Ernst Mayr's Influence on the History and Philosophy of Biology: A Personal Memoir

DAVID L. HULL

Department of Philosophy
Northwestern University
Evanston, IL 60208

ABSTRACT: Mayr has made both conceptual and professional contributions to the establishment of the history and philosophy of biology. His conceptual contributions include, among many others, the notion of population thinking. He has also played an important role in the establishment of history and philosophy of biology as viable professional disciplines.

KEY WORDS: essentialism, history, philosophy, population thinking, whiggism.

When I received my Ph.D. from Indiana University in 1964, quite a bit had been written on the history of biology but very little on anything that might be termed the philosophy of biology. In 1929 J. H. Woodger published a book on the philosophy of biology written in the discursive style common at the time, but soon thereafter he was bitten by the bug of symbolic logic. All his subsequent works were written using this notation (e.g. Woodger 1937, 1952). It seemed that in this literature, too often the medium became the message. Woodger pursued logical snarls with great enthusiasm, no matter how irrelevant they might be to biology, but dismissed empirical issues with an indignant snort, no matter how central they might be to the science that he claimed to be professing the philosophy of. As a result, Woodger had very little impact on biology or biologists. Back in the early sixties, I was not especially intrigued by this way of doing philosophy of biology, nor were very many other philosophers of biology. To this day the formal approach to philosophy of biology remains a minority position in philosophy of biology.

Morton Beckner might well have served as the catalyst for an upsurge in the philosophy of biology. His *The Biological Way of Thought* (1959) was a revision of a dissertation that he wrote under the direction of Ernst Nagel. As a result his methods and conclusions were of the sort that budding young philosophers of science might find promising. I for one was extremely excited by his book. However, my attempts to engage him in a correspondence were met with no encouragement. Goudge's *The Ascent of Life* (1961) was also easily accessible. However, in this book Goudge was interested primarily in the implications of evolutionary theory for the human species, an emphasis that had long irritated me. As I saw it, the human species is one species out of millions. In discussing evolution as a biological phenomenon, it deserves that much attention and no more. What I was searching for, without realizing it, was a mentor, someone already well-established in the field, a biologist, a philosopher, I did not care.

While still in graduate school, I submitted a manuscript on G. G. Simpson's principle of monophyly to *Systematic Zoology*. The editor at the time was Libbie Henrietta Hyman. Because I had used her text in my comparative anatomy laboratory, I felt as if I knew her. She returned my manuscript rejected without even sending it out to be reviewed. I asked one of my professors, Michael Scriven, for advice. He sent the manuscript to Simpson with a note explaining the situation. Simpson returned the manuscript to Hyman, and this time she had it reviewed. The referee's comments were sufficiently positive that she published the paper (Hull 1964). Although Simpson wrote me a very supportive letter in response to this manuscript, he begged off when Indiana University Press approached him to read my dissertation to see if it might be suitable for publication. Through the years I had only a very limited correspondence with Simpson. In fact, I never met him until July 1978 when I interviewed him in his Tucson home for a book I was writing.

Indiana University Press approached a second eminent biologist about reading my dissertation. This reviewer provided five pages of detailed comments. In general, he found the manuscript too heavy on logic and too short on biology. He recommended that I extract a series of journal articles from my dissertation rather than rewriting it as a book. This reviewer sounded suspiciously like Ernst Mayr. I was treating biological species as if they were "classes, sets of individuals like classes or sets of any kind of inanimate objects" (April 27, 1965). I had also mistakenly attributed the Biological Species Concept to Dobzhansky instead of Mayr.

Shortly thereafter I found myself at a meeting with Mayr. I sat down next to him at a session and at the break thanked him for his anonymous review. He did not hesitate or feign ignorance but engaged me in a discussion of the points that he had criticized in my manuscript. Shortly after I returned home, I received a packet of Mayr's reprints and a request for reprints of my own publications. He also asked for references to philosophers who discussed certain issues because in the future he intended to move "more and more in the direction of the history and philosophy of biological science" (June 2, 1966). He ended one of his letters with the subtle hint that it "might not be a bad idea if some day some philosopher" would write on the type of problems that an "operational approach" engenders in systematics (August 16, 1966). I complied (Hull 1968).

In reading over my early correspondence with Mayr, I can clearly see two workers engaged in mutual exploitation. Mayr at the time was a well-established biologist, a leader in his field, but he was expanding into foreign territory, while I was a young philosopher attempting to write papers that biologists would find comprehensible and useful. Although our relationship was far from symmetrical, Mayr was never condescending. He said lots of hard things about "philosophers," or worse yet, "logicians," but I agreed with most of his criticisms, and he presented them in ways to indicate that I was exempt or, if I took his advice, would eventually find myself exempt. The one issue that Mayr found very important at the time but which I found myself unable to appreciate was the superiority of the ascending, empirical, inductive approach in science over the descending, essentialist, deductive approach. I also did not understand the cues Mayr was giving me about species not being "classes" or "sets." As I saw it:

> ...taxa are not *just* classes. They are *at least* classes but not *just* classes. What I mean by this is that a species is not just a collection of individuals and their simple properties (such as, wing length, number of body bristles, etc.). Their relational properties must also be taken into account. Individual organisms are descended from one another, interbreed with one another, compete with each other for food and mates, and so on. It is these relational properties that make species "organic wholes" (July 15, 1966).

Mayr also urged me to discuss this issue with Michael Ghiselin. As it turned out, I was already communicating with Ghiselin because I had been sent his 1966 paper to review for *Systematic Zoology*.

Mayr did not always find himself in total agreement with my views. For example, he did not like the manuscript of my *Philosophy of Biological Science* (1974) that I sent him prior to publication. He found it too laden with the sort of arguments that philosophers (and only philosophers) find interesting. In this connection, he quoted Schopenhauer's characterization of Hegel's philosophy: "the conscious and continuous misuse of a terminology especially invented for that purpose" (November 15, 1971). In arguing for his position, Mayr was relentless, but he was willing to continue our correspondence in spite of any disagreements we might have. He never went off in a huff. Mayr struck many of his contemporaries as being formidable. He struck me that way too but not so formidable that I felt unable to argue with him, once wagging my finger in front of his nose to emphasize a point. I noticed a slight smile creep across his face in appreciation of my having the courage to stand up to him. On the occasion of Mayr's receiving the Balzan Prize in 1983, Stephen Jay Gould (1984) ended his eulogy by relating an incident that occurred two years earlier at a talk given by a graduate student on some arcane issue in systematics. Mayr:

> ...joined the subsequent discussion with a vigor and definiteness that was simply intimidating. Initially, I became annoyed that he would so assert his authority against such a younger colleague. But then I understood that I had it all backwards and that I was seeing the essence ... of Mayr's greatness. He remains so in love with his subject, so enthusiastic about its promise and intellectual content, that he couldn't hold back. He was urging with all the verve of a graduate student because, by God, he remains one himself in heart, energy, and commitment.

One important lesson I learned from Mayr is that, if you are not willing to push your own ideas long and hard, no one else is likely to do it for you. If your views are not important enough for you to be committed to them, they are not important enough for others to take seriously.

Mayr's assistance was not all intellectual. In 1967 I was up for tenure in a department of philosophy on the basis of six papers, four published in biological journals, one in a history journal, and only one in a philosophy journal. In such circumstances, I needed an eminent biologist to vouch for the substance of the biological papers. Mayr happily complied. A couple of years later, while working on an anthology dealing with Darwin and his critics, I asked him to check the translations I had made of two German papers that I was including in this anthology. As presumptuous as this request now strikes me, Mayr complied without a whimper of protest.

Mayr also taught me by example some strategies in scientific debate. For example, because he found the writings of Paul Ehrlich so "unerhlich," he never bothered to respond to Ehrlich's criticisms. In writing a paper on the philosophy of systematics, I was having a very difficult time in characterizing the views of Donald Colless in such a way that he recognized them as his own. Out of frustration, I simply deleted most references to his work. Without realizing it, I was following Mayr's lead. The most powerful tool that any author possesses is silence. (Happily I eventually came to understand Colless's views.)

I recount my early relations with Mayr, not because I think that I am in the least bit special. To the contrary, he had similar relations with nearly all early historians and philosophers of biology. I was by far not his only correspondent. Those of us engaged in the study of biology as a scientific discipline are foolish if we pretend that we have anything like the professional influence of the biologists whose work we study. At times, I think that we are about as consequential as gnats scurrying about on the hide of a tough old rhinoceros. Mayr used his not inconsiderable power to promote the cause of history and philosophy of biology. He helped establish the study of biology as a viable and distinct professional discipline, characterized now by several journals devoted specifically to the history, philosophy and social studies of biology as well as our own society – the International Society for the History, Philosophy, and Social Studies of Biology. Mayr and Marjorie Grene were chosen to be retroactive honorary presidents of the society.[1]

MAYR'S HISTORY OF SCIENCE

Of course, Mayr's support was not entirely disinterested. It also had an element of what we termed in the sixties "cooptation." One reason he helped establish the study of biology as a profession was that he himself was increasingly engaged in this activity. In 1976 Mayr published an anthology of his papers that included sections on the history of biology and the philosophy of biology, followed by his monumental *The Growth of Biological Thought* in 1982, his *Toward a New Philosophy of Biology* six years later, and an analysis of Darwin's argument in the *Origin of Species* in 1991. He is currently working on yet another book!

On the face of it, *The Growth of Biological Thought* is a comprehensive history of the sort published by Erik Nordenskiöld in 1928. Nordenskiöld's book is chronological, starting with classical antiquity and progressing through the Renaissance to modern biology. Mayr's book is organized initially with respect to topics – beginning with the diversity of life (taxonomy), followed by evolution and, finally, variation and its inheritance – but, within each of these sections, Mayr treats the topics chronologically. Thus, one might be surprised to read in his preface that this "volume is not, and this must be stressed, a history of biology." It is a "developmental, not a purely descriptive, history. Such a treatment justifies, indeed necessitates, the neglect of certain temporary developments in biology that left no impact on the subsequent history of ideas" (Mayr 1982:vii).

The contrast between "purely descriptive" and "developmental" histories calls for some discussion. The issue is "whiggism." Other than making errors in transcription or citing the wrong edition of a publication, about the worst thing that historians can do as historians is to allow "whiggism" to creep into their works, but this term covers a lot of territory. In its most pernicious form, whiggism is the denigration of early scientists for not holding the views on empirical or theoretical matters that we currently hold or reading our current understanding into the past. For example, many early biologists from Aristotle to Agassiz rejected the evolution of species, and they were not fools for doing so. Furthermore, the "evolution" that they rejected was very different from evolution as we understand it today. Just as at the outbreak of hostilities in 1914, no one declared that World War One had begun, Mendel did not think of himself as initiating what came to be known as Mendelian genetics.

Certainly Mendel did not term his research "genetics," let alone "Mendelian genetics," but we do. Historians write *about* earlier times and people, but they are writing *for* present-day readers. Hence, I continue to be puzzled by those historians who claim that we cannot refer to anyone as being a "biologist" until the term "biologie" was coined in 1802 or anyone a "scientist" until 1834 when Whewell coined this term.[2] If this peculiar terminological convention is designed to keep us from assuming that Aristotle, Harvey and Linnaeus were all engaged in essentially the same activity, the goal is desirable but the method is misconceived. In the first place, just because "biologie" and "biology" are formed from the same root, it does not follow that they mean the same thing either then or now. For example, "essence" in French looks very much like "essence" in English, but these two terms do not come close to having the same range of meanings in the two languages (e.g., "gasoline" in French). For those authors who write in English, the issue is the introduction of "biology" into English, not "biologie" into French or German. Even within the *same* language, problems arise. Certainly the activities in which "biologists" have engaged vary through time, but they also are quite different at anyone time. Certainly 19th and 20th century biologists were engaged in quite different activities, but in 1802 various "biologists" were also engaged in quite different activities. Declining to term anyone before 1802 a "biologist" does not even begin to solve these terminological problems.

Strangely enough, among those historians who are most vocal in condemning whiggish tendencies with respect to the *content* of science can be found some who treat *morals* in as whiggish a way as can be imagined. Although they would not dream of chastising Lamarck for thinking that acquired characters can be inherited, they freely point out instances when earlier scientists did not behave according to the historian's own code of moral beliefs. A genre that has become extremely popular in the past couple of decades consists in documenting how sexist, racist, elitist, etc. scientists in previous generations have been, not sexist given the views of the scientist's own day, but sexist from the historian's own perspective. The only justification that I can think of for such blatant whiggism is the belief that morals are universal, regardless of time, place, or circumstance. Even though we may be mistaken to some extent about what these moral universals actually are, they nevertheless exist. But the same observation can be made with respect to empirical and theoretical issues, and the existence of moral universals is a good deal more problematic than the existence of scientific laws. Anyone who rejects scientific laws is likely to be hard put to defend moral universals (for a discussion of moral whiggism, see Gould's (1993), "The Moral State of Tahiti – and of Darwin").

In his *The Growth of Biological Thought*, Mayr is not "whiggish" in either of the preceding senses. He does not complain that earlier scientists held views different from our own or read them as if they did. However, "whiggism" is also taken to denote writing history of science as if it homed in on our present-day beliefs. Officially, historians are committed not only to evaluate the science of a period in its own terms but also to follow all avenues of research regardless of where they might lead. The history of science, like biological evolution, is more like a bush than a tree. Perhaps in the best of all possible worlds, historians can and should give equal weight to all scientists as well as everything that these scientists may have done and said, but histories also have to be read, and readers want historians to provide them with paths through the woods (see for example Kay's 1993 review of Holmes' 1992 book). Different authors might trace different paths. For example, Adrian Desmond (1989) tells a nonstandard story of the reception of evolution in early nineteenth-century England when he expands his interest beyond the usual big names to include numerous lesser scientists of the day. But a history that provides no paths whatsoever is likely to be incomprehensible not to mention unread.

People who read histories of science are likely to know most about the science of their own day, at least textbook versions. They have a rough idea of which ideas turned out to have had an impact and which just disappeared. Any historian who ignores this fact about his potential readers is likely not to communicate very successfully. Martin Rudwick (1985) was fortunate that very few of his readers ever heard of the "Great Devonian Controversy." I for one had not. I had no idea who turned out to be right on which issues. Certainly I found Henry T. De le Beche a much more likeable human being than the imperious Roderick I. Murchison, but in science as elsewhere nice guys do not always finish first. Because Rudwick's readers were likely to be ignorant of the science under investigation, Rudwick was spared one source of misunderstanding – his readers viewing the past from present-day knowledge. Thus, Rudwick was able to write his narrative almost as if it were a murder mystery.

An equally serious source of bias is the author pruning the tangled path of history on the basis of who contributed to current understanding. As Desmond (1989) demonstrates, one can trace paths without limiting oneself only to those paths that led to present-day knowledge. Certainly Rudwick knew who, in the Great Devonian Controversy, turned out to produce important advances and who had no impact or lead us down dead ends. With respect to the scientific points at issue, Rudwick is relentlessly non-whiggish, so much so that periodically, while reading his book, I found myself discombobulated. I could not see the branches for the twigs. Twigs, twigs, twigs. All I could see were twigs. I am glad that at least one historian has written a totally non-whiggish book so that we can all see what one looks like, but I am not so sure that I want all histories to be so impartial. At the risk of sounding like a pluralist, I think that important roles exist for both sorts of histories – those that tell it like it was as exhaustively and impartially as is possible and those who make greater concessions to the present-day reader (for further discussion, see Hull 1979).

Mayr explicitly notes that his book is of the second sort. If only descriptive histories count as genuine history, then he has not written a history of biology. However, I see no reason to limit the term "history" so narrowly. Developmental histories such as the one produced by Mayr certainly count as genuine history. But yet another source of bias confronts Mayr because not only is he writing about the course of biology but also he was one of the primary actors in this play. Historians are supposed to mask their own preferences on the controversies that they chronicle, and some are very good at it. For example, I had not noticed the slightest hint of John Greene's own religious beliefs in his writings through the years until he himself made them explicit (Greene 1981). Mayr is not very good at masking his own beliefs. I myself see nothing wrong with historians being up front about their own views, though I would just as soon that they did not allow them to intrude too centrally or too often in their published works. As a contribution to the secondary literature, *The Growth of Biological Thought* exhibits Mayr's own preferences a bit too obviously and pervasively, but Mayr's book is also a contribution to the *primary* literature. After all, Mayr himself is an actor in the some of the stories that he tells. The last thing that I would want Mayr to do is to hide his own preferences behind a feigned mask of impartially. I want to know what Mayr thinks about the issues that he addresses, and I doubt that I am alone in this predilection.

Thus, in the most negative senses of the term, Mayr's *The Growth of Biological Thought* is not "whiggish." But, in the two senses specified above, it is. Mayr explicitly notes that he is pruning the history of biology to make certain developments stand out more clearly. I see nothing wrong with developmental histories, as long as different historians trace different paths. By reading these alternative and complementary histories, a reader can get a fuller picture of what went on. Because Mayr *is* Mayr, his evaluations will also reflect his own priorities as an evolutionary biologist, priorities that historians who themselves have not been engaged in any of the controversies that they chronicle will not necessarily share. There are good reasons for third parties to discuss controversies. They bring with them third-party objectivity – which is not to say absolute objectivity – but histories written by protagonists are also valuable. Numerous historians have reviewed Mayr's *The Growth of Biological Thought* but only a couple have protested about its "whiggishness." This reticence may be a result of their viewing Mayr's book as a developmental history or possibly of the appreciation, affection, and even fear that they feel toward him.[3]

PHILOSOPHY OF BIOLOGY

Happily the issue of whiggism is not all that central to philosophy of biology. Gould (1984:257) complains that Mayr (1982) tends to view the 'entire pageant of historical biology as a great battle between Platonic "essentialists" who focus on unvarying types or, if evolutionarily inclined, must view the process as saltation from one essence to another, and "population thinkers" who understand that variation is irreducible reality and become receptive to a Darwinian model of change.' Perhaps the contrast between essentialism and population thinking was not as pervasive throughout the history of biology as Mayr claims. I think that it was at least one major theme. But from the perspective of the philosophy of biology, this historical issue is beside the point. Whether major, minor or non-existent, Mayr's contrast is sufficiently central to present-day biology to warrant discussion. To my way of thinking, it is *the* central conceptual change in recent biology.

According to Mayr (1988:193), population thinking requires that we view the living world as consisting not of "types but of variable populations in which each individual is unique." For Mayr, "population" has both a particular and a general sense. In the particular sense, populations are groups of organisms more inclusive than kinship groups and less inclusive than entire species. Even though a hive of bees is composed of numerous organisms, it does not serve the same function as populations in the evolutionary process, because it is too restricted. Species, to the contrary, are usually composed of numerous populations. They are too unrestricted. Populations in this sense are spatiotemporally localized groups of organisms all belonging to the same species. They occupy the same pond, valley, forest or what have you.

If populations are to serve the functions that they do in the evolutionary process, they must be genetically variable. More importantly, this variation cannot be conceived in terms of typical and deviant members. As Mayr has argued for decades, one of the chief challenges that Darwin's theory posed to the thought of his age (and ours) is the replacement of essentialism with population thinking. Essentialists acknowledge that entities in the natural world vary. Not every star, atom, or organism perfectly exemplifies its kind, but any variation from type is a *deviation*, and science deals primarily with regularities among typical individuals and only secondarily with deviations from these norms. Deviations can be explained, but solely in terms of accidents that result in departures from the natural order. In the populations of organisms that function in the evolutionary process, the essentialist distinction between essence and accident makes no sense. Variations are not deviations.

Nor can the essentialist position be saved by resorting to clusters. In the nineteenth century, William Whewell (1840) attempted to deal with variation in terms of "exemplars," particular instances that delineate borders between natural kinds (see also Herschel 1830:94). In response, Darwin noted, 'On my theory an "*Exemplar*" is no more wanted than to account for the likeness of members of one Family' (Darwin Archives, volume 105.5, item 143). A century later, another philosopher, Ludwig Wittgenstein, returned to this metaphor. Wittgenstein argued that natural kinds need not be defined in terms of universally covariant traits. Instead, they can be defined only in terms of statistically covariant traits. In order for a horse to count as a horse, all it need do is to exhibit enough of the most important characteristics of horses. Comparable observations hold for games, truth and beauty.

Wittgenstein and his followers treat family resemblances in a *metaphorical* sense of "family." Not all games need to be descended from ancestral games to count as games. Darwin and Mayr treat the notion of family resemblance *literally*. Entities belong together because of common descent. Because of common descent, the traits that characterize these entities tend to covary, but descent is primary, not the covariation of traits. Thus, an organism that possesses none of the traits most characteristic of its species may still belong to that species. More importantly, the covariation that characterizes biological populations need not be unimodal. When mapped, characteristics need not form a distribution with a single hump. The notion of a "wild type" is a fiction. It generally denotes those forms found nearest major roads. Instead, the traits that characterize species tend to form distributions with several peaks. They are multimodal, and any mathematical function that obscures this characteristic of species destroys information that is crucial in understanding the evolution of species.

One consequence of Mayr's population thinking with respect to species (as the things that evolve primarily through the action of natural selection) is that species have no essences. In Darwin's day Marx made a big deal about the human species having no essence. More recently Sartre echoed this claim. But evolutionary biologists are not in the least surprised by this apparently momentous proclamation. If no biological species has an essence and *Homo sapiens* is a biological species, then one need not be a logical wiz to conclude that *Homo sapiens* has no essence. If it has no essence, then those ethical systems that turn on human nature are in real trouble. Either there is no foundation to ethics or else they are talking about "human" nature in some other sense.

Mayr thinks that his notion of a population is so important that he elevates it to a metaphysical category between class and individual. Traditionally, biological species have been treated as classes defined in terms of character covariation. Changing from this covariation being universal to statistical was a step in the right direction but did not go far enough because of the priority of descent – and descent is not just another character. Biological taxa, whether particular lineages or more inclusive chunks of phylogenetic trees, are necessarily spatiotemporally localized. One might be willing to treat biological taxa as classes, but these "classes" are not the sort of classes that can function in laws of nature – and Mayr thinks that there are few, if any, generalizations in biology that count as "laws" of the sort that can be found in physics. However, if laws of nature are held to be spatiotemporally unrestricted and taxa are spatiotemporally restricted, it follows that no law of nature can include reference to particular taxa.

The contrast is marked in biology by the contrast between monophyletic and paraphyletic or polyphyletic taxa. At the level of characters this distinction is mirrored in the contrast between homologies and homoplasies. "Monophyly" and "homology" are wedded to the genealogical perspective. No genuine laws of nature can include reference to monophyletic taxa or homologous characters. Only groups and traits not tied to genealogy have any hope of functioning in laws of nature. For example, evolutionary biologists are currently trying to explain why a particular chromosomal pattern governing sex seems to emerge so frequently (Morell 1994). XY sex determination is a homoplasy. As such, it is at least a candidate for a general explanation in terms of general evolutionary forces. The vertebrate kidney, as an homology, is not.

Another metaphysical category is the individual – entities that are spatiotemporally localized. Individuals may exemplify natural kinds, but they themselves are particulars, instances, and no more. Because of the similarities between species as evolving lineages and the traditional characteristics of individuals, some biologists and philosophers have argued that species are individuals – not organisms but individuals in the generic sense of this term (Ghiselin 1966, 1974, Hull 1976). Although Mayr finds this position appealing, he thinks a better alternative is to introduce a third category between class and individual – populations in a general sense. Any group whose constituent entities are classified together primarily because of descent counts as a population. Species clearly count as populations in this general sense, but Mayr thinks that other entities traditionally treated as "groups" equally count as populations in his generic sense. For example, Mayr argues that scientific research programs such as Darwinism are also best construed as populations. Like biological populations, "Darwin's evolutionary paradigm is highly composite," and the fates of these various parts have been highly variable (Mayr 1988:165, 196, 211).

Mayr is willing to extend population thinking beyond evolving species but only so far. For example, Mayr contends that natural selection is of the *essence* of Darwin's theory. Anyone who did not (or does not) view natural selection as the primary, directive force in biological evolution cannot count as a Darwinian, regardless of what the people involved might have claimed. As a result, most of the people who called themselves "Darwinians" in Darwin's day were not true Darwinians. Here, Mayr the historian confronts Mayr the scientist.

CONCLUSION

In the early days, when historians of science and philosophers of science were striving to become "professional," we were more than a little territorial. Philosophers who ventured into history and historians who turned their hand at a little philosophy were likely to be ignored or treated quite harshly. For example, L. Pearce Williams (1975) was so irate over Joseph Agassi's (1972) book on Michael Faraday and the section on Faraday in a book by William Berkson (1974) that he was moved to entitle his review "Should Philosophers Be Allowed to Write History?" His answer was firmly in the negative. Kuhn (1970:198) was especially irked by philosophers. They seemed peculiarly unable to understand his philosophical views. Philosophers in turn tend to cast aspersions on the mental capacities of historians. Historians are good at collecting data, but they just don't seem able to comprehend even the most basic philosophical distinctions (see also Richards 1993).

If we have treated each other less than charitably, we have been even less receptive when scientists ventured into our emerging territories. One of the explicitly stated goals of professional historians of science was *not* to write histories the way that scientists have written them in the past. However, history of science and philosophy of science are now sufficiently secure as professional disciplines that we can afford to tone down our exclusionary rhetoric. Being trained initially in one field does not preclude a scholar from making important contributions in another. I am happy to say that those of us in the history and philosophy of biology have encouraged biologists to join in our activities, and biologists have been extremely receptive of the work produced by historians and philosophers of biology. Ernst Mayr has played a central role in this continuing rapprochement.

NOTES

[1] Although Marjorie Grene is somewhat older than I am, I have always considered her a professional contemporary because we both began trying to establish ourselves in the philosophy of biology at roughly the same time (Grene 1958). I am also happy to say that we became good friends. For a similar reminiscence on the early days of the history and philosophy of biology, see Ruse (1993).

[2] I have since run across a reference to "biologie" being used in 1800. As a result, the ranks of biologists have been increased.

[3] Mayr (1990) cites W.F. Bynum (1985) and Peter Bowler (1988) as two historians who complained of his whiggish tendencies. See Mayr (1990) for his response.

REFERENCES

Agassi, J.: 1972, *Faraday as a Natural Philosopher*, University of Chicago Press, Chicago.
Beckner, M.: 1959, *The Biological Way of Thought*, Columbia University Press, New York.
Berkson, W.: 1974, *Fields of Force*, Routledge and Kegan Paul, London.

Bowler, P. J.: 1988, *The Non-Darwinian Revolution*, Johns Hopkins University Press, Baltimore.
Bynum, W. F.: 1985, On the Written Authority of Ernst Mayr, *Nature* **317**, 585.
Desmond, A.: 1989, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London*, The University of Chicago Press, Chicago.
Ghiselin, M.: 1966, On Psychologism in the Logic of Taxonomic Principles, *Systematic Zoology* **15**, 207–215.
Ghiselin, M.: 1974, A Radical Solution to the Species Problem, *Systematic Zoology* **23**, 536–544.
Goudge, T. A.: 1961, *The Ascent of Life*, University of Toronto Press, Toronto.
Gould, S. J.: 1984, Balzan Prize to Ernst Mayr, *Science* **223**, 255–257.
Gould, S. J.: 1993, *Eight Little Piggies*, W. W. Norton & Company, New York and London.
Greene, J. C.: 1981, *Science, Ideology, and World View*, University of California Press, Berkeley.
Grene, M.: 1958, Two Evolutionary theories, *British Journal for the Philosophy of Science* **9**, 110–127 & 185–193.
Herschel, J.: 1830, *Preliminary Discourse on the Study of Natural Philosophy*, Longman Rees, Orme, Brown and Green, London.
Holmes, F. L.: 1992, *Hans Krebs: The Formation of a Scientific Life, 1900–1933*, Oxford University Press, New York.
Hull, D. L.: 1964, Consistency and Monophyly, *Systematic Zoology* **13**, 1–11.
Hull, D. L.: 1968, The Operational Imperative: Sense and Nonsense in Operationism, *Systematic Zoology* **17**, 4378–457.
Hull, D. L.: 1974, Are Species Really Individuals? *Systematic Zoology* **25**, 174–191.
Hull, D. L.: 1979, In Defense of Presentism, *History and Theory* **18**, 1–15.
Kay, L. E.: 1993, Review of Holmes (1992), *Journal of the History of Biology* **26**, 369–373.
Kuhn, T. S.: 1970, *The Structure of Scientific Revolutions* (2nd ed.), University of Chicago Press, Chicago.
Mayr, E.: 1976, *Evolution and the Diversity of Life*, Harvard University Press, Cambridge, MA.
Mayr, E.: 1982, *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*, Harvard University Press, Cambridge, MA.
Mayr, E.: 1988, *Toward a New Philosophy of Biology: Observations of an Evolutionist*, Harvard University Press, Cambridge, MA.
Mayr, E.: 1991, *One Long Argument: Charles Darwin and the Genesis of Modern Evolutionary Thought*, Harvard University Press, Cambridge, MA.
Mayr, E.: 1990, When is Historiography Whiggish? *Journal of the History of Ideas* **51**, 301–309.
Morrell, V.: 1994, Rise and Fall of the Y Chromosome, *Science* **263**, 171–172.
Richards, R. R.: 1993, Arguments in a Sartorial Mode, or The Asymmetries of History and Philosophy of Science, *PSA 1992*, Vol. II., D. Hull, M. Forbes, and K. Okruhlik (eds.), pp. 482–489, East Lansing, MI: Philosophy of Science Association.
Rudwick, M. J. S.: 1985, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists*, The University of Chicago Press, Chicago.
Ruse, M.: 1993, Booknotes, *Biology & Philosophy* **8**, 477–483.
Whewell, W.: 1840, *The Philosophy of the Inductive Sciences, founded upon their History*, J. W. Parker, London.
Williams, L. Pearce, 1975, Should Philosophers Be Allowed to Write History? *The British Journal for the Philosophy of Science* **26**, 241–253.
Woodger, J. H.: 1929, *Biological Principles*, Routledge & Kegan Paul, London.
Woodger, J. H.: 1937, *The Axiomatic Method in Biology*, Cambridge University Press, Cambridge.
Woodger, J. H.: *Biology and Language*, Cambridge University Press, Cambridge.