

The Creation of the Essentialism Story: An Exercise in Metahistory

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ABSTRACT – The essentialism story is a version of the history of biological classification that was fabricated between 1953 and 1968 by Ernst Mayr, who combined contributions from Arthur Cain and David Hull with his own grudge against Plato. It portrays pre-Darwinian taxonomists as caught in the grip of an ancient philosophy called essentialism, from which they were not released until Charles Darwin's 1859 *Origin of Species*. Mayr's motive was to promote the Modern Synthesis in opposition to the typology of idealist morphologists; demonizing Plato served this end. Arthur Cain's picture of Linnaeus as a follower of 'Aristotelian' (scholastic) logic was woven into the story, along with David Hull's application of Karl Popper's term, 'essentialism', which Mayr accepted in 1968 as a synonym for what he had called 'typological thinking'. Although Mayr also pointed out the importance of empiricism in the history of taxonomy, the essentialism story still dominates the secondary literature. The history of the first telling of the essentialism story exposes its scant basis in fact.

KEYWORDS – Essentialism, essentialism story, Ernst Mayr, David Hull, Arthur Cain, Karl Popper, species concept, typology, history of systematics, history of taxonomy, historiography.

Introduction

There is a story about the development of biological classification that has become so deeply entrenched that textbook writers and college teachers are sure it is historical fact. For years I taught it myself, but now I am convinced it is little more than myth. The story has pre-Darwinian taxonomists imprisoned by an ancient philosophy called essentialism, from which they were released, or at least their release was begun, by Darwin's 1859 *Origin of Species*. Essentialism, we are told, was set out by Plato and Aristotle and elaborated by scholastic philosophers in the Dark Ages; these men held that every organism belongs to whatever sort

it does because it is governed by an immaterial essence. Since essences exist eternally and are distinct one from another, they render gradual evolution inconceivable.¹ In this saga, Darwin, whose theory contradicted essentialism, is the champion, even though the defeat of essentialism was delayed until the mid-20th century and even though pockets of this venerable error survive to this day, deserving to be stamped out.

The most extraordinary thing about the essentialism story is the contrast between the enormity of its reputation and the flimsiness of its basis in historical evidence. The story's vitality surely owes much to certain important truths to which it calls attention. Human beings doubtless are prone to the error of reification; we tend to ascribe power to an abstraction, because our use of common nouns creates categories, and these tempt us to assume that characteristics associated with a prototype will be present in each instance. There may also be some degree of truth in the notion that the now discredited philosophical version of essentialism, the one that ascribes reality to ideals or universals, was at some time commonplace among intellectuals. If that is so, however, then this textbook story about the history of taxonomy is worse than false, for it turns the order of causation upside down. The people who most directly confronted the inadequacies of scholastic essentialism, beginning at least as early as the 17th century, were the practical naturalists who had to deal with the ever-increasing number of kinds of seashells, butterflies, fruit, beasts and other natural wonders that were astounding European collectors. Botanists and zoologists, far from being held in the grip of a philosophical doctrine, shied away from metaphysics and concentrated on developing practical techniques for recording and making available data about living things (Stevens 1994; McMahon 2001; Scharf 2007). Although the reputation of those naturalists in the annals of science has always been low, their work was certainly the foundation upon which Darwin built his theory. I believe that this other story, which we may call the empiricism story, would now stand as the backbone narrative of the history of taxonomy if the essentialism story had not upstaged it.

Challenging the essentialist version of the history of taxonomy will be a large and complex project, requiring research into Greek philosophy (reviewing recent scholarly views on what Plato and Aristotle actually thought and taught), the metahistory of Greek philosophy (what interpretation was given to various Platonic and Aristotelian doctrines in later centuries), botany and zoology from the Renaissance onwards (the beliefs and also the practices of those who classified plants and animals), and

¹ The essentialism story is clearly stated in Futuyma (1998, 6, 448). A search of the internet turns up numerous undergraduate courses in which the essentialism story is used to introduce the concept of species.

metahistory, that is, the history of the writing of history. In this paper my aim is limited to metahistory, looking into when, where, why, and by whom the essentialism story was first told.

Calling the essentialism story into question may be a foolhardy thing to do, for it has been endorsed by many authorities and, above all, there are powerful forces, political and emotional, that make it attractive. Within one of the great dichotomies of Western history, idealism versus materialism, essentialism keeps company with the divine right of kings, with the everlasting damnation of atheists, and with racism. Many academics now treat essentialism as a valid category of analysis within their own particular discipline. Gender essentialism, for example, is a label that helps reveal how misguided are concepts like manhood. Anyone unwilling to condemn essentialism must mount a very clever explanation to prove she is not a bigot, or at best, terribly reactionary. In my own case the appropriate defence is close at hand, for far from supporting essentialism, I am attacking an instance of historical essentialism; that is, I am pointing out and condemning the habit of slapping glib labels on complex ideas of the past. I maintain that the anti-essentialist historian should pay respectful attention to the interesting differences between the ideas of diverse naturalists instead of lumping them together.

It may seem obvious that we ought to begin our historical work with a rigorous definition of essentialism, so that we will be able to recognize it, or its absence, when we come upon it, or fail to find it, in the writings of a dead taxonomist. The great irony of the subject, however, is that once you grasp the concept of essentialism, you discover that it is naive to expect a precise definition of such a word (Ghiselin 1997, 73-80). Asking for a precise definition is to imagine that there is an essence to essentialism itself. Some leading philosophers are now using 'essentialism' for something nearly opposite to what it meant to the subjects of my study (Boyd 1999). This historical sketch will concentrate on describing the concerns and ideas of the biologists who, in the middle of the Twentieth Century, first used this word in the context of the history of systematics. My subject is limited to the creation of the historical claim that essentialism dominated systematics before Darwin. I will not attempt to deal here with the spread of the story in later decades, nor with the question of what picture we are left with when this widely-accepted story vanishes.

Mayr on Plato

In its inception, there were three largely independent strands that, when interwoven, formed the essentialism story. One was contributed

by Arthur J. Cain in 1958, another by Ernst Mayr in 1959, and another by David Hull in 1965; it was Mayr who combined these strands in 1968. His handsome volume of collected biological essays, *Evolution and the Diversity of Life*, published in 1976, and his impressive 1982 work of history, *Growth of Biological Thought*, repeated the essentialism story vividly and with authority. (It was also Mayr who named and articulated the empiricism story, an important point I will put aside for now.)

Although Mayr did not distinguish the separate strands as I am doing, he was well aware that there was a history to the telling of the story, for he introduced one item in his 1976 collection thus:

So far as I know, the following essay, excerpted from a paper originally published in 1959, was the first presentation of the contrast between essentialist and population thinking, the first full articulation of this revolutionary change in the philosophy of biology. (Mayr 1976, 26)

Actually, however, what Mayr had written in 1959 was that the opposite of population thinking was 'typological thinking'. The words 'essentialist', 'essentialism', and even 'essence' did not appear in his writings until a decade after the 1959 'first full articulation'. Mayr's 1968 decision to accept 'essentialism' as a synonym of 'typology' contributed to the confusion of issues that has blighted the history of systematics ever since (Mayr 1968).

Mayr's 1959 essay was based on a public lecture he had delivered in October of 1957 in the Smithsonian Institution at the invitation of the Anthropological Society of Washington. There he had proposed a novel assessment of Darwin's significance. Besides the two achievements everyone knows about, evolution and natural selection, Mayr identified a third:

[Darwin] replaced typological thinking by population thinking.

The first two contributions of Darwin's are generally known and sufficiently stressed in the scientific literature. Equally important but almost consistently overlooked is the fact that Darwin introduced into the scientific literature a new way of thinking, 'population thinking'. . . .

Typological thinking no doubt had its roots in the earliest efforts of primitive man to classify the bewildering diversity of nature into categories. The *eidōs* of Plato is the formal philosophical codification of this form of thinking. According to it there are a limited number of fixed, unchangeable 'ideas' underlying the observed variability, with the *eidōs* (idea) being the only thing that is fixed and real while the observed variability has no more reality than the shadows of an object on a cave wall, as it is stated in Plato's allegory. The discontinuities between these natural 'ideas' (types), it was believed, account for the frequency of gaps in nature. Most of the great philosophers of the 17th, 18th, and 19th centuries were influenced by the idealistic philosophy of Plato, and the thinking of this school dominated the thinking of the period. Since there is no gradation between types, gradual evolution is

basically a logical impossibility for the typologist. Evolution, if it occurs at all, has to proceed in steps or jumps.

. . . For the typologist, the type (*eidos*) is real and the variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different. (Mayr 1959a, 2; reprinted in: Mayr 1976, 27-28)

Mayr repeated this claim, quoting himself, at the beginning of his 1963 textbook *Animal Species and Evolution* (pp. 5-6) and in his preface to the facsimile edition of the *Origin of Species* that Harvard University Press published in 1964, as well as in many other places, and his words have been quoted or summarized countless times ever since.

Whether we date Mayr's contribution to the essentialism story from the 1957 Washington lecture to anthropologists or the 1959 publication of it, we must note that neither one, strictly speaking, was 'the first presentation of the contrast' between typology and population thinking, for he had already made that contrast clear in his contribution to the 1953 *Methods and Principles of Systematic Zoology* (the book, but not this quotation, co-authored with entomologists Robert Usinger and Gorton Linsley). There he had said,

Taxonomy in its early history was completely dominated by the type concept. The type concept goes back to Greek philosophy. The 'ideas' of Plato are such 'types'. *Applied to taxonomy, the type concept postulates that all members of a taxonomic category conform to a 'type'. Whether a taxonomist adhered to the type concept consciously or unconsciously, it inevitably affected his methods and results. In particular, the type concept tended to exaggerate the constancy of the categories and the gaps that separate them and to minimize variability. Typologists have often either denied evolution altogether or explained its operation by macromutations. * [Mayr's footnote:] The early nineteenth century was the heyday of the 'typologist', the adherents of the type concept. Cuvier was an outstanding representative of this school, and so were the German *Naturphilosophen* (Schelling, Oken, Carus, etc.) of that period. Students of the higher categories were particularly strong typologists, but this philosophy also affected taxonomists who worked at the species level. (Mayr, Linsley, Usinger 1953, 15)

This contrast between population thinking and typology was also clearly set out in his important paper, 'Species concepts and definitions', delivered in Atlanta, Georgia, in December 1955 and published in 1957.

The significant difference between these statements by Mayr and his Washington lecture is that the earlier versions utterly lacked a hero. In the 1953 textbook Mayr said that 'During the past seventy-five years the population concept has gradually replaced the type concept' (Mayr, Linsley, Usinger 1953, 15), leaving it up to the reader to calculate that

the population concept began around 1878. He may have had in mind the work of Friedrich Heincke on variability in herring populations, which began to appear in that year, but in 1953 Mayr mentioned no landmark figures at all.² Of course *Methods and Principles of Systematic Zoology* did credit Darwin with evolution by natural selection, but it did not credit him with establishing a populational viewpoint in place of typology. Positioning Darwin as the pivotal figure was what made the Washington lecture the first full articulation of Mayr's strand of the three-strand story. The revolutionary role of Darwin has remained one of the core elements of the essentialism story.

In the 1950s, Ernst Mayr had already achieved eminence as an evolutionary biologist (he became Director of the Museum of Comparative Zoology in 1953), but in that decade no one thought of him as an historian of biology. In the preceding decades his vast capacity for work had been almost entirely consumed by ornithology; it is quite striking, for those who know his later work, to see how very few historical remarks can be found in his landmark 1942 *Systematics and the Origin of Species*. He stepped into the role of historian cautiously, by a series of small steps, and those mostly in response to some need or situation.³ Seeing the need of a textbook on taxonomy, he began to write one, including a few pages of historical introduction, and then learned about two entomologists, Linsley and Usinger, who had been working on the same project. In 1953, the biologist Walter Zimmermann published, in German, a thick history of evolutionary ideas, and Mayr's review of it came out the next year. In 1954, friends of Karl Jordan who were organizing a *Festschrift* to celebrate the Tring entomologist's 94th birthday invited Mayr to describe Jordan's contributions to the species concept (Mayr 1955). The American Association for the Advancement of Science chose the species question as a symposium topic for their 1955 Atlanta meeting and asked Mayr to organize it. To prepare his introductory essay for the resulting volume, 'Species Concepts and Definitions', he read what he could find on the history and philosophy of the issue. In 1956 he accepted the Anthropological Society of Washington's invitation to give a lecture celebrating the centenary of the *Origin of Species* (which would appear in print in 1959) partly because it gave him 'the opportunity to make an effort of re-evaluating Darwin's role'.⁴ A careful rereading of the *Origin of Species* convinced Mayr that

² Mayr named Heincke appreciatively in a 1959 paper reprinted in Mayr 1976, 311.

³ Junker (1996) documents these steps, while suggesting (p. 33) that the historical style of thinking was always important to Mayr.

⁴ E. Mayr to Marshall T. Newman, 8 Nov. 1956, Mayr Papers, Harvard University Archives.

Darwin had understood ‘population thinking’, and this supplied Mayr with a novel point for the lecture. Nevertheless, only the first few pages, a mere 16 per cent, of his Washington lecture were historical. Much later, he recalled that at the time he was ‘not at all aware of the importance’ that would later accrue to his brief claim.⁵

Quite obviously the occasions, contexts, purposes and format of Mayr’s remarks about Plato, Darwin, and typology did not belong to the category of scholarly history. His audiences did not expect from him, nor did Mayr pretend to have conducted, original research, nor even a careful review of historical sources. He was speaking and writing *ex cathedra*, from his position of eminence in science, and his historical claims were not submitted to peer review nor did he always supply full citations. The 1953 footnote mentioning Cuvier, Schelling, Oken, and Carus could have been based on the 1951 *Geschichte der Ornithologie* of his teacher Erwin Stresemann or on Zimmermann’s book.⁶ Mayr’s contribution to the 1957 *Species Problem* has a bibliography of 54 items, but these were all directly relevant to the history of species concepts. Broader histories of systematics, such as Zimmermann’s book, Francis Bather’s article, or even Henri Daudin’s substantial volumes, are not cited.

There is not the slightest doubt that Mayr’s readings, thoughts, and writings on history were undertaken for the service of his prime interest, the promotion of the modern evolutionary synthesis and, in particular, in support of his view that the process of speciation should hold the limelight in neo-Darwinism. Neither Mayr nor most of his colleagues saw his purposeful style of history as anything to apologize for. For example, in his Atlanta AAAS talk of December 1955, he declared,

To give a well-documented history of the stated controversies [on the nature of species] would fill a book. As interesting as this chapter in the history of human thought is, the detailed presentation of the gropings and errors of former generations would add little to the task before us. Let us concentrate therefore on the gradual emergence of the ideas which we, today, consider as central and essential. (Mayr 1957, 6)

The Washington anthropologists had also wanted from Mayr not the errors and gropings of the past, but useful history. His was the first of a series of lectures whose purpose was ‘to review historically the significance of evolutionary concepts in anthropology, and to assess the relevance to current thinking in the field’.⁷

⁵ E. Mayr to M. P. Winsor, personal communication, 16 November 2001.

⁶ Carl Gustav Carus (1789-1869), who remained obscure in the English literature, was mentioned in both works.

⁷ Marshall T. Newman to E. Mayr, 25 October 1956. Mayr Papers, Harvard University Archives.

Mayr's brief statement in his Washington lecture about how population thinking differs from typological thinking led him immediately to two issues of importance to mid-twentieth-century anthropologists: natural selection and race. No typologist could understand natural selection, he insisted, yet natural selection is 'the cornerstone of the modern, synthetic theory of evolution' (Mayr 1959, 10). Race, he said, when properly understood as a statistical statement about variable populations, 'is a universal phenomenon of nature occurring not only in man but in two-thirds of all species of animals and plants' (Mayr 1959, 3), but the typologist thinks that members of a race must conform to its type and that there must be a gap between races. 'All racist theories are built on this foundation,' he asserted (Mayr 1959, 3). Doubtless the memory, less than two decades old, of Nazi pronouncements and atrocities against Jews provided a powerful reason for civilized people to learn what modern evolutionary science could teach. In the following decades, when the American civil rights movement moved racism to the forefront of liberal thought, Mayr's words grew ever more relevant and profound.

Population thinking and typology were matters of central importance to Mayr because he was engaged in a complex and delicate rescue operation. Trained in Berlin as a museum taxonomist in the 1920s, Mayr discovered when he came to New York in the 1930s that the leaders of academic biology considered the collection-based research he was doing to be little better than stamp collecting. Zoologists and botanists who had tried to contribute to evolutionary biology by sketching a great tree of life, exploring the phylogeny of higher taxa (Which invertebrate most resembles the ancestor of the vertebrates? Which flowering plant is the most primitive?) had been ridiculed as too speculative (Bowler 1996). Taxonomists who thought they were being good Darwinians by giving names to minute variants of familiar species were treated with scorn for wasting time on details that were subjective and irrelevant. The superiority of experimental over comparative science had been so successfully promoted by physiologists that the catchword for the future was 'experimental evolution', a concept which attracted funding to a laboratory in Cold Spring Harbor, New York (Cook 1999). Another glaring weakness of taxonomy was the flimsy metaphysical status of its framework, its nested sets of categories from species and genera up to classes and phyla, which biologists who did genetics or physiology regarded as purely artificial devices. Mayr contradicted these perceptions by arguing two points. First, species were not the inventions of taxonomists but real phenomena, natural objects that taxonomists were especially equipped to investigate and, second, the way species evolve, the process of speci-

ation, was at the heart of evolution and required the skills of a taxonomist to investigate. Mayr was tireless in repeating these points, though he had to battle not only the ill repute in which other biologists held taxonomists, he needed at the same time to convince taxonomists to fall in line with his campaign so that they would cease to do things deserving of scorn. He thought that taxonomists who were interested in the higher categories ought to redirect their attention to the species level and so he emphasized that only there was evolution taking place. He insisted that species were real entities rather than mere human inventions, but for the source of that reality he focussed on organisms' capacity for interbreeding rather than on their historical kinship. Consequently, the status of biological reality that Mayr accorded to species could not be extended to genera or higher categories. He therefore agreed with the critics who called the higher categories subjective, and he decided that the new systematics must focus on problems of speciation.

The contrast between population thinking and typology directly served this agenda. 'Population' was a concept employed by geneticists, ecologists, and natural historians (but not by museum taxonomists, whose lowest category, the variety, encompassed numerous populations). Mayr declared that the word 'population' was very hard to define precisely, but roughly meant the subset of individuals within a species which would usually breed with one another because they lived in the same area (Mayr 1942, 24; 1963, 136). Theodosius Dobzhansky and Julian Huxley, leaders of the modern synthesis, emphasized the genetic variability of the gene pool of a population as the necessary condition for natural selection. To Mayr, the term population also included the variability of its members, in contrast to the identical items in chemists' populations of atoms; he invented the term 'population thinking' to mean the appreciation of this variability. In a population of organisms, no feature is absolutely unvarying; every character can exist in more than one state (if eye color is normally red, it can also be white).

Mayr's decision to call the opposite of population thinking 'typological thinking' was a highly significant move and by no means an obvious one. He was aware that the word 'type' invited confusion, because it was already used in several different ways by taxonomists, with further meanings in genetics and other fields. The word had appeared in two quite unconnected senses in Mayr's 1942 *Systematics and the Origin of Species*. Taxonomists call an individual organism a 'type specimen' if the first description of a species was based on it (Mayr 1942, 15-16). But this, he was later careful to insist, 'should not be confused with' the pernicious notion he was tracing back to Plato; keeping track of type specimens was an extremely useful technique (Mayr, Linsley, Usinger 1953,

15). The second sort of type we find in *Systematics and the Origin of Species* had nothing at all to do with species; it occurred in Mayr's brief mention of the 1936 assertion of palaeontologist Otto Schindewolf that 'all or nearly all the major types of animals have appeared on the earth in a more or less finished form, with the links to the presumable ancestors missing' (Mayr 1942, 296). Mammals are one 'type' of vertebrate and birds are another. Schindewolf's assertion was profoundly anti-Darwinian because the evolution of taxa in the higher categories was said to require a different cause than what operates at the species level. A genus can evolve new species by natural selection working on ordinary variation, but a new order or class appears overnight, by means of a special leap or saltation. Mayr mentioned Schindewolf only to contradict him.

Schindewolf clung fast to his pre-War beliefs throughout the 1940s, unmoved by the arguments of Mayr, Huxley, Dobzhansky, or even fellow palaeontologist George Gaylord Simpson, and in the late 1940s he wrote a book aimed at a wide readership in which he repeated and developed his claims. He insisted that the *Typus* was something real and not merely conceptual, nor metaphysical. He attributed the appearance of new types to an evolutionary process he called *Typostrophismus*. 'These processes,' he wrote, 'occur independently of the mode of life of their carriers and without direct cooperation of selection (which is responsible for the origin of races)' (Schindewolf 1993, 186). He also utterly denied that the species category possessed any stronger kind of reality than did the higher taxonomic categories, locus of the types. His doctrine was anathema to Mayr. Although it is unlikely that by the 1950s Mayr considered Schindewolf's swan song a serious threat, especially since it was in German, the old paleontologists' new book must have stirred up memories of the years when Mayr was in his teens, the early 1920s, a period when German Darwinists were on the defensive, vitalism was on the rise, and much morphology was explicitly idealistic (Harrington 1996). The 1919 book *Idealistische Morphologie und Phylogenetik* by Swiss anatomist Adolf Naef 'was very influential for several decades' (Reif 1983, 176). To Naef and his followers, the transcendent meaningfulness of morphological types was an important antidote to the cold materialism of Darwinism. The idealistic morphologists proudly traced their intellectual ancestry, including the concept of types, back to famous 'authorities of the late eighteenth and early nineteenth centuries (Cuvier, Lamarck, Goethe, Schelling, Oken, and other German Romantics and *Naturphilosophen*)' (Reif 1983, 174). Although from our 21st century perspective, their version of history is easily seen to have been distorted by their desire to build a distinguished ancestry

for their own ideas, in the 1950s Mayr (and Zimmermann) had every reason to welcome their history at face value, for if the idealists' type was an old concept, that was all the more reason it should be consigned to the dustbin.

What Mayr needed in the 1950s was a vivid label for the opposite of population thinking. 'Typology' was the name that idealist morphologists used for their science of abstract form. Their types were distinct kinds separated by gaps and these kinds were always at the level of higher categories, like classes or orders (Reif 1986). Mayr's new term 'typological thinking' implied a transfer of these features of Schindewolf's types down to the species level, where no one believed they belonged. Actual typologists ignored the small differences that separated one species from another in the same genus, as well as the fluctuating differences among individuals within a species. Mayr's rhetorical ploy created a new and imaginary sort of typologist: someone who dealt with species as if they were logical sets rather than reproductive communities. This was an enormously effective bit of polemic, because rather than accurately describe the avowed position of anyone, typological thinking at the species level was an error no one would want to be accused of.

Mayr's central concern in the 1950s was to establish the biological species concept, around which there was still dissent and confusion. He was able to take advantage of the ambiguous meaning of the word 'morphology'. In addition to its meaning as the science of abstract comparative anatomy, 'morphology' was also used by taxonomists to mean the physical features that could be observed in a dead specimen, as opposed to information about a living organism, such as where it lives, its physiology, and its interactions with other organisms, whether as prey, predator, competitor, or potential mate. Many, perhaps most, taxonomists wanted to limit the characters permitted in determining species membership to morphological differences, for the very practical reason that only such characters could be observed, measured, and described indoors. Yet taxonomists' traditional policy of not granting specific status to differences that were not evident on dead specimens was facing the interesting challenge of sibling species. One example came from epidemiologists whose fieldwork showed that there were two kinds of mosquitoes, only one of which carries malaria, though these kinds looked identical in preserved adult specimens and so had been given only one species name in museum collections. Another came from geneticists, whose breeding experiments showed that apparently identical fruit flies consisted of two groups which would breed within their kind but not interbreed with the other kind. A. H.

Sturtevant designated such forms of *Drosophila pseudoobscura* with the letters A and B, rather than granting form B species status, because of the lack of morphological difference between the forms, whereas Carl Epling had christened form B as a good species, naming it *D. persimilis*.

To Mayr, sibling species were crucial tests for the modernization of systematics, for if museum workers refused to let biological properties trump morphological ones, they would be cutting themselves off from the only theory that could give them the status of scientists. Mayr thought they ought to choose whether their goal was to understand the natural world, or merely to catalogue storehouses of dead animals or plants. In this context, it served Mayr's purpose not to disentangle the two very different senses of 'morphology'. For example, in 1955 he claimed,

The application of the typological species concept to practical taxonomy results in the morphologically defined species; 'degree of morphological difference' is the criterion of species status. Species are defined on the basis of their observable morphological difference. . . . Most systematists have found this typological-morphological concept inadequate and have rejected it. (Mayr 1976, 501)

But Carl Epling, to whom he sent a draft of this paper, wrote back, 'You are suggesting or implying that Sturtevant's method is derived from Plato's conception.' Epling complained,

I doubt this. I suggest that his method of estimating species limits was no different from that of Linnaeus or Darwin. . . . I would resist the implication that the morphological criteria which I used in my own monographic work was 'typological', any more than yours. Look back my dear friend. What we both did, I believe, was to use morphological criteria as a guide to determining where the reproductive barriers lay.⁸

Mayr's response was that he would rather exaggerate and oversimplify than risk having any reader miss his point.

Furthermore, we all have our favorite scape-goats. Plato, under whose influence I have suffered throughout my high school and college career, and de Vries . . . are two of my pet aversions. It is good for my liver if I am permitted to knock [him] down from time to time. I may differ from you in evaluating their influence. The fact is that, at least as far as Plato is concerned, this influence is much greater and deeper than you seem to imagine. There is not a single philosopher in Germany who is not basically influenced by Plato and this is true even for the majority of naturalists. Goldschmidt's thinking in terms of systemic mutations is a clear product

⁸ Carl Epling to Ernst Mayr, 18 May 1956, Harvard University Archives. The late Ernst Mayr graciously gave me permission to quote from this letter.

of this school of thinking. And so are Schindewolf's speculations on the origin of new categories.⁹

At about this time, Mayr did a bit of primary research that seemed to confirm this view of the history of taxonomy thoroughly. A young historian, Edward Lurie, who was writing a doctoral dissertation at Northwestern University on Louis Agassiz, asked the director of the museum Agassiz founded, Al Romer, how Agassiz measured up as a zoologist, and Romer recommended that Lurie consult Mayr, which he did in the fall of 1954.¹⁰ Most likely it was this query that led Mayr to read Agassiz's 'Essay on classification', a text that handed Mayr a vivid picture of an opponent of evolution whose understanding of nature had been hopelessly distorted by an idealist philosophy. Although American tradition praised Agassiz as a great field naturalist, what impressed Mayr was his treatment of the reality of species. In the ordinary sense of reality, according to Agassiz, only individuals were real, physically existing things. Yet in a more important sense, species, as well as genera and higher categories, were even more real than individuals, existing as 'categories of thought' in the mind of the Creator. Agassiz, notorious for his unrelenting opposition to Darwin, argued that each level of the taxonomic hierarchy reflected the thoughts of God. To Mayr, this seemed to match perfectly the expected scenario (Mayr 1959a).

The fact that Agassiz made no mention of Plato's forms, ideas, type, or *eidōs* was no problem, for Mayr was learning how to do the history of ideas from a master, Arthur O. Lovejoy. His 1933 lectures, published as *The Great Chain of Being: A Study of the History of an Idea*, argued that assumptions or unconscious habits of thought could influence 'the course of man's reflections on almost any subject' over thousands of years (Lovejoy 1936, 10).

The most fundamental of the group of ideas of which we are to review the history appears first in Plato; and nearly all that follows might therefore serve as an illustration of a celebrated remark of Professor Whitehead's, that 'the safest general characterization of the European philosophical tradition is that it consists in a series of footnotes to Plato'. (Lovejoy 1936, 24)

Drawing upon German and English scholarly studies of the 1910s and 1920s, Lovejoy expounded Plato's theory of forms; that is, the existence of eternal entities of which the objects in our visible world are the

⁹ Letter from Ernst Mayr to Carl Epling, 18 May 1956, Harvard University Archives.

¹⁰ Lurie 1956; letter from E. Lurie to M.P. Winsor, personal communication 18 May 2005.

pale reflections. Further developed by Aristotle and the neoPlatonists, this 'otherworldliness' was still dominant in the 18th century, according to Lovejoy's last chapter. Lovejoy's thesis emphasized the continuity represented by the great chain of being and ignored the gaps between types so central to Mayr, but nevertheless the tone and style of the book pleased him greatly (Mayr 1976, 254n). In 1959 Mayr did not yet know that Agassiz, though a stubborn creationist, had had a keen eye for individual and varietal differences and had urged his students to study variation (Mayr later gave kind encouragement to the young historian who uncovered these facts).¹¹ No philosophically-competent historian nor historically-competent philosopher has yet confronted the intriguing issue of whether Agassiz's theological notions really had neoPlatonic connections, nor even if they logically excluded evolution as Agassiz and Mayr thought. Historians untrained in philosophy either assumed that Mayr must be correct, or noted that since many of Agassiz's contemporaries found his opinions peculiar, he should not be taken as typical of his time (Winsor 1991, 12-28).

Mayr was certainly not alone in his belief that idealism had stood in the way of Darwinism. We find it in another source celebrating the Darwin centenary, *A Book that Shook the World: Anniversary Essays on Charles Darwin's Origin of Species*. One of the essays in this collection was by professor Oliver L. Reiser, a philosopher at the University of Pittsburgh. Building upon a classic essay by John Dewey (which had celebrated the 50th anniversary of the *Origin of Species*), Reiser argued that

. . . Aristotelianism is in principle a non-evolutionary viewpoint. Aristotle's philosophy is an 'essentialist' doctrine in the sense that, as the Existentialists would say, 'essence precede existence'. . . To see why the Platonic-Aristotelian metaphysics must lead to a non-evolutionary type of explanation let us retrace the main outlines of the Aristotelian form of essentialist philosophy. . . . The Aristotelian schematism of forms in nature thus was embodied in a hierarchy of types or species arranged according to the essential characteristics (essences) which defined the classes. (Reiser 1958, 40-41)

Reiser was echoing a lecture given by his teacher John Dewey half a century earlier, and Reiser pointed out that the Aristotelian idea of essences was being promoted by the neo-Thomist Mortimer Adler (1940). Whether any of these were known to Mayr in the 1950s may be questioned. Mayr first cited Reiser in 1971 (p. 984), there is no evidence of correspondence between them, and Mayr never mentioned Dewey or

¹¹ E. Mayr to M. P. Winsor, personal communications, 7 September 1977, 14 September 1977, 27 October 1977, 4 May 1978; Winsor, 1979.

Adler. I suspect that Dewey's essay "Darwin's Influence upon Philosophy" was not noticed by biologists until Philip Appleman included an excerpted version in his reader (1970). It is worth pointing out, in support of Mayr's own belief that it was he who gave the "first full articulation" of the essentialism story, that these philosophers did not discuss the hallmark of population thinking, that is, that because individuals differ within a biological species, the distinction between accidental and essential characters is meaningless. Their subject was evolution in the broad sense.

By the end of the next decade, as he read and thought more about the history of biology, Mayr would learn that many pre-Darwinians had not after all worked in Plato's shadow. New scholarship on Michel Adanson left no doubt that it was possible for an 18th century taxonomist to be utterly untouched by typological thinking (Stafleu 1963). In the English-speaking world, Mayr acknowledged, empiricism had dominated (Mayr 1982, 110). Yet he would never relinquish the conviction that Plato must have been as influential in the 17th, 18th, and 19th centuries as he had been in Germany early in the 20th.

Cain on Linnaeus

The second strand in the three-stranded essentialism story was contributed by the zoologist Arthur J. Cain, who claimed in 1958 that Linnaeus had been following the dictates of Aristotelian logic.¹² There seems to be no connection between Cain's idea and Mayr's portion of the story, although Cain, fresh from a doctoral degree in physiology, had spent 6 months with Mayr in New York in 1950, apprenticing himself in taxonomy in preparation for a position at Oxford.¹³ Cain's appointment required him to teach taxonomy, but he had less interest in contributing to the taxonomic literature than in identifying the faults in its traditional practices. One was that every species be assigned to a genus, even if this meant creating a new single-member genus, a requirement

¹² I have traced this story in much more detail in Winsor, 2001.

¹³ It seems to me very likely that both men were stimulated by an article they both forgot to cite, W.R. Thompson's 1952 'Philosophical Foundations of Systematics'. Thompson's confident exposition of the medieval logic of genus and species was doubtless a result of his training in Catholic philosophy (Thorpe 1973). Although Thompson was no friend of the modern synthesis, his views on evolution introduced a 1958 reprint of Darwin's *Origin*. Though neither Mayr nor Cain mentioned Thompson, and there is no record of any correspondence with him in Mayr's papers in the Harvard University Archives nor in Cain's papers at the Library of the American Philosophical Society in Philadelphia, I doubt they would have missed an article so close to their interests, published by a taxonomist in a scientific journal.

originating with Linnaeus and codified by 20th century botanical and zoological congresses. Cain was annoyed by the rule's arbitrariness and he decided to look in some of Linnaeus's writings for its rationale. When he came across Swedish scholar Arvid Hj. Uggla's remark that the terms *genus*, *species*, and *differentia* used by Linnaeus were terms with special meanings in medieval logic, Cain was excited by the idea he had uncovered a neglected key to Linnaeus's mindset. A textbook *Introduction to Logic* taught Cain about Aristotelian definition, which stipulates that essential rather than accidental properties be identified. The only reason every Linnaean 'species' had to sit within a 'genus' was that Aristotle had so decreed, Cain thought, which seemed to him a terrible indictment against the mindless traditionalism of taxonomy.

Cain explained that Aristotelian definition required the essence of an entity to be known, because only its essential characters should be mentioned in a definition and not the many other properties that may also be present, whether incidentally or always. In mathematical systems, the full essence of each entity was fully known; convincing examples of proper definitions were always from geometry. The genus triangle contains the three species equilateral, scalene, and isosceles, defined by the ratio of the sides: all three sides of equal length, or only two sides equal, or no sides of the same length. Having three sides is the very essence of being a triangle.¹⁴ Cain invented the term 'analysed entities' for cases like that, where the essence of things is known, and he contrasted them with 'unanalysed entities', of which living things are the best examples, where the essence, the ultimate nature of their being, is not known. It was a fatal error, said Cain, for Linnaeus and other taxonomists to try to apply the logical method of Aristotle to unanalysed entities, where it could not work.

Although a few people expressed partial doubts about Cain's argument, before long his thesis became the new orthodoxy. Probably the mind-numbing detail of his exposition of medieval rules of definition intimidated most readers. One who knew Linnaeus's work well enough to be suspicious, William Stearn, demurred, but quietly, perhaps wishing to avoid making an enemy.¹⁵ G. G. Simpson felt Cain's argument impressive enough to report in detail in his 1961 *Principles of Animal Taxonomy*, while confessing himself unsure if Linnaeus was quite as

¹⁴ The triangle example derives from the neoplatonist Porphyry rather than from Plato or Aristotle.

¹⁵ Stearn wrote (1959, 94) '...Linnaeus's practice here was empirical rather than in accordance with formal logic (cf. Cain, 1958)'.

scholastic as Cain made him out to be (Simpson 1961, 24; 36-40). Cain's portrayal of Linnaeus's scholasticism quite convinced Frans Stafleu, who relayed it as fact in his highly-regarded book *Linnaeus and the Linnaeans*, in a style that may have led some readers not to recognize Cain as the source. David Hull (1965, 316) pointed out in a footnote a serious flaw in Cain's reasoning, but this was not enough to prevent its acceptance by other influential scholars (Larson 1971; Lindroth 1985).

Mayr in the 1960s was not happy with Cain's analysis. Although he did not quarrel in print with Cain's interpretation, Mayr had always had a positive view of Aristotle, as a remarkable early naturalist, a view for which there were plenty of sources in the German literature.¹⁶ Yet Aristotle, rather than Mayr's nemesis Plato, was Cain's villain. Mayr wrote Cain on 17 March 1960,

When I was preparing myself for the 1957 Linnaeus Celebration in Upsala, I went through the entire biographical literature on Linnaeus, but found nothing that would have satisfied the needs of a systematist. Neither of the two big biographies brings out your point of the Aristotelian basis of Linnaeus's taxonomic philosophy, nor what his real contribution was. In that respect I must admit I find your own treatment of Linnaeus also somewhat deficient. A man who had such a colossal impact on his contemporaries must have been more than a 'throwback' to Aristotle. . . . I hope you will take up Linnaeus once more and present a more comprehensive treatment of his contribution.¹⁷

But Cain's further research on Linnaeus was put off until his retirement many years later. In the 1990s Cain would characterize Linnaeus as an eclectic in philosophy and an empiricist in science, but by then it was too late, for Mayr's enormously influential *Growth of Biological Thought* was completed in 1982. Cain had let loose a genie that would prove very difficult to put back in its jar.¹⁸

Hull on Popper

The third strand in the creation of the essentialism story was the work of a graduate student enrolled in the history and philosophy of science program at Indiana University, David L. Hull. In a course taught by a visiting professor, the eminent philosopher of science Karl Popper (who was there between 1961 and 1963), Hull was required to write a paper

¹⁶ In his youth Mayr had read, in his father's library, Theodor Gomperz's *Griechische Denker*, which gives a very sympathetic description of Aristotle as a pioneer biologist.

¹⁷ E. Mayr to A.J. Cain, 17 March 1960. Mayr Papers, Harvard University Archives.

¹⁸ For one attempt to do so, see Winsor 2006.

that would support one of Popper's opinions. Hull, not fond of Popper, was already interested in evolution and knew how slight was the great man's knowledge of biology, so he decided to focus on a contemporary biological debate, the so-called 'species problem', for which there was a rich source in the AAAS collection (Mayr 1957a,b). Popper was so pleased with the result that he sent it off, unknown to Hull, to the *British Journal for the Philosophy of Science*. The surprised author managed to revise it to please himself, and it appeared in two issues of the journal in 1965 with the arresting title 'The Effect of Essentialism on Taxonomy – Two Thousand Years of Stasis'. This publication was the foundation of Mayr's mentorship of Hull. Hull, who earned his doctoral degree in 1964, met Mayr at the December 1965 AAAS meeting.¹⁹ Mayr thenceforth encouraged Hull's career in the philosophy of biology, which was a neglected branch of the philosophy of science at the time (Hull 1994).

Hull's paper began with Popper's eloquent contention that good science avoids the trap that ensnares philosophy, namely, endless bickering over the meaning of words. Using a 1950 reprinting of Popper's *The Open Society and Its Enemies* (first published in 1945), Hull quoted Popper's introduction of a new term, 'methodological essentialism', which is

the view, held by Plato and many of his followers, that it is the task of pure knowledge or 'science' to discover and to describe the true nature of things; i.e. their hidden reality or essence. It was Plato's peculiar belief that the essence of sensible things can be found in other and more real things – in their primogenitors or Forms. Many of the later methodological essentialists, for instance Aristotle, did not altogether follow him in determining this; but they all agreed with him in determining the task of pure knowledge as the discovery of the hidden nature or Form or essence of things. All these methodological essentialists also agreed with Plato in holding that these essences may be discovered and discerned with the help of intellectual intuition; that every essence has a name proper to it, the name after which the sensible things are called; and that it may be described in words. And a description of the essence of a thing they called a 'definition'. (Popper 1950, 1: 34)

Immediately following this quotation was Hull's assertion, 'In taxonomy this philosophical position became known as typology' (1965, 317).

Hull's paper was directed to the species question, that is, how the category 'species' should be defined, rather than to species taxa, that is, how individual species may be named and described. He concluded that the search for a simple definition was ill-conceived and that a cluster concept (as advocated by Michael Scriven) was more appropriate. This conclusion was consistent with the view Mayr set forth in the AAAS vol-

¹⁹ Hull to Mayr, 3 January 1966, Mayr Papers, Harvard University Archives.

ume (1957, 10) even though Mayr's name would later be associated only with the so-called 'biological species concept' he had adopted from Theodosius Dobzhansky. Mayr quickly recognized in the young philosopher a potential ally. Hull had already been brave enough to publish (1964) in a scientific journal an analysis of the logical relationship between phylogeny and taxonomy, and he responded very satisfactorily (1967) to Mayr's suggestion that the numerical taxonomists be taken to task for their poor philosophy.

I said earlier that the essentialism story was made by the joining of "three largely independent strands." In the case of Hull I had in mind primarily the fact that it was he who first equated essentialism as used by Popper with typology in taxonomy. Of course Cain's 1958 paper, so full of Aristotle, virtually cried out for connection to Popper's thesis, but the links to Mayr were not as strong as we might imagine. Hull's paper was written without reference to the story given its 'first full articulation' in Washington by Mayr (1959a), the one that credited Darwin with the revolutionary breakthrough of population thinking. The only work of Mayr's that Hull cited was *The Species Problem* (1957), which predated Mayr's new view of Darwin. In that book Mayr had expressed disappointment in Darwin, whose species concept was far too weak for Mayr's taste, and he had portrayed the typological species concept as virtually a straw man, held by very few taxonomists either today or in the past. Taxonomy has been criticized by other biologists, Mayr asserted, who 'mistakenly considered [the typological species concept] the basis of taxonomy' (1957a, 14). Instead of Mayr, Hull's source for the idea that taxonomy had been dominated by typology was Simpson's *Principles of Animal Taxonomy* (1961). (Simpson's sources were principally Cain and Zimmerman; doubtless it was Simpson's citation that led Hull to Cain's article.)

Hull made it very clear that it was not the history of taxonomy that was his topic but taxonomists' contemporary struggle to define species. Nevertheless, his reading of Cain and Simpson, coupled with Popper's rhetorical use of Aristotle, left Hull confident enough to use a simplistic historical claim as his title: that essentialism had caused 'two thousand years of stasis'²⁰ in the history of taxonomy. He believed that taxonomists had not understood their business.

Presented with the welter of diverse forms to be classified, a taxonomist can greatly simplify his task if he pretends that certain properties are 'essential' for defini-

²⁰ Surely most people understood this number to represent the period from Aristotle to the present, rounded off, even though that meant ignoring 287 years (1965 + 322 = 2,287-2,000). Whitehead had been somewhat more accurate when he said (1925, 29) 'Between the epoch which stretches from Pythagoras to Plato and the epoch comprised in the seventeenth century of the modern world nearly two thousands years elapsed.'

tion. But he would have to do just that – pretend – since the names of taxa cannot be defined in terms of essential characters without falsification on a scale which should have been evident even to the most uncritical investigator with only a limited knowledge of the organisms being classified.

The conflict between reality and theory was largely ignored by early taxonomists both because they did not understand the logic of Aristotelian definition very clearly and because even scientists have a way of not noticing what conflicts with their philosophical presuppositions. (Hull 1965, 316)

Hull did realize that there was a danger in carelessly applying labels that were not well defined, for he had noticed the absurdity of Simpson calling Darwin a typologist (Simpson 1961, 48; Hull 1965, 317). Perhaps taking a cue from Popper (who had said that people who expected too much of definitions were ‘methodological’ essentialists even if they did not believe in Plato’s world of ideal forms), Hull proposed to unpack his terms.

The three essentialistic tenets of typology are (1) the ontological assertion that Forms exist, (2) the methodological assertion that the task of taxonomy as a science is to discern the essences of species, and (3) the logical assertion concerning definition. (Hull 1965, 317)

Yet while Hull was sure that Darwin and many other modern taxonomists could not be convicted of (1) or (2), indeed they may have held only ‘part of’ (3), so that it was excessive to call them typologists. His analysis left the impression that only very recently and reluctantly had taxonomists given up essentialism with respect to defining taxa. With respect to defining the species category, reading his article would presumably finally free them from that last remnant of essentialism.

A few years after reading Hull’s paper, Mayr (1968) accepted the word ‘essentialism’ as a synonym for typological thinking. Ignoring Hull’s warning, he treated these as synonymns. The essentialism story now took on its canonical form (Mayr 1976, 1982).

These details about the creation of the essentialism story, by two biologists and a philosopher, none of whom had done any serious historical research, certainly cannot demonstrate by itself that the story is false. It is possible that they were catching hold, as each of them in different ways believed, of some profound and previously overlooked truth that sits at the center of the history of systematics. But structurally what is perfectly evident in the subsequent literature is that the story’s authority simply grew with repetition.

Personal Postscript

My methods in assembling this history of the early years of the essentialism story have been those I learned when I began my career as a professional historian in the 1960s. We were taught that an author's actions should be put into the context of his own era, particularly the writings he read, while connections to the historian's own later period should be avoided. Of course many historiographers have shown that such prescriptions are naïve, because a god's-eye view is quite unachievable, so that an historian claiming objectivity is duping herself and her readers. Certainly there is a great deal more to be said about the essentialism story than naming the people who first wrote about it. And on the most important question – how much of it is true? – surely the logic of justification is more relevant than the logic of discovery. But if I may be excused for leaving those larger issues out of this paper, I might not be forgiven if I do not admit my own connections to the history I have just related.

As an undergraduate at Harvard (1961-1965) and graduate student at Yale (1965-1969) I took no course from Ernst Mayr but did read, with admiration, some of his writings (1957a, 1957b, 1963, 1964). Everything he said seemed right, and it was encouraging to me, as I undertook a foray into the history of taxonomy for my doctoral dissertation, that he was celebrating the value of this subject at a time when leaders of our young discipline were saying it was devoid of interest.²¹ Yet I could not imitate Mayr's willingness to deal with great questions, because the professional standards that governed me required me to focus on primary sources and to control my material, that is, not to venture an opinion on matters about which I had no evidence.

When Imre Lakatos declared that 'Philosophy of science without history of science is empty, history of science without philosophy of science is blind' (1971, 91), the criticism certainly applied to me. I had never formally studied either philosophy or the philosophy of science, although I heard of Plato's cave and did read Kuhn. To comment upon what Cain said about Aristotle, or what Hull said about Popper, or what Mayr said about Plato, I regarded as hopelessly beyond my field of competence.²² Even when others raised serious questions about the essentialism story (Sober 1980, Atran 1990), I continued to limit my attention to modest bits of history (Winsor 2000a, 2000b).

²¹ On the first page of my dissertation I quoted Charles Gillispie, editor of the *Dictionary of Scientific Biography*, who had said, 'Taxonomy little tempts the historian of scientific ideas' (Gillispie 1960, 170).

²² Even after I had closely studied every publication of Louis Agassiz, plus much of his correspondence, which convinced me that there was something wrong with Mayr's claim that it was impossible for Agassiz, as an essentialist, to admit the variability of species, my article pointing out the contradiction (1979) was so cautious and technical that it had no impact whatever in dislodging Mayr's interpretation.

When I began my investigation into the mid-20th century writings of two biologists, Cain and Mayr, my interest was to discover what motivates a scientist to take up the study of history. Cain's case was a promising one, because his Linnaeus paper was early in his career, and he was frank about the scientific benefits he hoped to gain (Winsor 2001). I was surprised to find, in a paper I had long been impressed by, that the flaws in his historical method stood out once I looked at his sources and considered his motives. In taking up Mayr, I was again surprised to see how powerful these two elementary historical techniques can be. Simply putting Mayr's early statements about typology in context was enough to make me doubt their soundness.

Whether the essentialism story can be rebuilt upon a more sound foundation, as some scholars believe (Stamos 2005, 2007), or whether most of what went on in the history of taxonomy falsifies it, as others now argue (Wilkins 2004, Amundson 2005), remains to be seen. I hope in any case that my work will not be taken as a personal criticism of three men whom I hold in very high esteem: Arthur Cain, Ernst Mayr and David Hull.

Acknowledgements

This research would not have been possible without the generous support of the taxpayers of Canada through a grant from the Social Sciences and Humanities Research Council. Gordon McOuat's research and insights have contributed immeasurably to enlarging my understanding. I have also benefited greatly from discussions with Peter Stevens and his students at the Missouri Botanical Garden, and with my own students, Conor Burns, Darrin Durant, Brigit Ramsingh, Sara Scharf, and Charissa Varma. I am indebted to Robert S. Cox at the American Philosophical Society for checking Cain's paper for me. My greatest debt is certainly to the late Ernst Mayr, who always encouraged my efforts in the history of systematics, and responded to my departure from his views with graceful forbearance. In particular, he gave me permission to examine his papers and correspondence in the Harvard University Archives, and allowed me to publish the excerpts quoted here.

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