book called Laboratory Manual for Comparative Vertebrate Anatomy. There were at that time absolutely no grounds for this demand. The book was selling better than ever after having been out some 15 years or more and was constantly increasing its sales. . . . I was forced to yield. . . . I took off three years to write Comparative Vertebrate Anatomy. It was a loathsome job. I have never liked vertebrate anatomy . . . here is the Press again with the same old demands. . . . It has been a constant annoyance to me that the authoring of that book has associated me with vertebrate anatomy in the minds of many teachers and students. No, I am not proud of it. I had no right to undertake a book outside my field. I will do what I said I will do. I will remove the text parts and reduce the book to a laboratory manual, its original intention. . . . I don't understand what you mean by getting some one else to do a revision. . . . You don't seem to understand that this book consists of laboratory directions, which do not require any changing except for a few inevitable mistakes, and a textual part that does need alteration from time to time. But according to the Press the public does not want the entire book, it wants only the laboratory manual. . . . If anybody else is going to do it, I certainly would expect to retain major royalty rights. . . . I don't know anybody competent to revise the text. Vertebrate anatomy is in fact a dead subject in this country. Young people don't get interested in it and it stays alive only because it is a required premedical subject. Romer is just about the only real vertebrate anatomist we have and he is in fact a paleontologist. . . . I find myself very much puzzled by your statement as to the decline of the

> sales because everywhere I go I meet teachers and students who are using the book. There must be some other grounds for the sales slump than faults of the book.

Veith persisted a year later in inquiring about the status of a revision. Hyman responded

> Dear Mrs. Veith: Your go-getting tactics antagonize me. I told you over and over again that I am working on the fourth volume of my treatise on invertebrates and that I do not propose to delay the completion of this volume one iota to oblige the U. of C. Press. I also told you repeatedly that the entire subject of vertebrate anatomy is detestable to me. Apparently you did not take these statements seriously. That was your mistake. The probabilities are that I will never make a revision of this book. I . . . see now that I should have had it written into the contract that I was never again to be annoyed about this book.

Veith responded in complete defeat, saying that she

> . . . still should like to remind you of your promise to give it some thought. It might amuse you to know that the completion of your revision was one of my first projects when I joined the Press and that I am leaving it now in favor of a full-time position in the Department of Medicine without having completed this first project. Quite obviously you are much more effective than I am.

Correspondence after 1956 between Libbie Hyman and University of Chicago Press is not available, still protected by privacy laws. However, University of Chicago Press pursued the need for revision of Comparative Vertebrate Anatomy. In 1968, it contacted Richard Snyder, a distinguished comparative vertebrate anatomist at the University of Washington, and asked him to organize a revision, a third edition. A primary reason for the Press' interest was its knowledge that Dr. Hyman was ill and dependent on her royalties, largely from the vertebrate anatomy guide. Snyder contacted several vertebrate morphologists with expertise in different aspects of comparative anatomy, and asked them to consider doing a multiauthored revision, largely to update and modernize the textual material. It is no surprise that the laboratory directions needed relatively little revision. Eleven vertebrate (and invertebrate) morphologists in the United States and Britain agreed to undertake the task, primarily to help Libbie Hyman but also because we believed in the utility of the format of the book, and its contribution to the training of students. Because of her situation, the majority of the royalties were allocated to Dr. Hyman. The third edition had a long gestation. Dick Snyder had to withdraw as general editor for the project, and I assumed that role. Dr. Hyman died long before the volume appeared in 1979; her royalties from the third edition enrich the Invertebrate Library at the American Museum of Natural History, as bequeathed in her will.

With considerable trepidation, I hope some day to see the correspondence from Dr. Hyman to the Press as the revision was contracted, because it could hardly have pleased her, given the attitudes she had expressed about the idea. However, Dr. Hyman was right; it took a group of us to replace her in this effort. Many of us continue to subscribe to her notion that students should learn to do work themselves, with appropriate written guidance. The format of the third edition included an expansion of the textual material to emphasize functional morphology and evolution, but it retained, with some updating, the laboratory dissection directions. I am now frequently told that its approach, though some of its material is out of date, makes it a useful text, but often of more utility to instructors than to students. I find this sad. Libbie Hyman, feisty and cantankerous though she was, was so *right* about what teaching should accomplish, and how poor or misdirected teaching can dilute the initiative of students. Given that her great love and lifetime commitment was her treatise on invertebrate biology, she made a highly significant contribution to the teaching of vertebrate morphology. Even though, by her own admission, she detested the field, she could not give it short shrift as she produced the volume that constituted much of her income. Libbie Hyman's emphasis on rigor, precision, thoroughness, good writing, good pedagogy, and respect for the abilities of students constitute a legacy to all good teachers and researchers.

ACKNOWLEDGMENTS. I am especially grateful to the archivists at the University of Chicago Library, who made available to me copies of some 35 years' correspondence, from 1921 to 1956, between Hyman and the University of Chicago Press. The letters are the database for this report. I thank Rachel Fink

and Judith Winston for organizing the symposium at the 1991 annual meeting of the American Society of Zoologists, and for inviting me, a vertebrate morphologist, to participate in the celebration of the career of one of the world's great zoologists—vertebrate and invertebrate.

REFERENCES

Hyman, L. H.
1922. Preface, Comparative vertebrate anatomy. 1st ed. Chicago: Univ. Chicago Press.
1942. Preface, Comparative vertebrate anatomy. 2nd ed. Chicago: Univ. Chicago Press.

CONTRIBUTIONS OF LIBBIE H. HYMAN TO KNOWLEDGE OF LAND PLANARIANS: RELATING PERSONAL EXPERIENCES (TRICLADIDA: TERRICOLA)

ROBERT E. OGREN

INTRODUCTION

Libbie Henrietta Hyman has been an important American zoologist of the 20th century with significant influence in the field of invertebrate biology through the preparation and publication of the six-volume treatise, *The Invertebrates*. Since her work greatly influenced me, it is my intent to relate my encounters, along with some account of her contributions in the field of my research. Short biographical accounts can be consulted (Youngpeter, 1969; McConnel, 1970; Stunkard, 1970, 1974; Winston and Fink, 1991).

My experiences with Dr. Hyman began in college comparative anatomy class using her well-known manual of dissection. As I worked for my doctorate I had occasion to read Hyman's *The Invertebrates*, a major reference in zoology. However, it was not until I actually discovered my first land planarian that research encounters began. During the past 45 years, I have studied land planarians (Tricladida: Terricola), the flatworms found in terrestrial soil, beneath surface litter, logs, and stones, and in gardens or horticultural greenhouses. Libbie Hyman described and named several of these interesting flatworms. My studies progressed from discovering living specimens in new localities, doing histological work, then reading her accounts or using slides prepared by her.

DEVELOPING A CAREER. Libbie Hyman studied zoology at the University of Chicago, receiving her bachelor's degree in 1910 and Ph.D. in 1915. She then began to study with physiologist Charles Manning Child (Hyman, 1955b) as a research assistant conducting basic studies on physiology of freshwater planarians. Her papers of 1919, *Physiological Studies on Planaria,* introduced her as an expert on planarian research. The bibliography of articles is provided by McConnel (1970) and Riser and Morse (1974).

Libbie Hyman left the University of Chicago in 1931 at the age of 44, when Dr. Child became department chairman and reduced his research efforts. She entered a new career in 1932, moving to New York, and becoming a guest scholar of the Department of Experimental Biology in the American Museum of Natural History with access to its extensive library. In 1937 she became a research associate and was assigned an office and laboratory. Her treatise on *The Invertebrates* (1940c, 1951) was produced at the AMNH as well as numerous articles on planarians. Her involvement with taxonomy and systematics was gradual, although a requirement for classifying planarians. She already had begun an important series of papers on taxonomy of triclad freshwater Turbellaria (1928–1963) of North America and by 1937 eight significant articles had been published. In addition, she described a variety of marine microturbellarians and polyclads. Although she may not have formally chosen to be a systematic biologist, she became one by virtue of need, and had a strong influence on taxonomy of the Platyhelminthes. She was a member of the Society of Systematic Zoology, serving as president for 1959 and jour-

Fig. 23. LIBBIE HYMAN feeding the pigeons outside the American Museum of Natural History. In 1952, at the age of 65, Libbie gave up her house in Millwood and moved to an apartment hotel near the museum.

Fig. 24. LIBBIE HYMAN and DAVE FREY on the shore of Lake Cayuga, 1952. At this time Libbie had not yet begun to suffer from Parkinson's disease and could still enjoy travel and meeting with colleagues.

nal editor (1959–1963). She was listed as a zoological taxonomist in Blackwelder & Blackwelder (1961), willing to identify Turbellaria of the world.

Dr. Hyman developed her expertise with land planarians as a natural consequence of being a research associate at AMNH. She utilized special collections, made identifications, and was partly responsible for establishing the valuable AMNH flatworm reference collection.

Her first paper on land planarians described species collected in Yucatan by an expedition of the Carnegie Institute (Hyman, 1938). She described, in the family Rhynchodemidae, the new genus *Diporodemus* (Hyman, 1938), so named because the type species *Diporodemus yucatani* (Hyman, 1938) possessed a secondary sexual pore opening to the side from the seminal bursa. Additional papers of this period (Hyman, 1939a, b, 1940a, 1941) described land planarians from Hawaiian Islands, Panama, Micronesia, Central America, and South America. These early papers established Hyman's insight, authority, and expertise with planarians. She published a survey of North American land planarians (Hyman, 1940b), indicating the great lack of taxonomic work being done. She then filled the need with a definitive paper: *Endemic and Exotic Land Planarians in the United States* (Hyman, 1943). Her efforts continued for 25 years, from age 49–74, the last publications being on land planarians from South America, Canal Zone, and the Caribbean (Hyman, 1955a, 1957, 1962).

CONSULTING THE GREAT ZOOLOGIST: I found

my first land planarians in June of 1952, in the enclosed backyard of the Vivarium Building, University of Illinois, Champaign-Urbana, Illinois (Ogren, 1955) while searching for slugs for a parasitology experiment. I placed small boards around the yard on the grass or soil and beneath shrubs as collection traps. To my surprise, besides slugs and snails, I found a small, awl-shaped, unsegmented brown worm that was puzzling and interesting. In my attempt to identify this unknown "critter," my first reference was the classic *Fresh-Water Biology* by Ward and Whipple (1918: 360) which I had recently purchased. Dr. Henry B. Ward had been a famous zoology professor at University of Illinois. Using taxonomic keys, I learned that my specimen was a triclad terricolan, i.e., land planarian in phylum Platyhelminthes. The Ward and Whipple text included references and two drawings, one a dorsal external view showing two large anterior eyes and two brown stripes; the other a diagrammed body with shape and digestive system. The species was *Rhynchodemus sylvaticus* (Leidy, 1851a); then, I realized that I had discovered a new locality. As I found more technical literature, I discovered that the definitive work at the time was a description of exotic and endemic land planarians in the United States by Hyman (1943). I learned that my discovery was the first land planarian reported from Illinois and from the University of Illinois, Champaign-Urbana campus where it had escaped the notice of great zoologists. After reading Hyman's descriptions, I began to believe that I also had her recently described species *Rhynchodemus americanus* (Hyman, 1943) because of darker specimens I had collected.

I sent my letter of inquiry December 18, 1952, and received her quick reply written December 22, 1952. "I was very interested in your letter and quite surprised to learn about the occurrence of *Rhynchodemus americanus* out-doors. I had decided some time ago that it and sylvaticus are both imported species, since what has been called bilineatus in western Europe is in fact sylvaticus. I am also interest(ed) in the other specimen[1] you mention as I once spoiled a land planarian from Wisconsin[2] and was never able to get another from there. Of course you realize that the name *Rhynchodemus* now has to be restricted to forms similar to sylvaticus. . . . Yes, the land planarians seem to be not as rare as supposed if you know where to look for them. The person[3] who gave me the collection mentioned above finds them not infrequently while looking for other animals." She was very positive, helpful, and considerate of a young graduate student. Later I also sent her my prepared slides and indicated I was preparing a paper on my observations (Ogren, 1955). I had her helpful reply by March 17, 1953, but it was necessary to provide additional histological material. She did not like my tentative diagnosis of *R. americanus* and stated (letter April 20): "Nevertheless, there is no doubt in my mind that the specimen is Rhynchodemus sylvaticus. This was a foregone conclusion from the color pattern." She made several helpful suggestions and observations in each letter. Thus, as a graduate student, I was on my way into the study of land planarians and learning about Libbie H. Hyman. Later, as a young college professor, I would have my first personal experience.

My letters requesting information and guidance appear to have had a stimulating effect on Hyman's work at the time because she wrote on December 22, 1952 (in litt.): "I have on hand a large collection of U.S. land planarians from eastern U.S. but haven't had time to work them over. I feel that the whole matter of nomenclature in regard to these forms has to be gone over again. I am not too satisfied with what I said in the article on Endemic and exotic land planarians, etc." In June 10, 1954, Dr. Hyman published her new observations and remarks on nomenclature, some of which were suggested in her letter to me. I was especially pleased to get an honorable mention for having reported *Rhynchodemus sylvaticus* . . . "apparently the first finding in nature in many years." (Hyman, 1954: 2). Some years later I showed that *R. americanus* and *R. sylvaticus* were the same species (Ogren, 1989).

[1] Later Ogren (1955) identified it as *Rhynchodemus sylvaticus*.

[2] Hyman (1943:15) identified this as *Microplana terrestris* (now *Geodesmus*).

[3] The person was Leslie Hubricht.

My first personal contact with Dr. Hyman was in the spring of 1959, when I visited her laboratory at the American Museum of Natural History for consultation and advice on land planarians. I had published short studies (Ogren, 1955, 1956, 1957) on natural history, behavior, and development of *Rhynchodemus sylvaticus* with specimens found near my home in Collegeville, Pa. A large black species, sometimes bluish-black (atrocyane), from New York State appeared to be *Microplana atrocyanea*. I wanted to see the AMNH laboratory and meet her personally. Thus, I arranged for a visit, by letter, and made my way to New York and finally, with slides and notebook in hand, arrived at her office on the fifth floor of the old AMNH building. Through the open door I could see Dr. Hyman standing at a large group of oak library card files, which represented references for her work on *The Invertebrates* and planarian publications. I was impressed by the amount of time represented for this accumulation of references. The laboratory office was plain with laboratory table and work area along the wall in front of a large window. To one side was a large desk and chair and no room for a secretary. I was surprised since I had anticipated a larger more modern laboratory area. After a few words of introduction and social pleasantries, I handed her the slides to be examined. We went to the high wall table where she pulled out an old fashioned monocular microscope brass and black painted. Certainly this was a disappointment since I expected a large, fancy binocular research microscope. However, it was satisfactory for our purposes. She made her observations quickly and we discussed the results. Perhaps my visit was concluded in an hour. But I felt satisfied with her conclusions, with my new experience, and apparent acceptance as a colleague studying land planarians. I believed she understood the problems of a young zoologist. I guess I was rather bold to ask for this visit but she was very friendly, patient, and helpul, not at all as I had pictured her. I had the impression that she generally approved of my work and had an interest in what I was doing. In an earlier letter I suggested that I was interested in more serious specialization on the taxonomy of land planarians. Her response to this in a letter of January 10, 1959, was: "I advise you to stay away from taxonomy. A taxonomist cannot operate on a local basis. One must have knowledge of world literature. Planarians cannot be identified except by means of sagittal serial sections of sexually mature animals. Your natural history studies of *Rhynchodemus sylvaticus* are valuable but your taxonomic attempts are pretty feeble."

In another letter, October 29, 1961, she remarked: "This is a somewhat belated acknowledgement of the photos of Microplana atrocyanea. It doesn't look very blue, does it? You seem to have some attraction for land planarians. They park in your backyard when nobody else can find them by intensive search, except Leslie Hubricht who has turned up quite a number while looking for something else." This was her last letter to me and at the time of her death in 1969 I was deeply involved with college teaching, cell biology, and research on tapeworm hexacanth embryos (oncospheres). It was not until 1980 that I once again published papers dealing with land planarians discussed in that conference (Ogren, 1981, 1982, 1983, 1984), because *Bipalium adventitium* had appeared literally under my back porch step in Kingston, Pa. In the past decade I have spent much effort in restudy of her exotic species *Bipalium adventitium* (Hyman, 1943), *Geoplana vaga* (Hyman, 1943), and *Geoplana mexicana* (Hyman, 1939b) (Ogren, 1985, 1989, 1990).

SIGNIFICANCE OF LIBBIE HYMAN'S LAND PLANARIAN EFFORTS

1. She placed land planarian investigation in the United States on a solid basis. Previous studies of land planarians in America lacked details and necessary histology of body and copulatory apparatus. The scientist who first discovered a land planarian in America was Dr. Joseph Leidy, Academy of Natural Sciences of Philadelphia, who collected living specimens which he named *Planaria sylvatica*, in gardens of the Philadelphia area (Leidy, 1851a). These small, awl-shaped worms were renamed *Rhynchodemus sylvaticus* (Leidy) when he realized a new genus was necessary (Leidy, 1851b); but there were no histological studies or illustrations. Another

Philadelphia Academy of Science naturalist from 1852 to 1872, William Stimpson, traveled in the Pacific Area, collected a variety of Platyhelminths, and described several land planarians placed in his new genera *Bipalium* and *Geoplana*. A period of nearly 50 years passed before planarians were again collected in the United States during the years 1905–1912 and studied by Dr. Lee Barton Walton, Professor of Biology, Kenyon College, Gambier, Ohio. His three short papers identified land planarians found in gardens, but failed to include needed morphological details and illustrations. Libbie Hyman, when her land planarian studies began in 1938, made detailed, illustrated descriptions of new species, including the reproductive system. Her studies succeeded in making the species of land planarians in the United States better known (Hyman, 1940, 1943, 1954).

2. She was the first North America zoologist to become an international specialist in nonparasitic Platyhelminthes and an authority on land planarians. Dr. Hyman drew attention to the nonparasitic Platyhelminthes and perhaps led the way for new investigators.

3. She provided additional morphological details for species in all of three families: Bipaliidae (Stimpson, 1858), Rhynchodemidae (von Graff, 1896), and Geoplanidae (Stimpson, 1857).

4. She enhanced species identification by describing the copulatory apparatus, required for complete classification, for new and previously described species. Hyman (1943) reexamined *Rhynchodemus sylvaticus* (Leidy, 1851), describing its copulatory apparatus for the first time in 92 years (Ogren, 1983). Later developmental studies (Ogren, 1986) revealed that this was not the fully mature organ. Her study showed that *R. sylvaticus* was the type species of the genus *Rhynchodemus* and also type for the subfamily Rhynchodeminae Corrêa (Hyman, 1943; Ogren, 1983, 1989). Later, Pantin (1950) showed that *Geodesmus bilineatus* Mecznikoff, 1866, a European species greatly resembling *R. sylvaticus*, also belonged in genus *Rhynchodemus*. Specimens of *R. bilineatus* were sectioned and carefully examined by Dr. Hyman. "From this study of the copulatory apparatus it is clear that Pantin's specimens are distinct from R. sylvaticus, although the two species probably cannot be distinguished externally." (Hyman, 1954: 6). This restrained language modified her earlier opinion that *sylvaticus* and *bilineatus* were the same species. However, full agreement was finally announced by Pantin (1953) that the European *R. bilineatus* was indeed conspecific with and a synonym of *Rhynchodemus sylvaticus*. These decisions were important because they established the basis for classification in Rhynchodemidae.

5. She was an authority for identification of land planarians in the United States, providing access to literature and museum collections.

6. Her published papers include 27 new species and are reliable descriptions of land planarians from North America, the Caribbean region, South America and the Hawaiian Islands. In *The Invertebrates*, Vol. 2, 1951: 160–163, there is a detailed description of uborder Terricola with accounts of representative species.

7. She introduced taxonomic revisions in Rhynchodemidae that resulted in progress toward improved classification. Hyman (1943) described the new subfamilies Dolichoplaninae and Geodesminae, which later became synonyms of other taxa (Pantin, 1953), recognized by Hyman (1954) in a revised classification. This involved story is told elsewhere (Ogren and Kawakatsu, 1988).

8. She introduced the following two new genera into the family Rhynchodemidae, subfamily Microplaninae Pantin, 1953: *Diporodemus*; and *Orthodemus*. The genus *Diporodemus* provided a convenient taxon for certain species having the feature of two reproductive pores, but this concept has been challenged because one pore may close early. Distinctive features of the copulatory apparatus separate *Diporodemus indigenus* from *Microplana terrestris*, thus, providing stronger support for the genus *Diporodemus* (Ogren, 1991: 30, 33).

The introduction of *Orthodemus* was in response to the taxonomic revisions of Pantin (1953) since Hyman was not satisfied with the assignment of *Fasciola terrestris* to *Microplana* and recommended instead her new genus *Orthodemus*. She stated (Hyman, 1954:19): "I feel that nothing is gained by

making of Microplana the same "hodgepodge" that was formerly made by Rhynchodemus. I consider that the genus Microplana should be limited to species with a simple female tract without appendages. . . . Species with a bursa attached to the vagina and having but one exit canal for the bursa I would place in Orthodemus.

Unfortunately, even though the suggestion sounds reasonable, the genus *Orthodemus* has not been generally accepted pending further study and is considered a synonym of *Microplana* (Ball and Reynoldson, 1981; Ogren, 1984, 1988, 1991; Ogren and Kawakatsu, 1988, 1989; Ball and Sluys, 1990). Differences in copulatory apparatus between species in *Microplana* and *Orthodemus* are considered to be developmental (Ogren, 1988): Transitional stages of conspecific *M. atrocyanea & terrestris,* belong in one genus. Therefore, since *Microplana* had priority, *Orthodemus* became a synonym.

KEY INVESTIGATORS SUPPLYING SPECIMENS

The major source of land planarians in the United States studied by Libbie Hyman was Dr. Leslie Hubricht, who also collected terrestrial snails (Hyman, 1943, 1954). He remarked in a letter to me dated April 6, 1983: "I think that I am responsible for the use of the name Rhynchodemus atrocyaneus Walton. When I sent my first batch to Dr. Hyman I put that name on them. She wrote and asked where I got that name, and I told her 'Out of Ward & Whipple.' She investigated and obtained the types and decided that that was probably what they were." (Hubricht, 1983, in litt.).

Dr. Hubricht's collections are significant to the development of Dr. Hyman's interest in and contributions to land planarians. More than any other person he provided specimens of Rhynchodemidae from many localities. His efforts advanced and perhaps encouraged Hyman's investigation of land planarians in America with specimens for two papers (Hyman, 1943, 1954). Dr. Leslie Raymond Hubricht, born January 11, 1908, in Los Angeles, California, worked as Assistant to the Geneticist at Missouri Botanical Garden, 1936–1943, then served as Senior Systems Engineer, Univac Div., Sperry Rand Corp., 1943–1973. He is a well-known malacologist, specializing in land snails, with over 115 articles on land and freshwater snails of eastern United States (Abbott and Young, 1973:310). During his searches for terrestrial molluscs, he also found land planarians, which were sent to Dr. Hyman for identification. His collection efforts began about 1935 and Dr. Hyman (1943, 1954) documented that Hubricht provided specimens from various localities for *Rhynchodemus sylvaticus* Leidy; *Microplana atrocyanea* (olim *Rhynchodemus atrocyaneus* Walton) collected in eastern United States, 1935–1942, and February 1949; specimens for three new species: *Rhynchodemus americanus* Hyman, 1943, from Greenhouses, Forest Park, St. Louis, Missouri, collected January 18, 1936; *Microplana rufocephalata* Hyman, 1954, collected in 1952; and *Diporodemus indigenus* Hyman, 1943, collected from eastern United States, 1935–1940, 1943–1951 providing present knowledge of distribution.

In a letter to me dated October 23, 1991 he remarked: "I never met Dr. Libbie Hyman, just corresponded with her. I continued to send her specimens even after I learned to identify them, and she continued to send me her identifications. Once she got a little careless and misidentified a lot. I wrote about it but she would not admit that she had made a mistake . . . I do not remember who first recognized that Diporodemus indigenus was a new species." Known geographic ranges of these species in natural localities in eastern United States are largely due to the persistent successful collection efforts of Dr. Hubricht, his excellent locality data, and Dr. Hyman's careful listing of collection sites and specimens. Most of the specimens and types still exist in the AMNH Platyhelminth Reference Collection (Feinberg, 1970; Boyko, 1996).

The late Dr. G. Alan Solem, Curator of Invertebrates, Field Museum of Natural History, Chicago, was another well-known American malacologist (Abbott and Young, 1973) who provided specimens for Hyman's description. Her publication "*Some land planarians from Caribbean Countries*," included seven new species of the family Geoplanidae collected by Dr. Solem while searching for land snails during extensive field trips in

Panama, and sent to Hyman for identification. Dr. Solem, who died February 26, 1990, was curator for 33 years at the Field Museum, participating in 19 field trips outside the United States (Wenzel, 1990; Girardi, 1991).

ADDENDUM. NEW SPECIES OF TERRICOLA DESCRIBED BY LIBBIE H. HYMAN

RHYNCHODEMIDAE: Rhynchodeminae*

Rhynchodemus americanus Hyman, 1943 (=*R. sylvaticus*)

Rhynchodemus angustus (Hyman, 1941) olim *Desmorhynchus*

RHYNCHODEMIDAE: Microplaninae*

Diporodemus yucatani Hyman, 1938

Diporodemus indigenus Hyman, 1943

Diporodemus plenus Hyman, 1941

Microplana rufocephalata Hyman, 1954

***Note:** These are not the subfamilies used by Hyman (1943, 1954) because at the time she classified the family Rhynchodemidae into two subfamilies: Dolichoplaninae Hyman, 1943 (syn. of Rhynchodeminae Correa, 1947) and Geodesminae Hyman, 1943 (syn. of Microplaninae Pantin, 1953).

GEOPLANIDAE: 20 new species were originally described by her under the genus *Geoplana* from 1939 to 1962. These land planarians, all originally placed in the genus *Geoplana* Stimpson, 1857, are now assigned to various new genera by Ogren and Kawakatsu (1990, 1991).

Geoplana (*Geoplana*, n. sg.) (4), *Amaga* (1), *Gigantea* (4), *Pasipha* (2), *Pseudogeoplana* (3), *Notogynaphallia* (2), *Caenoplana* (1), *Endeavouria* (1), *Kontikia* (1), *Australopacifica* (1).

GEOPLANINAE Ogren and Kawakatasu, 1990.

Geoplana alterfusca Hyman, 1962. Panama [G. (*Geoplana*)]

Geoplana catherina Hyman, 1957. Brazil [G. (*Geoplana*)]

Geoplana fuhrmanni Hyman, 1962. Panama [G. (*Geoplana*)]

Geoplana fusca Hyman, 1962. Panama [G. (*Geoplana*)]

Geoplana andina Hyman, 1962. Colombia (*Notogynaphallia*)

Geoplana quinquestria Hyman, 1962. Panama (*Notogynaphallia*)

Geoplana contamanensis Hyman, 1955. Peru (*Amaga*)

Geoplana unicolor Hyman, 1955. Peru (*Gigantea*)

Geoplana chiriquii Hyman, 1962. Panama (*Gigantea*)

Geoplana bistriata Hyman, 1962. Panama (*Gigantea*)

Geoplana montana Hyman, 1939. Costa Rica (*Gigantea*)

Geoplana aphalla Hyman, 1941. Panama (*Pasipha*)

Geoplana diminutiva Hyman, 1955. Peru (*Pasipha*)

Geoplana panamensis Hyman, 1941. Panama (*Pseudogeoplana*)

Geoplana ucayalensis Hyman, 1955. Peru (*Pseudogeoplana*)

Geoplana sp.(juv.) Hyman, 1962. Panama (*Pseudogeoplana*)

CAENOPLANINAE Ogren and Kawakatsu, 1991.

Geoplana mexicana Hyman, 1939. Texas (=*Kontikia ventrolineata* (Dendy, 1892), Synonymy: Jones, Johns & Winsor (1998)

Geoplana subpallida Hyman, 1939. Hawaiian Island (*Australopacifica*)

Geoplana vaga Hyman, 1943. California, USA (*Caenoplana coerulea vaga*)

Geoplana septemlineata Hyman, 1939, Hawaiian Is. (*Endeavouria*)

BIPALIIDAE:

Bipalium adventitium Hyman, 1943

Bipalium costaricense Hyman, 1939 (olim *B. costaricensis*) [**Note**: This species is now considered a synonym of *Bipalium kewense* Moseley, 1878.]

NEW SPECIES OF LAND LAND PLANARIANS NAMED IN HER HONOR

Diporodemus hymenae E. M. Froehlich and C. G. Froehlich, 1958. Brazil

Amaga libbieae (du Bois-Reymond Marcus, 1958) (olim *Geoplana*). Peru

REFERENCES

Abbott, R. T., and M. E. Young
1973. American malacologists. 1st ed. Falls Church, VA: Am. Malacol.

Ball, I. R., and T. B. Reynoldson
1981. British planarians. Synop. Br. Fauna 19.

Ball, I. R. and R. Sluys
1990. Turbellaria: Tricladida: Terricola. *In*: D. L. Dindal, (ed.), *Soil biology guide*: 137–153. New York: Wiley.

Blackwelder, R. E., and R. M. Blackwelder
1961. Directory of zoological taxonomists of the world. Carbondale: Southern Illinois Univ. Press.

Boyko, C.
1996. Catalog of recent type specimens in the Department of Invertebrates, American Museum of Natural History, III. "Parasitica" (Phyla Platyhelminthes, Rhombozoa, and Pentastomida) amd Gastrotricha. (Supplement), Am. Mus. Novitates 3174: 59 pp.

Feinberg, H. S.
1970. A. catalogue of type specimens in the department of living invertebrates, the American Museum of Natural History, New York, U.S.A. The Phyla Platyhelminthes and Gnathostomulida. Issued by the Department of Living Invertebrates AMNH, N.Y. [Includes Dr. Hyman's types.]

Girardi, E-L.
1991. A bibliography and brief biography of G. Alan Solem, 1931–1990. The Veliger 34/4: 317–323.

Hyman, L. H.
1938. Land planarians from Yucatan. *In* A. S. Pearse (ed.), Fauna of the caves of Yucatan, Carnegie Inst. Washington Publ. 491: 23–32.
1939a. Land planarians from the Hawaiian Islands. Arch. Zool. Exp. Gen. Notes Rev. 80(3): 116–124.
1939b. New species of flatworms from North, Central and South America. Proc. U.S. Natl. Mus. 86/3055: 419–439.
1940a. Land planarians from Palau and Caroline Islands, Micronesia. Ann. Mag. Nat. Hist. (11) 5: 345–362.
1940b. Native and introduced land planarians in the United States. Science 92/ 2379: 105–106.
1940c. The invertebrates: Protozoa through Ctenophora. N. Y.: McGraw-Hill.
1941. Terrestrial flatworms from the Canal Zone, Panama. Am. Mus. Novitates 1105: 11 pp.
1943. Endemic and exotic land planarians in the United States with a discussion of necessary changes of names in the Rhynchodemidae. Ibid. 1241: 21 pp.
1951. The invertebrates: Platyhelminthes and Rhynchocoela vol, 2.: *the acelomate Bilateria*. New York: McGraw-Hill.
1954. Some land planarians of the United States and Europe, with remarks on nomenclature. Am. Mus. Novitates 1667: 21 pp.
1955a. Miscellaneous marine and terrestrial flatworms from South America. Ibid. 1742: 33 pp.
1955b. Charles Manning Child, 1865–1954. Biogr. Mem. Nat. Acad. Sci. 30: 73–103.
1957. A few turbellarians from Trinidad and the Canal Zone, with corrective remarks. Am. Mus. Novitates 1862: 8 pp.
1962. Some land planarians from Caribbean countries. Ibid. 2110: 25 pp.

Jones, H. D., P. M. Johns and L. Winsor
1998. The proposed synonymy of *Parakontikia ventrolineata* (Dendy, 1892) and *Kontikia mexicana* (Hyman, 1939): what is a penis papilla? Hydrobiologia 383: 91–96.

Leidy, J.
1851a. Helminthological contributions-no.3, Proc. Acad. Nat. Sci., Philadelphia, 5: 239–244.
1851b. Corrections and additions to former papers on Helminthology published in the Proceedings of the Academy, Ibid. 284–289.

McConnel, J. V. (ed.)
1970. In memoriam: The Libbie H. Hyman memorial issue. J. Biol. Psychol./Worm Runner's Dig., 12/1: 23 pp. [Includes biography, pictures, and anecdotes.]

Ogren, R. E.
1955. Ecological observations on the occurrence of *Rhynchodemus*, a terrestrial turbellarian. Trans. Am. Microsc. Soc. 74: 54–60.
1956. Physiological observations on movement and behavior of the land planarian *Rhynchodemus sylvaticus* (Leidy). Proc. Pennsylvania Acad. Sci. 30: 218-115.
1957. Developmental observations, the egg capsule and sexual maturity of the land *Rhynchodemus sylvaticus*. Trans. Am. Microsc. Soc. 76: 45–52.
1981. Land planarians in Pennsylvania. Proc. Pennsylvania Acad. Sci. 55: 52–56.
1982. The identification and reproductive apparatus of the land planarian, *Microplana atrocyaneus* (Walton) from Pennsylvania (Platyhelminthes: Turbellaria). Ibid. 56: 39–43.
1983. Reproductive complex and sexual maturity of the land planarian, *Rhynchodemus sylvaticus* (Leidy) in the United States. Ibid. 57: 696–73.
1984. The land planarian *Microplana terrestris* (Platyhelminthes: Turbellaria) from the United States. Trans. Am. Microsc. Soc. 103: 130–136.
1985. Exotic land planarians of the genus *Bipalium* (Platyhelminthes: Turbellaria) from Pennsylvania and the Academy of Natural Sciences, Philadelphia. Proc. Pennsylvania Acad. Sci. (for 1984) 58: 193–201 [Issued in 1985].
1986. Development of the reproductive apparatus of the land planarian *Rhynchodemus sylvaticus* (Turbellaria: Tricladida) and its significance for the classification of the genus. Hydrobiologia 132: 175–180.
1988. Developmental and Morphological evidences that the land planarian *Micro-*

plana terrestris and *M. atrocyaneus* are conspecific. Fortschritte der Zoologie 36: 395–398.

1989. Identification features of the two-lined land planarian *Rhynchodemus sylvaticus*, with evidence that *Rhynchodemus americanus* is conspecific. Trans. Am. Microsc. Soc. 108/1: 40–44.

1990. Redescription of the geoplanid land planarian *Kontikia mexicana* (olim *Geoplana mexicana* Hyman) from the United States (Turbellaria: Tricladida: Geoplanidae). *Proc. Pennsylvania Acad. Sci.* 64/1: 23–28.

1991. Land planarians from Ontario and Quebec, Canada. Trans. Am. Microsc. Soc. 110: 27–36.

Ogren, R. E., and M. Kawakatsu

1988. Index to the species of the family Rhynchodemidae (Turbellaria, Tricladida, Terricola). Part I: Rhynchodeminae. Bull. Fuji Women's Coll. Ser. 2, 26: 39–91.

1989. Op. Cit. Part II: Microplaninae. Ibid. 27, 53–111.

1990. Index to the species of the Geoplanidae (Turbellaria, Tricladida, Terricola). Part 1: Geoplaninae. 28, 79–166.

1991. Op. Cit. Part II: Caenoplaninae and Pelmatoplaninae. Ibid. 29: 25–102.

Pantin, C.F.A.

1950. Locomotion in British terrestrial nemertines and planarians with a discussion on the identity of *Rhynchodemus bilineatus* (Mecznikow) in Britain, and on the name *Fasciola terrestris* O.F. Müller. Proc. Linn. Soc., London 162/285: 207–218.

1953. On the name of the ground fluke *Fasciola terrestris* O.F. Müller, on *Othelosoma symondsi* Gray, and on the genus *Amblyplana* von Graff. J. Linn. Soc., Zool. 42: 207–218.

Riser, N. W., and P. M. Morse (eds.)

1974. *Biology of the Turbellaria*, Libbie H. Memorial Volume, New York: McGraw-Hill. [Proceedings of a Libbie H. Hyman Memorial Symposium, Division of Invertebrate Zoology, American Society of Zoologists, Chicago, December, 1970.]

Stunkard, H. W.

1970. Dr. Libbie Henrietta Hyman. Nature 225: 5230.

1974. In memoriam, Libbie Henrietta Hyman, 1888–1969. *In* N. W. Riser and M. P. Morse (eds.), Biology of the Turbellaria, Libbie H. Hyman memorial volume. New York: McGraw-Hill.

Ward, H. B. and G. C. Whipple

1918. Fresh-water biology. New York: Wiley.

Wenzel, R. L.

1990. G. Alan Solem 1931–1990. Field Mus. Nat. Hist. Bull. 61(3): 8–10.

Winston, J., and R. Fink

1991. Libbie Henrietta Hyman Exhibit. American Society of Zoologists, annual meeting program, December 27–30, 1991, GA, Atlanta. [This was a pictorial poster and biographical display prepared in association with the Hyman Symposium]

Youngpeter, J. M.

1969. Libbie Henrietta Hyman. Teacher workshop notes, Ward's Bull., 62(9): 7.

LIBBIE HENRIETTA HYMAN: HER INFLUENCE ON TEACHING AND RESEARCH IN INVERTEBRATE ZOOLOGY

M. PATRICIA MORSE

Fig. 25. LIBBIE HYMAN lecturing on invertebrate development.

Libbie Henrietta Hyman is truly an international name in zoology. Blackwelder (1970) noted "no other zoologist in modern times has made such an impression on both vertebrates and invertebrates." Hyman achieved this distinction by contributing in three major publication arenas: (1) scientific publications in peer-reviewed journals—about 140—with particular emphasis on physiology and taxonomy of invertebrates; (2) preparing laboratory manuals both in general zoology and in comparative vertebrate zoology; and (3) by writing the most comprehensive treatise of the vast invertebrate literature ever carried out by one person—her monumental six-volume series, *The Invertebrates*.

In her own words, quoted from her autobiographical memoir written with G. Evelyn Hutchinson, "Invertebrate Zoology was always from the start my preferred subject . . ." (1991) Her subsequent influence on research and teaching in the area of invertebrate zoology is vast, lasting from about 1925 on to the last volume published in 1962 and beyond. Anyone who has carried out research on the invertebrate phyla included in her publications or taught invertebrate zoology has individually experienced the works of Libbie Hyman. My experience with those

volumes ranges from undergraduate days of a thesis on the statocysts in invertebrates, to graduate school, to 26 years of teaching invertebrate zoology. It can undoubtedly be said that I never went through a year without referring to some illustration in those works. It was also my privilege, in conjunction with Drs. Nathan W. Riser and Horace W. Stunkard, to arrange a symposium on free-living flatworms, "Biology of the Turbellaria," for the American Society of Zoologists, to honor Libbie Hyman. It was at that meeting that I learned of the great respect which zoologists all over the world had for the accomplishments of Dr. Hyman and especially for her published papers on the phylum Platyhelminthes.

Libbie Hyman was a brilliant woman, who, against the odds put before most women of her time, succeeded in fording all rivers to become one of this country's preeminent zoologists. She has been a beacon to aspiring undergraduates and graduate students. She has served as a role model for young women, many of whom experienced few female mentors during their graduate years. Although her research papers were of great importance to specialists in the discipline, the six volumes of *The Invertebrates* are her greatest contribution to the field of invertebrate zoology.

As my part in this symposium celebrating the life and contributions of Libbie Hyman, let me highlight three aspects of her career: the production of the six volumes of *The Invertebrates* and their influence on teaching and research, her influence on Turbellaria research, and the appreciation among zoologists as noted during the Memorial Symposium on the Biology of Turbellaria in 1970. Finally, I wish to share with you her legacy from one person's view—namely mine.

In 1925, as a research assistant for Professor C. M. Child at the University of Chicago, Libbie Hyman found it necessary to put names on the various invertebrates that she and Dr. Child were using to study the physiology of the axial gradient. Thus, as her own research interests centered on flatworms—marine, freshwater, and land species, and on *Hydra*, she soon became the world's expert on the taxonomy and distribution of the North American species of those groups. It was also during this time at the University of Chicago that she developed the laboratory manuals, which, she found from her own experience, were needed to simplify assisting in both comparative anatomy and in general zoology. Publication of these scholarly manuals filled a national void and led to Hyman's financial independence.

Libbie Hyman set the tone for invertebrate zoology courses and for the publication of books on the subject. These volumes were eagerly sought by invertebrate zoologists at the time of their publication. In a review of volume 2 published in the Scientific Monthly in 1951, Professor Paul Illg wrote: "The second volume will reach an expectant public that has long eagerly awaited its appearance. The first volume has already taken its place as an indispensable reference work in the field." Dr. Illg, who subsequently taught invertebrate zoology to many of our colleagues with the ASZ, hosted Dr. Hyman when she visited the Friday Harbor Laboratories of the University of Washington. Let me further quote Professor Illg: "The author in her prefatory statement makes the important clarification that the contents of her volumes reflect a majority opinion of workers or the opinions of outstanding specialists in a given group, with her own stand on any matter, when expressed, explicitly stated as such. The consistency and lucidity of the presentation emphatically represent that the author has thorough mastery of the material. It is most unusually encountered and accordingly gratefully appreciated boon to workers having occasion to refer to such a treatise to find a positive stand crystallized out of the maze of published information in the field." There is no question that Hyman, who lived near the excellent library at the American Museum of Natural History, carefully considered the existing literature. Even more important, all of the illustrations in each volume were done by the author, either from her own observations on living organisms made during trips to marine laboratories in many countries, or as adaptations of existing drawings in the literature. Her concepts of phylogenetic relationships are important references to this day, and the uniformity of her pen facilitates the collective use of her books in

any marine laboratory invertebrate zoology course.

The format established in her volumes began to appear in major textbooks, notably, *Invertebrate Zoology* by Robert Barnes. Before Barnes's work appeared only Parker and Haswell's *Zoology* (rather lightweight), Borradaile, Eastman, Potts and Saunders (whose illustrations did not have an American orientation), and Meglitsch (this weighty text supposed you knew the whole vocabulary of the subject before you began) were available. Probably because of the familiarity of most faculty members with the Hyman books and because the illustrations included some American species, the Barnes volume became the book of choice for numerous classrooms. As time has gone on, other texts were published, often, as in the case of Pechenik, with an adaptation of less factual content (making them less overwhelming for the one-quarter invertebrate course often presented) and a new flair for bringing the current literature into students' hands; or Kozloff—who included so many new drawings of West Coast forms so familiar to Friday Harbor students, or new editions of Barnes, who revised his text on several occasions but with little change in format. All of these books are similar in concept, and share the Hyman approach, that is, to give a historical background of each phylum, followed by the characteristics of the phylum, the classification, morphology, physiology, development, and ecology, and to end with phylogenetic considerations. A bit of electron microscopy has crept in, but little has changed in basic format. Libbie Hyman believed there was a need for order—to further the knowledge and teaching of invertebrates. She provided it. It was not until the text of Brusca and Brusca (1990) that a distinct change in the format occurred. Here we find the concept of a Bauplan, as well as examples utilizing Hennigian phylogenetic analyses along with extensive discussions of these concepts.

Libbie Hyman wrote, "The treatise on the invertebrates has brought me much fame and many honors but has given the zoological public an exaggerated idea of my scientific abilities. The treatise is essentially a compilation from the literature. My assets are some fluency in translating the main European languages and an ability to select and organize material in the literature" (Hyman and Hutchinson, 1991). But Hutchinson goes on to say: ". . . it is a compilation by someone who had an extraordinary first-hand acquaintance with her materials . . . Actually, although she was technically correct, her enormous knowledge did make it the result of the workings of her own mind" (Hyman and Hutchinson, 1991).

It has often been said that the final volume was not up to the standards of the earlier volumes. The earlier volumes covered groups in which Hyman herself had contributed to the original literature, and her health had also deteriorated in the years before the final publication. However, the molluscan groups covered still gave the reader a compilation from the literature that has furthered our understanding of those groups, especially the chitons and the aplacophorans.

In the summer of 1969, at the meeting of the American Society of Zoologists in Burlington, Vermont, Dr. John Corliss convinced me that a symposium in honor of Dr. Libbie H. Hyman would be a significant tribute to this leading zoologist. This was three years after the establishment of the Northeastern University Marine Science Center in Nahant, and my interest in this woman's contributions and my own use of the Hyman volumes led me to take up the challenge. I was in contact with Dr. Horace Stunkard in Woods Hole—as a native of the village and having spent summers there while an undergraduate at Bates, I was "the brat down the street" to Stunkard. However, to my great surprise and delight, he agreed to be on the organizing committee and was very helpful in identifying possible participants. In addition, a colleague at Northeastern University, Professor Nathan W. Riser, who was the first director of the Nahant Marine Laboratory, also known for his work on cestodes and as an invertebrate zoology teacher, agreed to help and in particular to share in editing the symposium results. Although we by no means were able to enlist all the participants we might have, our results in getting together an international group of turbellarian specialists, representing various areas of the biology of the Turbellaria, was impressive. There are

Fig. 26. LIBBIE HYMAN receiving the Gold Medal of the American Museum of Natural History at its Centennial Banquet in April, 1969, shortly before her death. During her last years she was wheelchair-bound and dependent on nursing care, and, with the publication of Volume VI (1967), had to give up at last on her uncompleted task.

some interesting remembrances resulting from the symposium.

Perhaps the most important was the high regard her colleagues, especially the European workers (of which there were a large number in attendance), felt for Libbie Hyman. They spoke of her papers on the North American Turbellaria and how she had led the field in turbellarian taxonomy. They told endless stories about meeting her, including the circumstances and the impressions she had made on them. Dr. Eveline Marcus (a European-born biologist from the University of São Paulo) was one of the most interesting participants. Her husband, Professor Ernst Marcus, had died shortly before, and this was her first trip to the United States. She was one of the most sought-after participants in the symposium because of the many groups of invertebrates that she and her husband had investigated. She told how Libbie Hyman brought literature on flatworms to them in Brazil, a most precious gift. They, in turn, introduced her to interstitial meiofaunal organisms, a field just beginning to develop when she visited. The team of Marcus and Marcus had written many key papers on the Brazilian interstitial fauna. Dr. Joseph B. Jennings from Leeds, a leader in the physiology of digestion in Turbellaria, told of visiting her at the American Museum of Natural History. In awe of this woman, one of three to have received the Linnaean So-

ciety Medal from his country, he mentioned this to her. Her response was to pull open her left desk drawer, filled with stamps from all over the world, and rummage round until she pulled out the Linnaean gold medal with the comment, "Yes, this was indeed an impressive award; you know, this is real gold!"

Another important event of the symposium was the mixing of established workers such as Dr. Tor Karling from Sweden, Dr. J. B. Reynoldson from Wales, and Dr. Roman Kenk from the U. S. Museum of Natural History with the younger generation of investigators. One who may well be here, Reinhard Reiger, along with Dr. Sterrer and Antonius, we nicknamed the Austrian Mafia. Dr. Reiger went on to establish an important group of workers that broadened the field to include the use of electron microscopy and asks important phylogenetic questions utilizing ultrastructural evidence. In our own laboratory at Nahant we began studies on meiofaunal organisms—Riser on turbellarians, annelids, and nemerteans, and I on interstitial molluscs, hydroids, and priapulids. The National Science Foundation supported this symposium and McGraw Hill Book Company published the proceedings. Although they called it part of their "series on invertebrates," there was never any question in Riser's or my mind that this was a separate book, dedicated by many authors to the author of the series of six volumes on the invertebrates, Libbie Henrietta Hyman.

What is the legacy? Libbie Hyman, more than any other zoologist in the United States, established order and set high standards for studies of invertebrates with her monumental series. She left countless numbers of students, who utilize the volumes when searching for what is known of the groups she covered. She showed that one person could assimilate vast quantities of material in many languages, then use a critical and judicious approach toward development themes of new ideas.

REFERENCES

Blackwelder, R. E.
1970. Libbie Henrietta Hyman memorial: her life. J. Biol. Psych. 12(1): 4–12.

Hyman, L. H., and G. E. Hutchinson
1991. Libbie Henrietta Hyman, December 6, 1988–August 3, 1969. Biogr. Mem. Nat. Acad. Sci. 60: 103–114.

Illg, Paul L.
1951. The invertebrates: Platyhelminthes and Rhynchocoela. The acoelomate Bilateria, Vol. II. Rev. Scientific Monthly 72(5): 336–337.

Riser, N. W., and M. P. Morse (Eds.)
1974. Biology of the Turbellaria, (Libbie H. Hyman memorial volume). New York: McGraw-Hill, 530 pp.

SYSTEMATICS OF THE FLATWORMS—LIBBIE HYMAN'S INFLUENCE ON CURRENT VIEWS OF THE PLATYHELMINTHES

SETH TYLER

INTRODUCTION—HYMAN'S VIEW OF THE FLATWORMS

Libbie Hyman made her mark with her extraordinarily comprehensive view of the animal kingdom. She covered comparative morphology with encyclopedic breadth and explained that breadth in eminently readable style with her laboratory manual, *Comparative Vertebrate Anatomy,* and her multi-volume treatise, *The Invertebrates.* Her first love among the animals, however—the group to which she devoted over 60 research papers—was the free-living flatworms, the Turbellaria (fig. 27): small ciliated, soft-bodied worms that, despite their delicacy, range from aquatic habitats in the marine and freshwater environments to humid terrestrial habitats. She used her view of the comparative anatomy of these animals to expand knowledge of their diversity.

Hyman's papers on flatworms deal mainly with alpha taxonomy of turbellarians—that is, descriptions of species; and there is now a wealth of species that bear her name as

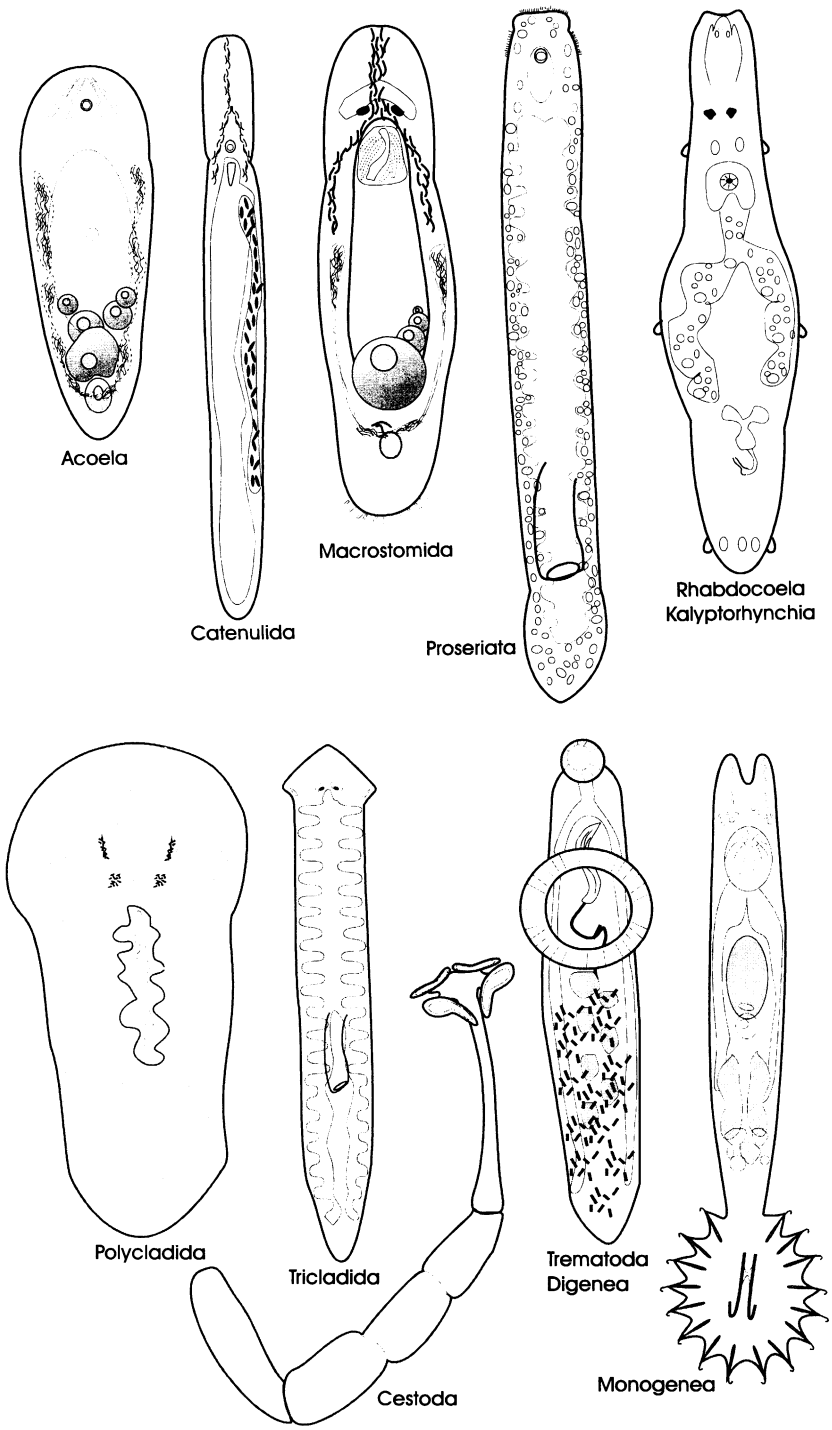

Fig. 27. Sketches of flatworms representing some of the major taxa in the phylum (Platyhelminthes). Libbie Hyman worked mostly with members of the Tricladida and Polycladida, the macroturbellarians. The major parasitic taxa are represented here with Cestoda, Digenea, and Monogenea. All the rest of the taxa depicted are smaller free-living turbellarians or microturbellarians.

authority: 116 polyclads, 80 triclads, 11 rhabdocoels, 3 proseriates, 3 acoels, and 1 each of prolecithophorans and lecithophorans. The highest taxonomic category that she dealt with was the family, having established four families, one of which (the polyclad family Callioplanidae Hyman, 1953) is still recognized, the other three now relegated to subfamily status. She established 32 new genera.

Despite this concentration on the lower levels of taxonomy, she had a profound influence on higher classification of the flatworms. Her treatise on the phylum—Volume II of *The Invertebrates* series (Hyman, 1951)—was so clear, so well-organized and concise, that the taxonomic system she put forth there was seen as her system and carried weight for at least 35 years after its publication date; it still appears in many recent introductory zoology and invertebrate-biology textbooks. Even the name of the phylum bears her indomitable stamp. Of the nine names proposed for the phylum since Fogt (1851) first suggested "Platyelmia," Hyman (1951) argued forcefully for the one that was etymologically correct, namely "Platyhelminthes" proposed by Claus (1887). Europeans have more frequently adopted the spelling that dropped the Y, i.e., Schneider's (1873) "Plathelminthes," a spelling that has priority by 14 years; but publications subsequent to Hyman's 1951 treatise, especially since more and more of them were in English, seemed to largely accept her arguments for "Platyhelminthes." Ehlers' new system (Ehlers, 1984, 1985) challenged that, not only with the point that "Plathelminthes" had priority but that rules of nomenclature do not necessarily bear on etymology. English-speaking authorities, citing Hyman *inter alia*, have balked at this, however, and it appears that the latest consensus would be to allow either spelling (Ehlers and Sopott-Ehlers, 1995). Hyman's authoritative voice continues to ring clear.

The system Hyman used (fig. 28) was that published by Bresslau (1928–33). In an aside, Hyman (1951) mentioned Meixner's (1938) scheme and noted that its logic was acceptable, but she opted for following the more conservative system of Bresslau, presumably because it is easier to understand and more practical. Hyman's and Bresslau's system categorizes the flatworms into three major groups (classes), the Turbellaria, the Trematoda, and the Cestoda. Within the Turbellaria, the major subdivisions, the orders, are distinguished largely by the gross shape of the digestive system: Acoela lacking an epithelially lined digestive cavity, Rhabdocoela having a saclike cavity, Tricladida having a three-branched cavity, Polycladida a multi-branched cavity, etc. This subdivision is more a matter of convenience, and I doubt that Hyman necessarily attached phylogenetic significance to it outside of her idea that the Acoela is a primitive group showing ties in its gut morphology with a planula-like ancestor.

Hyman never published a phylogenetic tree of flatworm taxa, but she did describe, at the end of her section on the Platyhelminthes (Hyman, 1951: 419–420), an outline of relationships that can be graphically represented as a tree (fig. 29A). She draws direct connections from the Acoela to the "Alloeocoela" and the Tricladida, but is less certain about the link to the Rhabdocoela. From the Rhabdocoela, she derives all the major parasitic groups (flukes, tapeworms, etc.), and specifically points to dalyellioid rhabdocoels as the ancestral form, not by way of a specific origin but through a vaguely defined polytomous branching to the various separate parasitic subgroups. If this tree were to be reconstructed in a form more acceptable to present-day phylogenetic systematists, i.e., with the more narrowly defined dichotomous branching of cladistics, it might show a tree like that of figure 28B. The link between dalyellioids and the parasitic classes, in Hyman's view, was evident especially in the strong similarity of their reproductive organs.

CLADISTIC SYSTEMS FOR THE PLATYHELMINTHES

The first cladistic analysis of the Turbellaria (fig. 30) was that of Karling (1974) and was presented at the first symposium honoring Libbie Hyman (see Riser and Morse, 1974); it remains the backbone of all subsequent platyhelminth systems. As Karling (1974) pointed out, cladistic analysis shows the difficulty in identifying characters that

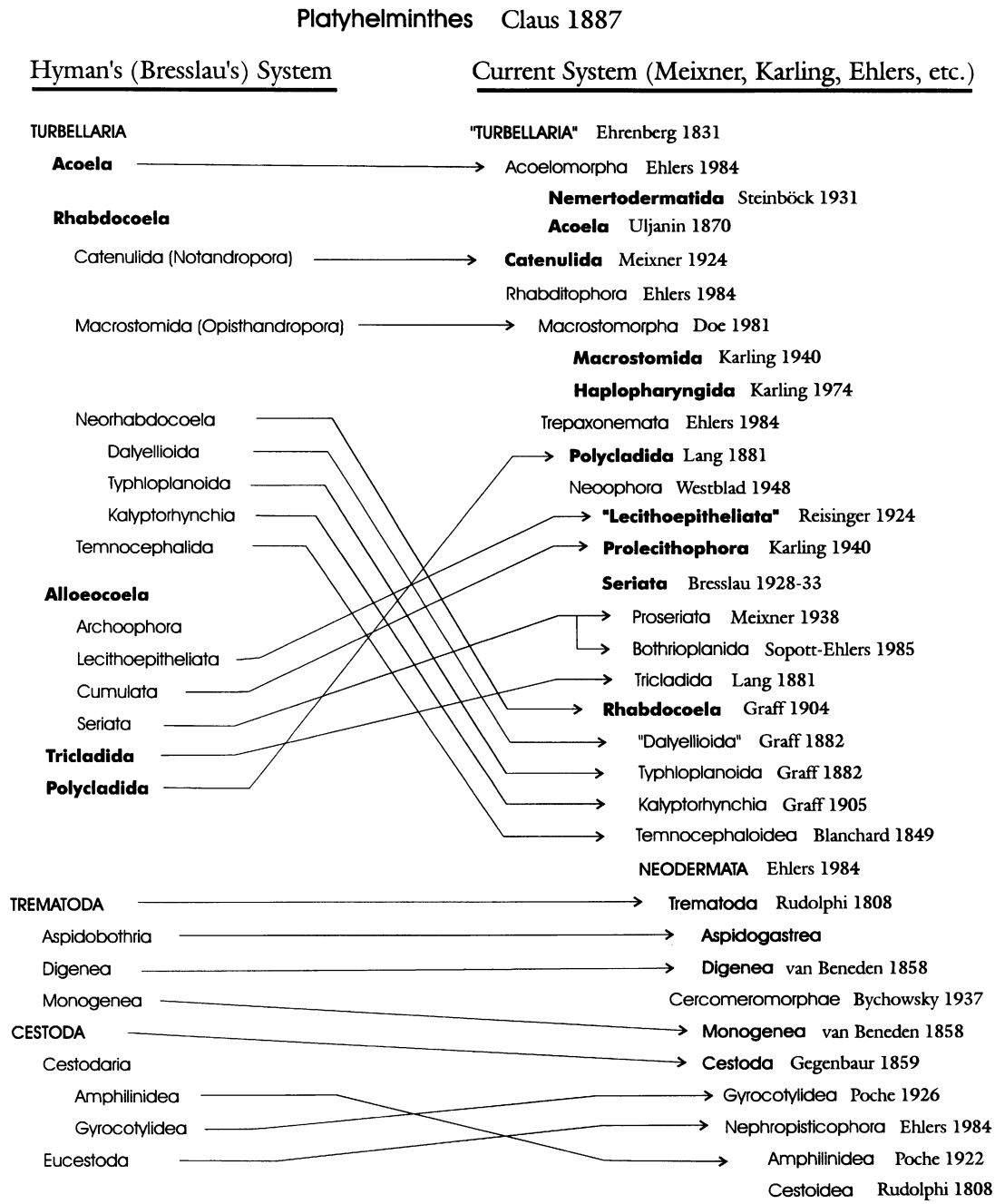

Fig. 28. Classification of the Platyhelminthes according to Hyman (1951) and, by comparison, a cladistic system of flatworm groups reflecting only those phylogenetic relationships that are now relatively well substantiated. Taxa at the rank of class in the Linnean hierarchy are printed in all capital letters; orders are in boldface. Hyman's system has 3 classes and 4 turbellarian orders; the current system recognizes 2 classes and 10 orders. Taxa of uncertain monophyly are placed in quotation marks. (Current system arrived at in consultation with Reinhard Rieger [see also Rieger et al. 1991].)

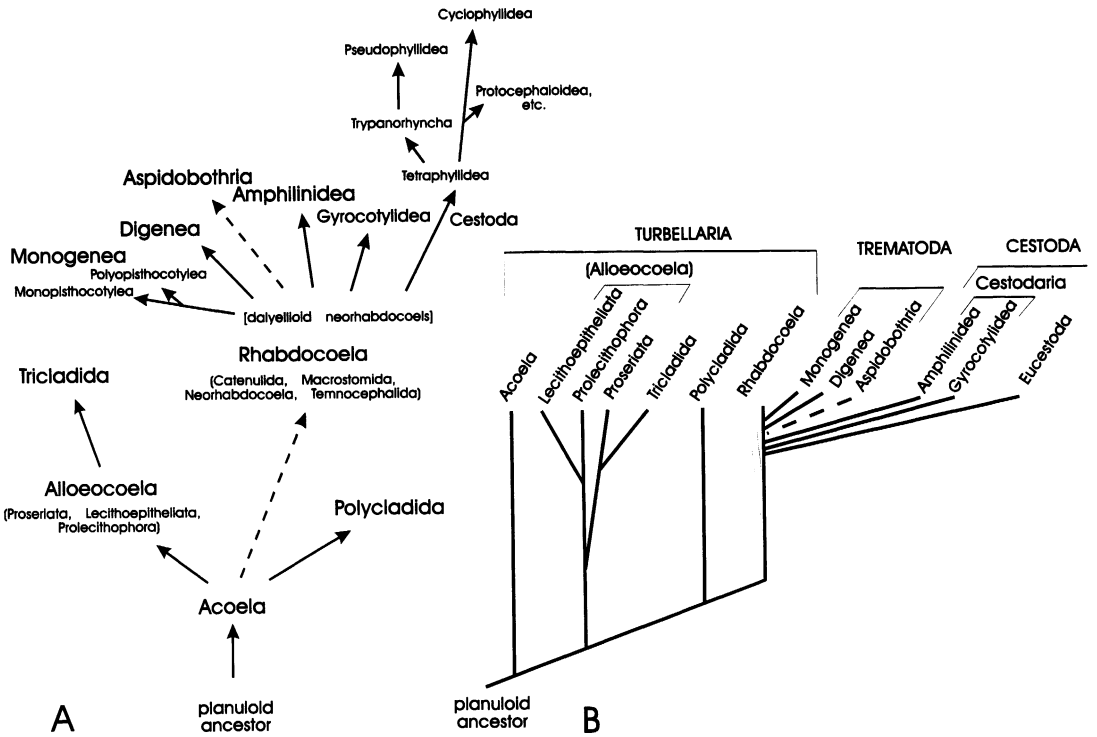

Fig. 29. A) Treelike representation of the evolutionary relationships of flatworm groups as Hyman described them (Hyman 1951: 419–422). B) Interpretative cladogram of the relationships depicted in fig. 28A for comparison with current cladograms of the Platyhelminthes.

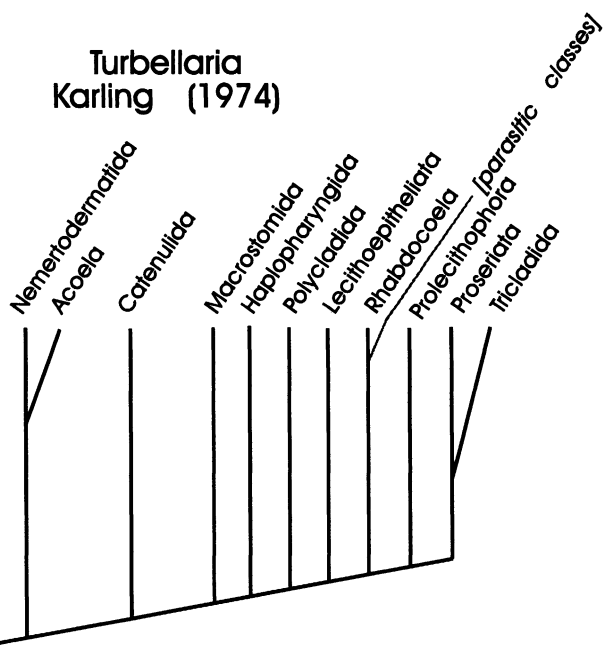

Fig. 30. Karling's (1974) cladogram for turbellarian orders. (After Karling, 1974.)

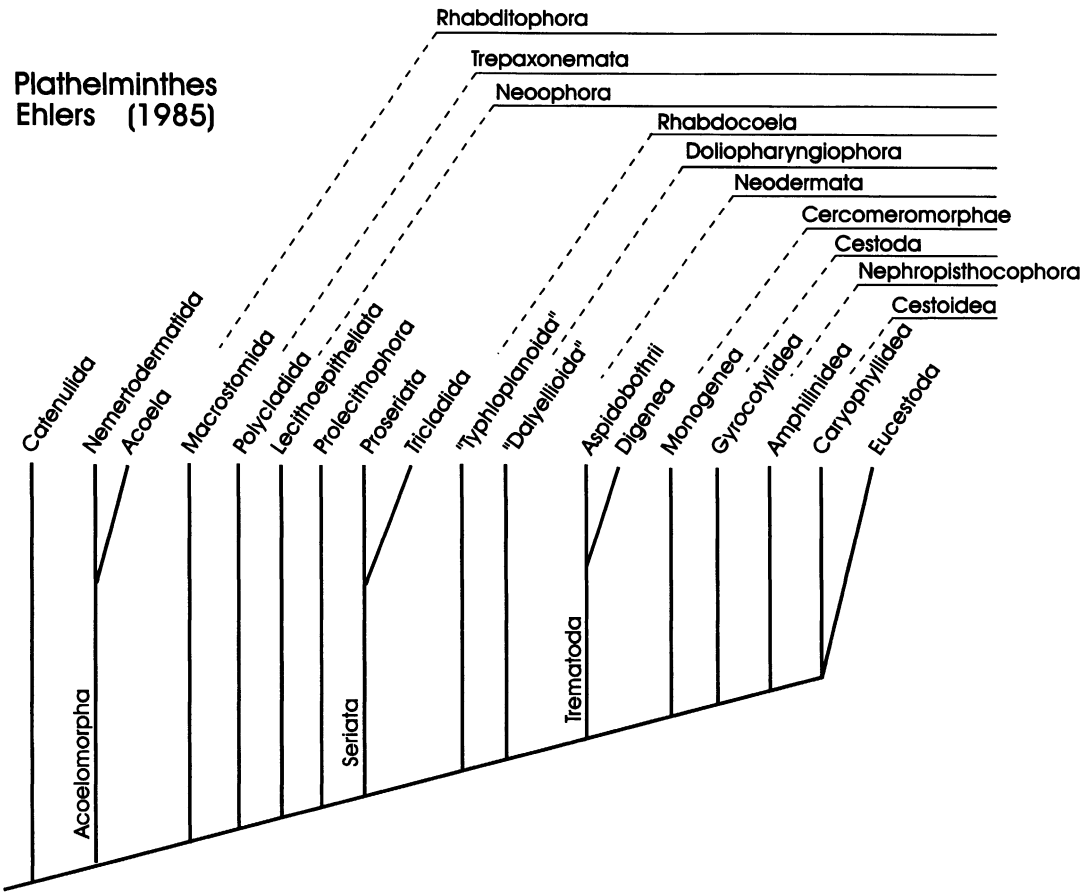

Fig. 31. Ehler's (1984) cladogram for the phylum Platyhelminthes (or, as he calls it, Plathelminthes). [Modified, from Ehlers, 1985]

unite the flatworms into a well-defined phylum or that can show relationships among the orders, which otherwise simply stand as monophyletic but vaguely related taxa. In other words, the class Turbellaria lacks synapomorphies and, by virtue of the evident descent of the parasitic classes from a rhabdocoel-like ancestor (something Hyman and Bresslau recognized), the taxon Turbellaria is invalidated by the standards of cladistics: it is a paraphyletic taxon.

Ehlers (1984, 1985) formalized the latter conclusion with a strictly cladistic system (fig. 30) that abandons the term "Turbellaria." His system also tries, by a careful cataloguing of many characters that have been discovered since Hyman's time, to show firmer phylogenetic links between the major taxa. The main impetus for this system comes from characters discerned by electron microscopy, starting with especially significant characters discovered by Reinhard Rieger's laboratory in the early 1970s through 1980s and further expanded by Ehlers' laboratory (see citations in Ehlers, 1985; Rieger et al., 1991). As bold as Ehler's system is, it is fair to say that the relationships he outlines for the turbellarian platyhelminths remain only weakly supported; at best only three monophyletic groups can be identified in the Turbellaria (see Rieger, 1981; Smith et al., 1986): the Catenulida, the Acoelomorpha (Acoela plus Nemertodermatida), and the Rhabditophora (all remaining orders, united by their possession of rhabdites, uniquely turbellarian glandular secretions). Beyond this, relationships among the orders within the Rhabditophora remain problematic, and

it is impossible to even verify that the Platyhelminthes is a monophyletic phylum. Julian Smith (Smith et al., 1986) phrased the question, "Are the Turbellaria monophyletic?" (Hyman, too, would have worded it "Are the Turbellaria . . . ," but by editorial prerogative I changed that to "Is the Turbellaria . . . " to reflect that "Turbellaria" is a singular entity, a taxonomic unit) and first thoroughly analyzed the relationships among Acoelomorpha, Catenulida, and Rhabditophora. Actually, we don't dispute that the Turbellaria/Platyhelminthes is monophyletic; it is simply that the cladists' need to identify exact connections among the three clearly monophyletic groups is not supported by well-established homologies. Even more recent tests of relationship among platyhelminths with molecular data do not solve the question of monophyly; some studies do statistially corroborate the prospect of monophyly (Rohde et al., 1993, 1996; Joffe et al., 1995; Litvaitis and Rohde, 1999) while others place the Acoela outside the Platyhelminthes (Hasz-prunar, 1996; Zrzavy et al., 1998; Ruiz-Trillo et al., 1999). In those studies supporting non-monophyly, the Acoela is problematic because only information from a single gene, the 18S rDNA gene, was applied, and acoel taxa appear to be quite long-branching in phylogenetic reconstructions with this gene.

Besides its abandonment of the class Turbellaria, Ehlers' (1985) system is most noteworthy for its impact on taxonomy of the major parasitic platyhelminth groups. Ehlers subsumes these taxa in the group Neodermata, so-called because the adults develop a "new skin" in their metamorphosis from the larva (the larva sheds its epidermis when contacting a host and the next stage in the life cycle develops a syncytial epidermis from cells that reach through the body-wall musculature from the underlying parenchyma). Like Hyman, Ehlers (1985) derived the parasitic taxa—the Neodermata—from a dalyellioid-like ancestor. While he viewed the Dalyellioida as a paraphyletic group, one lacking any defining autapomorphy, he placed taxa with a doliiform pharynx, including dalyellioids and Neodermata, together in a monophyletic taxon, the Doliopharyngiophora. (Hyman [1951] may have meant the same thing when she cited similarities in "digestive system" between the parasitic taxa and dalyellioids.)

Parasitologists applying cladistic methodology to these taxa have proposed competing schemes. One such scheme, appearing about the same time as Ehlers', is that of Brooks (1982, 1989; see fig. 32). He adopted the name "Cercomeridea" at that level where Ehlers would have Neodermata, emphasizing a curious tail-like appendage called the cercomer in the larvae of many parasites; and he brings one turbellarian taxon, the Temnocephalida, under the umbrella of cercomer-possessing platyhelminths. (Hyman [1951] herself acknowledged but dismissed an older taxonomic scheme that similarly used the cercomer to relate the parasitic groups.) In other respects, Brooks' (1989) scheme is similar to Ehlers' (1985) but adopts different names for some of its newly proposed taxa.

Another scheme concentrating on the parasitic platyhelminths is that of Rohde (1990; see fig. 33). He chose yet another point of origin for the major parasitic taxa from a turbellarian-like ancestor, namely a line encompassing turbellarians classified as Fecampiidae. Fecampiids are highly simplified parasites in the body cavities of crustaceans as adults, and their free-swimming larvae resemble rhabdocoel turbellarians, among which they are traditionally classified. Rohde and co-workers (Rohde, 1990; Watson et al., 1992a, 1992b) found evidence that features of the epidermal ciliation, protonephridia, photoreceptors, and sperm are less like those of rhabdocoels than monogeneans and other neodermatans (though their lack of a neo-

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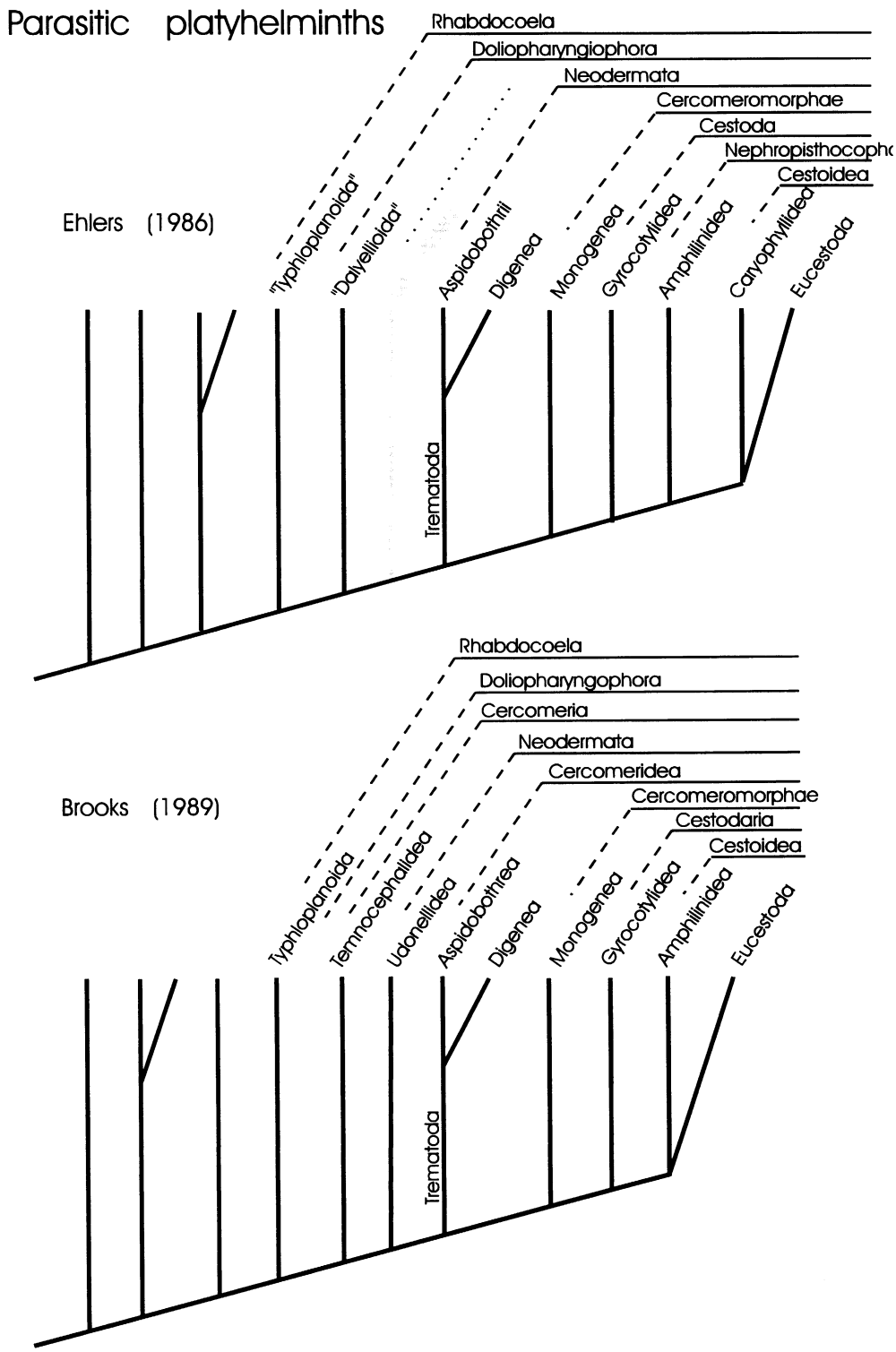
Fig. 32. Comparison of Ehlers' (1984) and Brooks' (1989) cladograms for the major groups of parasitic platyhelminths. The major distinctions lie in Brooks' choice of the Temnocephalidea as the sister group to the Neodermata and his choice of higher-grouping names reflecting his emphasis on the tail-like appendage, the cercomer. Ehlers' provisional placement of the Udonellida is indicated in shadowed font.


Parasitic platyhelminths
Ehlers (1986)
Rhabdocoela
Doliopharyngiophora
Neodermata
Cercomeromorphae
Cestoda
Nephropisthocoph
Cestoidea
"Typhloplanoida"
"Dalyelliida"
Aspidobothrii
Digenea
Monogenea
Gyrocotylidea
Amphilinidea
Caryophyllidea
Eucestoda
Trematoda
Brooks (1989)
Rhabdocoela
Doliopharyngophora
Cercomeria
Neodermata
Cercomeridea
Cercomeromorphae
Cestodaria
Cestoidea
Typhloplanoida
Temnocephalidea
Udonellidea
Aspidobothrea
Digenea
Monogenea
Gyrocotylidea
Amphilinidea
Eucestoda
Trematoda

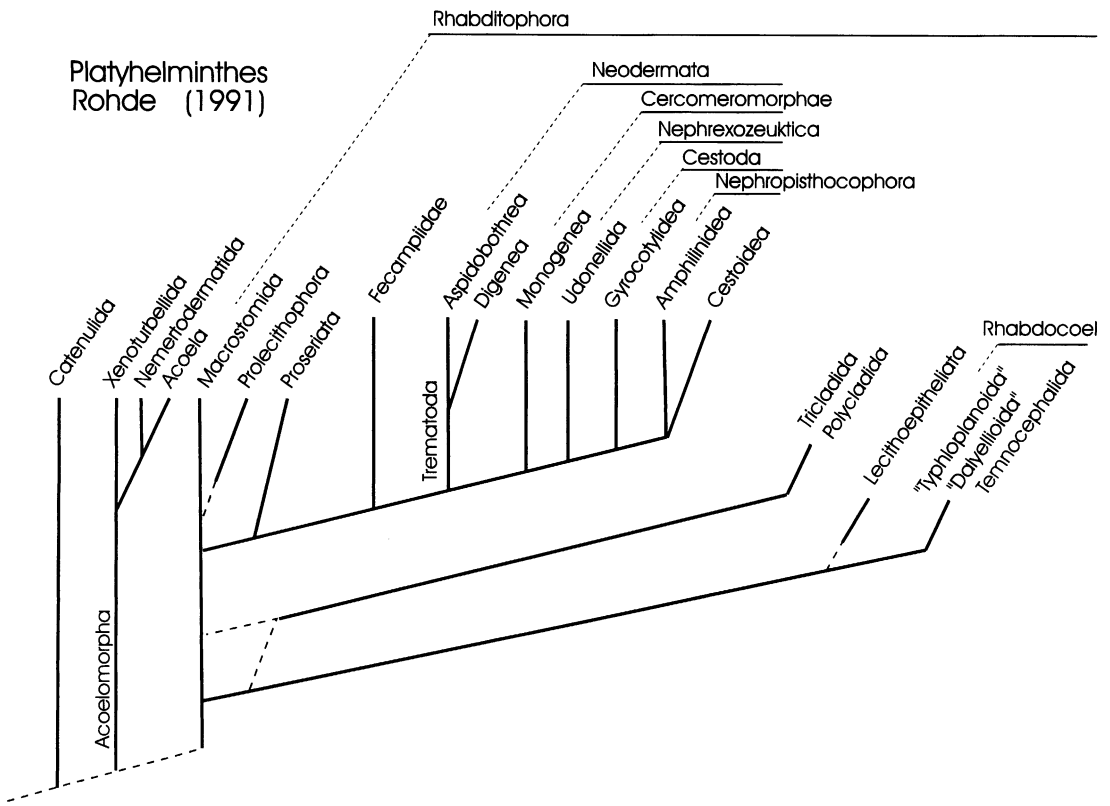


Fig. 33. Rohde's (1990) cladogram of the Platyhelminthes (modified).

dermis leaves them firmly among the turbellarians). Moreover, Rohde would conditionally place fecampiids closer to proseriate turbellarians than to the rhabdocoels. From the fecampiid-like ancestor, Rohde (1990) proposed a phylogenetic classification close to that of Ehlers (1985), with the technical exception of the Udonellida, a group of parasites found on parasitic copepods. (Rohde followed Ivanov [1952] in placing Udonellida as a major taxon within the Neodermata while Ehlers [1985] classified them outside the Neodermata close to the rhabdocoels.) Litvaitis and Rohde (1999) found support for the Fecampiida being the sister group of the Neodermata in sequence data from the 28S rDNA gene, but Rohde showed elsewhere that sequence data from the 18S rDNA gene support the idea that that sister group should be a branch of the Turbellaria comprising Proseriata + Rhabdocoela + Tricladida (see Rohde, 1994) or, essentially, all other neoophorans (Littlewood et al., 1999).

OTHER SCHEMES OF PLATYHELMINTH RELATIONSHIPS

Without formally proposing changes in the classification of platyhelminths, others have presented models for the evolution of Neodermata from turbellarian-like ancestors. Cannon (1986) postulated four separate origins of parasitic groups from different families of the Dalyellioida based on evidence in such trends as host infection site and symbiosis type. Malmberg (1974, 1986) viewed the evolution of platyhelminths as steps along a line of attaining specific organ systems or so-called evolutionary capacities (fig. 34). For example, Malmberg considered absence of an intestine to be a fundamental primitive feature that showed cestodes to be derived from acoel turbellarians. Logachev and Sokolova (1975) adopted a similar view. Stunkard (1975), on the basis of characters of life histories and postulations of how cestodes and digeneans acquired two hosts in

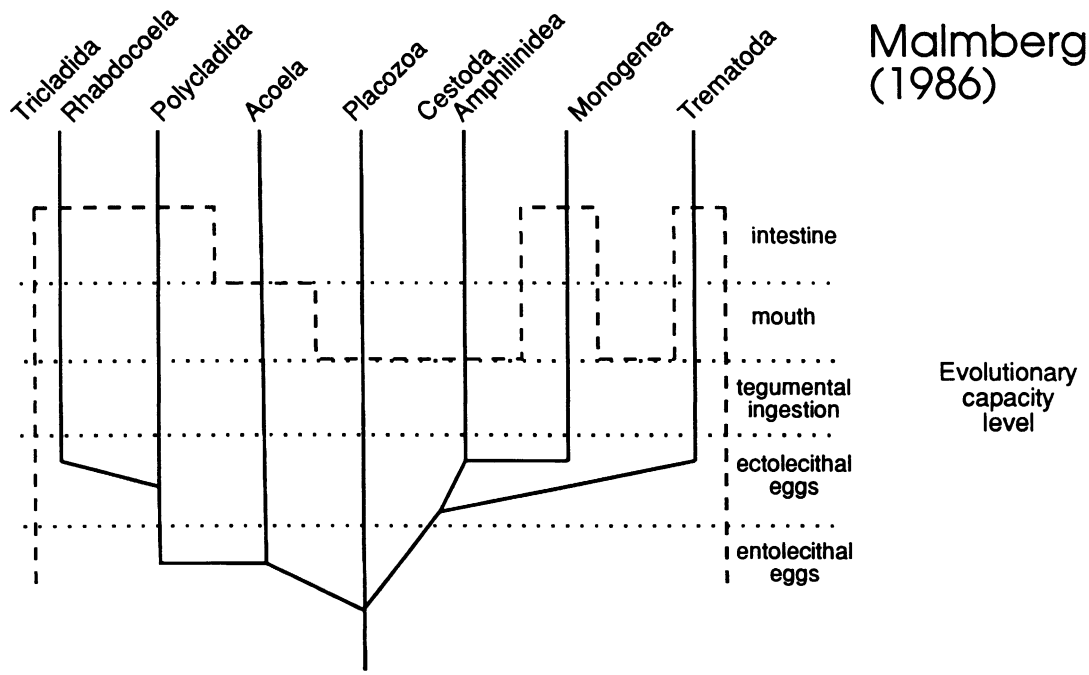


Fig. 34. Scheme of platyhelminth evolutionary relationships proposed by Malmberg (1986).

the life cycle, proposed that the Digenea and a group encompassing monogeans + cestodes had separate origins (see also Llewellyn [1986] for similar conclusion). None of these models of evolution entail formal taxonomic revisions.

TESTS OF THE PHYLOGENETIC HYPOTHESES

Newer data, including characters discerned by electron microscopy and characters from nucleic-acid sequences, have been used to test these hypotheses of relationships, including Hyman's. Hyman's broad-brush view of relationships among the Tricladida, Polycladida, and Rhabdocoela (i.e., Neorhabdocoela) finds support in ultrastructural characters (Rieger et al., 1991) and even the Acoela can be related in this broad scheme with nucleic-acid-sequence data (Joffe et al., 1995). These newer characters have certainly clarified the nature of taxa she recognized as "Rhabdocoela" and "Alloeocoela" (see below and fig. 28).

Hyman's view of the parasitic classes as having a common origin among a specific group of turbellarian ancestors is borne out by both ultrastructural and molecular data, and, more specifically, the Neodermata appears to be monophyletic. Ehlers (1985) presented a comprehensive and thorough review of the ultrastructural characters supporting this notion, and Rohde's (1990, 1991; Rohde et al., 1995) addition of ultrastructural characters from his study of protonephridia corroborate the monophyly. Nucleic-acid sequence data also statistically corroborate monophyly (Rohde et al., 1995; Joffe et al., 1995; Litvaitis and Rohde, 1999; Littlewood et al., 1999); but, despite hope that such data could resolve the question of where on the tree of other platyhelminths the neodermates may be derived, no such resolution is yet possible.

Origin of the Neodermata from a dalyellioid-like ancestor, as Hyman and Ehlers have it in their phylogenies, has been challenged first of all from characters of the pharynx: Joffe and co-workers (Joffe, 1987; Kotikova and Joffe, 1988) used detailed histological characterization of the pharynges of trematodes and monogeneans to show that they were less like the doliiform pharynx of dalyellioid rhabdocoels than the rosulate

pharynx of typhloplanoid rhabdocoels. Ultrastructural features of protonephridia also speak against a dalyellioid origin of neodermates according to Rohde (1990, 1991), forming the basis of his proposal for an origin lower on the platyhelminth tree than that of the rhabdocoels. And, finally, the most recent molecular data from both 5S rRNA (Joffe et al., 1995) and 18S rDNA (Riutort et al., 1993; Rohde et al., 1994, 1995; Zrzavy et al., 1998; Littlewood et al., 1999) show that dalyellioids are a less likely sister group to the Neodermata than some other turbellarian group. What the sister group to the Neodermata could be still remains unsolved. The 18S rDNA data contradict even an origin from other rhabdocoels and also rule out the temnocephalids, which by these data (as well as by ultrastructural data [see Williams, 1986]) are to be classified as rhabdocoel. Such evidence speaks against Brooks' (1989) scheme positing temnocephalids as the sister group of the Neodermata. The uncertain position of the Neodermata in these molecular phylogenies may be a result of a relatively low rate of base substitution in these parasitic flatworms relative to that of other platyhelminths (Riutort et al., 1993; Joffe et al., 1995).

The various hypotheses of phylogenetic relationships of major taxa in the Platyhelminthes are discussed further by Littlewood et al. (1999).

NONCLADISTIC SYSTEMS OF THE PLATYHELMINTHES

Although most of the newer proposals for a system of the Platyhelminthes are derived from cladograms, a cladistic (phylogenetic) system is not necessarily the final word. Haszprunar (1986), Ivanov (1991), Mamkaev (1991, 1995), and Cavalier-Smith (1998), in particular, have discounted the need for a strictly cladistic classification, citing the practical problems of constructing such a system. The theoretical need for dichotomous branching in lines of descent, for instance, can make cladistic systems unwieldy and problematic. Cladistics' strict avoidance of paraphyletic groupings means it must sometimes forego the advantage of having distinctly recognizable taxonomic groupings, the Turbellaria among them. Mamkaev (1991, 1995), offered an "evolutionary morphological" basis for classification, one that centers on formulation of morphological types to define taxonomic groups at all levels of the hierarchy. The classification, he says, is to be founded on analysis of morphological transformations in phylogenetic lines. Thus distinct morphologies can be recognized for higher taxonomic groups. Mamkaev's evolutionary morphological approach is akin to arguments for so-called evolutionary classification from Mayr (1974). Haszprunar (1986) married cladistics with the evolutionary approach in his "clado-evolutionary" approach to classification. This approach, exemplified in his system for the Plathelminthes (Haszprunar, 1986: 99) starts with a cladogram and constructs from it a system with further consideration of degree of divergence of the cladogram's branches as well as so-called practicability and compatibility with traditional systems. A paraphyletic taxon such as the Turbellaria is considered acceptable as long as it has only one lineage emerging from it (in this case, the Neodermata), and taxa with questionable relationships are designated as such by adding "sedis mutabilis" and by enclosing the taxon name in quotation marks. The clado-evolutionary system preserves Linnean categories and uses them to indicate degree, as well as order, of divergence. In other words, Hyman's system is not so quickly to be abandoned.

THE CURRENT STATUS OF THE PLATYHELMINTH SYSTEM

With these conflicting views of platyhelminth evolution and taxonomic methodology (as well as points of contention, not mentioned, about systematics of lower levels of the hierarchy), we have to admit that there is no universally accepted current system for the Platyhelminthes. Still, a system that has the better-supported groupings can be constructed, leaving aside for the moment certain problematic taxa. My own view of what that system would be (fig. 28, right side) is built on the framework that Hyman championed. By no means has her system been supplanted by newer cladistic or any other

systems, rather it has been incrementally improved with discovery of new characters of ultrastructure and molecular biology and with application of cladistic paradigms for defining evolutionary relationships as precisely as possible.

The most significant post-Hyman features of the current system are (1) reorganization of the Rhabdocoela and Alloeocoela into more discretely defined orders, (2) grouping of several orders into supra-ordinal taxa, and (3) arranging the taxa in a better-defined phylogenetic order. For the Rhabdocoela, reorganization has involved elevating most of the suborders Hyman had distinguished within it to ordinal status and, more importantly, better delineating their evolutionary relationships. The Rhabdocoela is now more narrowly defined as what Hyman designated the Neorhabdocoela (plus the Temnocephalida), and the other suborders are ranked as orders. As distinctive as are the temnocephalids—which are polyplike, tentacle-bearing commensals of freshwater crustaceans—both ultrastructural (Williams, 1986) and molecular characters (Rohde et al., 1994) showed that they fit within the present-day concept of the Rhabdocoela (systems presented since Hyman's had elevated this group to ordinal status, distinct from Rhabdocoela). Removal of the Catenulida from the Rhabdocoela is particularly important; that order now stands as one of three major clades of the Turbellaria, perhaps, as some have argued (Ehlers, 1985; Rohde, 1990; see also Karling, 1974; Smith et al., 1986), the sister group to all the remaining turbellarians (although Hyman's view of the Acoela occupying this position is still tenable). For the Alloeocoela, reorganization is more drastic: the largest group within it, the Seriata (or more precisely, the Proseriata), is now seen to be the sister group of a clade with the Tricladida (see Sopott-Ehlers, 1985), so the two are combined as suborders in a new order with that name (though Rohde and coworkers [see Rohde et al., 1995; Littlewood et al., 1999] found evidence in both ultrastructure of protonephridia and 18S rDNA data against this sister-group relationship). Hyman recognized the link between "seriates" and triclads herself—even pointing out the intermediate nature of *Bothrioplana*, now positioned in the Tricladida clade as sister group to the Tricladida (see Sopott-Ehlers, 1985). The other two main subgroups of Hyman's Alloeocoela, the Prolecithophora and Lecithoepitheliata, remain problematic, but they are now recognized as less likely to be related, neither being closely allied with the seriates.

Grouping of the Acoela and Nemertodermatida (which Hyman had subsumed in the Acoela as a family but it was subsequently raised to ordinal status) into a superorder Acoelomorpha is based on ultrastructural features, particularly of the body wall and ciliation (Tyler and Rieger, 1977). Similarly, ultrastructural features justify the superorder Macrostomorpha. The major clade encompassing the Macrostomorpha and all remaining platyhelminths, namely the Rhabditophora, is defined by ultrastructural identification of a characteristic glandular secretion, the rhabdite (Smith et al., 1982). The relationships of certain members of the Rhabditophora do remain problematic, as mentioned above, however; lack of a homolog of a rhabdite-producing cell in Prolecithophora and Lecithoepitheliata is an especially weak part of the platyhelminth tree. The clade Trepaxonemata is another major group defined on the basis of ultrastructural characters—in this case of the sperm, which are characteristically biflagellate with a distinctive 9 + 1 axonome. The Neoophora, which unites all those platyhelminths having the female gonad divided into a yolk-producing vitellarium and an oocyte-producing ovary, has been recognizable for these characters by light microscopy since Hyman's time (Karling, 1940). Finally, the major parasitic groups of platyhelminths are now seen as a single clade, the Neodermata, and that clade, by traditional ranking, would be a class.

The taxonomy of the Platyhelminthes continues in a state of flux, and reexamination of especially the higher-level taxonomic groupings has proceeded apace recently (see, especially, Littlewood et al., 1999). Revision of this taxonomy will doubtless come from discovery of new characters, particularly among ultrastructural features and molecular data. Currently available molecular (DNA-sequence) data contradict some of the conclusions mentioned above but at quite low levels of confidence, and further analysis of

these characters, as well as corroboration with more molecular data, is needed before they can be used to revise the taxonomy. Undoubtedly the system for the Platyhelminthes will continue to be refined, but the basic framework, so eloquently championed by Hyman (1951) and now bearing the logistic backbone of the cladistic system that Karling (1974) pioneered, is well established.

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REFERENCES

Bresslau, E., and E. Reisinger
1928. Allgemeine Einleitung zur Naturgeschichte der Plathelminthes. *In* W. Kükenthal and Th. Krumback (eds.), Handbuch der Zoologie 2(1): 34–51.

Brooks, D. R.
1982. Higher level classification of parasitic platyhelminths and fundamentals of cestode classification. *In* D. C. Mettrick and S. S. Desser (eds.), Parasites: their world and ours: 189–193. Amsterdam: Elsevier Biomedical Press.
1989. The phylogeny of the Cercomeria (Platyhelminthes: Rhabdocoela) and general evolutionary principles. J. Parasitol. 75: 606–616.

Cannon, L. R. G.
1986. The Pterastericolidae: parasitic turbellarians from starfish. *In* M. Cremin, C. Dobson, and D. E. Moorhouse (eds.), Parasite lives: papers on parasites, their hosts, and their associations to honour J. F. A. Sprent: 15–32. St. Lucia, Australia: Univ. Queensland Press.

Cavalier-Smith, T.
1998. A revised six-kingdom system of life. Biol. Rev. 73: 203–266.

Ehlers, U.
1984. Das phylogenetische System der Plathelminthes. Univ. Göttingen Habilitationsschrift [Abstract published in Verh. Naturwis. Ver. Hamburg 27: 291–294 (1984)].
1985. Das Phylogenetische System der Plathelminthes. Stuttgart and New York: Gustav Fischer. 317 pp.

Ehlers, U., and B. Sopott-Ehlers
1995. Plathelminthes or Platyhelminthes? Hydrobiologia 305: 1–2.

Haszprunar, G.
1986. Die klado-evolutionäre Klassifikation—Versuch einer Synthese. Z. Zool. Syst. Evolutionsforsch. 216: 89–109.
1996. Plathelminthes and Plathelminthomorpha–paraphyletic taxa. J. Zool. Syst. Evol. Res. 34: 41–48.

Hyman, L. H.
1951. The invertebrates, II. Platyhelminthes and Rhynchocoela. The acoelomate Bilateria. New York: McGraw-Hill. 550 pp.

Ivanov, A. V.
1952. *Udonella caligorum* Johnson, 1835. Representative of a new class of flatworms. Zool. Zh. 31: 175–178.
1991. The state-of-the-art system of Plathelminthes. Ibid. 79(8): 5–19.

Joffe, B. I.
1987. On the evolution of the pharynx in the Plathelminthes. Tr. Zool. Inst.Akad. Nauk USSR 167:34–71. [In Russian]

Joffe, B. I., K. M. Valiejo Roman, V. Ya. Birstein, and A. V. Troitsky
1995. 5S rRNA sequences of 12 species of flatworms: implications for the phylogeny of the Platyhelminthes. Hydrobiologia 305: 37–43.

Karling, T. G.
1940. Zur Morphologie und Systematik der Alloeocoela Cumulata and Rhabdocoela Lecithophora (Turbellaria). Acta Zool. Fenn. 26: 1–260.
1974. On the anatomy and affinities of the turbellarian orders. *In* N. W. Riser and M. P. Morse, (eds.), Biology of the Turbellaria: 1–16. New York: McGraw-Hill.

Kotikova, E. A., and B. I. Joffe
1988. Nervous system of rhabdocoeloid turbellarians *Graffilla* and *Pseudografilla* in the context of their taxonomy. Zool. Zh. 67: 1621–1630 [In Russian].

Littlewood, D. T. J., K. Rohde, and K. A. Clough
1999. The interrelationships of all major groups of Platyhelminthes: phylogenetic evidence from morphology and molecules. Biol. J. Linnean Soc. 66: 75–114.

Litvaitis, M. K., and K. Rohde
1999. A molecular test of platyhelminth phylogeny: inferences from partial 28S rDNA sequences. Invert. Biol. 118: 42–56.
Llewellyn, J.
1986. Phylogenetic inference from Platyhelminth life-cycle stages. Int. J. Parasitol. 17: 281–289.
Logachev, E. D., and L. A. Sokolova
1975. Some aspects of early development of cestodes related to their phylogeny. Tr. Nauch-Issled. Inst. Tomsk. Univ. 5: 145–152 [In Russian].
Malmberg, G.
1974. On the larval protonephridial system of *Gyrocotyle* and the evolution of Cercomeromorphae (Platyhelminthes). Zool. Scr. 3: 65–81.
1986. The major parasitic platyhelminth classes—progressive or regressive evolution? Hydrobiologia 132: 23–29.
Mamkaev, Yu. V.
1991. O morfologicheskikh osnovakh sistemy ploskikh chervej. [On morphological principles of the plathelminthes system]. Tr. Zool. Inst. Akad. Nauk USSR. 241: 3–25 [In Russian: English summary].
1995. An elaboration of the evolutionary morphological basis for the systematics of the Plathelminthes. Hydrobiologia 305: 15–19.
Mayr, E.
1974. Cladistic analysis or cladistic classification? Z. Zool. Syst. Evolutionsforsch. 12: 94–128.
Meixner, J.
1938. Turbellaria (Strudelwuermer), I. Allgemeines Teil. *In* G. Grimpe, E. Wagler, and A. Remane (eds.), Die Tierwelt der Nord- und Ostsee (33) 4b:1–146. Leipzig.
Rieger, R. M.
1981. Morphology of the Turbellaria at the ulstrastructural level. Hydrobiologia 84: 213–229.
Rieger, R. M., S. Tyler, J. P. S. Smith, III, and G. E. Rieger
1991. Platyhelminthes: Turbellaria. *In* F. W. Harrison and B. J. Bogitsh, (eds.), Microscopic anatomy of invertebrates, Vol. 3: Platyhelminthes and Nemertinea: 7–140. New York: Wiley-Liss.
Riser, N. W. and M. P. Morse
1974. Biology of the Turbellaria. (Libbie H. Hyman memorial volume). New York: McGraw-Hill, 530 pp.
Riutort, M., K. G. Field, R. A. Raff, and J. Baguna
1993. 18S rRNA sequences and phylogeny of Platyhelminthes. Biochem. Syst. Ecol. 21: 71–77.
Rohde, K.
1990. Phylogeny of Platyhelminthes, with special reference to parasitic groups. Int. J. Parasitol. 20: 979–1008.
1991. The evolution of protonephridia of the Platyhelminthes. Hydrobiologia 227: 315–321.
1994. The minor groups of parasitic Platyhelminthes. Adv. Parasitol. 33: 145–234.
1996. Robust phylogenies and adaptive radiations: a critical examination of methods used to identify key innovations. Am. Natur. 148: 481–500.
Rohde, K., C. Hefford, J. T. Ellis, P. R. Baverstock, A. M. Johnson, N. A. Watson, and S. Dittmann
1993. Contributions to the phylogeny of Platyhelminthes based on partial sequencing of 18S ribosomal DNA. Int. J. Parasitol. 23: 705–724.
Rohde, K., K. Luton, and A. M. Johnson
1994. The phylogenetic relationships of *Kronborgia* (Platyhelminthes, Fecampiidae) based on comparison of 18S ribosomal DNA sequences. Int. J. Parasitol. 24: 657–669.
Rohde, K., A. M. Johnson, P. R. Baverstock, and N. A. Watson
1995. Aspects of the phylogeny of Platyhelminthes based on 18S ribosomal DNA and protonephridial ultrastructure. Hydrobiologia 305: 27–35.
Ruiz-Trillo, I., M. Riutort, D. T. J. Littlewood, and J. Baguna
1999. Acoel flatworms: earliest extant bilaterian metazoans, not members of Platyhelminthes. Science 283: 1919–1923.
Smith, J. P. S., III, S. Tyler, M. B. Thomas, and R. M. Rieger
1982. The nature of turbellarian rhabdites: phylogenetic implications. Trans. Am. Microsc. Soc. 101: 209–228.
Smith, J. P. S., S. Tyler, and R. M. Rieger
1986. Is the Turbellaria polyphyletic? Hydrobiologia 132: 13–21.
Sopott-Ehlers, B.
1985. The phylogenetic relationships within the Seriata (Platyhelminthes). *In* S. Conway Morris, J. D. George, R. Gibson, and H. M. Platt, (eds.), The origins and relationships of lower invertebrates: 159–167. Oxford: Oxford Univ. Press.

Stunkard, H. W.
1975. Life-histories and systematics of parasitic flatworms. Syst. Zool. 24: 378–385.

Tyler, S., and R. M. Rieger
1977. Ultrastructural evidence for the systematic position of the Nemertodermatida (Turbellaria). Acta Zool. Fenn. 54: 193–207.

Watson, N. A., K. Rohde, and J. B. Williams
1992a. Ultrastructure of the protonephridial system of larval *Kronborgia isopodicola* (Platyhelminthes). J. Submicrosc. Cytol. Pathol. 24(1): 43–49.

Watson, N. A., J. B. Williams, and K. Rohde
1992b. Ultrastructure and development of the eyes of larval *Kronborgia isopodicola* (Platyhelminthes, Fecampiidae). Acta Zool. 73: 95–102.

Williams, J. B.
1986. Phylogenetic relationships of the Temnocephaloidea (Platyhelminthes). Hydrobiologia 132: 59–67.

Zrzavy, J., S. Muhulka, B. Kepka, and D. Tietz
1998. Phylogeny of the Metazoa based on morphological and 18S ribosomal evidence. Cladistics 14: 249–285.

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