
Eloge: Ernst Mayr, 1904–2005

Author(s): Mary P. Winsor

Source: *Isis*, Vol. 96, No. 3 (September 2005), pp. 415–418

Published by: The University of Chicago Press on behalf of The History of Science Society

Stable URL: <http://www.jstor.org/stable/10.1086/447752>

Accessed: 25-01-2018 18:28 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <http://about.jstor.org/terms>



JSTOR

The History of Science Society, The University of Chicago Press are collaborating with JSTOR to digitize, preserve and extend access to *Isis*

that it is better than the theory based on contradictions that Oswald was the lone assassin.

Perhaps the incessant pressure of that monster memory had something to do with the fact that Popkin was manic-depressive all his adult life. His mania reached its peak when Richard Nixon was impeached. Popkin became obsessed with studying the hearings of the House of Representatives Armed Forces Intelligence Committee, and concluded that Robert Bennett, director of the Mullen Company, for which E. Howard Hunt had worked, was “Deep Throat,” the source of Woodward’s data about the Watergate affair. We now know that Popkin was wrong, which explains why at the time Bennett threatened to sue *The New York Review of Books* for \$1,000,000 if they published the story, and no other major news outlet would take it. In the course of this, Popkin renewed his investigations into the Kennedy assassination and through many complicated connections concluded that he had discovered who killed Kennedy. He hired an agent, assistants, private investigators, and had many major publishers interested, when it all collapsed.

The doctors put Popkin on lithium, which sent him into a depressed state, he claimed for years, and yet he continued to do major work in the history of philosophy and science. After awhile, he quit taking lithium and came back to his old self again, except that he morphed from the thin, almost emaciated man who first published his work around 1950 into a fat man who, for the rest of his life, weighed twice as much as he did when he was

eighteen. It is well known that lithium increases appetite, but there was also the fact that after many years of smoking up to three packs of Gauloise cigarettes and drinking up to a liter of cognac a day, he suddenly quit, cold turkey. When people expressed astonishment at this, he simply said that they hadn’t noticed the agony of the seven years it took him to work up to quitting. Because he didn’t smoke or drink anymore, he ate. He was also known thereafter for taking younger colleagues who smoked out to lunch where he presented himself and his labored breathing as a barely living exhibit of the horrors of emphysema, which killed him on 14 April 2005 when he was eighty-one years old.

In a lifetime of association with such a man, reading his books and hundreds of papers, one of course notices that he is extraordinary, but it is only when one sits down with his bibliography and an outline of all he accomplished in his life that wonder sets in. The pages of *ISIS* are filled with eulogies about men like Popkin, but that there are many of them only increases one’s awe. Their lives testify to the glory of the human intellect, and gives joy to those of us privileged to have known them.

RICHARD A. WATSON

Philosophy Professor Emeritus,

Washington University

Philosophy Faculty Affiliate,

University of Montana

© 2005 by The History of Science Society. All rights reserved. 0021-1753/2005/9603-0005\$10.00

ERNST MAYR, 1904–2005



Ernst Mayr shaped twentieth-century thought in three distinct endeavors—science, history, and philosophy—through numerous and substantial publications in each of these areas, and through warm personal interactions with contemporaries and with those of later generations. At the end of his allotted threescore years and ten, appreciative remarks started to appear; back in 1979, on the occasion of his seventy-fifth birthday, I was one of eight professional historians of biology who contributed research articles to a *Festschrift* in gratitude for his kind but stern mentoring.¹ When he reached his ninetieth birthday

¹ *Studies in History of Biology*, 1979, 3; the others were Mark Adams, Gar Allen, Chip Burkhardt, Bill Coleman, Fred Churchill, Will Provine, and Frank Sulloway. Steve Gould also contributed, but his professional home was zoology (as was Mayr’s). This dis-

and again when he turned one hundred, celebratory reviews by former students and younger colleagues were printed in scores of journals and the popular press, so that besides the enormous volume of his own writings, there is already a substantial secondary literature about him.² In years to come, historians who try to evaluate Mayr's contributions will face a peculiarly subtle task, not only because they will have to build upon Mayr's own historical work, but because many of these historians will be students of historians whom he directly influenced.

Mayr's career as a historian will not be hard for some future historian of science to chronicle. His publications in this area through 1993 have already been cataloged by Thomas Junker, complete with graphs.³ Some time ago Mayr deposited his professional correspondence and manuscripts in the Harvard University Archives, and he made sure they were thoroughly indexed. It is clear that his move to history began only after his credentials as a zoologist were solidly in place, after his 1942 book *Systematics and the Origin of Species* and after his move to Harvard in 1953.⁴ In retrospect, it is noteworthy with what small and cautious steps he entered a field in which he knew he was "a greenhorn."⁵ One obvious stimulus was his determination to win adherents to his biological species concept, which led him to read some of the literature on the history of species ideas.⁶ Another was a 1953 book, surveying the history of evolutionary ideas, by the respected German biologist Walter Zimmerman.⁷ Al Romer, then Director of the Museum of Comparative Zoology, was keenly

interested in the history of biology and encouraged the researches of a doctoral candidate in American history from Northwestern University named Ed Lurie, and it was most likely the visit of this eager young man in 1954 that stimulated Mayr to look at Louis Agassiz's concept of species.⁸ Mayr's reading of Lovejoy's *The Great Chain of Being* gave him an enticing model of the importance and explanatory power of the history of ideas.⁹ A key event was the centenary of the *Origin of Species*, when an invitation to speak to anthropologists led him to reread Darwin's classic book.¹⁰

Mayr's reading of the historical literature on evolutionary topics for more than ten years, and his correspondence with historians, convinced him that although our skill at ferreting out long-forgotten details can be wonderful, we generally lack technical competence and so we ignore the questions that scientific readers want answered. So, in the spring of 1970, at the Institute for Advanced Study at Princeton, Mayr began a major project on the history of biology, and in 1974 he organized two workshops on the history of the evolutionary synthesis.¹¹ In 1976 (the year after his retirement from Harvard), an impressive and influential collection of his articles came out, which included six historical pieces and several others with significant historical content.¹² In the late 1970s he completed the first of a planned two-volume history of biological concepts, which appeared in 1982 as *The Growth of Biological Thought*. Its subtitle, "Diversity, Evolution, and Inheritance," made clear that this book did not claim to cover all biology. Before long,

inction has sociological interest but certainly implies no innate superiority on either side, as I sketched in Mary P. Winsor, "The Practitioner of Science: Everyone Her Own Historian," *J. Hist. Biol.*, 2001, 34:229–245.

² Most substantial was an entire number of *Biology and Philosophy*, 1994, 9:263–435, including Michael Ruse's insightful "Booknotes."

³ Thomas Junker, "Factors Shaping Ernst Mayr's Concepts in the History of Biology," *J. Hist. Bio.*, 1996, 29:29–77.

⁴ By "move to history" I refer to the publication of historical statements, not to his private interest, which began in his childhood, in his father's library. Contrary to Junker and to Mayr himself, however, I do not see much history in his 1942 book.

⁵ Ernst Mayr to John Greene, 10 Mar. 1960, Harvard Univ. Archives.

⁶ Ernst Mayr, ed., *The Species Problem* (Washington, D.C.: American Association for the Advancement of Science, 1957).

⁷ Walter Zimmermann, *Evolution: Die Geschichte ihrer Probleme und Erkenntnisse* (Freiburg: Karl Al-

ber, 1953); Mayr reviewed this in *Scientific Monthly*, 1954, 79:57–58.

⁸ Ernst Mayr, "Agassiz, Darwin, and Evolution," *Harvard Library Bulletin*, 1959, 13:165–194; and E. Lurie to M. P. Winsor, personal communication.

⁹ Arthur Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge, Mass.: Harvard Univ. Press, 1936). Mayr read this book before 1959, but later could not recall exactly when.

¹⁰ Ernst Mayr, "Darwin and the Evolutionary Theory in Biology," in *Evolution and Anthropology: A Centennial Appraisal*, ed. Betty J. Meggers (Washington, D.C.: Anthropological Society of Washington, 1959), pp. 1–10; and Ernst Mayr to Marshall T. Newman, 8 November 1956, Harvard Univ. Archives.

¹¹ Will Provine, "No Free Will," *Isis*, 1999, 90:S117–S132, on pp. S126–S127; and Ernst Mayr and William B. Provine, *The Evolutionary Synthesis: Perspectives on the Unification of Biology* (Cambridge, Mass.: Harvard Univ. Press, 1980).

¹² Ernst Mayr, *Evolution and the Diversity of Life: Selected Essays* (Cambridge, Mass.: Harvard Univ. Press, 1976).

though, he dropped the idea of a second volume, for he had already dealt with the issues close to his heart.

In presenting Mayr with the History of Science Society's Sarton Medal in 1986, William Coleman rightly called *The Growth of Biological Thought* "a bold exercise in the history of ideas." He pointedly described Mayr as having "no hidden agenda; he writes history not only with vigor and clarity but with a well-defined and evident purpose."¹³ Professional historians of science recognized this as a backhanded compliment: good that Mayr doesn't hide it, but his agenda keeps him firmly outside our inner circle. After all, in those days (I hope we've matured since then) we never tired of telling each other, especially our graduate students, that a scientist can never do history properly—that is, write it the way we historians would do it. Mayr's frank purpose was to clarify issues in current science, while a real historian strives to explore the past *wie es eigentlich gewesen*. Mayr was well aware of our scornful attitude, and he addressed it, as was his wont, head on.¹⁴

My own view is that the HSS did itself proud by choosing to honor Mayr, for his "magisterial" book (Coleman's word) met a need that we in the profession did not have the gumption to tackle. Where could we turn when lecturing out of our specialty, and what could we recommend to our students—Nordenskiöld?¹⁵ Gar Allen and Bill Coleman had accepted the challenge of one century apiece, but Mayr reached back to the Greeks and forward well into the twentieth century, his work based on thoughtful study of an impressive array of primary and secondary sources. While we ourselves shrank from attempting such a project, Mayr, fully aware of the pitfalls, grasped the nettle. Many years later, when I agreed to sketch a small version of the big picture and finally experienced how much harder it is to write an overview than to report on narrow research, my admiration for him doubled.¹⁶

¹³ William Coleman, "1986 Sarton Medal Citation," *Isis*, 1987, 78:239–241, on p. 240.

¹⁴ Ernst Mayr, *The Growth of Biological Thought* (Cambridge, Mass.: Harvard Univ. Press, 1982), pp. 9–11; Ernst Mayr, "When Is History Whiggish?" *Journal of the History of Ideas*, 1990, 51:301–309.

¹⁵ William Coleman, *Biology in the Nineteenth Century* (New York: Wiley, 1971); Garland E. Allen, *Life Science in the Twentieth Century* (New York: Wiley, 1975); and Erik Nordenskiöld, *The History of Biology* (New York: Knopf, 1928).

¹⁶ Mary P. Winsor, "Biology," in *History of Modern Science and Mathematics* (New York: Charles Scribner's Sons, 2002), Vol. 1, pp. 51–83.

I continue to admire Mayr for his bold breadth of vision, even though I have, at long last, convinced myself that his favorite historical thesis is largely false. Beginning in the Darwin centenary, prominently in his influential books of 1963 and 1976, and throughout *The Growth of Biological Thought*, Mayr declared that a mode of thought epitomized by Plato's theory of forms had paralyzed the mind of almost every biologist, all philosophers, and indeed nearly everyone else until Darwin. People believed every species had an essence, so that the characters distinguishing each individual were "accidents" in the logicians' sense—that is, irrelevant to its basic nature and meaningless to science. Species could be defined by listing their essential characters. (Mayr at first called this view "typological thinking" but soon adopted Popper's term "essentialism.") Darwin's view was utterly different and revolutionary, for to him there was no distinction between an essential and an accidental character. Mayr, as a leading promoter of the modern synthesis, insisted that only Darwin's view was truly evolutionary. For the essentialists, the *eidos* is unchanging, so they must either deny transmutation entirely, as most did, or adopt a step-wise (and unexplainable) leap from one type to the next, like De Vries's mutations. Mayr continually used this narrative to badger colleagues and students into falling in line with his version of evolution, because even in the twentieth (and twenty-first!) century Darwin's insights have not yet been universally adopted. Mayr valued history as a weapon in his ongoing war against unreconstructed essentialists.

In 1959, soon after Mayr first proposed this now-famous thesis, John Greene wrote to him warning that it was a severe oversimplification.¹⁷ Reviewing *Growth* in 1983, Jacques Roger and Michael Ghiselin tactfully dissented. "Mayr envisions the history of evolutionary biology as an epic struggle against 'essentialism, the most pernicious of all philosophies,'" said Ghiselin, yet in his simplistic application of the term to a variety of historical figures Mayr was committing the very sin he abjured in biology. Later Ghiselin wisely urged that we should, whether doing science or history, "step outside the circle . . . and realize that essentialism . . . does not have an essence."¹⁸ But the constant repetition of this

¹⁷ Ernst Mayr to John Greene, 10 Mar. 1960, Harvard Univ. Archives.

¹⁸ Jacques Roger and Michael Ghiselin, "More Maiorum (A Review Symposium)," *Isis*, 1983, 74:405–413, on p. 410. See also Ghiselin's *Metaphysics and the Origin of Species* (Albany: State Univ. New York Press, 1997), pp. 77–80 and *passim*.

narrative soon established it as a well-known fact in the history of biology. My own doubts about the essentialism story remained vague and inarticulate until five years ago.¹⁹

I was emboldened finally to challenge the essentialism story in no uncertain terms by the principle Mayr himself stated repeatedly, in correspondence as well as in print:

. . . my tactic is to make sweeping categorical statements. Whether or not this is a fault, in the free world of the interchange of scientific ideas, is debatable. My own feeling is that it leads more quickly to the ultimate solution of scientific problems than a cautious sitting on the fence. Indeed . . . history should even be polemical. Such histories will arouse contradiction and they will challenge the reader to come up with a refutation. By a dialectical process this will speed up a synthesis of perspective.²⁰

The world of the interchange of historical ideas would surely be very different if more of us would state our views unambiguously, but it would become more productive only if we could at the same time follow Mayr's example of gentlemanly courtesy and not taking criticism personally. History's judgment on the essentialism story, for which Mayr is largely responsible, remains for the future. I fully expect that my criti-

¹⁹ Mary P. Winsor, "Setting up Milestones: Sneath on Adanson and Mayr on Darwin," in *Milestones in Systematics*, ed. David M. Williams and Peter L. Forey (Boca Raton, Fla.: CRC Press, 2004), pp. 1–17; "Non-Essentialist Methods in Pre-Darwinian Taxonomy," *Biology and Philosophy*, 2003, 18:387–400; "Linnaeus's Biology Was Not Essentialist," *Annals of the Missouri Botanical Garden*, in press. Mayr patiently read and commented upon each of these papers.

²⁰ Mayr, *Growth of Biological Thought* (cit. n. 14), p. 9.

cism will challenge others to refute me, which will lead to a far richer version of the history of evolutionary biology than the one that now dominates the textbooks and lecture halls.

Mayr's correspondence at Harvard is full of examples of the great amount of time and care he devoted to cultivating and encouraging young historians and philosophers of biology. Beginning in the 1960s, he also actively supported the expanding profession of the history of science. He served on the Committee on History of Science that created Harvard's department and supervised several undergraduate theses.²¹ He often attended historical meetings, including the History of Science Society and the Joint Atlantic Seminar, where he would listen attentively, ask sharp questions, and urge us onward in our work. At the beginning of my career, when I knew him only through his writings, I held him in such awe that I assumed he would have no time for someone as junior as me. But when I did call on him, he chastised me for keeping away, and soon I came to understand that above all he loved to see progress in knowledge, not only in science but in history and philosophy too.

MARY P. WINSOR

*Institute for the History and Philosophy
of Science and Technology
Victoria College
University of Toronto
Toronto, Ontario M5S1K7*

© 2005 by The History of Science Society. All rights reserved. 0021-1753/2005/9603-0006\$10.00

²¹ Joy Harvey, "History of Science, History and Science, and Natural Sciences: Undergraduate Teaching of the History of Science at Harvard, 1938–1970," *Isis*, 1999, 90:S270–S294, on p. S291 [with illustration].