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# Libbie Henrietta Hyman: Life and Contributions*

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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

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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.