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Factors Shaping Ernst Mayr's Concepts in the History of Biology

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No organic being can be fully understood except by considering its history.

Ernst Mayr [1]

Ernst Mayr's central role in the emergence of the modern evolutionary synthesis is well-known and has been widely discussed in the historical literature.[2] Mayr's rather extensive work in the history of biology, on the other hand, has received comparatively little systematic attention. His major writings, especially *The Growth of Biological Thought* (1982), are frequently cited and were controversially discussed in numerous more or less extensive reviews. However, so far there has been no attempt to analyze Mayr's historical writings in a more systematic way and to relate them to the biographical, professional and scientific context, in which they were developed.[3] This surely is a formidable task, considering Mayr's long and varied career in ornithology, systematics, evolu-[30]tionary biology, philosophy of science and history of biology. In the subsequent analysis, I will try to approach these questions from a historical point of view and look for the origins of Mayr's concepts by studying his personal and professional

[1] Ernst Mayr, „Where are we?" *Cold Spring Harbor Symposia on Quantitative Biology*, 24 (1959), 1.

[2] See *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine (Cambridge, Mass. and London: Harvard University Press, 1980); Vassiliki Betty Smocovitis, „Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," *J. Hist. Biol.*, 25 (1992), 1-65; Joseph Allen Cain, „Common Problems and Cooperative Solutions: Organizational Activity in Evolutionary Studies, 1936-1947," *Isis*, 84 (1993), 1-25. See also Mayr's own accounts: *Toward a New Philosophy of Biology: Observations of an Evolutionist* (Cambridge, Mass.: Harvard University Press, 1988), pp. 525-554; „What was the Evolutionary Synthesis?" *Trends in Ecology and Evolution*, 8 (1992), 31-34.

[3] A partial exception is Malcolm Jay Kottler's review of *Growth of Biological Thought* (below, n. 5), where the attempt is made to link Mayr's history of biology to some of his other works. See Kottler, „A History of Biology: Diversity, Evolution, Inheritance," *Evolution*, 37 (1983), 868-872. G. G. Simpson, in his review of the same book, claims that it can be understood as „an intellectual, psychological, and conceptual autobiography" of Mayr, rather than as a history of biology. However, Simpson does not substantiate this point, but more or less elaborates how his own ideas differ from Mayr's. Simpson, „Autobiology," *Quart. Rev. Biol.*, 57 (1982), 438. For recent discussions of Mayr's various careers see „Special Issue on Ernst Mayr at Ninety," *Biol. Phil.*, 9, no. 3 (1994).

development. Consequently, it will only be mentioned in passing to what extent the historical facts can be considered as a cause for his concepts in the history of biology.[4]

A causal analysis is somewhat speculative and subjective and can hardly be proven in a strict sense. But, as Mayr himself has remarked, why-questions „force one into the ordering of observations and into the constant testing of one's conclusions consistent with the hypothetico-deductive method."[5] I want to emphasize that the fact that the beliefs of a scientist are determined by historical or other contingent causes does not mean that these beliefs are bound to be false or unjustified.

It might be useful to give a short preliminary overview of Mayr's theoretical perspectives in the history of science, before I suggest some reasons as to why he held certain ideas. The most comprehensive account of these questions by Mayr can be found in the introductory chapters to *The Growth of Biological Thought*, „How to Write History of Biology," „The Place of Biology in the Sciences and Its Conceptual Structure," and in the epilogue, „Toward a Science of Science."[6] Mayr is, first of all, a scientific realist. Science has the function of helping us to understand the world in which we live by solving the various problems we encounter. History of science is, consequently, the history of the problems of science and of their solutions or attempted solutions. Mayr calls his approach „problematic history" and distinguishes it from other, equally legitimate approaches, e.g., lexicographic, chronological, biographical, cultural and sociological histories. Science is a rational undertaking for Mayr, it is based on observations and logic, and the development of science can be described as an increasing emancipation of scientific knowledge from religious, philosophical and other ideological beliefs. However, science has not only a content, but also a context. The context of science (the 'external' factors) can have a considerable effect on its [31] development, but its major significance consists in slowing down scientific progress.

Darwin's theories marked the transition of the 'pre-history' of biology to its scientific stage. Biology not only began to emancipate itself from various ideologies with Darwin, but also from the predominance of physicalist approaches. The struggle for the autonomy of biology does, however, continue to the present day. In this context the past and the

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[4] The content of Mayr's history of biology is discussed in various reviews of his books. For an overview and sympathetic remarks on Mayr's personality, see Michael Ruse, „Admayration," *Quart. Rev. Biol.*, 60 (1985), 183-192. For recent rather detailed analyses, see John C. Greene, „From Aristotle to Darwin: Reflections on Ernst Mayr's Interpretation in *The Growth of Biological Thought*," *J. Hist. Biol*., 25 (1992), 257-284 and idem, „Science, Philosophy, and Metaphor in Ernst Mayr's Writings," *J. Hist. Biol*., 27 (1994), 311-347.

[5] Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution, and Inheritance* [GBT] (Cambridge, Mass. and London: The Belknap Press of Harvard University Press, 1982), p. 7.

[6] Ibid.

present of scientific biology can elucidate one another. While modern knowledge can help to understand problems of the past, the current framework of biological theories is illuminated by a historical analysis. The close interconnection between past and present is certainly one of the major characteristics of Mayr's historical approach. He is particularly interested in current controversies within biology and between biology and other sciences, especially physics: „Perhaps the major thesis of Mayr's book [*GBT*], and the most important one, is that biology can expand the philosophy of science beyond the limits defined by the physical sciences."[7] He analyzes the 'phylogeny' of various concepts that are important for modern evolutionary biology. Consequently, his preferred topics are the first and the second Darwinian revolution, as well as the debate about reductionism.[8] For Mayr the history of biology is not a narrative story telling nor a historicist recreation of the past, but its objective is conceptual elucidation.

One important point must be kept in mind, when we discuss Mayr's development as a historian of biology: his historical interest in biology arose comparatively late, and he is still regularly publishing on other subjects, for example, on systematics and evolutionary theory. From his first papers in the early 1920s until the 1940s Mayr worked primarily in ornithology and theoretical systematics. He increasingly turned to questions of evolutionary biology during the 1940s and especially after he moved from the *American Museum of Natural History* in New York to Harvard University in 1953.[9] It was not until the end of the 1950s that Mayr began to publish significantly in the history of biology and it was not before the 1970s that historical [32] questions took up a larger proportion of his overall publications. In the 1970s Mayr had already worked in different fields of biology for five decades. However, the statement that Mayr turned towards history comparatively late has to be qualified. It is only valid for the history of biology in a narrow sense, since, from the very beginning of Mayr's scientific career, his research in ornithology and systematics dealt with historical problems. From the time when he was a graduate student with Erwin Stresemann at Berlin University, the historical change of organisms and of their geographical distribution played a pivotal role in his papers.

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[7] Douglas J. Futuyma, „A Synthetic History of Biology," *Science*, 216 (1982), 843.

[8] Mayr has used the notion of a first and second Darwinian Revolution on two different levels: When discussing Darwin's discoveries of the theory of evolution versus the theory of natural selection (1837-38) and with respect to the reception of the Darwinian theories (see Mayr, „Darwin and Natural Selection. How Darwin May Have Discovered His Highly Unconventional Theory," *Amer. Scient.*, 65 (1977), 321-327); and idem, *One Long Argument: Charles Darwin and the Genesis of Modern Evolutionary Thought* (Cambridge, Mass.: Harvard University Press, 1991). I am referring to the latter version.

[9] For the early years of Mayr's career as a naturalist and systematist, see Walter Bock, „Ernst Mayr, Naturalist: His Contributions to Systematics and Evolution," *Biol. Phil.*, 9 (1994), 267-327; Jürgen Haffer, „'Es wäre Zeit, einen 'allgemeinen Hartert' zu schreiben': Die historischen Wurzeln

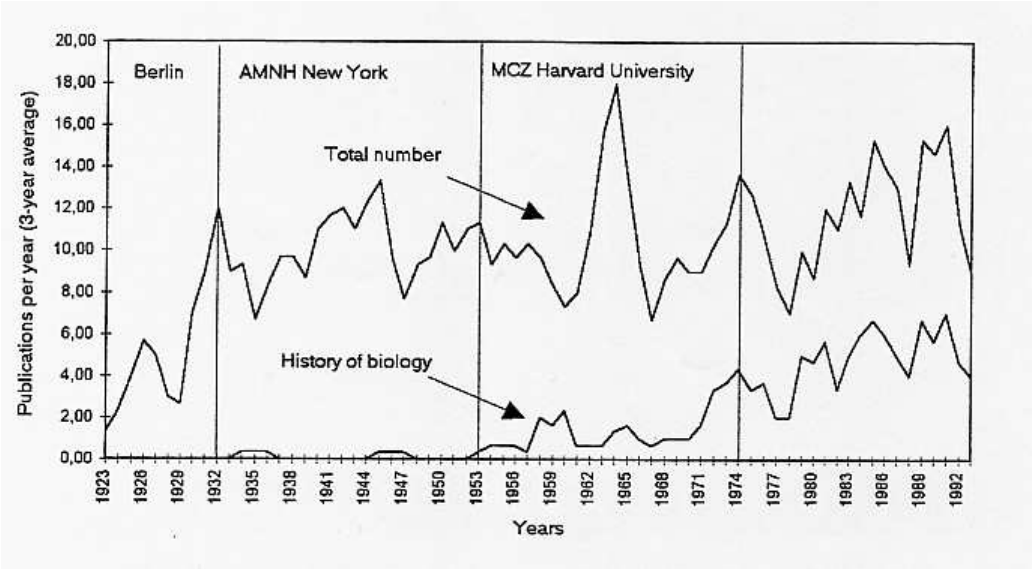

Figure 1. Number of publications by Ernst Mayr per year. (The figure given for a single year is an average including the preceding and following year.)

A few examples may illustrate this point: In Mayr's first major publication, his dissertation *Die Ausbreitung des Girlitz* (1926), he studied the change over time in the geographical distribution of the serin finch during the last 100 to 150 years. This problem involved collecting and commenting on the various historical reports of observations of the serin finch in the literature, – a classic historiographical problem.[10] In *Systematics and the Origin of Species* (1942), Mayr's major contribution to the evolutionary synthesis, he sought to establish a „new systematics," in which the traditional analysis and [33] classification should be complemented by aspects of evolutionary biology, especially speciation and factors of evolution.[11] As Walter Bock has pointed out, for Mayr biological systematics is an integral part of evolutionary biology, which again is an essentially historical science.[12] But even in a narrow sense, the historical approach is present in *Systematics and the Origin of Species*. For example, when Mayr is discussing

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von Ernst Mayr's Beiträgen zur Evolutionssynthese," *Bonner Zoologische Beiträge*, 45 (1994), 113-123.

[10] Mayr, „Die Ausbreitung des Girlitz (*Serinus canaria serinus* L.). Ein Beitrag zur Tiergeographie," *J. für Ornithologie*, 74 (1926), 571-671. See: „My main work consisted in ferreting out literally hundreds and hundreds of local natural histories from France to Poland and analyzing the competence of the various observers who had either not recorded the serin finch or had made definite statements about it." Mayr, *Autobiographical Notes* (ABN), p. 52. Private collection of Ernst Mayr. See also Richard W. Jr. Burkhardt, „Ernst Mayr: Biologist-Historian," *Biol. Phil.*, 9 (1994), 359-371.

[11] Mayr, *Systematics and the Origin of Species* (New York: Columbia University Press, 1942), pp. 10-11. The term „new systematics" was coined by Julian Huxley in 1940, see *The New Systematics*, ed. Julian Huxley (Oxford: Clarendon Press, 1940).

[12] „His [Mayr's] contributions in systematics and evolutionary biology are closely interwoven; they are difficult to unravel for separate analyses because they represent the empirical and

his ideas on systematics, he is frequently referring to the historical origins of various concepts.[13] It would not pose much of a problem to give similar examples that document the centrality of the historical style of thinking for Mayr, and it is hardly an exaggeration to say that the historical aspect is of great significance in practically all of Mayr's areas of research, except, perhaps, in the technical writings in ornithological systematics.

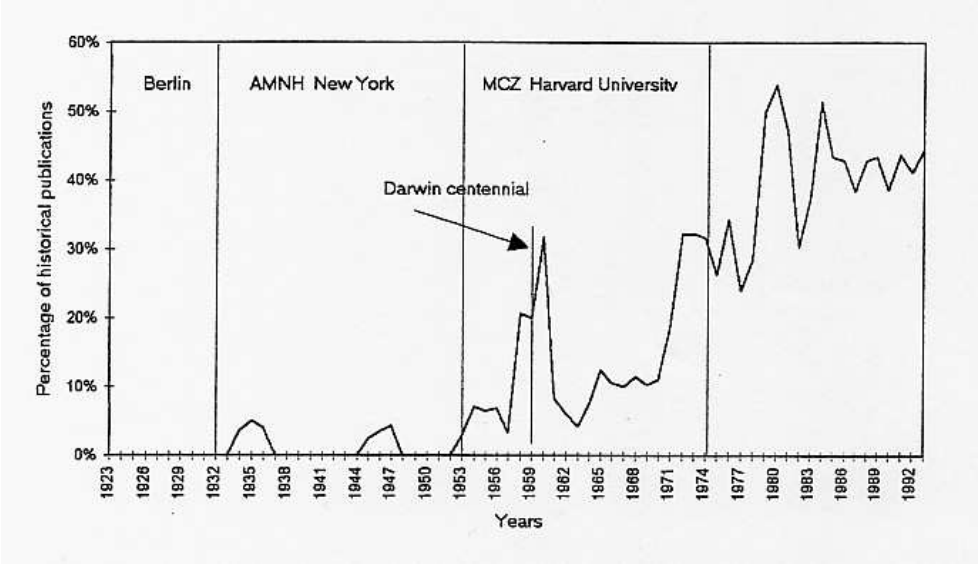

Figure 2. Mayr's publications in the history of biology as a percentage of the total number of publications per year. (The figure given for a single year is an average including the preceding and following year.)

Why does Mayr attach this importance to the historical approach, compared to a purely functional analysis? His basic observation is that organisms and [34] species evolve during a long historical process and that we can, as a consequence, only understand many of their features if we study them as historical phenomena (see the introductory quotation). To be sure, Mayr had originally developed his ideas about the importance of the historical approach within the context of evolutionary biology,[14] but this principle holds true for other disciplines as well. The most important observation in this context is that science itself is a historical phenomenon:

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theoretical aspects of the same research program." Bock, „Ernst Mayr, Naturalist" (above, n. 9), p. 269. See also Mayr, *Systematics and the Origin of Species* (above, n. 11), p. 3.

[13] „Already my first book, *Systematics and the Origin of Species* (1942) reveals my interest because I show again and again what the roots of some of the concepts and controversies were." Mayr, ABN (above, n. 10), p. 70.

[14] Mayr, „Cause and Effect in Biology: Kinds of Causes, Predictability, and Teleology are Viewed by a Practicing Biologist," *Science*, 134 (1961), 1501-1506.

> Anything that changes in time has, by definition, a history – the universe, countries, dynasties, art and philosophy, and ideas. Science also, ever since its emergence from myths and early philosophies, has experienced a steady historical change and is thus a legitimate subject for the historian.[15]

Two major aspects of Mayr's history of biology will not be discussed in this paper. The main focus will be first, to point out some of his basic concepts and to deal with underlying assumptions and ideas that remained more or less unchanged. As a result of this approach the constancy in Mayr's thinking will be overemphasized and the actual amount of change will be underrepresented.[16] The second point, which will only be mentioned in passing is the 'reception' of his writings by historians of biology.[17]

Biographical Background

Ernst Mayr's personal history was – compared to the unexciting life of most academics – eventful and sometimes even adventurous. This observation is especially true with respect to his youth and expeditions. It is not unduly speculative to assume that these experiences in part formed Mayr's character and, consequently, his world view. Mayr's biographical background will not be described in great detail, but it is necessary to keep in mind that many of his [35] most characteristic traits can only be understood from a biographical point of view.

It seems obvious that his education and youth as part of the educated middle class, the *Bildungsbürgertum*, in the German empire was of great importance to his later career. Fritz Ringer has referred to this social stratum as the German „mandarins."[18] Mayr

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[15] Mayr, *Growth of Biological Thought*, p. 1.

[16] Mayr stresses the point that he has changed his opinion on various questions during his long career and considers this flexibility a sign of scientific creativity (personal communication).

[17] See, for example, the *festschrift* on occasion of Mayr's seventy-fifth birthday, in *Stud. Hist. Biol.*, 3 (1979). The contributors were Richard W. Burkhardt, Jr., Frank J. Sulloway, William Coleman, Mary Pickard Winsor, Stephen Jay Gould, Frederick B. Churchill, Garland E. Allen, William B. Provine, and Mark B. Adams. A more comprehensive view of Mayr's actual influence may be gained by an analysis of the dedications and acknowledgments in writings in the history of biology. Even a casual survey will certainly document his widespread presence in this kind of source. A very readable and informative review of reviews of Mayr's *Growth of Biological Thought* is Ruse, „Admayration" (above, n. 4).

[18] Ringer defines the „mandarins" as „as a social and cultural elite which owes its status primarily to educational qualifications, rather than to hereditary rights or wealth." Fritz K. Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890-1933* (Cambridge, Mass.: Harvard University Press, 1969), p. 5. For a discussion of the mandarin spirit in biology, see Jonathan Harwood, *Styles of Scientific Thought. The German Genetics Community 1900-1933* (Chicago and London: The University of Chicago Press, 1993), pp. 274-314. Mayr describes the attitude of his family as „very much that of upper-class Germans, that one should never stop trying

himself has frequently stressed the relevance of the family context. One of his favorite examples is the importance of birth order in a family: Whereas the first-born child in a family tends to be more conservative, the later-born children tend to be more revolutionary.[19] Other decisive events were World War I and the subsequent revolution of November 1918 in Germany, the political and social tensions during the years of the Weimar Republic, Mayr's expeditions (1928-30), and finally, his emigration to the United States (1931). How may these events shaped Mayr's style of thinking?

One of Mayr's strongest characteristics is his 'rebelliousness' against authorities of all kinds. This point has already been mentioned with respect to the notion of birth order. We also have to keep another decisive period of his youth in mind. When Mayr was thirteen years old, his father died (1917), and shortly afterwards the political and social authorities in Germany partially collapsed during the revolution of 1918. He witnessed at this impressionable age how weak even powerful authorities can become over a short period of time. One of the few spheres of social life remaining comparatively unshaken during that time was science.[20] Another decisive experience may have been Mayr's personal situation during World War II (1941-1945). He was a German citizen during these years and was considered a potential danger to the safety of the United States. This kind of situation certainly sharpens a person's awareness [36] of the pitfalls of typological thinking concerning human beings and makes it a more than reasonable position to stress individuality.[21]

Finally, Mayr's familiarity with two different contexts of language, social structure and scientific tradition gave him the opportunity to bridge some of the gaps between German and Anglo-American science. This is evident in his *Systematics and the Origin of Species* (1942), where German papers constitute more than one third of the bibliography. Similarly, we see traces of Mayr's understanding of the two languages in his writings on the history of biology. He has, for instance, included rather extensive translations of German authors to make the English-speaking audience aware of certain important

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to add to one's 'Bildung'." Mayr, ABN (above, n. 10), p. 32. The comprehensive style of thought, characteristic for the German concept of *Bildung*, is certainly typical for Mayr.

[19] Mayr was the second of three sons. Mayr, *Growth of Biological Thought*, pp. 5, 831-832. The notion of birth order is currently analyzed by Frank Sulloway, „Orthodoxy and Innovation in Science: The Influence of Birth Order in a Multivariate Context," Paper presented at the AAAS, New Orleans, 16 February 1990, manuscript.

[20] Science, especially the exact sciences, were also under attack after World War I. The life sciences, however, could more easily adapt to the anti-'mechanistic' milieu. See Paul Forman, „Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Hist. Stud. Phys. Sci.*, 3 (1971), 1-115.

[21] For Mayr's critical attitude towards racist ideologies, see Mayr, „Discussion," in *Science and the Concept of Race*, ed. Margaret Mead, Theodosius Dobzhansky, Ethel Tobach, and Robert E.

concepts. A similar picture develops when we analyze Mayr's reviews of writings in the history of biology.[22] Another interesting point in this context deserves mentioning. It has been suggested by psychologists of science that 'marginality' can be one of the causes for increased scientific creativity. The notion of marginality implies that „persons who have been uprooted from traditional culture, or who have been thoroughly exposed to two or more cultures, seem to have an advantage in the range of hypotheses they are apt to consider, and through this means, in the frequency of creative innovations."[23]

The later stages of Mayr's personal history were primarily dominated by the professional context and his scientific career. The years during which he was curator at the *American Museum of Natural History* in New York (1932-53) and later a professor at Harvard University, gave him valuable insights into the politics of science. Last, but not least, I have to mention Mayr's activities as an editor of scientific journals,[24] as an administrator (Director of the *Museum of Comparative Zoology*, 1961-70), as a member or president of numerous societies and committees and as managing editor of the *Check-list of Birds of the World*. Despite all his other activities, Mayr's primary interest was always in science, and on the following pages I will discuss how his specific experiences as a [37] practicing ornithologist, systematist, and evolutionary biologist have shaped his world view and his understanding of history.

As an introduction to the analysis of his concepts in the history of biology, another question has to be answered: What were the reasons Mayr's attention turned towards the history of biology around 1959? As we can see in Figure 2, during these years there was a significant rise in the percentage of his historical writings, compared to the total number of his publications. After this early peak, for more than a decade (1960-1971) he turned his attention to other fields of research, before focusing on the history of biology again in the early 1970s – shortly before he became professor emeritus. Since that time, approximately 40% of his papers (with rising tendency) deal at least in part with

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Light (New York: Columbia University Press, 1968), pp. 103-105 and idem, „Letter to the Editor," *Perspect. Biol. Med*., 14 (1971), 505-506.

[22] See, for example: „Bernard Altum and the Territory Theory," *Proc. Linnaean Soc. New York*, Nos. 45, 46 (1935), 24-38; „Illiger and the Biological Species Concept," *J. Hist. Biol.*, 1 (1968), 163-178; „Weismann and Evolution," *J. Hist. Biol.*, 18 (1985), 295-329; „Joseph Gottlieb Kölreuter's Contributions to Biology," *Osiris*, 2d ser. 2 (1986), 135-176. For Mayr's reviews in the history of biology, see Appendix.

[23] Donald T. Campbell, „Blind Variation and Selective Retention in Creative Thought as in other Knowledge Processes," *Psychol. Rev.*, 67 (1960), 391. See also Dean Keith Simonton, *Scientific Genius: A Psychology of Science* (Cambridge: Cambridge University Press, 1988).

[24] Mayr served as editor of the *Proceedings* and *Transactions of the Linnaean Society of New York* (1934-41) and of *Evolution* (1947-49). See also Joseph Cain, „Ernst Mayr as Community Architect: Launching the Society for the Study of Evolution and the Journal *Evolution*," *Biol. Phil.*, 9 (1994), 387-427.

historical questions. Why do we see this significant peak in Mayr's historical activities around 1959?

1959: Lovejoy, Darwin, and the Evolutionary Synthesis

Mayr himself noted his interest in the history of biology as having arisen in connection with his reading of Lovejoy's *The Great Chain of Being*.[25] Admittedly, Lovejoy's book is a very influential and impressive piece of scholarship, but the kind of influence it had on Mayr is not evident at first sight. For example, in *The Great Chain of Being* Lovejoy is mentioning biological topics only briefly, whereas philosophical and theological questions receive thorough attention. In addition, Lovejoy is dealing with a time, which Mayr had dismissed as the less important 'pre-history' of scientific biology. There are two reasons why Lovejoy's analysis could gain great influence on Mayr despite the mentioned qualifications.

The first reason has to do with the contents of Lovejoy's book, the second with his historiographical methodology. If we look at Mayr's writings in the history of biology, we see that his first purely historical paper dealt with the relationship between Louis Agassiz and Charles Darwin.[26] This was an obvious choice considering the fact that at that time Mayr was professor at the *Museum of Comparative Zoology* (MCZ) and that Louis Agassiz, one of the most uncompromising opponents of Darwin, had been the founder of the MCZ a century earlier.[27] Agassiz' scientific outlook was strongly influenced by idealistic and *naturphilosophische* concepts and Lovejoy discusses the origin of these traditions. Mayr frequently refers to [38] Lovejoy's book in his analysis of Agassiz' ideas about the philosophy of science and especially emphasizes that Lovejoy „presents in a new light many of the concepts of the pre-Darwinian period customarily labeled as 'evolutionary'."[28] Mayr used Lovejoy's study of the different concepts that are related to 'the great chain of being' in subsequent papers too, for example, in his account of Lamarck's theory of evolution.[29]

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[25] Mayr, *Growth of Biological Thought*, p. 18; Arthur O. Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge, Mass. and London: Harvard University Press, 1936). Mayr's personal copy was printed in 1957.

[26] Mayr, „Agassiz, Darwin, and Evolution," *Harvard Library Bulletin*, 13 (1959), 165-194.

[27] See Mary Pickard Winsor, *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum* (Chicago and London: The University of Chicago Press, 1991).

[28] Mayr, „Agassiz, Darwin, and Evolution" (above, n. 26), 168 n.

[29] Mayr, „Lamarck Revisited," *J. Hist. Biol.*, 5 (1972), 55-94. For a discussion of Mayr's work on Lamarck see Burkhardt, „Ernst Mayr: Biologist-Historian" (above, n. 10).

Mayr's interest was not only stimulated by the subject matter of Lovejoy's writing, but even more by Lovejoy's historiographical methodology. Lovejoy's aim is to give a representation of the 'biography' of an idea.[30] Mayr links his 'phylogenetic' treatment of scientific problems in the history of science directly to Lovejoy's 'biographical' approach:

> In the case of the history of science, the focal points are problems rather than ideas, but the approach of the historian of science is not much different from that of a historian of ideas such as Lovejoy. Like Lovejoy, he attempts to trace the problem back to its beginning and to follow up its fate and its ramifications from such a beginning either to its solution or to the present time.[31]

Despite the overall similarities there are, of course, interesting differences between Lovejoy's and Mayr's approach. For instance, Mayr's substitution of „problems" for „ideas" may have been informed by Thomas S. Kuhn, who defined „normal science as puzzle-solving," or by Karl Popper's emphasis on problems in scientific research.[32] The 'phylogenetic' treatment of an idea or of a problem is intuitively plausible for an evolutionary biologist. I will discuss some consequences of this point of view for Mayr's history of biology below. An appropriate methodology is certainly an important prerequisite, but as such it is not necessarily a motive for a scientific study. Why exactly did Mayr publish in the year 1959 for the first time in the history of biology?

The Darwin Centennial of 1959

The direct motive undoubtedly was the Darwin centennial in 1959. Most of Mayr's writings in the history of biology during the following years deal [39] with Darwin, his forerunners (Lamarck), successors (Weismann), and opponents (Agassiz), a period that Mayr would later call the „first Darwinian revolution." Why was the Darwin centennial considered so important by many biologists and by Mayr? To answer this question, it is necessary to recall the situation of evolutionary biology in the 1950s. Evolutionary biology was a fragmented field during the first decades of the twentieth century having theoretical disagreements between the different biological disciplines and methodological approaches: geneticists, paleontologists and systematists held differing and in part contradictory concepts about organismic evolution. A broad consensus was reached

[30] Lovejoy, *The Great Chain of Being* (above, n. 25), p. 22.

[31] Mayr, *Growth of Biological Thought*, p. 18.

[32] Thomas S. Kuhn, *The Structure of Scientific Revolutions*. 2d ed. (Chicago and London: The University of Chicago Press, 1970), p. 35; „Thus we may say that *the growth of knowledge proceeds from old problems to new problems, by means of conjecture and refutations*." Karl R. Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon Press, 1972), p. 258.

between 1936-47 and the theory of evolution that emerged was called the modern synthetic theory of evolution.

I will not try to give an account of the various, still controversially discussed elements of the evolutionary synthesis. But one point has to be mentioned: the modern synthetic theory of evolution is considered the most recent branch of Darwinism and the Darwinian mechanism of natural selection is its theoretical core. The centennial of the publication of Darwin's *Origin of Species* in 1959 was celebrated, at least in part, because the architects of the modern synthesis saw Darwin as the founder of their research program:

> If we celebrate the centennial of the publication of the theory of evolution through natural selection, we do this not merely for historical reasons. We do it because natural selection has remained, since Darwin, the most important component of the theory of evolution and has become the cornerstone of the modern, synthetic theory of evolution.[33] [40]

Or, as Mayr has remarked pointedly, the history of biology before Darwin has just been the pre-history of scientific biology.[34] As early as his first review of a book in the history of biology – Walter Zimmermann's *Evolution* (1953) – Mayr draws a close connection between Darwin and the evolutionary synthesis and outlines his future research program in the history of biology:

> If I were to write a history of this field [the theory of evolution], I would try to show how growing maturity in the contributing fields eventually permitted this [the modern] synthesis. After an introductory chapter devoted to the period before Darwin, I would try to demonstrate how the publication of the *Origin of*

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[33] Mayr, „Darwin and the Evolutionary Theory in Biology," in *Evolution and Anthropology: A Centennial Appraisal*, ed. Betty J. Meggers (Washington, D. C.: The Anthropological Society of Washington, 1959), p. 10. The *Society for the Study of Evolution*, in which Mayr was one of the key organizers, co-sponsored the Darwin Centennial Celebration in Chicago. Mayr's role in the organization of the *Society for the Study of Evolution* is discussed in: Vassiliki Betty Smocovitis, „Organizing Evolution: Founding the Society for the Study of Evolution (1939-1950)," *J. Hist. Biol.*, 27 (1994), 241-309. For an account of the celebration, see *Issues in Evolution*, ed. Sol Tax and Charles Callender. Evolution after Darwin. The University of Chicago Centennial, vol. 3 (Chicago: University of Chicago Press, 1960), pp. 41-282. The Darwin-Centennial had not only a scientific significance (historical and contemporary), but it also reflected some of the *Zeitgeist* of this time: See Tax's comment: „The Centennial was a success because it celebrated something with deep meaning for the people of contemporary America." Ibid., p. 279. On the interplay between the history of science and modern biology in 1959 see, for example, Frederick B. Churchill, „Darwin and the Historian," *Biol. J. Linnean Soc.*, 17 (1982), 45-68. For an ethnographic account of the rituals involved in a scientific anniversary, see Pnina Abir-Am, „A Historical Ethnography of a Scientific Anniversary in Molecular Biology: The First Protein X-Ray Monography (1984, 1934)," *Soc. Epistemology*, 6 (1992), 323-354.

[34] „It has been pointed out recently that most 'histories' of evolution are actually 'pre-histories'. Their detailed treatment stops around 1860, precisely at the time when the development of the modern concepts began subsequently to the publication of Darwin's *Origin of Species* in 1859."

> *Species* stimulated an unprecedented amount of fact searching and theory building in biology.[35]

The Modern Synthesis

The preceding remarks point us to a second major topic of Mayr's work in the history of biology: the modern synthesis, the „second Darwinian revolution." The evolutionary synthesis had gained wide support in different areas of biology by 1959 and was acclaimed as the legitimate successor of the Darwinian research program and as its final point for the time being. However, a controversy began to develop with the success of the evolutionary synthesis over the question, as to which branch of evolutionary biology had been the most important for the triumph of modern Darwinism. The invitation for Mayr to give the introductory speech at the *Cold Spring Harbor Symposium* in 1959 gave him the opportunity to address this question. The first part of his speech with the title „Where are we?" is devoted to a historical discussion about the relationship between genetics and other branches of evolutionary biology.[36] In his speech, Mayr challenged the view that mathematical population genetics, more than anything else, had made the evolutionary synthesis possible, and demanded coexistence of the different branches of evolutionary biology:

> I have come to the end of my short historical survey of the relation between genetics and the other branches of evolutionary science. It seems evident that there is a happy symbiosis among these various fields. The [41] naturalist has access to a vast store of observational evidence on which he bases various empirical generalizations. It is the role of the geneticist to interpret these generalizations in terms of the genetic material and to test his conclusions by experiment. I can foresee no reason for a change in this historically established pattern of co-operation. The best evidence for its success is the modern synthetic theory of evolution.[37]

The controversy that developed after Mayr's „Where are we?"-speech contained a strong historical element, pointed out by W. Provine: „The real bone of contention between Mayr

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Mayr, „Karl Jordan's Contribution to Current Concepts in Systematics and Evolution," *Trans. Roy. Entomol. Soc. London*, 107 (1955), 45.

[35] Mayr, Review of *Evolution: Die Geschichte ihrer Probleme und Erkenntnisse*, by Walter Zimmermann (Freiburg and München: Karl Alber, 1953), *The Scientific Monthly*, 79 (1954), 57.

[36] Mayr, „Where are we?" (above, n. 1), p. 4.

[37] Ibid. The appeal for cooperation between different branches of biology was expressed by evolutionary biologists from the 1930s on. See Jonathan Harwood, „Metaphysical Foundations of the Evolutionary Synthesis: A Historiographical Note," *J. Hist. Biol.*, 27 (1994), 1-20; Cain, „Common Problems and Cooperative Solution" (above, n. 2).

and Wright was the historical interpretation of the evolutionary synthesis rather than substantive questions of evolutionary biology."[38] It would be misleading, however, to interpret this argument on the level of personal vanities or academic over-subtleties; it was rather about the future of various classical branches of biology. Systematics, for example, lost much of its previous importance during the 1950s and 1960s, mostly due to the prominent successes of molecular biology and other reductionist and experimental approaches. A major part of the available resources – graduate students and financial support – was diverted into the new research areas.[39]

The coincidence of the Darwin centennial, the triumph of the modern synthesis and the emergence of a controversy over the history and future of the evolutionary synthesis with the beginning of Mayr's interest in the history of biology is not accidental. Up to the present day, his writings focus on three related topics: the „first Darwinian revolution," triggered by Darwin's *Origin of Species* (1959), the „second Darwinian revolution" in the 1930s and 1940s (the evolutionary synthesis), and finally the importance of the classical biological disciplines in opposition to mathematical, reductionist and experimental approaches. Mayr's writings from 1959 show another typical feature of his historical approach: history is important, because it helps to understand the persistence of certain controversies in biology. It is the problems of the present that matter, and since Mayr is a biologist, [42] he deals with biological problems: „Because the great controversies of the past often reach into modern science, many current arguments cannot be fully understood unless one understands their history."[40]

[38] William B. Provine, *Sewall Wright and Evolutionary Biology* (Chicago and London: The University of Chicago Press, 1986), p. 483.

[39] See Mayr, „The New versus the Classical in Science," *Science*, 141 (1963), 765. On the situation in systematics, see Keith Vernon, „Desperately Seeking Status: Evolutionary Systematics and the Taxonomists' Search for Respectability 1940-60," *Brit. J. Hist. Sci*., 26 (1993), 207-227; Cain, „Common Problems and Cooperative Solution" (above, n. 2), pp. 16-22. On the rise of molecular biology, see Lily E. Kay, *The Molecular Vision of Life: Caltech, The Rockefeller Foundation, and the Rise of the New Biology* (Oxford and New York: Oxford University Press, 1993).

[40] Mayr, *Growth of Biological Thought*, p. 1.

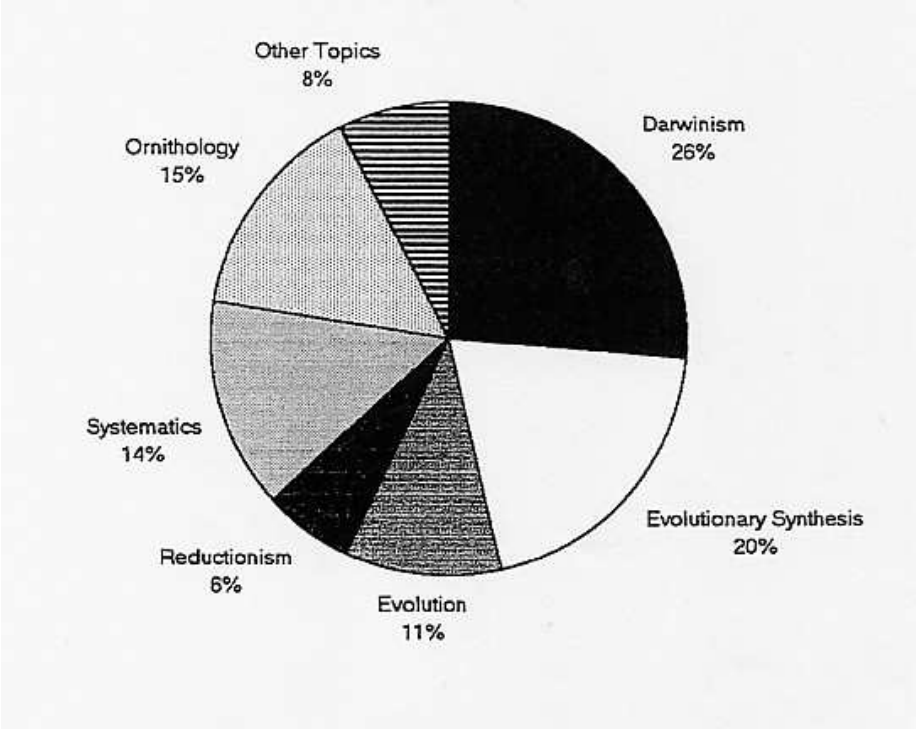

Figure 3. Percentages of different fields of interest in Mayr's publications in the history of biology.

In the following sections I will analyze various concepts that are predominant in Mayr's historical writings by comparing them with different contexts: „That such [scientific and cultural] background is important is evident even in the writings of the historians themselves."[41] The first section will show how the developments in biology that were consciously experienced (and shaped) by Mayr have influenced his view of history. Secondly, these experiences will be discussed under a somewhat different aspect: the professional context. This section will deal with experiences that are not specifically related to biology, but can be encountered in various scientific and cultural areas. A third section will be devoted to the general philosophical and ideological background. The final section will return to Mayr's experiences in biology and discuss to what extent he conceives of the history of biology in analogy to organismic evolution. [43]

It should be pointed out that the distinction between the different spheres of experiences is primarily a heuristic distinction. Frequently, the different aspects will blend into one another and a similar leitmotif will arise at different points. As Jonathan Harwood has recently pointed out, a scientist „is a member of many collectives, both inside and outside the scientific community: laboratory, specialty, discipline, family, social class, nation, etc." He continues: „At present we have no means of specifying which of these memberships will be most influential in shaping knowledge production under a given set

[41] Mayr, „Essay Review: The Recent Historiography of Genetics," *J. Hist. Biol.*, 6 (1973), 127.

of circumstances."[42] The present analysis of Mayr's writings confirms the view that a clear-cut discrimination of the effects of various contexts is not possible. This does not justify, however, focusing exclusively on one context or abandoning a causal analysis altogether.

The Biological Context

> Clearly all of Mayr's ideas in evolutionary biology, philosophy and history are grounded solidly in his experience as a naturalist and on his early studies in avian systematics.[43]

Mayr's training as a biologist has certainly influenced his historical perspective, not only in regard to his concepts, but it has determined the selection of what he considers interesting problems as well. It has already been mentioned that the history of biology is important for him insofar as it helps to understand current controversies. Under this premise, the history of biology has two major functions, it is „particularly suitable as a first introduction to science" and it „greatly assists in explaining the current framework of biological theories."[44] This approach has certain consequences. First, it implies that periods or problems that are only of limited relevance for modern science are also less important for the history of biology. Second, the focus of the history of biology on currently important biological questions requires a thorough understanding of modern biological theories by the historian.[45] Mayr's point [44] of view obviously results from his professional background: he sees himself primarily as a naturalist. A historian might give quite a different answer as to what has to be considered as currently important. Two examples will give an impression of how the biological perspective might have formed Mayr's historical approach.

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[42] Harwood, *Styles of Scientific Thought* (above, n. 18), pp. 306-307. Frank Sulloway has analyzed the effect of different contexts on the tendency of scientists to accept or reject new theories. See Sulloway, „Orthodoxy and Innovation in Science" (above, n. 19).

[43] Bock, „Ernst Mayr, Naturalist" (above, n. 10), p. 268. A similar observation was made by David Hull: „These tenets [of Mayr's philosophy of biology] stem, ... from Mayr's experience as an evolutionary biologist; all of us are influenced in our outlook by the contingencies of our early training." David L. Hull, „Ernst Mayr on the Philosophy of Biology: A Review Essay," *Historical Methods*, 23 (1990), 42.

[44] Mayr, *Growth of Biological Thought*, p. 19.

[45] „It is my conviction that one cannot understand the growth of biological thought unless one understands the thought-structure of biology." Ibid., p. 8.

Physicalism

One of Mayr's favorite observations is that modern biological research has documented how complex biological phenomena and, as a consequence, biological concepts are. In biology there are hardly any universal laws and „in striking contrast to physics and chemistry, nearly every situation and element has a unique aspect."[46] In biology history triumphs over logic, evolution over function. The same situation can be observed in the history of science: the special and individual dominates the general and there are constant changes and contingent combinations of various concepts.[47] It is not the logic of science that yields the deepest knowledge about the nature of science, but its history: „I had long come to the conclusion that it was quite impossible by logic to make genuine intellectual advances in the philosophy of biology."[48] The contingency of historical development has two consequences for biology, its philosophy, and its history: first, the futile search for laws has led biological research into many historical dead ends.[49] Second, it is not possible to base history on laws, but we have to settle with probabilistic generalizations.[50]

Mayr's view regards biological theories as being similar to organisms or species, historically developing, individual phenomena. Their uniqueness, however, does not result from the uniqueness of their elements, but from a different [45] combination of certain fundamental concepts. Mayr's notion is obviously influenced by Lovejoy, who had compared philosophical systems to complex chemical molecules. According to Lovejoy, heterogeneous concepts (the -isms) are best understood when they are broken up into „unit-ideas":

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[46] Mayr, „Integration of Genotypes: Synthesis," *Cold Spring Harbor Symposia on Quantitative Biology*, 20 (1955), 331.

[47] Mayr, *Growth of Biological Thought*, p. 17.

[48] Mayr, ABN (above, n. 10), p. 69-72. Mayr's approach is clearly different from what, for instance, Imre Lakatos suggested: „One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the footnotes* how actual history 'misbehaved' in the light of its rational reconstruction." Lakatos, „History of Science and Its Rational Reconstructions [1971]," in *The Methodology of Scientific Research Programmes*, ed. J. Worrall and G. Currie. Philosophical Papers, vol. 1. (Cambridge: Cambridge University Press, 1978), p. 120.

[49] „I have pointed out in this work many instances where physicalism has had a deleterious effect on developments in biology. ... In biology, where so many unique phenomena are encountered and where virtually all so-called laws have exceptions, the belief in the universality of laws has led to numerous invalid generalizations and to controversy." Mayr, *Growth of Biological Thought*, p. 846.

[50] Ibid., p. 847. For a discussion of this point from the positivist perspective, see Carl G. Hempel, „The Function of General Laws in History [1942]," in *Theories of History*, ed. Patrick Gardiner (Glencoe, Illinois: Free Press, 1959), pp. 344-356.

> Throughout this volume I have endeavored to carry the analysis of each problem as far as possible and to dissect heterogeneous theories and concepts into their individual components. Not all historians have been aware how complex many biological concepts are – in fact how complex the structure of biology as a whole is.[51]

As pertains to this complexity, biological concepts are very similar to organisms. In biology, we can also observe „extreme complexity. ... Every organic system is so rich in feedbacks, homeostatic devices, and potential multiple pathways that a complete description is quite impossible."[52] One of Mayr's widely discussed suggestions, as to how theories can be segregated into their theoretical components, was his dissection of Darwin's theory into five major components or subtheories.[53] Another important differentiation of Mayr was between proximate and ultimate [evolutionary] causes in biology. Whereas functional biology deals with the analysis of the proximate causes, evolutionary biology studies the ultimate causes. Both approaches have to be applied in order to gain a complete understanding of a given phenomenon.[54] Finally, Mayr's analysis of the multiple meanings of the term 'teleology' must be mentioned in this context.[55]

The intensity of Mayr's impatience with physicalism can only be understood, if we keep his experiences with the domination of biology by the functional (proximate) approach in the 1950s and 1960s in mind. Mayr had begun to fight as early as the 1950s against physicalism in biology – that [46] is, against the assertion that biological phenomena can be entirely described and explained in the vocabulary of physics. Although reduction was seen as a theoretical goal rather than as an actual possibility, it was supposed to be heuristically superior. Rudolf Carnap, for instance, maintained that the „explanation of

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[51] Mayr, *Growth of Biological Thought*, p. 8. For Lovejoy's notion of „unit-ideas" see Lovejoy, *The Great Chain of Being* (above, n. 25), pp. 3-23.

[52] Mayr, „Cause and Effect in Biology" (above, n. 14), p. 1505.

[53] See Mayr, „Epilogue," *Biol. J. Linnean Soc.*, 17 (1982), pp. 117-118; for a later version see idem, „Darwin's Five Theories of Evolution," in *The Darwinian Heritage*, ed. David Kohn (Princeton: Princeton University Press, 1985), pp. 755-772.

[54] See Mayr, „Cause and Effect in Biology" (above, n. 14), p. 1503. The importance of the distinction between proximate and ultimate causes for Mayr's philosophy of biology is stressed by John Beatty, „The Proximate/Ultimate Distinction in the Multiple Careers of Ernst Mayr," *Biol. Phil.*, 9 (1994), 333-356.

[55] See Mayr, „Teleological and Teleonomic: A New Analysis," in *Methodological and Historical Essays in the Natural and Social Sciences*, ed. Robert S. Cohen and Marx W. Wartofsky. Boston Studies in the Philosophy of Science, vol. 14 (Dordrecht, Boston: Reidel, 1974), pp. 91-117; Eve-Marie Engels, *Die Teleologie des Lebendigen* (Berlin: Duncker & Humblot, 1982).

more and more processes in organisms with the help of physics and chemistry – will be, as it has been, a very fruitful tendency in biological research."[56]

Soon Mayr extended his struggle against physicalism to the philosophy and history of biology. Physics was the paradigmatic science for the logical empiricists and the criterion for what was considered scientific was taken from the methods and theories of physics: „What annoyed me particularly was that philosophy of science in the 1950s and 1960s meant philosophy of physics. I rebelled against it in a lecture I gave at a AAAS meeting in Berkeley."[57] It was in this spirit that Mayr claimed in 1964 that a knowledge of biology is – since Darwin's *Origin of Species* – of the utmost importance to „man's concept of himself" and that the impact of the Darwinian revolution has been greater than that of „the great physicists," namely Newton and Einstein.[58] Similarly, for Mayr, the history of biology is as independent from the history of physics, as is biology from physics:

> Most general histories of 'science' have been written by historians of physics who have never quite gotten over the parochial attitude that anything that is not applicable to physics is not science. Physical scientists tend to rate biologists on a scale of values depending on the extent to which each biologist has used 'laws,' measurements, experiments, and [47] other aspects of scientific research that are rated highly in the physical sciences.[59]

We can certainly presume that it had been Mayr's personal experiences as a biologist in a time dominated by physics that convinced him of the historical relevance of this problem. When Mayr is describing *Darwin's* struggle against physicalism, we can, at the same moment, understand this statement as a biographical hint referring to Mayr's own

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[56] Rudolf Carnap, „Logical Foundations of the Unity of Science," in *International Encyclopedia of Unified Science*, ed. Otto Neurath, Rudolf Carnap, and Charles W. Morris, vol. 1, no. 1, (Chicago: University of Chicago Press, 1938), 60. Physicalism is defined by Mayr as the attempt „to explain all biological processes in terms of movements and forces. Everything was mechanistic, everything was deterministic, and there was no unexplained residue." Mayr, *Toward a New Philosophy of Biology*, (above, n. 2), p. 9.

[57] Mayr, ABN (above, n. 10), p. 69-72. Mayr refers to the meeting of the American Association for the Advancement of Science at Berkeley, 27 December 1965. The speech was published as, „Discussion: Footnotes on the philosophy of biology," *Phil. Sci*., 36 (1969), 197-202. See also: „The philosophy of science has been dominated by physics to such an extent during past decades that any discussion of theory formation in biology must start out with the similarities between physics and biology." Mayr, „Comments on 'Theories and hypotheses in biology'," in *Proceedings of the Boston Colloquium for the Philosophy of Science 1966/1968*, ed. Robert S. Cohen and Marx W. Wartofsky. Boston Studies in the Philosophy of Science, vol. 5 (Dordrecht: Reidel, 1969), p. 450.

[58] Mayr, „Introduction," in *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, by Charles Darwin. A Facsimile of the First Edition (Cambridge, Mass. and London: Harvard University Press. 1964), p. [vii].

[59] Mayr, *Growth of Biological Thought*, p. 14.

analogous controversy.[60] It might be worth mentioning that physics has not always been looked upon as the unchallenged leader of science, but during Mayr's youth in Germany it was rather biology that was considered most important.[61]

Progress in Science

As a second example of how Mayr's experiences as a biologist have shaped his views in the history of biology, the question of progress in science and how it can be defined shall be mentioned. Mayr's remarks on this point are obviously determined by his personal experience of a long and productive career in what Kuhn called „normal science." He observed (and took part himself) how scientific puzzles were solved and how a better understanding of numerous biological phenomena was achieved. Progress in science for Mayr is „characterized by an improved understanding of previously puzzling phenomena, by the removal of contradictions, by the opening of black boxes, by the possibility of making better probabilistic predictions, and by the establishment of causal connections between previously unconnected phenomena."[62]

Mayr's personal experience had shown him that in spite of occasional disruptions and different rates of improvement there is something like progress in science: „In spite of the difficulty of definition, a working scientist is rarely in doubt as to whether or not a new discovery, theory, or concept contributes to the progress of science."[63] Progress in science can, according to Mayr, result when the elements of different previously competing theories are combined in a new synthetic theory. This is considered to be one of the characteristics of the evolutionary synthesis, in which Mayr himself took part.[64] [48]

The Professional Context

Mayr was not just a naturalist, but he also had great practical experience in the politics of science. He had extensive opportunities to observe the rhetorical and political side of the

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[60] See „The Struggle Against Physicists and Philosophers" in Mayr, *One Long Argument* (above, n. 8); Mayr, *Growth of Biological Thought*, p. 830; Sandra Herbert, „Essay Review," *Isis*, 84 (1993), 119.

[61] See Heinrich Rickert, „Lebenswerte und Kulturwerte," *Logos. Internationale Zeitschrift für Philosophie der Kultur*, 2 (1911/12), 131-166. For an analysis of the anti-mathematical and anti-physicalist currents during the Weimar years, see Forman, „Weimar Culture, Causality, and Quantum Theory, 1918-1927" (above, n. 20).

[62] Mayr, *Growth of Biological Thought*, p. 856.

[63] Ibid.

[64] See Mayr, „What was the Evolutionary Synthesis?" (above, n. 2), pp. 31-34.

scientific community as an editor, administrator, and member of numerous scientific societies and committees. Science is not only determined by reason, but there are fashions, conflicts between generations, and power struggles. What is ultimately crucial for the success or failure of a scientific theory is frequently not its empirical or logical content, but the place and time of publication, the more or less skillful reference to authorities, the (e.g., mathematical) treatment of data, the playing-down of rival theories and the appropriate language and imagery. *Rhetoric* plays a very important role in science, because scientists are not just interested in scientific knowledge, but because they also „crave recognition."[65]

It is important for the historian to keep in mind that science is shaped by this rhetorical element, otherwise it will not be possible to discriminate genuine influences from ritual citations. Mayr's examples document it has been his experience with this kind of rhetoric during his active career as a biologist, which helps him grasp the rhetoric in historical developments. Many evolutionary biologists of the 1940s and 1950s followed, according to Mayr, Dobzhansky (1937) in citing the writings of the mathematical population geneticists (namely Fisher, Haldane and Wright), although they had not read these papers, let alone understood them:

> I am pointing this out as a warning to those who are trying to reconstruct influences. The mere fact that an author cites a certain work or says he is following the principles of this or that philosopher or scientist does not necessarily mean that this cited work had a decisive influence on his thinking.[66]

The real or supposed precision of mathematical statements tempts empirically working scientists to increase the authority of their own results: „As long as mathematics, physics, and chemistry enjoyed high prestige throughout the [49] eighteenth and nineteenth centuries, it was a sound strategy for a scientist to use the appropriate labels

[65] Mayr, *Growth of Biological Thought*, p. 850. For recent discussions of rhetoric in science, see Charles Bazerman, *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* (Madison and London: University of Wisconsin Press, 1988); Greg Myers, *Writing Biology: Texts in the Social Construction of Scientific Knowledge* (Madison: University of Wisconsin Press, 1990); *Textual Dynamics of the Professions. Historical and Contemporary Studies of Writing in Professional Communities*, ed. Charles Bazerman and James Paradis (Madison: University of Wisconsin Press, 1991).

[66] Mayr, *Growth of Biological Thought*, p. 850. Mayr is referring to Th. Dobzhansky's, *Genetics and the Origin of Species* (New York: Columbia University Press, 1937). See also: Nigel Gilbert, „Referencing as Persuasion," *Soc. Stud. Sci.*, 7 (1977), 113-122.

in order to give visibility to his work."[67] The use of mathematics as a rhetorical device is often very subtle and difficult to discriminate from useful applications.[68]

A similar problem emerges, when scientists discuss the theories of their scientific opponents:

> Traditionally there has been a tendency among scholars to refer to the approaches of their opponents in terms that are meant to be unflattering if not derogatory .... This must be mentioned here because a historian, looking at such statements from the outside, might fail to realize that such claims were purely psychological weapons."[69]

Finally, the tendency of a victorious scientific community to rewrite history – leading to the classical whiggishness – has to be considered by a historian of science, as Mayr points out.[70]

The *institutional structure* of science is as important as scientific rhetoric. There have been numerous promising concepts in the history of biology, which were not accepted by scientists of the same time and did not until later gain acceptance. Examples are – aside from the obligatory reference to Mendel – Mayr's own paper from 1954, where he had suggested a theory of genetic revolutions in founder populations.[71] As to why certain concepts were ignored, Mayr mentions that particular research programs do not fit into the predominant fashion of a certain time, highly specialized scientists are not informed about the developments in neighboring disciplines, and many scientists are only interested in their own narrow field.[72] The professional and personal background of a particular scientist will often result – more or less unintended – in certain distortions. National and language traditions have [50] to be mentioned in this context as well as the

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[67] Mayr, *Growth of Biological Thought*, p. 851.

[68] See Neal Koblitz, „Mathematics as Propaganda," in *Mathematics Tomorrow*, ed. Lynn Arthur Steen (New York, Heidelberg, Berlin: Springer-Verlag, 1981), pp. 111-120; Philip J. Davis and Reuben Hersh, „Rhetoric and Mathematics," in *The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs*, ed. John S. Nelson, Allan Megill, and Donald N. McCloskey (Madison: The University of Wisconsin Press, 1987), pp. 53-68.

[69] Mayr, *Growth of Biological Thought*, pp. 851-852.

[70] „Whenever there is a scientific controversy, the views of the losing side are almost invariably later misrepresented by the victors." Ibid., p. 12. See also Kuhn's similar statement: „When it repudiates a past paradigm, a scientific community simultaneously renounces, as a fit subject for professional scrutiny, most of the books and articles in which that paradigm had been embodied. ... the result is a sometimes drastic distortion in the scientist's perception of his discipline's past." Kuhn, *The Structure of Scientific Revolutions* (above, n. 32), p. 167.

[71] Mayr, „Change of Genetic Environment and Evolution," in *Evolution as a Process*, ed. Julian Huxley, A. C. Hardy, and E. B. Ford (London: Allen & Unwin, 1954), pp. 157-180.

scientific training. The methods and concepts in which a scientist was trained are seen as superior: „Not just the physicist but every specialist, quite naturally, considers his particular field of research to be the most interesting and its methods to be the most productive."[73]

Many – perhaps even the great majority – of Mayr's concepts in the history of biology are obviously based on his personal experiences as a naturalist and member of the scientific community. His long career as a biologist gives him the opportunity to gain a direct empirical access to many questions in the history of biology. Rachel Laudan has recently pointed out that eminent historians of science of the eighteenth and nineteenth century also tended to be very interested in the politics of science of their time.[74] There is a similar intimate connection of Mayr's interests in the historical and the political aspects of science.

The Philosophical Context

Even a scientist who has a narrow outlook does not live in a vacuum, and an author who has such broad interests as Mayr has, will be subject to miscellaneous and divergent influences.[75] I will not try to give a comprehensive picture of various contextual factors, but instead focus on a few important developments in the philosophy of science during the 1950s and 1960s, the time when Mayr became engaged in his study of the philosophy and history of biology. By comparing Mayr's historical concepts with contemporary notions, no direct influence is implied, in the sense that Mayr read a particular author and subsequently adopted a specific concept. I would rather suggest that most of the similarities are due to the common *Zeitgeist* of the period.[76] This

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[72] Mayr, *Growth of Biological Thought*, pp. 837-838.

[73] Ibid., p. 15.

[74] See Rachel Laudan, „Histories of the Sciences and Their Uses: A Review to 1913," *Hist. Sci*., 31 (1993), 21. Mayr's organizational activities as a member of the „New York Circle" during the 1930s and 1940s are discussed in Cain, „Common Problems and Cooperative Solution" (above, n. 2).

[75] Jonathan Harwood has emphasized the „wide range of intellectual and 'cultural' interests" displayed by most of the architects of the evolutionary synthesis. See Harwood, „Metaphysical Foundations of the Evolutionary Synthesis" (above, n. 37), p. 14.

[76] For the broader world view of the architects of the evolutionary synthesis, see John C. Greene, „The Interaction of Science and World View in Sir Julian Huxley's Evolutionary Biology," *J. Hist. Biol*., 23 (1990), 39-55; Sahotra Sarkar, „Science, Philosophy, and Politics in the Work of J. B. S. Haldane, 1922-1937," *Biol. Phil.*, 7 (1992), 385-409; M. J. S. Hodge, „Biology and Philosophy (Including Ideology): A Study of Fisher and Wright," in *The Founders of Evolutionary Genetics. A Centenary Reappraisal*, ed. Sahotra Sarkar. Boston Studies in the Philosophy of Science, vol. 142. (Dordrecht: Kluwer Academic Publishers, 1992), pp. 231-293; *The Evolution of Theodosius Dobzhansky*, ed. Mark B. Adams (Princeton: Princeton University Press, 1994); Smocovitis, „Unifying Biology" (above, n. 2); Harwood, „Metaphysical Foundations of the Evolutionary

question [51] will be discussed in relation to Mayr's notion of a comprehensive „science of science": a synthetic theory of science that not only encompasses its history and philosophy, but its psychology and sociology as well.[77]

A comparison of Mayr's concepts in the history of biology with those of the logical empiricists and T. Kuhn should document some of the general notions of that time. We will see, however, that Mayr cannot be assigned to either camp, but that he could preserve a fair amount of conceptual independence. One of the reasons for the unorthodoxy of some of his ideas probably lies in his biological background: the logical empiricists as well as Kuhn and Popper focused their attention very much on physics.

Logical Empiricism

In the early 1960s logical empiricism was the dominating philosophy of science in the United States. There was no serious rival.[78] It is important to keep this in mind, in order to understand the radical nature of Mayr's struggle for the independence of biology, its philosophy and history. It is evident from Mayr's frequent statements that Darwin had been an exceptionally creative naturalist, not *in spite of*, but *because* of the fact that he did not care too much about the philosophy of his time, that Mayr himself does not expect much inspiration from philosophy: „Dr. Himmelfarb criticizes Darwin repeatedly for his 'ineptness' as a philosopher. Yet she does not seem to realize that this very weakness was Darwin's strength."[79] However, Mayr's assessment of the principles of logical empiricism is not completely negative, but rather ambivalent. I will first discuss some of his objections and, subsequently, some common tenets.

At three points Mayr's opposition against physicalist notions of the logical empiricists were bound to arise: (1) Logical empiricism considered mathematics to be the model and paradigm for all other sciences; (2) within the natural sciences, again, it was the most mathematical science, physics, that was [52] supposed to be the most scientific.; and

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Synthesis" (above, n. 37). A more comprehensive account of Mayr's philosophical concepts would not only have to consider his readings in philosophy, but also trace the numerous lectures and informal discussions at Harvard University.

[77] Mayr, *Growth of Biological Thought*, pp. 829-858. The term „science of science" was used widely before 1982; see Thomas S. Kuhn, „The History of Science [1968]," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago and London: The University of Chicago Press, 1977), p. 122.

[78] See Ronald N. Giere, *Explaining Science: A Cognitive Approach* (Chicago and London: The University of Chicago Press, 1988), p. 22.

[79] Mayr, „Concerning a New Biography of Charles Darwin, and Its Scientific Shortcomings. Review of *Darwin and the Darwinian Revolution*, by Gertrude Himmelfarb (New York: Doubleday and Co., 1959)," *Sci. Amer.*, 201 (November 1959), 215. Mayr was one of the first to point out Darwin's originality in philosophy: „Darwin had violated all the rules of the game by placing his

(3), the unity of science was sought to be achieved by reducing the biological and social sciences to one common method and language, based on the physical sciences. Mayr considered it very important to fight against the predominance of physicalism in biology and it is in this connection that he also objected to the above tenets of logical empiricism. Mathematical models, for instance, are of a very limited value in biology, because it is not clear how they can be related to the actual biological phenomena.[80] The representatives of logical empiricism had little interest in the actual development of science, but rather dealt with the logical and epistemological foundations of scientific knowledge. Mayr, on the other hand, regards the historical viewpoint as predominant: "This must be stressed particularly in the face of the static views of the logical positivists who thought that logical structure was the real problem of science. ... Actually most scientific problems are far better understood by studying their history than their logic."[81]

It is worth mentioning that Mayr's writings in the philosophy of biology, in which he first expressed his anti-positivist notions, were published before 1962. In 1962 Kuhn published his extremely influential book *The Structure of Scientific Revolutions*. This point in time is considered to be the beginning of a turn from logic to history in the philosophy of science: „History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed."[82]

There also exist various points of contact between Mayr's concepts in the history of science and those of the logical empiricists in spite of these fundamental differences of opinion. Mayr, for example, defines problems in the history of science very much in the positivist tradition:

> The backbone of science is the system of generalizations, theories, and concepts which form the explanatory framework of the observed phenomena. It has been the major objective of the philosophy of science to determine how theories are formed and tested; how one distinguishes between hypotheses, laws, and theories; what differences there are between the logic of discovery and that of explanation.[83]

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argument entirely outside the traditional framework of classical philosophical concepts and terminologies." Mayr, „Introduction" (above, n. 58), p. [xi].

[80] Mayr, „Integration of Genotypes" (above, n. 46), p. 327.

[81] Mayr, *Growth of Biological Thought*, p. 6.

[82] Kuhn, *The Structure of Scientific Revolutions* (above, n. 32), p. 1.

[83] Mayr, *Growth of Biological Thought*, pp. 839-840.

And there are common values, like the high regard for science as a rational attempt to understand nature: „The complexities of biological causality do not justify embracing nonscientific ideologies, such as vitalism or finalism, [53] but should encourage all those who have been trying to give a broader basis to the concept of causality."[84]

The Internalism-Externalism Debate

One element common both to Mayr's history of biology and logical empiricism is the notion of the so-called 'internal' and 'external' factors. Today references to internal and external factors have become less popular, but this dichotomy fundamentally structured debates in the history of science up to the 1970s. The internalism-externalism debate centers on the question of the cultural demarcation of science, on how science defines itself in relation to rival forms of knowledge, for instance religious or ideological beliefs.[85] Where does Mayr draw the line between the scientific and the non-scientific? What does he regard as internal, what as external? Internal are empirical observations and logical relationships. According to Mayr, experience alone can provide the foundation for scientific knowledge and give rise to a new theory:

> As a young naturalist and explorer, Darwin had made a series of observations that he was unable to explain on the basis of the prevailing ideologies. Convinced that he had to start on an entirely new track, he realized that he must avoid at all costs the danger of being deflected from his true course by prevailing dogmas.[86]

External factors, on the other hand, can be everything that is retarding or sometimes accelerating the rational development of a particular research program: general ideologies (religious systems), widespread philosophical concepts (*scala naturae*, essentialism), the wider cultural context, and political developments (e.g., military conflicts). There can also be external influences that originate in neighboring scientific

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[84] Mayr, „Cause and Effect in Biology" (above, n. 14), p. 1506. For an account of the efforts of the architects of the evolutionary synthesis to make biology „scientific" (in positivist terms), without, at the same time, losing its subject matter through reduction, see Smocovitis, „Unifying Biology" (above, n. 2). There seem to be few reasons, however, to agree with Smocovitis's assertion that only „within a positivist theory of knowledge ... the unification of science [was] desirable." (Ibid., p. 4). Most of the architects, especially Mayr, vehemently opposed any attempt to unify science in a reductionist way, as it was suggested by logical empiricists. For a discussion of positivist and recent conceptions of reduction, see C. A. Hooker, „Towards a General Theory of Reduction," *Dialogue*, 20 (1981), 38-59, 201-236, 496-529.

[85] See Karl R. Popper, *The Logic of Scientific Discovery* [1934] (London and New York: Routledge, 1959); Imre Lakatos, „Introduction: Science and Pseudoscience," in *The Methodology of Scientific Research Programmes* (above, n. 48), pp. 1-7.

[86] Mayr, „Introduction" (above, n. 58), p. [xi]. See also Mayr, „The Naturalist in Leidy's Time and Today," *Proc. Acad. Nat. Sci. Philadelphia*, 98 (1946), pp. 271-272.

disciplines, in institutional [54] structures or technological needs.[87] General ideologies or world views can become external influences, because they are so fundamental that the single scientist cannot separate himself from them: „The sociologists of science ... have correctly stressed the fact that science is not going on in a vacuum but reflects inevitably the general *Zeitgeist* of the period."[88]

Every influence which impedes the free development of the single specialist in his area of expertise is considered external: Mayr's own controversy with reductionist tendencies within biology, for instance, constitutes an external influence for him. In the discussion of external factors the retarding and creative effects have to be distinguished, and, „there is far more evidence for an inhibiting rather than an innovative influence of the Zeitgeist."[89] The ideological external factors have a predominantly negative effect on scientific progress, but in this respect they can be very influential. According to Mayr, the socioeconomic environment, on the other hand, has no significance for the content of science at all.[90]

The problem for the historians of science is that ideologies are frequently unconscious. This unconsciousness gives rise to illusions about the origin of ideas on part of the scientists. It is not observations and facts as such that determine in many cases the answer to certain questions, but the theoretical and ideological perspective.[91] Mayr had accepted during the 1960s the notion of logical empiricism that scientific knowledge can be built up from neutral observations. He emphasized the theory-ladenness of observations in his later writings, again following the general shift in the history of science after Kuhn and others: „Scientific fact and theory are not categorically separable, except perhaps within a single tradition of normal-scientific practice."[92]

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[87] Mayr, *Growth of Biological Thought*, p. 4.

[88] Ibid., p. 848.

[89] Mayr, „The Ideological Resistance to Darwin's Theory of Natural Selection," *Proc. Amer. Phil. Soc.*, 135 (1991), 123.

[90] Socioeconomic factors can, however, have an effect on the level of scientific activity. The distinction between ideological and socioeconomic factors is not present in 1982. But see Mayr, *One Long Argument* (above, n. 8), pp. 39-40.

[91] „A further difficulty for the historian is posed by most scientists' unawareness of their own framework of ideas. They rarely articulate – if they think about it at all – what truths or concepts they accept without question and what others they totally reject. In many cases the historian can piece this together only by reconstructing the total intellectual milieu of the period. And yet an understanding of these silent assumptions may be necessary in order to answer previously puzzling questions." Mayr, *Growth of Biological Thought*, pp. 17-18.

[92] Kuhn, *The Structure of Scientific Revolutions* (above, n. 32), p. 7.

The internalism-externalism debate for Mayr has a deeply professional component. As a systematist he was confronted with the attempts by experimental and reductionist biologists to redefine the nature of biological science. He clearly considered this as an external factor, because it could not be [55] justified from within the problems and methods of biological systematics. Science as a whole, on the other hand, is internal when it is compared to the socioeconomic environment. The external-internal demarcation can be observed at different levels because scientific communities exist at various levels. An individual scientist usually belongs to several such groups, from the scientific community as a whole to restricted and informal networks.

Sociology of Science and Sociology of Scientific Knowledge

A sociology of science emerged in the 1960s to complement the logic of science. Sociology was supposed to analyze, for example, which scientific institutions and norms seemed most appropriate to guarantee progress in science. One of the basic principles of the so-called Mertonian program was that sociology can only study the social structure of science, but not its theoretical content and methods: "It is important to realize that external factors influence science in two entirely different ways: They may either affect the overall level of scientific activity at a given place at a given time, or they may affect or even give rise to a particular scientific theory."[93]

The first notion, describing the effect of various external factors on the intensity of scientific activity can, according to Mayr, hardly be doubted: „The effect of environmental conditions on the level of scientific activities has been appreciated as long as there has been a history of science."[94] The answer will be completely different, however, if we ask whether external factors can actually cause the emergence of a specific scientific theory. Contextual and sociological approaches in the history of science have a certain attractiveness, because this „approach stresses the point that science is a form of human endeavor and is therefore inseparable from the intellectual and institutional milieu of the period."[95] But Mayr himself is not convinced that „such external factors have favored or inhibited *specific* scientific theories."[96]

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[93] Mayr, *Growth of Biological Thought*, p. 4.

[94] Ibid., pp. 4-5.

[95] Ibid., p. 3.

[96] „Of course no one lives in a vacuum, and anyone who reads voraciously, as for example Darwin did ... is bound to be influenced by his reading. Darwin's notebooks are ample evidence for the correctness of this inference. But ... this by itself does not prove the thesis of the Marxists that 'Darwin and Wallace were extending the laissez-faire capitalist ethos from society to all nature.'„ Ibid., pp. 5-6.

The Mertonian program (the „sociology of science") is acceptable for Mayr, because it employs the constraint that only the prerequisites of scientific knowledge can be a subject matter of sociology, but not the content of knowledge. By rejecting the legitimacy of a sociology of scientific knowledge Mayr adopts the same demarcation between the logic of science and its sociology [56] as it was put forward by the representatives of logical empiricism. One reason why sociology cannot properly analyze the content of science is that during the historical development of science an increasing liberation from external influences has occurred:

> This trend toward the emancipation of science had a considerable effect on the writing of the history of science. The farther back we go in time, the less important becomes the store of scientific knowledge of the period and the more important the general intellectual atmosphere.[97]

This trend toward emancipation has the consequence that historians interested in the social background of science should turn to the pre-history of a science, i.e., to the period before 1800 with regards to biology.[98]

Psychology

A demarcation similar to the one between the sociology of science and the sociology of scientific knowledge is at the basis of Mayr's understanding of the role of psychology in the history of science as well. His ideas on this topic are analogous to the concepts of logical empiricism, although there seems to be no direct influence. Mayr stresses the fact that he arrived at his conclusions independently,[99] so that we have to assume that this is one of the general views within reach of all persons in this given historical situation. According to logical empiricism in the context of discovery, there is no logic, only psychology. On the other hand, in the context of justification there can be no psychology, just logic. The supposedly illegitimate application of psychology in the

[97] Ibid., p. 14. „Nothing signaled the emancipation of science from religion and philosophy more definitely than the Darwinian revolution. Since that time it has become quite impossible to say by looking at an author's scientific publications whether he was a devout Christian or an atheist." Ibid., p. 13. For an alternative view see Kuhn: „The more carefully they [the historians] study ... the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today." Kuhn, *The Structure of Scientific Revolutions* (above, n. 32), p. 2. The possibility to tell the ideological background of an individual scientist from the content and logical form of his arguments was actually considered the touchstone for a sociological analysis; see Karl Mannheim, „Die Bedeutung der Konkurrenz im Gebiete des Geistigen," in *Verhandlungen des Sechsten Deutschen Soziologentages vom 17. bis 19. September 1928 in Zürich* (Tübingen: J. C. B. Mohr, 1929), pp. 35-83; English trans., in *From Karl Mannheim*. 2d ed. Ed. Kurt H. Wolff. (New Brunswick and London: Transaction Publishers, 1993), pp. 401-402.

[98] Mayr, *Growth of Biological Thought*, p. 6.

context of justification was labeled „psychologism."[100] The same demarcation is applied by Mayr. Psychology can be useful in the context of discovery: for instance in answering questions like How [57] does scientific creativity originate, or what are the characteristics of fertile research programs?

I have mentioned above that one of Mayr's preferred psychological examples is birth order in a family. The rebellion of a younger generation against the parent's generation can also be an important factor in scientific revolutions.[101] Other psychological characteristics that might explain scientific creativity are the tendency to speculate, intellectual flexibility and breadth of interest.[102] Finally, it should be mentioned that for Mayr genetic, inherited characteristics play an important role in determining the character of a person, hardly a surprising position for a biologist.[103] All in all, Mayr treats psychological explanations more favorably than sociological ones and he discusses them with stronger interest.[104]

Science and Religion

Before I briefly discuss the general background of Mayr's philosophical concepts, I want to touch on his view of the relationship between science and religion. Mayr's treatment of this point has drawn criticism on him, second only to the alleged 'whiggishness' of his historical work. The critics claim that Mayr sees the relationship between science and religion „with the traditional (but now historically discarded) metaphor of warfare."[105] Mayr does not use the term 'warfare,' but he clearly describes natural theology as one of

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[99] Mayr, pers. comm.

[100] See Popper, *The Logic of Scientific Discovery* (above, n. 85); Lakatos, „History of Science and Its Rational Reconstructions" (above, n. 48).

[101] Mayr, „Comments," in *Proceedings of the Boston Colloquium for the Philosophy of Science, 1962-1964*, ed. Robert S. Cohen and Marx W. Wartofsky. Boston Studies in the Philosophy of Science, vol. 2. (New York: Humanities Press, 1965), pp. 151-156.

[102] Mayr, *Growth of Biological Thought*, p. 832.

[103] „His [Lord Rothschild's] parents were first cousins and an above-average amount of homozygosity may be part of the explanation for the incongruous mixture of seemingly incompatible traits." Mayr, Review of *Dear Lord Rothschild: Birds, Butterflies, and History*, by Miriam Rothschild (London: Hutchinson, 1983), *Isis*, 75 (1984), 602.

[104] Mayr's generally rather positive stance towards psychology applies especially to cognitive psychology, whereas he is very doubtful about the concepts of psychoanalysis. See for example: „Indeed, even Sigmund Freud based, in part, his theory of psychoanalysis on it [the biological concept of recapitulation]. Was it a sound foundation? In other words, is the theory valid? The answer is no." Mayr, Review of *Ontogeny and Phylogeny*, by Stephen Jay Gould (Cambridge, Mass. and London: The Belknap Press of Harvard University Press, 1977), *MCZ Newsletter*, 7 (1977), 3.

[105] W. F. Bynum, „On the Written Authority of Ernst Mayr," *Nature*, 317 (1985), 585.

the major opposing ideologies to Darwin's theory of natural selection.[106] What may have been the reasons for Mayr to stick to this allegedly „historically discarded" picture? [58]

First of all, it is an empirical question whether there was a tension between religious and scientific beliefs at certain times, which cannot be decided by wishful thinking on either side. The image that science and religion have been in opposition at all times may be historically discarded. But it is equally obvious that there have been certain historical situations when scientists expressed a strong sense of aversion against religious thinking. Let me give one example that may shed light on Mayr's awareness of conflicting interests, represented by religious versus scientific writers. In 1933, in his lecture *Über eine Weltanschauung*, Sigmund Freud expressed his conviction that religion „is to be taken seriously as an enemy" of science.[107] In 1933, Mayr was 29 years old, and, although he had not read Freud at that time, he was perfectly aware of the general disdain a considerable segment of the German intellectuals felt towards religious beliefs.[108] To sum up, I do not think that Mayr's critical attitude towards religion is caused by the aggressiveness of contemporary creationism in the United States, but is part of the anti-religious tradition in European thought stemming back to the Enlightenment.

Discussion

The various notions of the logical empiricists and of Mayr I have mentioned can only be fully understood if we look at their historical origin. These concepts gained widespread acceptance during World War II and the following Cold War. It is hardly an accident that

[106] See for example Mayr, „The Ideological Resistance to Darwin's Theory of Natural Selection" (above, n. 89), p. 126. For a different interpretation of Darwin's relation to natural theology, see Dov Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859* (Cambridge: Cambridge University Press, 1981); Greene, „From Aristotle to Darwin" (above, n. 4).

[107] Sigmund Freud, „The Question of a Weltanschauung [1933]," in *Standard Edition*, ed. James Strachey, vol. 22 (London: The Hogarth Press, 1964), p. 160. See also Peter Gay, *A Godless Jew: Freud, Atheism, and the Making of Psychoanalysis* (New Haven and London: Yale University Press, 1987).

[108] See Mayr's autobiographical notes: „... both of my parents were more less agnostic. ... I don't think I was more than 13 years old when I became very rebellious about all the miracles and other seemingly improbable events that were reported. I forever raised questions which embarrassed the teacher. Most of my class was behind me and I would try to get some reading that would strengthen my hand. Unfortunately I found in my father's library a copy of Haeckel's *Welträtsel* with numerous references to the apocryphal parts of the Bible. I read Haeckel for this and probably more or less ignored his evolutionary discussions. In later years I always said that I fully supported the ethics of Christianity but not its metaphysics. Still later I began to be disturbed at the strictly anthropocentric teachings of the Bible, including the New Testament, and the disregard for the environment." Mayr, ABN (above, n. 10), p. 45. Richard Goldschmidt, who was born 26 years earlier than Mayr, gave a vivid description of the anti-religious zeal of his youth, which was also

the architects of logical empiricism, as well as Mayr, were mostly emigrants from the German-speaking countries. The claim that science has to be independent from national or social conditions was directed against the notion of a „German" science as propagated in Nazi-Germany and, especially, against the Marxist assertion [59] that the content of science is determined by the economic situation. This political background explains why the question of the demarcation of science (the internalism-externalism debate) could gain this predominance.

During World War II, science seemed to be one of the few cultural areas that could guarantee rationality and internationalism: "I still remember that we [Mayr and David Lack, August 1939] wistfully remarked how much better it would be for the world if internationally-minded scientists could arbitrate all conflicts between nations, not leaving such important matters to politicians and generals."[109]

After the German capitulation in 1945, the concept of a „German" science had resolved itself to a large extent and the struggle against Marxism moved to the forefront. The sociology of scientific knowledge and externalism were considered as more or less identical with Marxism: „In recent years Marxist historiographers in particular have voiced the thesis that social ideologies influence the ideas of scientists, and that the history of science as practiced until now has totally neglected the social context."[110]

At this point, I cannot discuss Mayr's attempt to refute Marxist notions. Rather, it is important to demonstrate that the demarcation between the logic of science and the sociology of science, i.e., the belief that science is basically non-political, had eminent political significance.[111] Mayr and the logical empiricists represent a liberal science, a science that was seen as an alternative to the 'totalitarian' ideologies of communism and national socialism. Karl Mannheim has pointed out that one of the outstanding features of the liberal style of thinking in science is the attempt to eliminate all of its irrational elements.[112] Scientists stressed the *value* of science to oppose a perceived threat to the rationality of scientific knowledge.[113] Another important principle of a liberal society is

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inspired by Haeckel's writings. See Goldschmidt, *Portraits from Memory: Recollections of a Zoologist* (Seattle: University of Washington Press, 1956), pp. 34-36.

[109] Mayr, „In Appreciation – David Lack," *Ibis*, 115 (1973), 432.

[110] Mayr, *Growth of Biological Thought*, p. 5.

[111] See Steven Shapin, „Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism-Internalism Debate," *Hist. Sci.*, 30 (1992), 333-369.

[112] See: Mannheim, „Die Bedeutung der Konkurrenz im Gebiete des Geistigen," (above, n. 97), (English trans.), p. 424.

[113] This historic background explains why Mayr might not be inclined to follow John Greene's suggestion to recognize „that there is no such thing as a purely scientific approach to scientific

individualism, the emphasis of the single naturalist, [60] „the working scientist and his conceptual world." The world of science is the Western world, „the free world of interchange of scientific ideas."[114]

Considering the above tenets, Mayr is certainly well grounded in the liberal style of thought. I would suggest that his point of departure from the empiricist philosophy of science is due in part to his background as a naturalist, in part to his training during a more holistically minded time.[115] The physicalism of the Vienna Circle was developed in opposition to theories of biological and sociological holism, prominent, for example, in both fascist and Marxist philosophies of science. During World War II and the following years holistic concepts fell, consequently, into disrepute. In the 1960s they emerged again in what might be called a 'return of the repressed.'[116]

Evolution in Biology and in the History of Biology

In the section on „The biological context," I have discussed the influence of Mayr's experiences as a biologist on his historical writings. I will now return to biology with a

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problems of general scope." Greene, „From Aristotle to Darwin" (above, n. 4), p. 260. For an impression of the interplay of political and scientific issues during the 1940s and 1950s see, for example, Robert K. Merton, „The Normative Structure of Science [originally: „Science and Technology in a Democratic Order," 1942]," in idem, *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago and London: The University of Chicago Press, 1973), pp. 267-278; Karl R. Popper, *The Open Society and Its Enemies*. 2 vols. (London: Routledge & Kegan Paul, 1945). Very important for biology was, of course, the case of Lysenkoism; see Julian Huxley, *Heredity East and West: Lysenko and World Science* (New York: Henry Schuman, 1949). See also David Bloor, *Knowledge and Social Imagery*. 2d ed. (Chicago and London: Chicago University Press, 1991), pp. 76-79.

[114] Mayr, *Growth of Biological Thought*, pp. 7 and 9. The liberal style of thinking of a scientist does not necessarily have to be connected with a political liberal conviction. The reason is that current political conservative thought in the US has absorbed many originally liberal (whig) tenets like individualism. In Mayr's case, however, both fields coincide and he considers himself a liberal (personal communication).

[115] The independence of Mayr's concepts in the philosophy and history of biology has been criticized. However, if we consider the frequent methodological changes in the history of biology during the last decades, Mayr's rather idiosyncratic approach has its advantages. Recently, Mayr was made a fellow honoris causae by the *Center for Philosophy of Science* (University of Pittsburgh) and awarded an honorary degree by the University of Konstanz for his contributions to *philosophy*.

[116] During the 1920s, a whole array of holistic theories existed in the German-speaking countries that cannot be reduced to either fascist or Marxist approaches. See Anne Harrington, „Interwar 'German' Psychobiology: Between Nationalism and the Irrational," *Sci. Context*, 4 (1991), 429-447. Jonathan Harwood has recently argued that there may be similar metaphysical assumptions at the basis of both the interwar German holism and the evolutionary synthesis in the United States, namely anti-reductionism; see Harwood, „Metaphysical Foundations of the Evolutionary Synthesis" (above, n. 37). See also: Forman, „Weimar Culture, Causality, and Quantum Theory, 1918-1927" (above, n. 20); Peter Galison, „Aufbau/Bauhaus: Logical Positivism and Architectural Modernism," *Crit. Inq.*, 16 (1990), 709-752. In this context Mayr made an interesting observation: „At Harvard University it was in the year of unrest (1968) that student interest rather suddenly began to veer away from molecular biology, toward ecology, behavior, and evolution." Mayr, *Growth of Biological Thought*, p. 892.

somewhat different perspective and give examples of how he understands the growth of scientific thought in analogy to biological evolution. With this analogy Mayr does not stand alone. Th. Dobzhansky, for instance, wrote in 1955: „There is more than a superficial analogy between the evolution of ideas and the evolution of a group of organisms."[117] T. Kuhn also considered this analogy useful: [61]

> The analogy that relates the evolution of organisms to the evolution of scientific ideas can easily be pushed too far. But with respect to the issues of this closing section it is very nearly perfect. ... And the entire process [the development of scientific knowledge] may have occurred, as we suppose biological evolution did.[118]

Similarly, Mayr is transferring concepts that were originally biological into the historical context. In Mayr's case the direct connection between the two fields is, of course, that he had studied historical (evolutionary) phenomena in biology as well as in history.

Evolution and Selection

As an evolutionist Mayr is well aware that there are two different types of organismic change, individual development and evolution. Both forms are important in the history of biology. First of all, every organism constantly changes during its lifetime. This observation holds true for the intellectual development of a scientist as well: "One can never understand the impact of a thinker throughout his lifetime if one does not understand the permutations of his thought. ... Scientists who never changed their major ideas from the beginning to the end of their career are probably a small minority."[119]

In context with the reception of Lovejoy's historical methodology, I have mentioned that Mayr applies an analogy between biological evolution and the evolution of ideas. We can observe in the history of science, as we do in (biological) phylogeny, the transformation and branching of problems.[120] The analogy between biological and historical development can even be pursued in specific questions. The problem emerges for the

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[117] Theodosius Dobzhansky, „A Review of some Fundamental Concepts and Problems of Population Genetics," *Cold Spring Harbor Symposia on Quantitative Biology*, 20 (1955), 1.

[118] Kuhn, *The Structure of Scientific Revolutions* (above, n. 32), pp. 172-173. For recent evolutionary accounts of the development of science, see Robert John Richards, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (Chicago and London: University of Chicago Press, 1987), pp. 559-593; David Hull, *Science as a Process. An Evolutionary Account of the Social and Conceptual Development of Science* (Chicago and London: The University of Chicago Press, 1988).

[119] Mayr, *Growth of Biological Thought*, pp. 830-831.

[120] Ibid., p. 18.

evolutionary systematist how a necessarily constant term can be applied to a changing entity, for example, a biological species:

> The delimitation of species which do not belong to the same time level ... is difficult. In fact, it would be completely impossible if the fossil record were complete. ... The change is slight and gradual and should, at least theoretically, not permit the delimitation of definite species.[121]

'Fortunately,' the fossil record is not complete, so that gaps [62] arise that make the demarcation of species easier. The same problem occurs in the history of science observing a constant change in the meaning of terms: „A gradual evolution of terms, sometimes resulting in drastically different end points, is characteristic for almost all research traditions."[122]

The analogy between the history of science and evolution is drawn by Mayr in an even more direct way, when he transfers the biological mechanism of natural selection into history. For example, while discussing the question of which processes can result in constructive theoretical changes, Mayr mentions five cases. Three of these cases are characterized by the elimination (= selection) of inappropriate theoretical concepts; two cases by the addition of new elements. Progress can, for instance, be a result of the elimination of invalid theories or concepts, of the elimination of inconsistencies and contradictions and finally of the elimination of semantic confusion.[123] Mayr obviously applies a selection model to scientific progress. Another incentive, besides the biological theory of natural selection, to use the selection model may have been Popper's notion of conjectures and refutations in science.[124]

The selection model seems to be also at the basis of Mayr's understanding of the internalism-externalism debate. The context of science, its 'environment,' is clearly influenced by science. The context/environment cannot, on the other hand, induce the contents of new scientific theories. It can just determine its success (survival). The origin of scientific/genetic innovations by means of direct adaptation to the context/environment can be considered a Lamarckian mechanism. At least in biology, this notion has been refuted. But it is unclear if the development of science can actually

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[121] Mayr, *Systematics and the Origin of Species* (above, n. 11), p. 153.

[122] Mayr, „Attaching names to objects," in *What the Philosophy of Biology is*, ed. Michael Ruse (Dordrecht: Kluwer, 1989), p. 238.

[123] See Mayr, *Growth of Biological Thought*, p. 840-843.

[124] Mayr's selection model seems to be a combination of both, see, for example: „Active areas of biology experience a steady proposal of new conjectures (Darwinian variation) and some of them are more successful than others." Mayr, „The Advance of Science and Scientific Revolutions," *J. Hist. Behav. Sci.*, 30 (1994), 333.

be explained by a pure selection mechanism, based on chance variations/conjectures, as perceived by Mayr, or in addition displays 'Lamarckian' features.

Population Thinking and Scientific Revolutions

A very interesting and not so clear-cut parallel between the biological theory of evolution and the history of biology can be drawn between scientific revolutions and evolutionary saltations. One of Darwin's major theories was gradualism and one of the various anti-Darwinian theories that were refuted by the architects of the modern synthesis was saltationism.[125] To what [63] degree are Mayr's ideas on the subject of revolutions in the history of science shaped by the biological analogy? According to Mayr, there are revolutions in science, however these revolutions are not necessarily sudden events, but frequently long-term processes. This clearly is different from Kuhn, who sees revolutions as rather short-term switches of gestalt. In a recent paper, Mayr has linked Kuhn's notion of scientific revolutions to his training as a physicist: „It is perhaps no surprise that the theory of scientific revolutions was advanced by a physicist. It reflects the essentialistic-saltationist thinking of physicalism."[126]

On the other hand, gradualism is a direct result of population thinking for Mayr. Population thinking, as it was proposed by Mayr and other authors of the modern synthesis, contradicts the notion that single big mutations, e.g., Goldschmidt's „hopeful monsters" are the cause of macroevolution.[127] If evolution is a populational phenomenon, individual variations cannot become too large, because this would prevent the successful interbreeding with other members of the population: „A single mutation does not make a new species except in the case of polyploidy. New species are due to gradual accumulation and integration of small genetic differences."[128] Population thinking is one of Mayr's most basic theoretical concepts and can be documented in all

[125] Mayr, *Growth of Biological Thought*, p. 840.

[126] Mayr, „The Advance of Science and Scientific Revolutions" (above, n. 124). See also Mayr, „The Nature of the Darwinian revolution: Acceptance of Evolution by Natural Selection Required the Rejection of many Previously Held Concepts," *Science*, 176 (1972), 981. On Kuhn's understanding of scientific revolutions see Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*. Trans. Alexander T. Levine (Chicago and London: Chicago University Press, 1993), pp. 201-206.

[127] Mayr defines population thinking as: „Living nature does not consist of types but of variable populations in which each individual is unique." Mayr, „Epilogue" (above, n. 53), p. 124. The first full elaboration (1959) of the notion of population thinking by Mayr can be found in: Mayr, „Darwin and the Evolutionary Theory in Biology" (above, n. 34), p. 2. On the notion of „hopeful monsters," see Richard Goldschmidt, *The Material Basis of Evolution* (New Haven and London: Yale University Press, 1940), p. 390.

[128] Mayr, *Systematics and the Origin of Species* (above, n. 11), p. 225.

his areas of interest. According to Walter Bock, Mayr's belief in population thinking is primarily caused by his research in microsystematics:

> Nothing in Mayr's most general theoretical writings has been more important than his stand against typology in biology in the broadest sense, including his stance against racism .... Mayr's unwavering belief in 'population thinking', a term he coined, stems directly from his systematic work.[129]

Bock's analysis certainly brings up a very important point, but it is doubtful, if it is the only explanation. The connection between population thinking and the rejection of racist ideologies in Mayr's thought points to a broader causation. I have mentioned that [64] individualism is a general principle of western societies. Mayr's personal history has presumably made him more sensitive to this problem, because collective (typological) characterizations are not only applied to racial, but also, for example, to national minorities.

If population thinking is fundamental to Mayr's way of thinking, it should have found expression in his historical writings. Gradualism, which is closely related to population thinking, can actually be documented in Mayr's description of theoretical change: he speaks of „the gradual replacement of ... scientific beliefs by better based scientific theories and concepts."[130] It is conceivable that in the history of science as well, revolutionary changes cannot become too extreme, because new theories have to be understood and appreciated. Lovejoy, for instance, held that it is a „characteristic of the history of ideas ... that it is especially concerned with the manifestation of specific unit-ideas in the collective thought of large groups of persons, not merely in the doctrines or opinions of a small number of profound thinkers or eminent writers."[131] Mayr as well talks about „the great army of other scientists who make contributions to the gradual progress of our knowledge and understanding."[132] In this instance he links gradualism to the social character of science. Furthermore, he describes several cultural phenomena that have to be referred to as populational phenomena:

> ... it is quite legitimate to apply evolutionary concepts to certain group phenomena that are not, strictly speaking, genetic in nature, as for instance the evolutionary change of language and other cultural attributes of man. Concepts like 'isolation,' 'mutation,' 'gene flow,' 'selective advantage' have their close and

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[129] Bock, „Ernst Mayr, Naturalist" (above, n. 10), p. 285.

[130] Mayr, *Growth of Biological Thought*, p. 19.

[131] Lovejoy, *The Great Chain of Being* (above, n. 25), p. 19.

[132] Mayr, *Growth of Biological Thought*, p. 829.

> legitimate analogies in many of the phenomena studied by the cultural anthropologist.[133]

However, in his historical writings Mayr clearly stresses the individual and the influence of the „great leaders of science."[134] Biological evolution is, according to Mayr, a phenomenon that primarily occurs in populations – there are no „hopeful monsters." In the history of science at least with respect to its cognitive aspects, the social character is of minor relevance. Analogous to a biological population would be, for example, a „thought collective," a „community of persons mutually exchanging ideas or maintaining intellectual interaction."[135] Kuhn has emphasized the social [65] character of science in a similar way: „Both normal science and revolutions are, however, community-based activities."[136]

A closer look at Mayr's understanding of biological evolution reveals, however, that the seeming contradiction between his elitist conception of scientific progress and biological population thinking, is rather a difference in emphasis. Mayr's biological gradualism is 'gradual' only, if compared to the sudden changes suggested by mutationists. Although evolution can be considered as gradual and populational in the sense described above, he does, at the same time, allow for rapid changes, „genetic revolutions," in small populations. One of the factors that affect the rate of evolution is population size, i.e., evolution occurs most slowly in large panmictic populations and most rapidly in small isolated populations.[137] In this sense Mayr's notion of genetic revolutions in small „founder populations," consisting of few individuals, is as 'elitist' as his understanding of the role of the „great scientists." In both cases it is geographical isolation respectively individuality, that makes radical changes possible. The change in emphasis may be due to the generally reserved attitude of Mayr toward the social context, discussed above.

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[133] Mayr, „Darwin and the Evolutionary Theory in Biology" (above, n. 34), p. 8.

[134] Mayr, *Growth of Biological Thought*, pp. 829-831.

[135] Ludwik Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkkollektiv* (Basel: Benno Schwabe, 1935); English trans., *Genesis and Development of a Scientific Fact*, ed. Thaddeus J. Trenn and Robert K. Merton. (Chicago and London: The University of Chicago Press, 1979), see p. 39.

[136] Kuhn, *The Structure of Scientific Revolutions* (above, n. 32), p. 180. There is an interesting crossing over of ideas: Kuhn holds that science is characterized by „saltations" and is a community-based activity, while Mayr maintains that there is „gradualism" in science and it is based primarily on the activity of individuals. For an account of a scientific controversy as a group phenomenon, see Hull, *Science as a Process* (above n. 118), pp. 232-276.

[137] „Isolating a few individuals (the 'founders') from a variable population which is situated in the midst of the stream of genes which flows ceaselessly through every widespread species will produce a sudden change of the genetic environment of most loci. ... Indeed, it may have the character of a veritable 'genetic revolution'." Mayr, „Change of Genetic Environment and Evolution" (above, n. 71), p. 170.

Conclusion

As frequently pointed out in this discussion, one of the most characteristic features of Mayr's approach to the history of biology is related to the fact that he is dealing to a considerable degree with his own professional history. Furthermore, his main criterion for the selection of historical episodes is their relevance for modern biological theory. As W. F. Bynum and others have noted, the general impression of his reviewers is, that „one of the towering figures of evolutionary biology has now written a towering history of his discipline."[138] Bynum is referring to *The Growth of Biological Thought*, but this observation equally holds true for Mayr's other historical writings: [66]

> One must surely read this book [*One Long Argument*] not only for its content in itself, but for what it tells of its author. And certainly as one does so, one comes away full-handed. Many, if not all, of the disputes and controversies that have driven Mayr through his long intellectual life reappear, stated as forcefully and elegantly as ever.[139]

Up to this point most reviewers agree; the bone of contention is rather, how to evaluate his historical work, considering this observation. The two related characteristics in Mayr's work, I will call them subjectivity and presentism, stand in opposition to a widespread approach in the history of science, which is exemplified by Kuhn's suggestion that „insofar as possible ..., the historian should set aside the science that he knows. His science should be learned from the textbooks and journals of the period he studies."[140] There are, however, historians who consider the close connection between Mayr and the subject matter of his historical studies as an advantage.[141]

On the other hand, it is assumed that the connection between past and present must result in a distortion of the historical truth and lead to a historiographical fallacy, commonly referred to as „whig history." Butterfield (1931), who gave the term its now generally accepted meaning, believed that „real historical understanding is not achieved by the subordination of the past to the present, but rather by our making the past our

[138] Bynum, „On the Written Authority of Ernst Mayr" (above, n. 105), p. 585.

[139] Michael Ruse, Review of *One Long Argument*, *Amer. Scient.*, 81 (1993), 199.

[140] Kuhn, „The History of Science" (above, n. 77), p. 110.

[141] „Mayr is, of course, exceptionally well qualified to comment on his subject, for his affinity with Darwin is a deep one: he has worked as a field naturalist himself, and he served as one of the architects of the modern synthesis." Herbert, „Essay Review" (above, n. 60), 119. See also: Greene, „From Aristotle to Darwin" (above, n. 4), p. 282.

present and attempting to see life with the eyes of another century than our own."[142] Unfortunately, Butterfield's definition of what he considers whig history remains somewhat vague and modern authors have emphasized what they consider most important. Butterfield's „subordination of the past to the present" is referred to in relation to the selection of subjects (there are more biographies of Charles Darwin than let's say Louis Agassiz)[143], with respect to the evaluation of historical authors,[144] or, more generally, denotes all kinds of histories „with one eye, so to speak, upon the present."[145] The underlying tendency of whig historians is to produce a „historical account told from the viewpoint of [67] those in power"[146], leading to a „glorification of the present."[147] It is obvious that Mayr's strongly presentist approach to the history of biology can be called whiggish, if we apply the criteria of 'selection' or 'reference'. However, it might be worth mentioning that the program of writing a strictly historicist account of the history of science is challenged by various authors.[148] For Mayr it is not only legitimate, but necessary to compare the present situation with the past. Whiggish is only the evaluation of an author in terms of our time.[149]

I cannot discuss the whig/anti-whig controversy in any detail here, apart from the fact that Mayr has defended himself rather extensively against the charges of being whiggish.[150] Nevertheless, it may be useful to touch on some of the criticisms that are predominant in reviews of Mayr's writings. First, we encounter the notion that a historian can only write a true and convincing historical account, if she has no personal interest or

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[142] Herbert Butterfield, *The Whig Interpretation of History* (London: G. Bell and Sons., 1931), p. 16.

[143] G. C. Williams, Review of *The Evolutionary Synthesis* (above, n. 2), *Quart. Rev. Biol.*, 56 (1981), 445.

[144] „Whig history constructs the past as a series of steps leading to the present and awards points to past figures who 'got it right', or who thought most nearly like we do today." Bynum, „On the Written Authority of Ernst Mayr" (above, n. 105), p. 585.

[145] Butterfield, *The Whig Interpretation of History* (above, n. 142), pp. 31-32.

[146] Peter J. Bowler, *The Non-Darwinian Revolution. Reinterpreting a Historical Myth* (Baltimore and London: The Johns Hopkins University Press, 1988), p. 16.

[147] Butterfield, *The Whig Interpretation of History* (above, n. 142), p. v.

[148] For a critique of the historicist position, see David L. Hull, „In Defense of Presentism," *Hist. Theory*, 18 (1979), 1-15; Edward Harrison, „Whigs, Prigs and Historians of Science," *Nature*, 329 (1987), 213-214; Michael Ruse, „Booknotes," *Biol. Phil.*, 2 (1987), 377-381; Shapin, „Discipline and Bounding" (above, n. 111), p. 358. On Mayr and 'whiggism' see David L. Hull, „Ernst Mayr's Influence on the History and Philosophy of Biology: A Personal Memoir," *Biol. Phil.*, 9 (1994), 378-382.

[149] Mayr, „When is Historiography Whiggish?" *J. Hist. Ideas*, 51 (1990), 303.

interpretation of her own. Mayr, on the other hand, because he „has such strong interpretations of his own, ... cannot possibly convince everyone that he is right about everything."[151] It makes one wonder, which historian has ever been able to convince everyone that he is right about everything, but apart from this peculiar idea, it unquestionably poses certain dangers if the subject matter of historical scrutiny and the author are identical. On the other hand, this identity brings certain advantages with it, especially first-hand experiences of the period in discussion. Whether these personal memories ultimately result in a distorted picture of the past has to be decided in every particular instance. The notion that a scientific study can be conducted by a completely detached observer from a neutral standpoint has been shown to be impossible in physics, [68] and is also an illusion in historiography. The question is not whether, but which kind of interests are the underlying motivation for a historian. At this point, Mayr is ahead of his critics when he suggests that our understanding of the past always has a subjective component:

> The main reason, however, why histories are in constant need of revision is that at any given time they merely reflect the present state of understanding; they depend on how the author interpreted the current zeitgeist of biology and on his own conceptual framework and background. Thus, by necessity the writing of history is subjective and ephemeral.[152]

Second, the temporal proximity between the event and the historical analysis makes difficulties inevitable and will finally result in certain false assessments. But this applies to all historians when they discuss recent problems, regardless of whether they are personally involved or not:

> As long as the battle between Darwinism and Lamarckism was raging, it was quite impossible to undertake an unbiased evaluation of Lamarck. ... [The] definite refutation of Lamarck's theory of evolutionary causation clears the air.

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[150] Ibid. See also Phillip R. Sloan, „Essay Review: Ernst Mayr on the History of Biology," *J. Hist. Biol.*, 18 (1985), 145-153.

[151] Jane Maienschein, Review of *Toward a New Philosophy of Biology* (above, n. 2), *Isis*, 80 (1989), 569. Amazingly enough, the only really scornful attack on Mayr's historiographical abilities that I found was articulated by a paleontologist, Niles Eldredge. Mayr's *Growth of Biological Thought* is supposed to be „'bad' history," because it is „not the objective analysis of a detached historian." Eldredge, „A Biological Urge to Oversimplify," *The Philadelphia Inquirer*, 7 November 1982, section P, p. 3. A similar point (but without the sarcasm) is raised by John C. Greene, „Warfare of Nature," *Times Higher Education Supplement*, 29 May 1992, p. 25.

[152] Mayr, *Growth of Biological Thought*, p. 1.

> We can now study him without bias and emotion and give him the attention that this major figure in the history of biology clearly deserves.[153]

Third, Mayr is primarily interested in biological problems and not in, for instance, historiographical, sociological or psychological questions. Several authors have remarked that since the beginning of the professionalization of the history of science in the 1960s, a rift between two groups has developed, resulting from the heterogeneous professional backgrounds and interests of the people involved.[154] On the one hand, there are authors who had originally been biologists, and became interested in the history of their discipline only later on. On the other hand, there are authors who were trained as historians. Whereas the first group, the „biologists," tend to be laymen in history proper, the „historians" are in most cases laymen in biology. Different professional backgrounds obviously shape the historical perspective in both groups, but neither approach is necessarily superior. The great number of important books in the history of biology written by „biologists," documents how important this point of view can be. On the other hand, it cannot be denied that the writings of biologists in the history of science tend to have a strong „internalist" tendency and often neglect the professional, [69] cultural, and political context of science. Mayr's approach is that of a „biologist," it is „internalist" and typical for scientists who turn to the history of their discipline. I want to conclude my analysis with a quotation from a review by Douglas J. Futuyma, which gives a perceptive glimpse of Mayr's personality and style:

> One cannot help standing in awe of the Germanic capacity for vast, all-embracing synthesis: consider the lifelong devotion of Goethe to *Faust*, or Wagner's integration of the arts into a *Gesamtkunstwerk* in which all of human history and experience is wrought into epic myth. It is perhaps in this tradition that Ernst Mayr's *The Growth of Biological Thought* stands: a history of all of biology, a *Ring des Nibelungen* complete with leitmotivs such as the failures of reductionism, the struggle of biology for independence from physics, and the liberation of population thinking from the bounds of essentialism.[155]

Within this style of thinking Mayr has „to offer ... nothing less than a vision of biology that places neodarwinian evolutionary theory firmly at the centre."[156] There may be

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[153] Mayr, „Lamarck Revisited" (above, n. 29), p. 55.

[154] See, for example, Kuhn, „The History of Science" (above, n. 77); Mayr, *Growth of Biological Thought*, pp. 13-14; Harrison, „Whigs, Prigs and Historians of Science" (above, n. 148).

[155] Douglas J. Futuyma, „A Synthetic History of Biology" (above, n. 7), 842.

[156] Philip Kitcher, „The Importance of Being Ernst," *Nature*, 333 (1988): 25. See also, Greene, „From Aristotle to Darwin" (above, n. 4), p. 283.

other visions of biology, but few of them have as indefatigable and able representatives, as Darwinism has in Ernst Mayr.

Acknowledgments

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Appendix: Publications by Ernst Mayr in the History of Biology

The compilation of Mayr's historiographical writings is based on his private bibliography. The numbers given refer to this bibliography. In many cases a clear-[70]cut discrimination between historical and biological papers is not possible, because Mayr's historiographical method is largely based on the interconnection of past and present problems. I have tried to include all papers that contain a significant historical element.

1935 [73] Bernard Altum and the Territory Theory. *Proc. Linnaean Soc. New York*, Nos. 45, 46: 24-38.

1946 [188] The Naturalist in Leidy's Time and Today. *Proc. Acad. Nat. Sci. Philadelphia*, 98: 271-276.

1954 [257] Review of *Evolution: Die Geschichte ihrer Probleme und Erkenntnisse*, by Walter Zimmermann (Freiburg and München: Karl Alber, 1953). *The Scientific Monthly*, 79: 57-58.

1955 [264] Karl Jordan's Contribution to Current Concepts in Systematics and Evolution. *Trans. Roy. Entomol. Soc. London*, 107: 45-66.

1957 [285] Species Concepts and Definitions. In: *The Species Problem*, ed. E. Mayr. Publication no. 50 of the American Association for the Advancement of Science. Washington, D. C.: American Association for the Advancement of Science, pp. 1-22.

1959 [296] Isolation as an Evolutionary Factor. *Proc. Amer. Phil. Soc.*, 103: 221-230.

[297] Darwin and the Evolutionary Theory in Biology. In *Evolution and Anthropology: A Centennial Appraisal*, ed. Betty J. Meggers. Washington, D. C.: The Anthropological Society of Washington, pp. 1-10.

[298] Agassiz, Darwin, and Evolution. *Harvard Library Bulletin*, 13: 165-194.

[301] Where are we? *Cold Spring Harbor Symposia on Quantitative Biology*, 24: 1-14.

[302] Concerning a New Biography of Charles Darwin, and Its Scientific Shortcomings. Review of *Darwin and the Darwinian Revolution*, by Gertrude Himmelfarb (New York: Doubleday and Co., 1959). *Scientific Amer.*, 201 (November): 209-216.

1961 [311] Review of *Darwin's Biological Work: Some Aspects Reconsidered*, ed. P. R. Bell (Cambridge and New York: Cambridge University Press, 1959). *Isis*, 52: 433-435.

[317] Review of *Charles Darwin: The Founder of the Theory of Evolution and Natural Selection*, by Gerhard Wichler (New York: Pergamon Press, 1961). *Science*, 134: 607.

1964 [338] Introduction [pp. vii-xxvii], Bibliography, and Subject Index [pp. 491-513]. In *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, by Charles Darwin (London: John Murray, 1859). A Facsimile of the First Edition. Cambridge, Mass. and London: Harvard University Press.

[344d] Review of *Hundert Jahre Evolutionsforschung. Das wissenschaftliche Vermächtnis Charles Darwins*, ed. Gerhard Heberer und Franz Schwanitz (Stuttgart: Gustav Fischer, 1960). *Quart. Rev. Biol.*, 39: 384.

1965 [349] Foreword. In *Charles Darwin: A Scientific Biography*, by Sir Gavin de Beer. The Natural History Library. Garden City, New York: The American Museum of Natural History/Anchor Books, Doubleday, pp. VII-IX.

[360] Comments. In *Proceedings of the Boston Colloquium for the Philosophy of Science, 1962-1964*, ed. Robert S. Cohen and Marx W. Wartofsky. Boston Studies in the Philosophy of Science, vol. 2. New York: Humanities Press, pp. 151-156 [on Everett Mendelsohn, „Physical Models and Physiological Concepts: Explanation in Nineteenth Century Biology," ibid., pp. 127-150].

1966 [362] The Proper Spelling of Taxonomy. *Systematic Zoology*, 15: 88.

1968 [379] Illiger and the Biological Species Concept. *J. Hist. Biol.*, 1: 163-178.

1969 [382] Comments on 'Theories and Hypotheses in Biology'. In *Proceedings of the Boston Colloquium for the Philosophy of Science 1966/1968*, ed. Robert S. Cohen and Marx W. Wartofsky. Boston Studies in the Philosophy of Science, vol. 5. Dordrecht: Reidel, pp. 450-456.

[388] Erwin Stresemann zum 80. Geburtstag. *J. für Ornithologie*, 110: 377-378.

1971 [407] Essay Review: Open Problems of Darwin Research. *Stud. Hist. Phil. Sci.*, 2: 273-280.

1972 [411] The Nature of the Darwinian Revolution: Acceptance of Evolution by Natural Selection Required the Rejection of many Previously Held Concepts. *Science*, 176: 981-989.

[412] Review of *Oskar Heinroth. Vater der Verhaltensforschung, 1871-1945*, by Katharina Heinroth, with a preface by Konrad Lorenz. Große Naturforscher, Bd. 35 (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1971). *Isis*, 63: 127-128.

[413] Lamarck Revisited. *J. Hist. Biol.*, 5: 55-94.

[414] Sexual Selection and Natural Selection. In *Sexual Selection and the Descent of Man 1871-1971*, ed. Bernard Campbell. Chicago: Aldine Publishing Co., pp. 87-104.

1973 [418] Alden Holmes Miller (1906-1965). *Biogr. Mem. Nat. Acad. Sci.*, 43: 177-214.

[419] Erwin Stresemann. *Ibis*, 115: 282-283.

[423] Essay Review: The Recent Historiography of Genetics [review of *Origins of Mendelism*, by Robert C. Olby (London: Constable, 1966) and *The Origins of Theoretical Population Genetics*, by William B. Provine (Chicago and London: The University of Chicago Press, 1971)]. *J. Hist. Biol*., 6: 125-154.

[424] In Appreciation – David Lack. *Ibis*, 115: 432-434.

[427a] Robert Cushman Murphy (1887-1973). *Year Book Amer. Phil. Soc. 1973*, pp. 131-135.

1974 [428b] Comment. *Curr. Anthropol*., 15: 227-228 [on Derek Freeman, „The Evolutionary Theories of Charles Darwin and Herbert Spencer," ibid., pp. 211-221].

[435] Evolution and God [review of *Darwin and His Critics: The Reception of Darwin's Theory of Evolution by the Scientific Community*, by David L. Hull (Cambridge, Mass.: Harvard University Press, 1973)]. *Nature*, 240: 285-286.

1975 [437] Erwin Stresemann. *Verhandlungsberichte der Deutschen Zoologischen Gesellschaft*: 411-412.

[442] Foreword. In *Ornithology from Aristotle to the Present*, by Erwin Stresemann. Translated by Hans J. and Cathleen Epstein, ed. G. William Cottrell. Cambridge, Mass.: Harvard University Press, pp. IX-X.

[443] Epilogue: Materials for a History of American ornithology. In *Ornithology from Aristotle to the Present*, by Erwin Stresemann. Translated by Hans J. and Cathleen Epstein, ed. G. William Cottrell. Cambridge, Mass.: Harvard University Press, pp. 365-396, 414-419.

[444] Sars, Michael. In *Dictionary of Scientific Biography*, ed. Charles Coulston Gillispie, vol. 12. New York: Charles Scribner's Sons, pp. 106-107.

[445] Semper, Carl Gottfried. In *Dictionary of Scientific Biography*, ed. Charles Coulston Gillispie, vol. 12. New York: Charles Scribner's Sons, p. 299.

[450] Ridgway, Robert. In *Dictionary of Scientific Biography*, ed. Charles Coulston Gillispie, vol. 11. New York: Charles Scribner's Sons, pp. 443-444.

1976 [459b] Whitman, Charles Otis. In *Dictionary of Scientific Biography*, ed. Charles Coulston Gillispie, vol. 14. New York: Charles Scribner's Sons, pp. 313-315.

[459c] *Evolution and the Diversity of Life. Selected Essays.* Cambridge, Mass. and London: The Belknap Press of Harvard University Press.

1977 [460] Darwin and Natural Selection. How Darwin May Have Discovered His Highly Unconventional Theory. *Amer. Scientist*, 65: 321-327.

[462] Review of *Ontogeny and Phylogeny*, by Stephen Jay Gould (Cambridge, Mass. and London: The Belknap Press of Harvard University Press, 1977). *MCZ Newsletter*, 7: 3.

[467] The Study of Evolution, Historically Viewed. In *The Changing Scenes in the Natural Sciences, 1776-1976*, ed. Clyde E. Goulden. Academy of Natural Sciences, Special Publication 12. Philadelphia: Academy of Natural Sciences, pp. 39-58.

1978 [469] Origin and History of Some Terms in Systematic and Evolutionary Biology. *Systematic Zoology*, 27: 83-88.

1979 [479] Review of *Rudolf Leuckart. Weg und Werk*, by K. Wunderlich. Biographien bedeutender Biologen, Bd. 2 (Jena: Gustav Fischer, 1978). *Isis*, 70: 336.

[480] Obituary: Alexander Wetmore. *Ibis*, 121: 519-520.

1980 [482] The Evolution of Darwin's Theory. Review of: *A Delicate Arrangement: The Strange Case of Charles Darwin and Alfred Russel Wallace*, by Arnold C. Brackman (New York: Times Books, 1980); *Darwin and the Beagle*, by Alan Moorehead (London: Penguin, 1980); *The Voyage of Charles Darwin: His Autobiographical Writings*, ed. Christopher Ralling (New York: Mayflower Books, 1979); *The Collected Papers of Charles Darwin*, ed. Paul H. Barrett. With a Foreword by T. Dobzhansky. 2 vols. in 1 (Chicago: University of Chicago Press, 1977). *Washington Post, Book World*, 22 June 1980, pp. 4 and 10.

[484a] *The Evolutionary Synthesis*, ed. Ernst Mayr and William Provine. Cambridge, Mass.: Harvard University Press.

[484b] Prologue: Some Thoughts on the History of the Evolutionary Synthesis. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, pp. 1-48.

[484c] The Role of Systematics in the Evolutionary Synthesis. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, pp. 123-136.

[484d] Botany. Introduction. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, pp. 137-138.

[484e] Paleontology. Introduction. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, p. 153.

[484f] Morphology. Introduction. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, p. 173.

[484g] Germany. Introduction. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, pp. 279-283.

[484h] France. Introduction. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, p. 309.

[484i] The Arrival of Neo-Darwinism in France. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, p. 321.

[484k] How I became a Darwinian. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, pp. 413-423.

[484l] G. G. Simpson. In *The Evolutionary Synthesis. Perspectives on the Unification of Biology*, ed. Ernst Mayr and William B. Provine. Cambridge, Mass. and London: Harvard University Press, pp. 452-466.

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[491] *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*. Cambridge, Mass. and London: The Belknap Press of Harvard University Press.

[492] Epilogue. *Biol. J. Linnean Soc.*, 17: 115-125.

[497] Review of *A Concordance to Darwin's Origin of Species, First Edition*, ed. Paul Barrett, Donald J. Weinshank, and Timothy T. Gottleber (Ithaca, N. Y.: Cornell University Press, 1981). *Isis*, 73: 476-477.

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1983 [506] Geleitwort. In *Allgemeine Betrachtungen über die Triebe der Thiere, hauptsächlich über ihre Kunsttriebe*, by Hermann Samuel Reimarus (Facsimile of the 2nd. ed., 1762), ed. Jürgen von Kempski. Veröffentlichung der Joachim Jungius-Gesellschaft der Wissenschaften Hamburg, Nr. 46. 2 vols. Göttingen: Vandenhoeck & Ruprecht, 1982, pp. 9-18.

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[510] Darwin, Intellectual Revolutionary. In *Evolution from Molecules to Men*, ed. D. S. Bendall. Cambridge: Cambridge University Press, pp. 23-41.

[514] Review of *Allgemeine Betrachtungen über die Triebe der Thiere, hauptsächlich über ihre Kunsttriebe*, by Hermann Samuel Reimarus (Facsimile of the 2nd. ed., 1762), ed. Jürgen von Kempski. Veröffentlichung der Joachim Jungius-Gesellschaft der Wissenschaften Hamburg, Nr. 46. 2 vols. (Göttingen: Vandenhoeck & Ruprecht, 1982). *Isis*, 74: 596-597.

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