

DESCENDED FROM DARWIN
INSIGHTS INTO THE HISTORY OF
EVOLUTIONARY STUDIES, 1900–1970

Joe Cain and Michael Ruse, Editors

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CHAPTER 6

ERNST MAYR AND THE “BIOLOGY OF BIRDS”

Joe Cain

INTRODUCTION

Too often, formal publications structure the narratives we historians build. It is our legacy from intellectual history and the desire for declarative statements. It is also a measure of the convenience of published books and papers. We often forget, however, how these literary products stand in for activities that go much further—tips of icebergs comprising lives as lived. Sometimes those tips represent pinnacles; sometimes, peripheral edges. Sometimes, too, they are unrepresentative and distract biographers away from more revealing paths. Historical studies risk failure when the relationship between books and lived lives goes unexamined.

The narrative of Ernst Mayr’s life (1904–2005) has been grounded firmly in literary products that carry his name (Mayr, 1942, 1963; Mayr, Linsley, & Usinger, 1953). Bock (1994, 2005) and Haffer (1997, 2002, 2005) have done splendid work stretching our biographical knowledge beyond Mayr’s main publications to give a robust sense of overarching research programs and legacies inherited from mentors. Others, too, had added significant pieces to this biographical puzzle (Beatty, 1994; Burkhardt, 1994; Gill, 1994; LeCroy, 2005; Lein, 2005).

Two series of studies—one by me (Cain, 1993, 1994, 2000, 2002a, 2004); another by Smocovitis (1994a, 1994b)—approached Mayr in a different way. They reconstructed his considerable activities as “community architect” in evolutionary studies. This focused attention on efforts to create and co-opt professional and research infrastructure to advance evolutionary theory and evolutionary systematics. This was work “behind the scenes” of traditional publication-based research. Of course, Mayr was not the only person doing this, but he certainly was one of the most productive. Later work on the same line by Mayr in systematics continued that story (Cain, 2004a). Collectively, these studies made the point fairly plain. Mayr was no mere administrator,

and this attention to infrastructure added important elements to his intellectual goals. This was active, purposeful management, even though it sometimes included rather individualistic ends within cooperative and communal means.

It is hard to imagine Ernst Mayr without the book that made him famous, *Systematics and the Origin of Species* (Mayr, 1942b). After all, it is what *he* wanted us to remember, especially after moving in 1953 to a Harvard professorship. It was no accident he wrote little by way of personal autobiography, but much intellectual autobiography (e.g., Mayr, 1980, 1982, 1984, 1992a). (And we must not forget how much we spectators pushed it upon him for our own reasons.) So strong is the emphasis on this book in ours and his own recollections that many of us have been perfectly content to have Mayr appear in our narratives *deus ex machina* circa 1940 to set straight a confused gaggle of biologists and to launch his geographical theory of speciation.

In my own writing and speaking about Mayr, I have frequently noted the importance of his career prior to writing *Systematics and the Origin of Species*, offering tidbits but no systematic analysis. In the present chapter, I aim to flesh out this picture more fully. Following Bock and Haffer, I argue a rather different Mayr emerges if we build our picture starting from the beginning of his career. Some specific turns in his life grow in importance. Crucially, Mayr's turn into evolutionary theory around 1940 came as something of a surprise, a road taken when numerous alternatives were equally likely. What Mayr imported into evolutionary biology was far from novel. This is true both for specific concepts and for the overall approach he advocated. Focusing on the later consequences of this turn obscures Mayr's role in this period. It also obscures a much larger intellectual program, the biology of birds.

GETTING STARTED

The general shape of Mayr's start is well known (Bock, 1994; 2005; Cain, 2002a; Haffer, 1997; 2005). His formal training began in medicine. After earning a preliminary degree in 1925, Mayr abandoned medical training for ornithology. While at university, he spent term breaks and summers as a volunteer at the Natural History Museum in Berlin (Zoologische Museum der Universität zu Berlin), encouraged by the ornithologist Erwin Stresemann. It was Stresemann who recruited Mayr into professional ornithology and shaped his initial training.

Stresemann identified Mayr as suitable for collecting work on an expedition planned for Africa. Mayr was keen to go. Stresemann insisted he complete a doctoral thesis before embarking. This meant working quickly, which Mayr did. Sixteen months after starting, he completed a thesis on the European distribution and biogeography of the serin finch, *Serinus canaria serinus*. In the meantime, the African expedition had fallen through. Stresemann arranged an assistantship for Mayr at the museum, meant to hold him in place until another field opportunity arose.

That happened in 1928, when Stresemann convinced several competitors to hire Mayr in a cooperative venture. First, he was to collect in Dutch New Guinea jointly for Walter Rothschild's museum at Tring and the American Museum of Natural History (AMNH). Second, he was to collect in the former German Mandated New Guinea for Stresemann. In February 1928 Mayr left Berlin for the Pacific. Before returning, he was asked to join the Whitney Expedition in the Solomon Islands, again collecting

for the AMNH. This added a year to his travels. Returning to Berlin in 1930, Mayr set to work curating and studying the New Guinea material.¹

Reports of a diligent, efficient, and enterprising young Doctor Mayr preceded his return from the South Seas. As a result, he developed a reputation. A proven man, Mayr was considered by Walter Rothschild as a possible successor for Ernst Hartert at his museum in Tring. He certainly would have taken the post if it was offered, but Rothschild's financial problems prevented him from offering the post to anyone. As a second choice, Mayr was recruited by the AMNH to process its backlog of ornithological materials from the Pacific, some of which he had collected personally. Mayr began in January 1931. At the start, he was a contract employee, hired as short-term technical laborer and not as permanent curatorial staff. No one supposed he would stay in Manhattan for the long term. Mayr quickly proved himself in technical work with the specimens. With a good memory and an eye for detail, he also was productive with the classification and nomenclatural work of microtaxonomy. In this short-term contract, Mayr was kept focused on the collections—not expected, or encouraged, to do anything else. Then, within a year of his arrival, the unexpected happened.

In 1932, Rothschild sold his ornithological collection to the AMNH.² Numbering 280,000 skins and other materials, this was a massive acquisition. Assimilating it into the AMNH collection was an enormous task. Impressed by Mayr's curatorial abilities, the Department of Birds offered him an expansion of duties and a promotion. With no alternative yet on the horizon in Germany, Mayr accepted, little predicting that for the next 20 years his principal responsibility would be curation of this material. The duties attached to this post constrained Mayr in many ways. For instance, when he wanted to return to expedition work and add to the AMNH's collections, his proposals were declined. He was hired, managers said, to do a specific, local job. He needed to concentrate on that.³ Moreover, his position was funded by patrons, and this meant a considerable amount of work along the lines of someone else's priorities.

It is easy to jump ahead in Mayr's biography to *circa* 1940 and the start of his activism in evolutionary studies. That is a mistake. It leads to a fundamental distortion of Mayr's intellectual program, and it skips over several failed attempts Mayr made to break free from his low-ranking shackles.

A RESIDENT ALIEN

In 1931, Mayr was more emigrant than immigrant, more transplanted than uprooted (Bodnar, 1987). His scientific personality and intellectual allegiances were shaped by the intellectual environment of the Natural History Museum in Berlin. Mayr saw himself as a disciple of Stresemann, an advocate of what Haffer (1994, 1997) called the "Stresemann school."⁴ As systematists, this school approached microtaxonomy with an emphasis on wide species boundaries, biogeographical variation, and an interest in intermediate groups between subspecies and species. Haffer traces Stresemann's own lineage to Henry Seebohm and Ernst Hertert's "school" of European ornithology, with the Berlin museum and Rothschild's museum in Tring as important centers. As Mayr described his affiliation near the end of his life,

I belonged to a German school of evolutionary taxonomists that was unrepresented in the United States. Our tradition placed great stress on geographic variation within species,

and particularly on the importance of geographic isolation and its role in leading to the origin of new species. (Mayr, 2004, p. 47)

This affiliation comes through throughout Mayr's historical writings: from obituaries he wrote (Johnson, 2005; Mayr, 1934, 1973, 1975b, 1992b), to biographical commentaries (Mayr, 1955), to his larger historical work (Mayr, 1975a, 1982, 1984, 1992a). In early correspondence, Mayr described a good deal of American ornithology as arcane and unsophisticated. He complained much of the discipline in America was little more than bird-spotting: an obsession with naming, regional guides, collecting, identification, and unusual occurrences. Collections were item based. Facts were privileged over explanation. Taxonomic splitters created species names with seemingly no interest in underlying biology and raised groups to higher ranks seemingly on a whim.

For the historian, it is crucially important to view Mayr in 1931 with his European mindset: an emigrant importing into New York traditions from his German home. He arrived with Stresemann's confidence in their approach and was told to "lead the way" for other ornithologists to follow (Haffer, 1997, p. 2). No wonder Mayr spent his life attempting to reform ornithology. Also no wonder he described himself as delivering improvement under the framework of a "scientific" ornithology.⁵

Mayr's affiliation with this research school explains the extraordinary potential offered by employment at the AMNH. It was not simply *any* job in *any* museum. By adding the Rothschild collection, the American Museum now held the largest and most comprehensive bird collections in the world. Many of its species, subspecies, and varieties were represented by *multiple* rather than *single* specimens, some by dozens or even hundreds of individuals. For a systematist with Mayr's training and interests in geographic variation, nowhere were the opportunities so obvious. Thus, no matter what the employment prospects at home, Mayr had every reason to stay in New York.

The extent of Mayr's commitment to his Berlin training is clear. However, the biogeographical approach to microtaxonomy was only one element of that training. A second involved an allegiance to biology over zoology, process over object, dynamic over static themes, and explanations over observations or descriptions.

Whereas zoologically oriented ornithologists gave priority to knowledge of objects and particulars, *biologists* shifted their attention to abstracted processes and principles, then to their many instantiations. Hence, while Mayr daily worked within the minutia of nomenclature, description, and identification while he was curating material, he kept his priorities on other matters. He frequently complained of pedantry and nominalism in the profession, citing a general failure to appreciate the biological and ecological settings in which their museum specimens originally lived. Consider his attempts to encourage colleagues to shift their focus, such as Alden Miller (Mayr, 1942a), or later, Waldo LaSalle Schmitt:

The trouble with us taxonomists is that we are so busy with all sorts of jobs we don't get around to doing research, and when we do we usually don't have enough leisure to produce a good, all-around biological paper but have to be satisfied more or less with taxonomic revision. On the other hand, the production of papers of general interest by taxonomists is practically a survival necessity. We must impress on general biologists that

taxonomic work is not merely a clerk's job but real, genuine biology. Furthermore, we must impress on them that it yields results that are inaccessible to any other branch of biology. I think we have made a good beginning and taxonomy enjoys a greater prestige now than it had for a generation or more. We have to keep at it to improve our position, otherwise we will slide back again.⁶

Gill (1994) called this a commitment to "total" biology; Haffer (1997), "holistic" biology. Mayr himself simply referred to this perspective as the "biological" approach, as in his contrasting the "biological" and "morphological" species concept.

This "biological" approach showed while Mayr collected in the Pacific. Stresemann placed great emphasis on understanding birds as *living* creatures. He expected his students to know their organisms inside and out: their physiology, behavior, migration patterns, ecology, and distribution over the globe. Identification and classification were just the start of a serious taxonomist's work. Mayr followed suit, aspiring to what he called "scientific" standards, meaning biological, rather than zoological ones. Along this line, he kept extensive field notes on biological subjects. When he or his trackers shot and skinned a bird, for instance, Mayr carefully recorded its ecological setting, its stomach contents, the time of the day and the date it was collected, and anything special observed about the bird's behavior. Later, when giving advice to an ornithologist collecting in New Guinea in 1934, Mayr passed on the same advice, emphasizing the need to learn as much as possible about the "life history and ecology" of the fauna being collected. "Don't forget to make extensive notes on stomach contents." "There seems to be a very definite correlation in [a particular family of birds] between the structure of the tongue and the food. Notes on the food are therefore very welcome." "A thorough anatomical study . . . would be highly worth while." "I don't need to emphasize the importance of the study of the breeding conditions of New Guinea birds. Also, if possible make notes on the molt of your specimens." His collecting instructions continued for three full pages.⁷

The breadth of Mayr's biological interests is often forgotten by historians (though not by ornithologists like Bock, Haffer, and a few others). The singular focus on evolutionary theory has distorted our perspective. Consider the biological interests Mayr showed in 1938, for example, when he agreed to deliver a lecture series at Ohio State University. He proposed five topics: genetics and geographical variation, functional anatomy, orientation/homing/bird migration, hormones, and behavior. "In every one of these five fields, ornithologists (also zoologists working with birds) have made significant contributions toward the advance of zoological science."⁸ By late 1939 and early 1940, theoretical discussion of speciation and evolutionary processes were beginning to dominate Mayr's attention. This represented a constriction of his biological interests, not an expansion of his taxonomist ones.

A REFORMER'S ZEAL

As he would do in the 1940s with evolutionary studies, the 1950s with systematics, and the 1960s with organismal biology, in the 1930s Mayr focused some of his energies on reforming community and research infrastructure. This first round of activism involved American ornithology in general and efforts to "lead" it toward a more biological perspective. Mayr did not work alone. He met with uneven success.

Mayr arrived in New York in January 1931. Shortly thereafter, he joined with a small number of biologically oriented ornithologists working hard to break the image of their discipline as mere stamp collecting (Johnson, 2007). This small coterie included Percey Taverner, Herbert Friedmann, and Ludlow Griscom.⁹ Amateur bird watchers and enthusiasts may spend their time noting unusual sightings and building catalog-style checklists. Meanwhile, “real” ornithologists—whom this group identified with adjectives like “younger,” “modern,” “scientific,” and “active”—were students of what they called bird *biology*. Ornithologists needed to present themselves *not* as birding enthusiasts, but as highly skilled biological researchers whose expertise happened to come from studying birds. Their research focused on life processes: behavior, ecology, functional anatomy, physiology, migration, geographic distribution, plumage, and evolution. It made use of mass collections and biogeographical information. It made use of both lab and field techniques, such as bird banding, film, and experiments under controlled conditions. Their approach was to be as analytical and interventionist as it was descriptive and observational. They sought underlying mechanisms, fundamental principles, and generalizations about abstract processes. They wanted to coordinate their knowledge with what other specialists knew about the biology of other taxonomic groups. Systematics was the glue holding this larger enterprise together.

Reform manifested just about everywhere in Mayr’s professional life. A small but illustrative example relates to the increasingly important journal, *Bird-banding*. Its book review editor, Margaret Nice, was entirely in sympathy with Mayr’s goals.¹⁰ As he explained to another colleague, “The majority of American bird students are averse to serious work.” “They consider birding a recreation, and do not bother to learn what other ornithologists are doing. It is a slow job to educate them to a deeper appreciation of the subject.”¹¹ *Bird-banding* had a reputation for supporting bird biology, and through it Mayr hoped to raise awareness about the best in Continental ornithology and thereby broaden “interest in the biological significance of ornithological work.”¹² Nice was perfectly happy to assist. Mayr reciprocated, promoting Nice and *Bird-banding* to his German colleagues, as discussed below.

In his curatorial work and microtaxonomy, Mayr campaigned in similar fashion. When publishing species descriptions, for instance, he deliberately included distribution maps and information about regional variation, thus giving data to anyone interested in biogeographical problems (e.g., his thesis: Mayr, 1926). Moreover, distribution maps were hints of biological processes and dynamics. Not surprisingly, when he reviewed taxonomic manuscripts in the 1930s, Mayr expected others to follow this path as well.

BIOLOGICAL ABSTRACTS

Near the end of 1932, Mayr began contributing to *Biological Abstracts*. In its first years, this project asked its section editors to recruit specialists who could volunteer to write abstracts for all articles in a particular journal. Mayr was invited to abstract articles for the *Journal für Ornithologie* beginning with volume 78, number 4. Stresemann edited this journal, and it is no surprise Mayr quickly accepted an invitation to promote it. Here was another means for introducing Americans to European work, i.e., biologically oriented ornithology.¹³

Writing for *Biological Abstracts* made Mayr conscious of how the structure of information limited interchange. The ornithology articles were filed under "ornithology." This limited their exposure and reinforced stereotypes, something unhelpful to a reforming bird biologist. After six months of abstract writing, Mayr complained to the editor, "I have noticed in the *Biological Abstracts* that there were not as many cross references from one section to another as might be advisable." This was no trivial point; lack of cross references discouraged communication and information exchange. "There are many papers regarding birds abstracted in the anatomical, physiological and genetic section . . . but there are no references to them in the bird section, and vice versa. A good many of the papers in the bird section should [have] cross references in the sections of anatomy, genetics, zoogeography, etc. I think more frequent cross references would make the *Biological Abstracts* much more valuable."¹⁴ The editor agreed, but complained that detailed cross referencing was logistically difficult.

Mayr pressed this concern again several years later, when a new editor arrived for *Biological Abstracts* and asked Mayr for an assessment of the project. Whereas the *Zoological Record* served as the world standard for detailed taxonomic literature, Mayr replied, the "chief services of the *Biological Abstracts* are its reviews of papers with a more general biological interest." But those services were hampered by poor cross referencing (still!) and by poor coverage of foreign language journals. Mayr told the new editor he was tired of seeing ornithological work overlooked by "geneticists, the physiologists, and anatomists." "It seems to me very futile," Mayr wrote in frustration, "to review for the ornithological section a detailed paper on the thyroids of birds and have it appear in the ornithological section . . . and then see some physiologist's paper on the same subject a year or two later, from the purely hormonal point of view and not even quote an abstracted paper because he only reads his section on hormones."¹⁵

LINNAEAN SOCIETY OF NEW YORK

The social landscape for scientists in 1930s New York was a complex maze. The Linnaean Society of New York (LSNY) was one of many social clubs. Organized in 1878 by a group of amateurs interested in ornithology and the natural sciences, the LSNY was closely tied to the AMNH. Indeed, boundaries between the two bodies often were blurred.¹⁶ LSNY ornithologists, amateurs and enthusiasts in the richest sense, functioned as an informal extension of the museum's bird department. For Mayr, the LSNY offered an escape from narrow curatorial work. He participated in, and often led, bird-watching excursions. He gave talks and demonstrations. He suggested readings. But trying "to lift" the society "to a higher level" proved difficult. "I tried for a year or two to change the Linnaean Society," he later wrote, "but with little result. I finally hit upon the idea of starting the ornithological seminar to which I invited only young and active ornithologists [and] students."¹⁷ (This was 1932.) "I expect to review in these seminars," he told Nice and prospective attendees, "all important papers in European journals, and hope to do my share in advancing European literature to American ornithologists."¹⁸

The reading group met in the museum. "What we did was to pick certain subjects or recent publications and let one member report on them. The rest of the group would join in the subsequent discussion. What I did was merely to pick the appropriate publications and suggest who should review them."¹⁹ Participants included Joseph

Hickey, William Vogt, Allen Cruickshank, Roger Peterson, and Arthur McBride, among others. “This nucleus cleared the way for a restoration of the Linnaean Society, and we have now as active a group as you might find anywhere in this country.”²⁰ Later, Mayr described the society as a “training school” for young naturalists in New York City. If he had his way, these would be the first American progeny of the transplanted Stresemann school.

Mayr used all the resources of the LSNY in his efforts to reform and encourage. He encouraged the publication of biologically oriented research through the society’s *Transactions*. For example, he shepherded Nice’s (1937, 1943) important work on song sparrows through the press.²¹ As he would later be with *Evolution*, Mayr was an invasive editor. He justified this by explaining to one junior colleague whose manuscript he had just severely criticized, “You can believe me that it does not give me a bit of pleasure to criticize you as extensively as I have done . . . and that I have only done it in order to help you. Dr. Stresemann used to tear my papers to pieces when I was his student, and I got very mad about it many times. Eventually I realized that it was for my best, and I am now deeply grateful to him.”²²

Mayr’s regular attention on “young” workers is significant. When giving advice for a similar group elsewhere, Mayr advised a colleague,

There is one point which you will have to remember and which was the whole secret of our success. . . . This secret is that you never get anything from the older members except a benevolent cooperation while the younger men really take up your suggestions and put them into practice. In the seminar, which I organized, nine out of ten were less than twenty-five years of age. . . . The beginner in ornithology will do whatever is suggested to him. The old-timer will continue to do what he did for the last fifteen or twenty years. There are, unfortunately, very few exceptions to this rule.²³

AMERICAN ORNITHOLOGISTS’ UNION AND *THE AUK*

Nowhere were Mayr’s efforts to promote bird biology through infrastructure more determined than with the American Ornithologists’ Union (AOU) and its journal, *The Auk*. From the first meeting he attended, in Detroit in 1931, Mayr was sour to the group, complaining it was obsessed with “triviality” and “single facts” (Taylor, 2004).

Nominally a national organization, the AOU in the 1930s leaned sharply toward the East Coast.²⁴ To reformers—including Mayr, Nice, Herbert Friedmann, and others—it seemed dominated by preservationists, amateur bird-watchers, and rather static, nomenclature-oriented taxonomists. As Joseph Grinnell complained to Mayr, “Among zoologists in university circles I too frequently get the insinuation that AOU fellowship merely involves amateurish or recreational interest in birds.”²⁵ Like the others, Grinnell wanted people elected as AOU fellows (the highest membership rank) for their “scholarly ornithological work” and not for their “social or political prerequisites.”²⁶ The reformers had high hopes. They wanted the AOU to serve as America’s premier ornithological association—“assuming leadership” in ornithological research and operating as *the* national ornithological association. Yet, they knew the society had degenerated badly and had been eclipsed by others.²⁷ “As it is now, it is only a congenial social group . . . not an active organization.”²⁸ It was the same sense of frustration that brought Friedmann to conclude the AOU was “hopeless as far as science was concerned.”²⁹

The Auk was no better. Pointing to the journal's "recent trend to emphasize abnormal occurrences, unusual data and 'records' instead of studying and describing the normal range and normal occurrences of our birds," Mayr complained,

Practically no papers can be found in recent issues of *The Auk* and other American journals monographing the range of a certain species or distribution maps. Such maps are being published in nearly every issue of the *Ibis* and continental ornithological journals, and certainly help to improve the standard of the faunistic literature. Unusual occurrences, stragglers, etc. are of little significance, and the stress laid on such abnormal data reminds me of the doctor who defined the human species as being blind, lame, having [tuberculosis], etc., picking out the abnormal occurrences.³⁰

Friedmann was far more blunt in his criticism, describing *The Auk* as being "as incapable of progress as its equally dead name-sake":

My tentative solution was to start a new journal, but until times improve that will be hardly feasible. If it were only possible to get [AOU] people to study the change in the [*Journal für Ornithologie*] since Stresemann took charge, it might be possible to talk to them, but it isn't. The trouble is that there are not enough real ornithologists to make a scientific journal self-supporting.³¹

Mayr knew Friedmann's example well. While a graduate student in Berlin, he had watched Stresemann reform the Deutsche Ornithologische Gesellschaft. "Under his energetic leadership" as secretary, the impressionable Mayr later recounted, the society quadrupled its active membership. As editor, Stresemann turned *Journal für Ornithologie* from "a secondary journal" to "what most ornithologists consider the leading ornithological journal of the world." "This was accomplished not by popularizing ornithology, but rather by emphasizing the scientific aspects of it. Dr. Stresemann's aim was, and is, to raise ornithology from the level of hobby to that of a biological science."³²

While conspiring to alter the AOU and *The Auk*, these would-be reformers met many obstacles. As for the journal, Mayr complained in 1933, change seemed impossible. The sitting editor, Witmer Stone (who served as editor between 1912 and 1936), had been in place for 20 years and showed no sign of leaving. As Mayr complained, "nobody wants to injure Stone's feelings because everybody respects his personality. Consequently, the editorial policy of *The Auk* will remain unaltered as long as he is the editor." Neither could the reformers secure change to editorial policies.

As Stone's retirement approached, Mayr maneuvered to have a candidate ready. "He must be somebody who is not too specialized either in field ornithology or migration studies, in nomenclature or any other single subject of ornithology. He must be somebody who is fairly well acquainted with the whole field and he should also be trained in general biology." Though he preferred a cooperating editorial board because he thought the job was "too big" for one person, Mayr settled on Friedmann as his candidate.³³ When Stone stepped down in 1936, Mayr was vocal and quick in his efforts to influence the selection of a replacement. As he told the chair of the nominating committee,

I personally feel that someone should be elected as editor who is a good modern field ornithologist in the best sense of the word. He should be a person who is familiar with

what is going on in the field of ornithology in this country and abroad, and who is not a specialist in any particular phase of ornithology. He should be a person with enough energy to go out of his way to get new contributors and to get papers of general interest. He should not be just a clerk who sends papers to the printers in the sequence in which they arrive by the mail. He also should be capable of weeding out unnecessary papers from the "Notes." There is a lot being published in *The Auk* which does not belong to the national journal of American ornithologists.³⁴

Glover Allen (Museum of Comparative Zoology) was elected editor of *The Auk* in 1936. Sorely disappointed, Mayr privately complained that Allen was really "a mammalogist" who "has not done any active work in ornithology for over twenty years" and that those controlling the election were "old-timers."³⁵

Before Allen had much time to ponder the job he had taken on, Mayr sent him a long letter describing a new vision for *The Auk's* editorial policy. On the selection of manuscripts, "*The Auk* should not lower its standards to the level of the only mildly interest[ed] majority of the membership, but it should try to educate them and raise them to a higher standard." "Nothing can more truly reflect the scientific status of the ornithologist of a country than its national publication. It should represent all active lines of research. It should inform the working ornithologist of everything of interest that goes on in America and abroad, and should thus stimulate him to more and better work." The editor should "go out of his way to secure valuable material." To increase the quality of individual papers, the "efficient" editor also should "use red ink generously, and then return the paper for improvements to the usually very grateful author." Allen should add more color plates and photographs, too. He also should create a "cooperative association" with local ornithological journals, which should be publishing most basic field notes so that room can be made in *The Auk* for more general research, and clean up the secretary reports in each volume. Furthermore, the editor need not work alone. He could appoint an editorial board (suggestions for which Mayr offered) with "at least some of the actively publishing members of the AOU, not the same old crowd of retired ornithologists that now fills most of the committees of the Union."³⁶ Clearly, Mayr saw control of *The Auk* as a potential triumph for bird biology.

Ultimately, this campaigning came to nothing, and Allen was unpersuaded. Allen's election as editor in 1936 was the final provocation for Mayr with respect to the "antiquity of the AOU constitution." He prepared a series of "improvements" in the society's constitutional structure that would reduce the role of the amateurs and increase the influence of professional, active, scientific workers. He planned to propose these at the next annual AOU meeting, scheduled for Charleston, South Carolina, in the autumn of 1937. After circulating his plan to reform-oriented colleagues, Mayr received important tactical advice from Grinnell. "You men who are in attendance at the Charleston meeting—everyone sympathetic with our points of view—should get together in advance of the elections, and arrive at an agreed-upon plan of action."³⁷

Attending the birth of his daughter prevented Mayr from attending the Charleston meeting. But that did not prevent the reformers from making significant inroads into the AOU's structure. Mayr and Nice were elected Fellows. Friedmann was elected president, James Chapin and James Peters—both sympathetic to the reform efforts—were elected vice presidents. Describing the results to Alden Miller, also interested in

reform, Mayr was enthusiastic, "All in all, one might say that the elections were better than they have been at the most recent meetings. The time was apparently ripe for an attack on the old people who were in power."³⁸

One of the changes these reformers sought while in office was the invigoration of the AOU as a facilitator of research. As (now president) Friedmann explained, in an open letter on "The Role of the AOU in Ornithology Today,"

in the field of scientific endeavor and progress, [the AOU] should again assert its influence and attempt to mold, to encourage or discourage as the case may be, different trends or developments. If the Union, composed as it is, of the best ornithological experience and knowledge in America, cannot play a progressive and vigorous role in formulating or directing general tendencies in modern ornithology, then it is high time we call in the doctor. . . . The old, well established fields will take care of themselves; it is the new ones [elsewhere described as "avian physiology, genetics, psychology"] that need encouragement and aid. . . . The AOU is precisely the body that should serve this function—never authoritarian, always advisory, tolerant, but with the courage of its own convictions.³⁹

To pursue such interests, a Research Committee with various specialized subdivisions was organized and put to work.

The reformers' success was neither exhaustive nor lasting. They simply were too few in number and not positioned to sustain real authority. Their struggles continued over the following years, and entrenchment remained elusive. In 1943, for example, Joseph Hickey worried to Mayr the AOU was "going to pot" when compared with the Wilson Ornithological Club and its publications.⁴⁰ In reply, Mayr continued to complain that "My plea for the election of young men fell on deaf ears," and that "there is no hope for a striking improvement in *The Auk*." "As I see it," he told Hickey, "there is no use trying as long as most of the younger and more energetic men are in the Army. I hope to take up the matter again as soon as times improve."⁴¹

Though never as intense as in the thirties, Mayr returned to the goal of reforming the AOU throughout his long career.⁴² Importantly, in the overall chronology of the 1930s and 1940s, Mayr's frustration with failures relating to the AOU coincide with his shifting attentions to organizational activities in species studies, namely the Society for the Study of Speciation and the Committee on Common Problems of Genetics and Paleontology (Cain, 2002a, 2004b, 2007). There, his success was spectacular.

"BIOLOGY OF BIRDS" EXHIBITION AND BOOK

The emphasis most historians place on Mayr's literary creations has led them to miss a different type of intellectual product, one that became an important expression of his biological ornithology. In the late 1930s, Mayr was assigned the task of assisting with a new exhibition at the AMNH, titled "the biology of birds." In 1945 he was made full-time chair of the exhibition committee, then he played a significant role in shaping the final product.⁴³

In 1929 the AMNH announced plans to build a new wing, ultimately named in honor of Harry Payne Whitney.⁴⁴ The new Whitney Wing was a expansion north along Central Park West, connected to the rest of the museum by the new Theodore Roosevelt Memorial Hall (opened October 1933). The new wing would be devoted exclusively to the Department of Birds and its collections. Space allocation was clear

from the start. Of the six floors, three were set as exhibit space; three for collections and offices.⁴⁵ For exhibits, the main level (their second floor) would be a permanent display of dioramas depicting oceanic birds from the Pacific, a focus for the Whitney family's patronage over the years. One floor below these dioramas (their first floor) was to be a hall dedicated to education and informative displays.⁴⁶

The "Biology of Birds" exhibit opened in 1948 in the first floor hall (Cain, 2006; Mayr, 1948b). The floor plan was straightforward: 16 alcoves around the walls with a central series of displays dividing the room into two long segments. In its content, the exhibit featured three distinctly different elements. Mayr worked hard to ensure his biological approach was its centerpiece.

One sequence of panels offered a synoptic display, presenting bird diversity at the family or subfamily level. It also introduced basic anatomical concepts. This fit into the museum's educational agenda. It also replaced an existing exhibition elsewhere in the museum, in the "Birds of the World" hall. Mayr's intellectual role in shaping the synoptic displays is not clear.

A second element of the "Biology of Birds" exhibition involved spectacle and the display of symbolic objects. As spectacle, for instance, a pair of white-thighed hornbills was installed in an alcove, complete with their nest and the original tree in which it was embedded, hauled from near the Congo River with considerable effort. This was tied to a strong message about parental responsibility. Likewise, a set of panels considered relations between "birds and man," celebrating the value of birds as food and illustrating the use of birds as cultural symbols. These panels seem to have been imposed on Mayr. Some were carryovers from temporary exhibitions in the museum (Chapman, 1943). Others were important departmental acquisitions, too good not to put on show.

Mayr's principal contributions involved the third segment. More than half the area of the exhibition involved diagrammatic panels illustrating basic biological concepts using birds as exemplars. Panels included migration, geographic variation, evolution, ecology, plumage, courtship displays, and the physics of flight. Predictably, Mayr devoted a whole alcove to evolution and speciation, closely tied to Mayr (1940; 1942b). No fewer than 15 biological subjects were given attention, presented as the scientific edge, and pinnacle, of ornithology.⁴⁷ His diagrammatic panels were arguments for the contributions "museum men" like him could make to the top biological problems of the day. They were demonstrations of how curators could extract information from their collections. Mayr took full advantage of this opportunity to give his vision a physical manifestation. Mayr repeated the same assertion whenever he had a platform (e.g., Mayr, 1942a; 1946; 1948a).

Mayr's freedom to expand the biological aspects of this exhibition was related to Albert Parr's arrival as new AMNH Director (1942–1959). The shifts from object to process and things to ideas were precisely the modernizing changes Parr was called upon to implement at the museum. Trained in biogeochemistry under G. Evelyn Hutchinson and an oceanographer by affiliation, Parr stressed "ecological biology," meaning studies of relations between living things and between them and their environments (Hutchinson, 1979, p. 239; Kennedy, 1968, pp. 240–244). Long after his departure from the AMNH, exhibition halls still manifested Parr's intellectual agenda.⁴⁸

Much has been made of Parr's antagonism to evolution.⁴⁹ But Parr's antagonism concentrated on a vision of evolution as a descriptive and narrative project,

i.e., "proofs" and "demonstrations" of evolution such as those described by Bowler (1996). For his part, Mayr exploited the new director's desire for a strategic shift in emphasis at the museum. And he did so brilliantly. Speciation was a process—or "subject" or "idea" in Parr's terminology. The myriad of isolating and diverging mechanisms made up the dynamic system driving evolution. The biological species concept brought to evolution and systematics an objective standard and a demonstrable test. Streams in Mayr's research program clearly already flowed in sympathetic directions. The intellectual substance of "The Biology of Birds" further played on these themes and added more, with each analytical panel drawing connections to facets of a Parr-like vision of biology. Mayr just happened to be using birds as his research tool.

In the broader frame of Mayr's career in the 1940s, this shift in emphasis—birds to evolutionary biology—became Mayr's ticket out of "mere" curation and toward more freedom to pursue bird biology. Reading the introduction to *Systematics and the Origin of Species*, Parr would have found a sympathetic plea in Mayr's chastising his colleagues about identification being "basic"—the "lowest' task"—for "museum men." Above that was classification (grouping species into categories and ranks). At the top were biological processes such as the study of species formation and factors of evolution. These highest-level tasks were synthetic in nature, Mayr said. They were only available to experts with panoramic vision, such as the broad-minded systematists working in museums whose collections put the world at their fingertips.⁵⁰

Connected with this exhibition, Mayr wanted to publish a companion book. In 1940, Mayr signed a contract with Oxford University Press to produce "The Natural History of Birds," later retitled, "The Biology of Birds." Mayr's initial plan was for this to be his first big book. It predated his commitment to the Jesup lectures by roughly a year. This book was meant as an American replacement to the British *Biology of Birds*, by J. Arthur Thomson (1923) and the German *Aves* by Stresemann (1927–1934), with a more analytical treatment compared with Allen (1925). Mayr discussed this project with considerable pride and ambition. He worked determinedly on it through the early 1940s. But, for several reasons, the book never materialized from Mayr's pen.

The most important reason was overwork. Aside from an increasing curatorial load in the department, including contingency planning for evacuation and bombing, the exhibition design dragged on. Designs and budgets constantly changed; labor shortages shifted priorities; museum politics intervened. To this, Mayr had taken on more editing work for sections of Peter's *Check-list of Birds of the World*. Plus, as war in the Pacific arrived, his locality expertise was drafted into service (Mayr & Jaques, 1945). Meanwhile, the war in Europe, and then reconstruction, were impossible to ignore. Mayr and his wife worked hard to help family and colleagues engulfed by those events back home.⁵¹ Another reason was Theodosius Dobzhansky, who encouraged, then pushed, Mayr into emphasizing speciation and evolution. In the early 1940s, Mayr's biological interests were still Mandarin, but decreasingly so. Largely this was because speciation came to dominate. Dobzhansky also dropped onto Mayr infrastructure projects aimed at expanding evolutionary studies. First, there were committee jobs Dobzhansky did not want to do, and then it was secretary of a society, then founder and editor of a journal. Mayr also was drawn into the real success his focus on speciation and evolution was producing. It proved to be a highly productive specialization for him. *Systematics and the Origin of Species* was a hit. People were responding to his

ideas. He was winning awards and recognition. With success on intellectual, professional, and social levels, it was hard to break away.

The *Biology of Birds* textbook slipped from Mayr's grasp in 1947–1948, just as the exhibition was reaching its completion and interest in speciation exploded. After the opening, the overload on Mayr led to health problems. At the time he described these as “heart trouble”; late in life, he said it was a “nervous breakdown.” He arranged some time away from New York, teaching in the Midwest and West Coast. The idea was completely abandoned when Mayr learned Van Tyne was producing a similar book (Van Tyne & Berger, 1959). When the AMNH refurbished the *Biology of Birds* exhibition in the late 1950s, Mayr encouraged Dean Amadon and Wesley Lanyon to complete a companion volume as he once hoped to do (Lanyon, 1964).

SPECIATION STUDIES

As a historian of the synthesis period, I formerly thought Mayr seemed out of place where most narratives first introduced him: in 1935, corresponding with Dobzhansky, a geneticist in Morgan's fly group (why read about beetles?), then attending seminars at Columbia University at the invitation of L. C. Dunn (why follow genetics?). And why would he agree to represent geneticists in 1943 when co-opted by Dobzhansky to play a role in the Committee on Common Problems?

Appreciating Mayr's affiliation with Stresemann's biological ornithology makes his foraging for information outside traditional ornithology perfectly understandable. He was more than a familiar face; he was adding to his own interests. Indeed, energized by contact with Dobzhansky, Dunn, and Milislav Demerec, Mayr found ways to apply recent genetics research to his broad program.⁵² He became a frequent visitor to Dunn's department. “I am anxious to keep my knowledge of Genetics up-to-date and would like to attend the Genetics Seminar whenever possible.”⁵³

And Mayr's interest was warmly received. In 1938, he had prepared a manuscript on sex ratios in wild birds that he hoped would contribute to several types of genetics studies. Dunn called it “extremely interesting and provocative” and thought it “would stir up new interest.”⁵⁴ At the speciation symposium in 1939, Mayr included genetic elements freely as he described processes of isolation and divergence (Mayr, 1940). After his own Jesup lectures (delivered March 1941), Mayr began attending the annual summer gathering of geneticists, the Cold Spring Harbor Symposia.⁵⁵ In 1942, he proposed some behavioral experiments, on mating preferences in pigeons. When this did not come to fruition, he turned to Dobzhansky and Demerec, who suggested similar work using *Drosophila*. Seeking approval from his managers, Mayr explained,

I have come to the conclusion in connection with my work on the origin of species that a study of the factors which control the mating of animals is one of the most badly needed research jobs in this field. There are certain factors, the so-called isolating mechanisms, which prevent the mating of individuals which do not belong to the same species. I'm planning to conduct a number of experiments . . . through which I want to determine how far the isolating mechanisms are inborn and to what extent they are conditioned during the early life of the individual.⁵⁶

Work with *Drosophila* would "make the problems clearer." Then, "as soon as I have completed these preliminary experiments, I am planning to continue the work with three species of oriental finches."⁵⁷ Mayr's request was approved.⁵⁸ The *Drosophila* phase was completed; the finch work was not.

CONCLUSION

When Mayr arrived in New York in 1931, he carried considerable intellectual baggage. Here was a disciple of Stresemann, an expert, sent to America on short-term contract work. He not only imported considerable factual knowledge about birds in the South Pacific but also arrived fully intending to transplant a research tradition deriving from the Seebohm-Hartert and Stresemann "schools." In his early correspondence, Mayr expressed great concern over the state of American ornithology. While he encountered many bird enthusiasts and nomenclature-based workers, he found only few "scientific" or biological workers. For a decade, he worked to improve this situation. Many activities show the imprint of his effort to convert American ornithology into a discipline focused on the biology of birds. The AMNH exhibition he worked on, the Sanford Hall's "Biology of Birds," offered a physical manifestation of this agenda.

Mayr's case reminds us how mistaken we are when we focus too strongly on literary products when we write our biographies. The lived life must take precedence.

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NOTES

1. Mayr (1932, 1943) provides some flavor to his experiences. Haffer (1997) reproduces related correspondence.
2. The background to this sale is often told. Murphy (1932) provides a sense of the excitement at the AMNH regarding this acquisition. Rothschild (1983) describes the British perspective.
3. For example, Frank Chapman to Ernst Mayr, June 17, 1933, Mayr Papers, Harvard University Library, Ernst Mayr Collection (hereafter MPH), folder 3.
4. For Mayr's assessment of Stresemann's work with *Journal für Ornithologie*, see Mayr to Fleming, March 6, 1936, MPH, folder 38. He called it "the leading ornithological journal in the world." Mayr's respect for his mentor is clear, see Mayr (1975b). Compare this with changes in *Ibis*, the British journal (Johnson, 2004).
5. Mayr knew his criticisms did not apply particularly to the Department of Birds at the AMNH, whose members were largely in sympathy with the Seebohm-Hartert-Stresemann approach. Senior colleagues such as Frank Chapman and R. C. Murphy held Mayr in high regard. Mayr also knew his criticisms did not apply to *all* American ornithologists (e.g., Barrow, 1992, pp. 558–578; Friedmann, 1931; Lincoln, 1931). This division in American ornithology deserves much further investigation. Indeed, the same must be said for all the object-based specialties in the life sciences.

6. Mayr to Waldo LaSalle Schmitt, March 9, 1948, Society for the Study of Evolution Records, Series V. *Evolution* Editorial Records, American Philosophical Society Library, Philadelphia, folder: "Schmitt."
7. Mayr to Austin Rand, February 8, 1934, MPH, folder 30. Also see Mayr (1932, 1997).
8. Notes on the lecture series are in MPH, folder 70.
9. This convergence took place by the end of the first summer Mayr was in the U.S. (1931); see Herbert Friedmann to Mayr, August 10, 1931, MPH, folder 6.
10. Mayr met Margaret Nice at the 1931 Detroit AOU meeting (Taylor, 2004) and they corresponded frequently, e.g., Mayr to Nice, March 7, 1932, MPH, folder 12. He became a strong supporter of her work; see Mayr to Charles Whittle, October 1935, MPH, folder 28.
11. Mayr to Charles Whittle, October 1935, MPH, folder 28.
12. Three years after beginning his work with Nice for *Bird-banding*, Mayr believed measurable success had been achieved, in large part through Nice's reviews for that journal. See Mayr to Whittle, October 1935, MPH, folder 28.
13. In a 1994 interview, Mayr told the author he did most of this abstracting at home late at night so as not to interfere with his official museum duties. Later, Schramm did not hesitate to ask Mayr—one of the few ornithologists he knew who was fluent in German and willing to volunteer their time—for additional assistance with other German-language publications, such as the *Beiträge zur Fortpflanzungsbiologie*. However, an overloaded Mayr declined.
14. For correspondence between Mayr and Schramm, see MPH, folder 15. Quote from Mayr to Schramm, May 31, 1933.
15. Mayr to Flynn, April 27, 1937, MPH, folder 38.
16. On relations between the Department of Birds and the Linnaean Society of New York, see Mayr to Murphy, January 19, 1943, MPH, folder 82. Mayr told Murphy that shortly after he arrived at the museum, James Chapin told him to work through the LSNY when it was possible. Barrow (1998) describes the Society. The *Proceedings of the Linnaean Society of New York* provides membership lists and notes on activities.
17. Mayr to Nice, December 6, 1937, MPH, folder 45.
18. Mayr to Nice, January 16, 1933, MPH, folder 12.
19. Mayr to Serventy, July 12, 1943, MPH, folder 87.
20. Mayr to Nice, December 6, 1937, MPH, folder 45. Hickey (1943) was an outgrowth of the LSNY training, among others.
21. See letters in MPH, folder 45, and Mayr (1975).
22. Mayr to Charles Kendeigh, July 12, 1937, MPH, folder 42.
23. Mayr to Serventy, July 12, 1943, MPH, folder 87.
24. The AOU has its origins in the 1870s and 1880s. Growing out of this group were sister societies in the Midwest (the Wilson Ornithological Club, with the *Wilson Bulletin*) and the West Coast (the Cooper Ornithological Club, with its *Condor*). For a basic sketch of the AOU's history and relations with these other groups—especially as they relate to the discussion in this section—see Mayr (1975a, pp. 378–382). For a general history of the AOU, see Barrow (1998) and Chapman and Palmer (1933).
25. Grinnell exercised considerable influence over the rival Cooper Ornithological Union on the West Coast. Joseph Grinnell to Mayr, June 26, 1937, MPH, folder 38.
26. Grinnell to Mayr, November 25, 1937, MPH, folder 38.
27. For example, see Taverner to Mayr, November 26, 1935; and Mayr to Taverner, December 7, 1935, MPH, folder 31.
28. Mayr to Grinnell, March 17, 1938, MPH, folder 58.
29. Friedmann to Mayr, February 4, 1932, MPH, folder 6.
30. Mayr to Fleming, October 26, 1933, MPH, folder 6.
31. Friedmann to Mayr, February 4, 1932, MPH, folder 6.
32. Mayr to Fleming, 6 March, 1936, MPH, folder 38.
33. Mayr to Fleming, October 26, 1933, MPH, folder 6.
34. Mayr to Fleming, April 1, 1936, MPH, folder 38.

35. Mayr to C. B. Ticehurst, January 22, 1937, MPH, folder 49.
36. Mayr to Grover Allen, September 10, 1936, MPH, folder 39. Also see additional suggestions in Mayr to Allen, December 6, 1937, MPH, folder 34. Importantly, this bold letter came six years *before* Mayr achieved his status as a major contributor to evolutionary studies with Mayr (1942). Also compare this with Mayr's later activity as editor of *Evolution* (1947–1949).
37. Mayr to Grinnell, August 19, 1937, and Grinnell to Mayr, November 6, 1937, MPH, folder 39. Other advice came from Ludlow Griscom to Mayr, November 5, 1937, MPH, folder 39, and Alden Miller to Mayr, October 22, 1937, MPH, folder 44.
38. Mayr to Miller, November 24, 1937, MPH, folder 44.
39. Undated, single-page document accompanying Friedmann to Mayr, March 30, 1938, MPH, folder 57.
40. "Joe" to "Ernie," October 16, 1943, MPH, folder 78.
41. Mayr to Hickey, October 30, 1943, MPH, folder 78.
42. Mayr returned to a campaign for *The Auk* in 1942 following the unexpected death of Allen and a search for his replacement. This time, Mayr and the other reformers successfully lobbied to have Friedmann assume control of the journal. By this point he also had to convince Friedmann to accept the offer. "The editorship of *The Auk* is the most important office in the Union," Mayr told his friend, "since it is the best means for educating the younger men to become the leaders of the next generation." Mayr to Friedmann, February 25, 1942, and March 6, 1942, MPH, folder 76.
43. Mayr later said he modeled the displays on a similar exhibition by Rensch at the Berlin Museum. At the same time, he did not have an entirely free hand. Significant input also came from many parts within the museum. For a sense of the internal influences on exhibit design, see Mayr (1948b). A preliminary plan was in place as early as 1935, probably without a great deal of input from Mayr, see Gregory to Chapman, July 2, 1935, with 8-page "Biology of Birds" outline. A revised plan came in 1944, "Departmental Plan for the LC Sanford Hall of Birds." Both are archival material in AMNH Department of Ornithology. Mayr was made chair of the exhibition committee in 1945.
44. AMNH (1933) and Murphy (1939) discuss the Whitney Wing in detail. Announcements appeared in the newspapers, e.g., *New York Herald-Tribune*, January 27, 1929, and *New York Evening Post*, March 14, 1929. The Whitney family were longstanding patrons of the AMNH. William C. Whitney (Secretary of the Navy under President Cleveland) was Trustee of the Museum for many years. His eldest son, Harry Payne Whitney, had enthusiast interests in the museum. After his death in October 1930, his wife continued to support activities in the bird department. R. C. Murphy and Leonard Sanford were principal negotiators in these arrangements.
45. Allocation of space changed significantly with the arrival of the massive Rothschild collection. In essence, the purchase of this material dashed hopes the new wing would solve chronic space problems and give room for the next generation of collecting.
46. The Hall was dedicated to Leonard Sanford, museum trustee and long-time patron of ornithology at the AMNH. Sanford played a crucial role in acquiring the Rothschild collection.
47. Useful comparisons are Lockley (1934) and Huxley (1936). The display of similar mindedness explains why the intellectual camaraderie between Mayr and Huxley went well beyond the notion of biological species, natural selection, or new systematics.
48. Exhibitions at the AMNH in the 1930s and 1940s deserve much further study, as other exhibitions developed similar concentrations on process rather than objects. For instance, animal behavior (Mitman, 1993), human biology, and ecology. Especially see AMNH Special Collections, Box "Central Archives. 1232. 1934–1952," for discussion by the Committee on Plan and Scope. 1942 was a critical year. "Modernization" was considered across the whole museum.
49. In fact, he dissolved the Department of Paleontology, claiming it was an antiquated topic and that evolution was a "settled question" and "no longer a burning issue." See Parr (1944, p. 16) and Kennedy (1968). Parr reversed this decision several years later.

50. Mayr made the same argument in the museum's main visitor's guide, where he asserted, "we may confidently report that ornithology leads in several branches of biological investigation" (AMNH, 1953, pp. 107–108).
51. The Mayrs' status as resident aliens did not help either. Reporting requirements were imposed. Mayr had been denounced by an informant at the American Museum as unpatriotic and as a risk to national security. He and his wife were arrested at their home in 1942. Their house was searched. Investigations followed. No corroborating evidence was found. His family likely would have been interned had the museum not intervened.
52. Mayr to Dunn, November 5, 1937, MPH, folder 37. Mayr began corresponding with Dunn in May 1937. Mayr repeatedly claimed that Dobzhansky (1937) was an important inspiration for him to work through the genetics literature (e.g., Mayr, 1992a, p. 4), but this should not be taken to mean he had *no* interest in genetics or experimental biology before this. Rather, Dobzhansky was key for converting Mayr away from neo-Lamarckian views.
53. Dunn to Mayr, November 12, 1937, Dunn Papers, folder "Mayr, Ernst."
54. Dunn to Mayr, January 30, 1938, Dunn Papers, folder "Mayr, Ernst." This manuscript was published as Mayr (1939).
55. In a photograph book for the 1941 symposium on "Genes and Chromosome Structure," there is a photograph of Mayr attending the conference though he is not listed on the conference attendance list; see American Philosophical Society Library item B:D917.p 1972 2485pr gift of Dr. Dunn, 1972.
56. Mayr to Murphy, March 18, 1943, MPH, folder 82.
57. Mayr to Murphy, March 18, 1943, MPH, folder 82.
58. Mayr spent mid-June to mid-July 1943 in Cold Spring Harbor. He hoped to develop additional experiments on isolating mechanisms in birds at the museum in the Autumn. Reports of Mayr's research appeared in the series "Experiments on Sexual Isolation" over the next three years with Dobzhansky as coauthor (Mayr & Dobzhansky, 1944, 1945; Mayr, 1946a, 1946b). Dobzhansky produced other research in this series too. Responsibilities at the museum forced Mayr to drop this research after the second summer; see Mayr to Hickey, May 24, 1945, MPH, folder 111. It was during later summers at Cold Spring Harbor that Mayr's daughter met James Watson (Watson, 2002).

REFERENCES

- Allen, G. M. (1925). *Birds and their attributes*. New York: Marshall Jones.
- AMNH. (1933). The American Museum and defeatism. *64th annual report of the trustees for the year (1932)*. New York: American Museum of Natural History and the City of New York.
- AMNH. (1953). *General guide to the American Museum of Natural History*. New York: American Museum of Natural History.
- Barrow, M. (1992). *Birds and boundaries: Community, practice, and conservation in North American ornithology, 1865–1935*. Cambridge, MA: History of Science, Harvard University.
- Barrow, M. (1998). *A passion for birds: American ornithology after Audubon*. Princeton, NJ: Princeton University Press.
- Beatty, J. (1994). The proximate/ultimate distinction in the multiple careers of Ernst Mayr. *Biology and Philosophy*, 9, 333–356.
- Bock, W. J. (1994). Ernst Mayr, naturalist: His contributions to systematics and evolution. *Biology and Philosophy*, 9, 267–327.
- Bock, W. J. (2005). Ernst Mayr at 100: A life inside and outside ornithology (Rev. ed.). *Ornithological Monographs*, 58, 2–16.

- Bodnar, J. (1987). *The transplanted: A history of urban immigrants in urban America*. Bloomington: Indiana University Press.
- Bowler, P. J. (1996). *Life's splendid drama: Evolutionary biology and the reconstruction of life's ancestry, 1860–1940*. Chicago: University of Chicago Press.
- Burkhardt, R. (1994). Ernst Mayr: Biologist-historian. *Biology and Philosophy*, 9, 359–371.
- Cain, J. (1993). Common problems and cooperative solutions: Organizational activity in evolutionary studies, 1936–1947. *Isis*, 84, 1–25.
- Cain, J. (1994). Ernst Mayr as *community architect*: Launching the Society for the Study of Evolution and the journal *Evolution*. *Biology and Philosophy*, 9, 387–427.
- Cain, J. (2000). For the “promotion” and “integration” of various fields: First years of *Evolution*, 1947–1949. *Archives of Natural History*, 27, 231–259.
- Cain, J. (2002a). Epistemic and community transition in American evolutionary studies: The “Committee on Common Problems of Genetics, Paleontology, and Systematics” (1942–1949). *Studies in History and Philosophy of Biological and Biomedical Sciences*, 33, 283–313.
- Cain, J. (2002b). Ernst Walter Mayr, 1904–. In W. Bynum, (Ed.), *Encyclopedia of life sciences*. London: Macmillan Reference Ltd.
- Cain, J. (2004a). Launching the society of systematic zoology in 1947. In D. M. Williams & P. L. Forey, (Eds.), *Milestones in systematics* (pp. 19–48). London: CRC Press.
- Cain, J. (Ed.). (2004b). *Transactions of the American Philosophical Society: Vol. 94. Exploring the borderlands: Documents of the Committee on Common Problems of Genetics, Paleontology, and Systematics, 1943–1944*. Philadelphia, PA: American Philosophical Society.
- Cain, J. (2006). *A display of ideas, not things: The “Biology of Birds” exhibition at the American Museum of Natural History*. Paper presented at the 2006 conference “A Century of Evolution: Ernst Mayr 1904–2005,” Linnean Society, London.
- Cain, J. (Ed.). (2007). *Regular contact with anyone interested: Documents of the Society for the Study of Speciation* (2nd ed.). London: Euston Grove Press.
- Chapman, F. (1943). *Birds and man: A guide to the exhibit illustrating the relations between birds and man. Guide Leaflet Series 115*. New York: American Museum of Natural History.
- Chapman, F. M., & Palmer, T. S. (Eds.). (1933). *Fifty years' progress of American ornithology, 1883–1933*. Lancaster, PA: American Ornithologists' Union.
- Dobzhansky, T. (1982). *Genetics and the origin of species* (L. C. Dunn, Ed.). Facsimile in *Columbia Biological Series: Vol. 11. Columbia Classics in Evolution Series*. New York: Columbia University Press. (Original work published 1937)
- Friedmann, H. (1931). Bird distribution and bird-banding. *Bird-banding*, 2, 45–51.
- Gill, F. B. (1994). Ernst Mayr, the ornithologist. *Evolution*, 48, 12–18.
- Haffer, J. (1994). The genesis of Erwin Stresemann's *Aves* (1923–1934) in the *Handbuch der Zoologie*, and his contribution to the evolutionary synthesis. *Archives of Natural History*, 21, 201–216.
- Haffer, J. (1997). “We must lead the way on new paths”: The word and correspondence of Hartert, Stresemann, Ernst Mayr—international ornithologists. *Ökologie der Vögel*, 19, 1–980.
- Haffer, J. (2002). Ernst Mayr: Ornithologist, evolutionary biologist, historian and philosopher of science. *Verhandlungen zur Geschichte und Theorie der Biologie*, 9, 125–132.
- Haffer, J. (2005). Ernst Mayr: Bibliography. *Ornithological Monographs*, 58, 73–108.
- Hickey, J. (1943). *A guide to bird watching*. New York: Oxford University Press.

- Hutchinson, G. E. (1979). *The kindly fruits of the earth: Recollections of an embryo ecologist*. New Haven, CT: Yale University Press.
- Huxley, J. (Director/Producer). (1934). *Private life of the gannets* [Motion picture]. R. Lockley, writer. London: London Film Productions.
- Huxley, J. (1936). *At the zoo*. London: George Allen and Unwin.
- Johnson, K. (2004). *The Ibis*: Transformations in a twentieth century British natural history journal. *Journal of the History of Biology*, 37, 515–555.
- Johnson, K. (2005). Ernst Mayr, Karl Jordan, and the history of systematics. *History of Science*, 43, 1–35.
- Johnson, K. (2007). Natural history as stamp collecting: A brief history. *Archives of Natural History* 34, 244–258.
- Kennedy, J. M. (1968). *Philanthropy and science in New York City: The American Museum of Natural History, 1868–1968*. Unpublished doctoral dissertation, Yale University, New Haven, CT.
- Lanyon, W. E. (1964). *Biology of birds*. London: Thomas Nelson and Sons.
- LeCroy, M. (2005). Ernst Mayr at the American Museum of Natural History. *Ornithological Monographs*, 58, 30–49.
- Lein, M. R. (2005). Ernst Mayr as a life-long naturalist. *Ornithological Monographs*, 58, 17–29.
- Lincoln, F. (1931). Bird-banding; its first decade under the Biological Survey. *Bird-banding*, 2, 27–32.
- Mayr, E. (1926). Die Ausbreitung des Girlitz (*Serinus canaria serinus* L.): Ein Beitrag zur Tiergeographie. *Journal für Ornithologie*, 74, 571–671.
- Mayr, E. (1932). A tenderfoot explorer in New Guinea. Reminiscences of an expedition for birds in the primeval forests of the Arfak Mountains. *Natural History*, 32, 83–97.
- Mayr, E. (1934). Ernst Johann Otto Hartert [obituary]. *The Auk*, 51, 283–285.
- Mayr, E. (1939). The sex ratio in wild birds. *American Naturalist*, 73, 156–179.
- Mayr, E. (1940). Speciation phenomena in birds. *American Naturalist*, 74, 249–278.
- Mayr, E. (1942a). Speciation in the *Junco*. [Review of the book *Speciation in the Avian genus Junco*]. *Ecology*, 23, 378–379.
- Mayr, E. (1942b). *Systematics and the origin of species: From the viewpoint of a zoologist*. Vol. 13, *Columbia Biological Series*. New York: Columbia University Press.
- Mayr, E. (1943). A journey to the Solomons. *Natural History*, 52, 30–37, 48.
- Mayr, E. (1946a). Experiments on sexual isolation in *Drosophila*. VI. Isolation between *Drosophila pseudoobscura* and *Drosophila persimilis* and their hybrids. *Proceedings of the National Academy of Science (USA)*, 32, 57–59.
- Mayr, E. (1946b). Experiments on sexual isolation in *Drosophila*. VII. The nature of the isolating mechanisms between *Drosophila pseudoobscura* and *Drosophila persimilis*. *Proceedings of the National Academy of Science (USA)*, 32, 128–137.
- Mayr, E. (1946c). The naturalist in Leidy's time and today. *Proceedings of the Academy of Natural Sciences of Philadelphia*, 98, 271–276.
- Mayr, E. (1948a). The bearing of the new systematics on genetical problems: The nature of species. *Advances in Genetics*, 2, 205–237.
- Mayr, E. (1948b). The New Sanford Hall. *Natural History*, 57, 248–254.
- Mayr, E. (1955). Karl Jordan's contribution to current concepts in systematics and evolution. *Transactions of the Royal Entomological Society of London*, 107, 45–66.

- Mayr, E. (1963). *Animal species and evolution*. Cambridge, MA: Belknap Press of Harvard University Press.
- Mayr, E. (1973). Erwin Stresemann. *Ibis*, 115, 282–283.
- Mayr, E. (1975a). Epilogue: Materials for a history of American ornithology. In E. Stresemann, (Ed.), *Ornithology from Aristotle to the present*. Cambridge, MA: Harvard University Press.
- Mayr, E. (1975b). Erwin Stresemann. *Verhandlungen der Deutschen Zoologischen Gesellschaft*, 67, 411–412.
- Mayr, E. (1980). Prologue: Some thoughts on the history of the evolutionary synthesis. In E. Mayr & W. Provine, (Eds.), *The evolutionary synthesis*. Cambridge, MA: Harvard University Press.
- Mayr, E. (1982). *The growth of biological thought: Diversity, evolution, and inheritance*. Cambridge, MA: Belknap Press of Harvard University Press.
- Mayr, E. (1984). The contributions of ornithology to biology. *BioScience*, 34, 250–255.
- Mayr, E. (1992a). Controversies in retrospect. *Oxford Surveys in Evolutionary Biology*, 8, 1–34.
- Mayr, E. (1992b). In memoriam: Bernhard Rensch (1900–1990). *The Auk*, 109, 188.
- Mayr, E. (1997). Reminiscences from the first curator of the Whitney-Rothschild Collection. *BioEssays*, 19, 175–179.
- Mayr, E. (2004). 80 years of watching the evolutionary scenery. *Science*, 305, 46–47.
- Mayr, E., & Dobzhansky, T. (1944). Experiments on sexual isolation in *Drosophila*. I. Geographic strains of *Drosophila willistoni*. *Proceedings of the National Academy of Science (USA)*, 30, 238–244.
- Mayr, E., & Dobzhansky, T. (1945). Experiments on sexual isolation in *Drosophila*. IV. Modification of the degree of isolation between *Drosophila pseudoobscura* and *Drosophila persimilis* and of sexual preferences in *Drosophila prosaltans*. *Proceedings of the National Academy of Science (USA)*, 31, 75–82.
- Mayr, E., & Jaques, F. L. (1945). *Birds of the Southwest Pacific, a field guide to the birds of the area between Samoa, New Caledonia, and Micronesia* ("First printing" ed.). New York: Macmillan.
- Mayr, E., Linsley, E. G., & Usinger, R. L. (1953). *Methods and principles of systematic zoology. McGraw-Hill publications in the zoological sciences*. New York: McGraw-Hill.
- Mitman, G. (1993). Cinematic nature: Hollywood technology, popular culture, and the American Museum of Natural History. *Isis*, 84, 637–661.
- Murphy, R. C. (1932). Moving a museum. *Natural History*, 32, 497–511.
- Murphy, R. C. (1939). Whitney Wing: The new home of the American Museum's Department of Birds. *Natural History*, 44 (September), 98–106.
- Nice, M. M. (1937). Studies in the life history of the song sparrow. I. *Transactions of the Linnaean Society of New York*, 4, 1–247.
- Nice, M. M. (1943). Studies in the life history of the song sparrow. II. The behavior of the song sparrow and other passerines. *Transactions of the Linnaean Society of New York*, 6, 1–328.
- Parr, A. (1944). Times and the museum. *American Museum of Natural History Annual Report*, 1944, 9–39.
- Rothschild, M. (1983). *Dear Lord Rothschild: Birds, butterflies and history*. London: Hutchinson.

- Smocovitis, V. B. (1994a). Disciplining evolutionary biology: Ernst Mayr and the founding of the Society for the Study of Evolution and *Evolution* (1939–1950). *Evolution*, 48, 1–8.
- Smocovitis, V. B. (1994b). Organizing evolution: Founding the Society for the Study of Evolution (1939–1950). *Journal of the History of Biology*, 27, 241–309.
- Stresemann, E. (1927–1934). *Handbuch der Zoologie: Aves*. Berlin: Walter de Gruyter.
- Taylor, S. (Ed.). (2004). Ernst Mayr at 100: Ornithologist and naturalist [Videotaped interview]. Washington, DC: American Ornithologists' Union.
- Thomson, J. A. (1923). *The biology of birds*. London: Sidgwick and Jackson.
- Van Tyne, J., & Berger, A. (1959). *Fundamentals of ornithology*. New York: John Wiley and Sons.
- Watson, J. (2002). *Genes, girls and Gamow*. New York: Knopf.