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# Organizing Evolution: Founding the Society for the Study of Evolution (1939–1950)

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There are many instances in the history of biology where the founding of a society or of a journal has signaled a new development. This surely can also be claimed for the founding of the Society for the Study of Evolution and of the journal *Evolution*.

Ernst Mayr<sup>1</sup>

## INTRODUCTION

In a previous article I offered an interpretive framework for understanding the emergence of evolutionary biology following the historical event recognized as the evolutionary synthesis.<sup>2</sup> In that article I stressed the wider process of unifying the biological sciences in the interwar and postwar period of American science through the emergence of a central science of evolution within the positivist theory of knowledge that then held sway.<sup>3</sup> In this paper, I will focus on a more local feature of the evolutionary synthesis, the reconfiguration and institutionalization of evolutionary practice through efforts made to organize evolution by the founding of the Society for the Study of Evolution (SEE). These efforts to unify evolution were simultaneous with the wider efforts made to unify biology. Following a brief introduction to its precursor societies – the Society for the Study of Speciation, and the

1. Ernst Mayr to A. E. Emerson, October 28, 1965, Society for the Study of Evolution Papers (SSE Papers), Library of the American Philosophical Society, Philadelphia.

2. For the most comprehensive account see Ernst Mayr and William B. Provine, eds., *The Evolutionary Synthesis* (Cambridge, Mass.: Harvard University Press, 1980). See also Ernst Mayr, *The Growth of Biological Thought* (Cambridge, Mass.: Harvard University Press, 1982).

3. See V. B. Smocovitis, "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," *J. Hist. Biol.*, 25 (1992), 1–65; for Ernst Mayr's response see "What Was the Evolutionary Synthesis?" *Trends Ecol. Evol.*, 8:1 (1993), 31–34.

Committee on Common Problems in Genetics, Paleontology, and Systematics – I will recount the story of the detailed negotiations leading to the founding of the SSE through a close examination of documents compiled initially by Alfred E. Emerson in 1965, preserved by successive secretaries of the society, and recently deposited at the Library of the American Philosophical Society.<sup>4</sup>

The founding of the Society for the Study of Evolution was part of a process that organized evolution, drew together evolutionists from the wider sciences, cross-linked and extended communications networks, mobilized and redirected available resources, legitimated the experimental science of evolution, and redefined as it reconfigured evolutionary practice within the maturing biological sciences. This society would in turn negotiate its location as a central science and cross-link with wider scientific societies. The community of evolutionists and their *sensus communus* emerged through the use of communications technology such as mimeographed questionnaires, bulletins, correspondence networks, and an international journal that facilitated information transfer between members; also instrumental were sponsored conferences, meetings, textbooks, courses of instruction, and ritualized practices that helped to bind together the heterogeneous practices of evolutionary studies. These efforts were part of the process that led to the construction of a common language, a *disciplinary discourse* which would lead in turn to the emergence of a new “central” science of evolutionary biology that would redefine the identities of the members. Critical to the process was the organizational role played by one chief “architect” of the evolutionary synthesis, Ernst Mayr.

4. The documents were given to the American Philosophical Society by the secretary of the SSE. It was the wish of both Emerson and Ernst Mayr that these papers be preserved so that the history of the society could be written. Neither was able to write a historical account of the founding of the society in the mid-1960s. See the letters between Emerson and Mayr on the founding of the society: Mayr to Emerson, October 28, 1965; Emerson to Mayr, November 25, 1965, SSE papers. The SSE papers have not yet been catalogued at the time of writing. I have also used the catalogued Ernst Mayr papers in the Library of the American Philosophical Society, along with documents from other locales, to reconstruct the story. For a history of efforts to organize evolution that focuses on documents pertaining to the Committee on Common Problems in Genetics, Paleontology, and Systematics see Joseph Allen Cain, “Common Problems and Cooperative Solutions: Organizational Activity in Evolutionary Studies, 1936–1947” *Isis*, 84 (1993), 1–25 (While this paper was in press, it came to my attention that Joe Cain presented a paper entitled “Building a Center of Attention: Ernst Mayr’s Editorship of *Evolution*, 1947–1949,” at the meetings of the International Society for the History, Philosophy, and Social Studies of Biology in July 1993.)

### THE GROUNDING OF EVOLUTION: DOBZHANSKY'S EVOLUTIONARY GENETICS AND HUXLEY'S NEW SYSTEMATICS

Moves to reconfigure evolutionary practice and construct an experimental science of evolution were well under way by the time of the evolutionary synthesis of the 1930s and 1940s. In the United States as early as 1904, at the height of the period recognized as the "eclipse of Darwin," efforts to experimentalize the declining area of inquiry associated with evolution and natural history were institutionalized with the formation of the Carnegie Institution for Experimental Evolution at Cold Spring Harbor. The Carnegie Institution extended its support even further when, in the 1920s, it backed Harvey Monroe Hall's efforts to create an experimental science of taxonomy at Stanford University.<sup>5</sup>

In England, similar moves to experimentalize the study of evolution took place. Julian Huxley, who was a keen organizer and had helped found the Society for Experimental Biology, brought together British systematists-naturalists into a central and cooperative organization that would aid the legitimization of systematics as an experimental science. In 1936, along with a company of systematists that included J. B. Turrill and J. S. L. Gilmour, he helped found the Association for the Study of Systematics in Relation to General Biology.<sup>6</sup> This "new systematics," as he envisioned it, would draw together workers interested in problems of speciation from the areas of genetics, ecology, and systematics into a common ground of biological practice that was heavily experimental.<sup>7</sup> As his grounding for the new "synthesis" of disciplines, Huxley drew extensively on his knowledge of the evolutionary

5. For a discussion of Harvey Monroe Hall, Frederic Clements, and the organization of the Carnegie Institution's team of Jens Clausen, David Keck, and William Hiesey see Joel Hagen, "Experimentalists and Naturalists in Twentieth-Century Botany: Experimental Taxonomy, 1920–1950," *J. Hist. Biol.*, 17 (1984), 249–270; see also chap. 5, "The Origins of Biosystematics," in V. B. Smocovitis, "Botany and the Evolutionary Synthesis: The Life and Work of G. Ledyard Stebbins Jr.," Ph.D. diss., Cornell University, 1988.

6. The Association for the Study of Systematics in Relation to General Biology entered into a "loose affiliation" with the Linnean Society of London, and its annual reports were published in the *Proceedings of the Linnean Society of London*. See the annual reports 1938–1940 for the organization of the association, a description of the difficulties encountered under wartime conditions, and the treasurer's statement, in *Proc. Linn. Soc. London*, 152 Session (1939–40), part 4, pp. 399–403.

7. See Julian Huxley, "Genetics and Ecology in Relation to Selection," *Nature*, 138 (1936), 748–749.

framework that was being articulated by the interaction of R. A. Fisher and E. B. Ford as well as Sewall Wright and Theodosius Dobzhansky. For Huxley, this new systematics emerging from taxonomy would serve as the “focal point” of the biological sciences:

Even a quarter a century ago it was possible to think of systematics as a specialized, rather narrow branch of biology, on the whole empirical and lacking in unifying principles, indispensable as a basis for all biological workers, but without much general interest or application to other branches of their science. To-day, on the other hand, systematics has become one of the focal points of biology. Here we can check our theories concerning selection and gene-spread against concrete instances, find material for innumerable experiments, build up new inductions: the world is our laboratory, evolution itself our guinea pig.<sup>8</sup>

Elsewhere in Europe, as well as in the United States, systematists-naturalists were to increase their efforts to solve the problems of speciation by reconfiguring genetics, ecology, and systematics. The efforts of systematists-naturalists like Erwin Baur in Germany, Scandinavian genecologists like Göte Turesson, Russians like Sergei Chetverikov and N. Timofeef-Ressovsky, and Americans like Francis Sumner and Edgar Anderson have long been recognized by Ernst Mayr, Mark Adams, William Provine, and Jonathan Harwood as playing an important role in the history of evolutionary biology.<sup>9</sup>

Simultaneously with these efforts to experimentalize evolution and construct the “new systematics,” the collaboration of mathematical theorists like Sewall Wright and R. A. Fisher with field biologists like Dobzhansky and E. B. Ford led to the making of an experimental and quantifiable science of evolution. Most attractive in the framework Dobzhansky provided in his 1937 book

8. Julian Huxley, ed., *The New Systematics* (London: Oxford University Press, 1940), pp. 1–2.

9. The relative contributions made by systematists-naturalists vs. theoretical population geneticists form the basis of the interpretive differences between Ernst Mayr and Will Provine. For a thorough discussion of the Russian context see Mark Adams, “The Founding of Population Genetics: Contributions of the Chetverikov School, 1924–1934,” *J. Hist. Biol.*, 1 (1968), 23–39; and idem, “Towards a Synthesis: Population Concepts in Russian Evolutionary Thought, 1925–1935,” *J. Hist. Biol.*, 3 (1970), 107–129. For the German context see Jonathan Harwood, “Geneticists and the Evolutionary Synthesis in Interwar Germany,” *Annals of Science*, 42 (1985), 279–301; and idem, *Styles of Scientific Thought: The German Genetics Community* (Chicago: University of Chicago Press, 1993).

*Genetics and the Origin of Species* was the grounding in genetics that would account for mechanisms of speciation in natural populations of organisms. With this emphasis on natural populations and the close consideration of geographic variation patterns in races, subspecies, and species, Dobzhansky's evolutionary genetics made tractable the long-standing problem of species, offering practical solutions to long-standing problems of evolution for a wide audience interested in speciation. The publication of *Genetics and the Origin of Species* thus served as a foundation for the consolidation of the network that would increasingly draw in a greater number of biologists.

Along with the consensus that evolutionists shared this common ground there simultaneously came a consensus that the new practices should be secured, sustained, and institutionalized through a collaborative and cooperative organization. This initial push to organize evolution came from those systematists-naturalists who saw in the recent developments common ground to solve persistent common problems of speciation. On December 28, 1939, at a special symposium entitled "Speciation"<sup>10</sup> organized by Dobzhansky at the American Association for the Advancement of Science (AAAS) meetings in Columbus, Ohio, Huxley, who was visiting the United States on behalf of European war efforts, met with Dobzhansky, Mayr, Emerson, and Carl Epling to suggest the formation of an official society for the study of speciation. Huxley's suggestion set in motion the process that would eventually lead to the founding of the Society for the Study of Evolution.

#### AN INFORMAL "INFORMATION SERVICE": THE SOCIETY FOR THE STUDY OF SPECIATION

The suggestion met with an instant favorable reaction, and a consensus arose that "some form of organization" would in fact "serve the interests of the rather large and growing number of speciation workers" who shared common problems in understanding speciation. Although the formation of a full-blown "journal-issuing society" was not yet called for, an informal organization that would "help to organize speciation as a definite and progressive field of

10. See *American Naturalist*, 74 (1940), 193–278, 289–321 for the published papers. Papers were given by Leon J. Cole, Sewall Wright, Ernst Mayr, Lee R. Dice, Warren Spencer, and Theodosius Dobzhansky. This was a joint symposium held by the American Society of Zoologists and the Genetics Society of America under the auspices of the AAAS. An earlier meeting (June 20, 1939) was held at Milwaukee and entitled "The Relation of Genetics to Geographical Distribution and Speciation."

biological endeavor" was thought to be desirable; in 1940 Alfred Emerson, professor of zoology at the University of Chicago, took on the task of organizing such an informal society by issuing a questionnaire to gauge support for a cooperative organization that would serve as an informal information service, distributing notes, news, and bibliographies on recent work from laboratories and museums across the country.<sup>11</sup> The responses to the questionnaire appeared to be favorable, and with the financial assistance of an anonymous donor Emerson launched the Society for the Study of Speciation (SSS). From its inception, the newly formed society was thus intended to facilitate the dissemination of pertinent information. The official notice of the SSS stated that its objective was "to institute an informal information service which will tend to correlate the various approaches" of workers in different fields interested in "an understanding of the factors influencing speciation." The society would make possible "a greater degree of integration between the various fields" and would allow the "attacking of problems" of speciation from "somewhat different angles" and with "different techniques."<sup>12</sup>

The major field of interest of the SSS was stated to be the "dynamics of the origin of species"; it therefore sought to bring together a disparate body of literature generated by workers in different institutional settings and locales, which would help solve the "central problem of the origin of species."<sup>13</sup> Evolutionists interested in conditions leading to divergence, population structure, and origins of local populations, races, and subspecies as well as the evolution of higher taxonomic categories were included. Within these sets of concerns, variation, isolation, and selection, which were the "major factor complexes," were singled out as the areas that needed the most clarification through examples from both plant and animal groups. The society was to facilitate information transfer, not only between the more standard fields long interested in the problem of species – bacteriology, botany, zoology, and anthropology – but between the fields of morphology, cytology, genetics, biogeography, ecology, paleontology, comparative psychology, comparative physiology, embryology, population biology, and taxonomy as well.<sup>14</sup> This information transfer would serve

11. Announcement by Alfred E. Emerson dated March 18, 1940. The questionnaire was soon circulated to those interested in the organization; L. C. Dunn Papers, American Philosophical Society, Philadelphia.

12. A. E. Emerson, "Evolution News," *Amer. Nat.*, 75 (1941), 86–89.

13. *Ibid.*, p. 87.

14. This list is reproduced from the official notice of the society.

to construct linkages between these disparate biological communities.

Emerson took on the task as the first secretary responsible for the general organization, collection, publication, and dissemination of relevant information; the Executive Committee included Edgar Anderson, John M. Beal, William Burrows, L. J. Cole, Lee R. Dice, Th. Dobzhansky, A. C. Kinsey, W. M. Krogman, Karl P. Schmidt, George G. Simpson, and Sewall Wright in addition to Emerson himself. Given the large number of scientific societies then in existence, the new society intended itself to be as informal as possible while having enough organizational structure to function effectively. In this way the group would be an "informal cooperative group of scientists willing to pass information from one to the other."

In the early 1940s the Society for the Study of Speciation thus served as an informal information service that facilitated communication between interested evolutionists. At its peak, the society had approximately 375 members.<sup>15</sup> In 1940 a mimeographed bulletin was circulated to interested workers across the United States. In addition to a discussion of the objectives of the SSS, the 29-page bulletin contained recent news from laboratories, notes and comments, short critical reviews, and a major 11-page review of Julian Huxley's *New Systematics* by Emerson.<sup>16</sup> Most important in the bulletins were the semi-autobiographical reports of recent activities by workers across the country, which introduced speciation researchers to each other and served to link up those who shared common interests.<sup>17</sup> Two additional issues

15. Alfred E. Emerson, undated document entitled "The Society for the Study of Speciation," SSE Papers.

16. A. E. Emerson, "A Critical Review of 'The New Systematics,'" ed. by Julian Huxley. 1940. Oxford," in *Bulletin of the Society for the Study of Speciation*, SSE Papers.

17. The following is an example of the entries under "News and Comments" of the bulletin:

Fox, H. Since 1936 I have accumulated much data on the relative abundance of local populations of species of Orthoptera, including some cases of seasonal and annual fluctuations; also on their habitat distribution in relation to types of vegetation and soil.

In 1938-39 I gathered additional data on egg content and nymphal emergence in oothecae of two introduced species of Asiatic Mantids, as well as in the relative abundance of their oothecae in local areas.

In 1938-39 I conducted experiments testing the effects of modified feeding procedure upon the survival of Japanese beetle larvae at constant high temperature. (*Bulletin of the Society for the Study of Speciation*, SSE Papers.)



of a primarily bibliographical bulletin were prepared and distributed in 1940.<sup>18</sup>

The Society for the Study of Speciation was off to a good start, but the initial momentum was not sufficient to sustain the group. The outbreak of the war in Europe thwarted and distracted further collaboration and interrupted international communications. This served to undermine the purpose of providing an information service. With the entry of the United States into the theater of the world war, communication between U.S. workers became even more difficult due to the consequent gas rationing and conservation measures. Some of the workers redirected their research efforts to wartime operations or engaged in wartime research, and others went into military service. Emerson's leadership as secretary was not sufficient to carry the society through this difficult phase of the war, and the nascent society began to flounder.<sup>19</sup>

The Society for the Study of Speciation thus lost momentum, but two more local organizations continued to support the study of evolution in the early 1940s. On the West Coast in the San Francisco Bay Area an informally organized group, begun sometime in 1937 under the name of the "Biosystematists," actively supported the continuation of the SSS and of evolutionary studies as a whole.<sup>20</sup> That the Bay Area supported many researchers interested in evolution is not surprising, given the region's demographic expansion which called for the increased institutionalization of the biological sciences. Many of the "Biosystematists" were in programs and departments that were heavily compartmentalized in comparable East Coast institutions.<sup>21</sup> The members of this group represented diverse backgrounds in all branches of systematics

18. These bibliographic bulletins classified listed papers into categories that would direct interested researchers to relevant recent publications. The five major categories were: I. Distinctions between Species and Other Categories; II. Causes of Variation; III. Isolation; IV. Natural Selection; V. Artificial Selection. Each of these major categories had subcategories; e.g., Ib under "Distinctions between Species and Other Categories" was based on "Cytological Distinctions," and hence a recent paper, listed as item 409, "Flory, W. S. 1939. Cytological confirmation of taxonomy in *Cooporia*. *Herbertia* 6: 194-196," was classified as Ib. The entries totaled 1249 items.

19. Carl Hubbs complained of this to Mayr: "I thought there was a society along these lines in embryonic stages at least - though Emerson hasn't pushed it. Success will depend on the initial impetus given by a leader who will sacrifice time" (Hubbs to Mayr, January 6, 1946, SSE Papers).

20. See Smocovitis, "Botany and the Evolutionary Synthesis" (above, n. 5), and Hagen, "Experimentalists and Naturalists" (above, n. 5).

21. Reasons for the interdisciplinarity of West Coast botanists are discussed in Smocovitis, "Botany and the Evolutionary Synthesis."

but had an especially strong representation in the botanical sciences; they included E. B. Babcock, G. Ledyard Stebbins, Jr., and Lincoln Constance, along with the Stanford-based Carnegie Institution team of Jens Clausen, David Keck, and William Hiesey.<sup>22</sup> In the 1940s the Biosystematists officially supported Emerson's Society for the Study of Speciation,<sup>23</sup> and as an organized West Coast contingent they kept in close touch with developments on the East Coast. But the organization that fed eventually into the Society for the Study of Evolution centered on an initially informal group of evolutionists who lived in the New York area.

#### WORKING OUT COMMON PROBLEMS: THE COMMITTEE ON COMMON PROBLEMS IN GENETICS, PALEONTOLOGY, AND SYSTEMATICS

In the early 1940s an attempt was made once again to launch a "synthetic attack" on the "common problems of evolution" by forming a cooperative and coordinated organization. The move to form such an organization came from evolutionists living in the New York City area, which by the 1930s and 1940s had become a hub of intellectual activity. While evolutionists worked from numerous institutional bases, the primary centers of research were Columbia University and the American Museum of Natural History.

22. The Biosystematists were a very informal organization and did not keep membership records for the early years. A very brief description of the group is given by Jens Clausen in Emerson's first mimeographed bulletin, in which he states that it was founded in the fall of 1935 and numbered approximately 25 workers. According to oral interviews with Stebbins and Lincoln Constance, the Biosystematists officially began to meet in 1937 with the first lecture on plant biogeography given by David Keck. A photograph of the group taken at Placerville Forest Genetics Station in May 1946 gives the most inclusive list of the members immediately following the war: H. E. McMinn (Mills), G. F. Ferris (Stanford), E. G. Linsley (Berkeley), Herbert Graham (Mills), LeRoy Adams (Stanford), C. Y. Chang, E. B. Babcock (Berkeley), W. E. Castle (ex-Harvard), R. H. Weidman (station), R. Goldschmidt (Berkeley), G. S. Meyers (Stanford), R. C. Miller (California Academy), G. L. Stebbins (Berkeley), C. O. Sauer (Berkeley), H. L. Mason (Berkeley), I. L. Wiggins (Stanford), L. Constance (Berkeley), Nicholas Mirov (station), Palmer Stockwell (station), Bill Cummings (station), and Harold Kirby (Berkeley). The Carnegie group – Jens Clausen, David Keck, and William Hiesey – were not present, or was Alden Miller. The membership in the group fluctuated and friendly visitors to the Bay area were welcome. In 1943 Edgar Anderson, on leave from the Missouri Botanic Garden, took part in many of the activities. Women and graduate students were strictly excluded from the society until the early 1970s, when they were admitted after much discussion. See Smocovitis, "Botany and the Evolutionary Synthesis."

23. G. Ledyard Stebbins to Ernst Mayr, January 2, 1946, SSE Papers.

Various researchers, including Dobzhansky (newly settled at Columbia), L. C. Dunn, Walter Bucher, Mayr, and Simpson, among others, came together in a loose community as they began to plan activities such as symposia and lectures on common evolutionary themes. Among these lectures were the prestigious Jesup Lectures at Columbia University, which Dunn was resuscitating following H. F. Osborn's final lecture.<sup>24</sup> Dunn played an important role in selecting speakers and choosing topics, and also in serving as editor in the Columbia Biological Series of the Columbia University Press, which published the lectures in book form. It was out of Dunn's organizational efforts that what would function as the major synthetic "texts" in evolution would emerge, the earliest and most widely read of which was Dobzhansky's *Genetics and the Origin of Species*.<sup>25</sup>

Unlike the Society for the Study of Speciation and the Biosystematists, this East Coast contingent had a large representation of paleontologists, many of whom shared the feeling that the new evolutionary genetics had to incorporate paleontology, the field that provided *direct* evidence of evolution; at the same time, by cross-linking with genetics, paleontology would be legitimated by this experimentally rigorous science. The presence of the paleontologists along with the geneticists therefore played an important role in expanding what now seemed a narrower interest in speciation that had interested systematists-naturalists in the earlier Society for the Study of Speciation. With paleontology on board, the objectives of this newer group of evolutionists began to widen to include not just speciation, which paleontologists could not address easily, but also more general problems of evolution, especially the tempo and mode of evolution and problems of evolutionary trends, both of which were of especial interest to paleontologists. Simpson, who was working on his own contribution to the Columbia Biological Series, *Tempo and Mode in Evolution*, was especially vocal in demanding that paleontology be included in any efforts to organize evolution. His own book was effectively serving the same process, by cross-linking with Dobzhansky's earlier text.

The other prominent geologist who was active in initiating the

24. See Ronald Rainger, *An Agenda for Antiquity* (Tuscaloosa: University of Alabama Press, 1991), on the history of H. F. Osborn and the American Museum of Natural History.

25. It was L. C. Dunn who invited Dobzhansky to give the Jesup Lectures with the title "Genetics and the Origin of Species." See W. B. Provine's account of the history of *Genetics and the Origin of Species* in "Origins of Dobzhansky's *Genetics and the Origin of Species*," unpublished manuscript.

organization of evolution in the New York area was Walter Bucher at Columbia. Along with Simpson, Bucher made efforts to integrate the paleontologists with other interested individuals, initially in genetics and then in systematics. In 1941 he announced to the annual meeting of the Geological Society of America that persistent problems in evolution might be solved if geneticists and paleontologists cooperated to synthesize their two fields of research. In 1942 Bucher, who was also chair of the Division of Geology and Geography at the National Research Council, used this institutional backing to commission from Simpson and Dobzhansky a proposal to form an NRC-sponsored committee that would study common problems in paleontology and genetics and thus possibly fuse the two disciplines together. Dobzhansky and Simpson took great pains to include the paleontologists in their proposal. With Bucher's initiative, a group of geneticists, paleontologists, and systematists met on October 17, 1942, in the library of the zoology department of Columbia University to form an evolutionary organization entitled "The Committee on Common Problems of Genetics and Paleontology," which would help to solve the common problems between genetics and evolution. These initial plans were put into effect on February 6, 1943, when the committee was established officially under the auspices of the National Research Council.<sup>26</sup> In 1944 the title of the committee was amended to include systematists, who would function as the "obvious links" for informational exchange between the paleontologists and geneticists, and it became the Committee on Common Problems in Genetics, Paleontology, and Systematics.<sup>27</sup>

This committee officially supported two meetings in the summer of 1943 organized by two geographically delineated subcommittees. The first group met on June 14–16 at the American Museum of Natural History in New York, and was heavily represented by animal paleontologists and geneticists; the second meeting, which took place at the University of California at Berkeley on June 24–25, drew on the "Biosystematists," and especially the botanists. The committee heartily supported the extension of these local meetings to the national level, but more ambitious plans were thwarted by wartime conditions.

26. This was a joint or interdivisional committee organized by the Division of Geology and Geography and the Division of Biology and Agriculture.

27. See the historical Foreword to the edited volume of the Committee's final meeting at Princeton written by Glenn L. Jepsen in which he discusses the incorporation of systematics: Glenn L. Jepsen, Ernst Mayr, and George Gaylord Simpson, eds., *Genetics, Paleontology, and Evolution* (1949; repr. New York: Atheneum, 1963), p. ix.

During the war years, communication between interested members of the committee, many of whom had also been part of the now-defunct Society for the Study of Speciation, took place through a series of mimeographed bulletins whose contents were often solicited, collected, and edited by Mayr on a voluntary basis. It was in these bulletins that “common problems” were defined and discussed by the local network of evolutionary practitioners, thus making them accessible to a wider interested audience.<sup>28</sup> The topics discussed in the bulletins indicate that interest had expanded to represent a wider set of issues that were emerging in the evolutionary synthesis. Not only were the initial problems of speciation, divergence, and isolation discussed, but also problems of evolutionary rates and higher-order evolutionary phenomena. This extension of the evolutionary perspective was the outcome of the strong presence on the committee of paleontologists and of the organization of West Coast botanists, which included Ralph Chaney, E. B. Babcock, Herbert Mason, and G. Ledyard Stebbins, all of whom were seeking to understand the complex patterns of variation and evolution in plants – organismic systems that seemed to defy generalizations drawn from animals.

The bulletins also occasionally gave news and commentaries, in addition to ongoing discussion on the common problems of systematics, paleontology, and genetics, and hence served as a conduit for recent developments as well as a consistent dialogue between evolutionists. The importance of these bulletins as a medium for informational exchange and as a site for negotiating common problems should not be underestimated. It was through these communication bulletins – comprising a series of letters or rapid exchanges between members of a local group that thus extended itself to and cross-linked with a wider group of biologists – that a consensus emerged that there was in fact not only a common ground for evolution but also a common *field*, which should be institutionalized. Returning from military service abroad, Simpson identified the emergence of this common field in the final mimeographed bulletin of 1944:

This series of bulletins, compiled and edited by Dr. Mayr who continues this task, has accomplished a great deal more than

28. I am using the set of mimeographed volumes in the holdings of the Provine evolution collection in Marathon, N.Y., as well as an unpublished manuscript “History of the Society for the Study of Evolution” written by Ernst Mayr (dated most likely around 1947) included with the bundle of mimeographed bulletins.

the expression of a few facts and opinions, useful as these have also been. From the whole series of letters in the bulletin there has emerged concrete evidence that a field common to the disciplines of genetics, paleontology, and systematics does really exist and this field is beginning to be clearly defined. Some, at least, of the more helpful approaches to these common problems are indicated and exemplified. The existence of geneticists, paleontologists, and systematists interested in these problems and competent to attack them has been demonstrated. Their interest has been stimulated and made more concrete and their competence in the joint field has been increased by the exchange of views with students of other specialities. Thus great progress toward the goal of the committee has been made.<sup>29</sup>

By this time, too, the Columbia Biological Series was beginning to reach the wider biological audience to garner further support and belief in the emergence of a common field of evolutionary studies. With the publication of the Columbia Series and of such semipopular books as Julian Huxley's *Evolution: The Modern Synthesis* (1942), and with Huxley's numerous other publications and public appearances, an ever-growing audience was coming to acknowledge that a modern synthesis of evolution had taken place and that a common ground existed for researchers from the formerly disparate biological fields. But it was only after the war that major moves could be made to redirect available resources to the planning of major conferences and the formation of new societies. It was the editor of the bulletins, Ernst Mayr, who began to play the most active role in facilitating the communication that would lead to the founding of the central organ of the community, the Society for the Study of Evolution.

#### FOUNDING THE SOCIETY: ERNST MAYR LAYS THE GROUNDWORK

The success of the bulletins made all the more obvious the lack of suitable outlets for publication for those interested in the newer field. The primary journal for the work of systematists-naturalists had long been the *American Naturalist*, but that journal had begun to shift its policy in the 1930s and 1940s in order to increase circulation. The American Society of Naturalists (ASN) did not have

29. Introductory remarks by G. G. Simpson, Bulletin no. 4, November 13, 1944.

official control of the journal, which was privately owned by the firm of Jaques Cattell Press. Taking over the responsibility from J. McKeen Cattell, Jaques Cattell first became coeditor, and then sole editor of the *American Naturalist* in 1944; he thus served not only as publisher, but also as editor. In the late 1930s the journal had begun to encompass an ever-increasing set of issues and to exclude the more traditional evolution-related articles. With the founding of the Genetics Society of America in 1932, and the institutionalization of the work of practical geneticists, genetics began to predominate even in this naturalist-oriented journal. With the accompanying rise of experimental biology, physiology, psychology, and even sociology, the *American Naturalist* began to reflect these newer practices and was catering increasingly to a wider and more popular audience. By the end of the war, the need for a journal that would serve as a “unified outlet” for evolutionists took on top priority, especially with those very systematists-naturalists who had made the initial moves to organize evolution and draw the community of workers together.<sup>30</sup>

Approaching Huxley on his visit to the United States in early December 1945, Mayr made an official inquiry about the “possibility of publishing a journal devoted to the interrelations of systematics, genetics and evolution” through a joint venture with British systematists, who had had a head-start in supporting evolution. Huxley responded to Mayr’s query by stating that he was firmly behind such organizational efforts and that “the Royal Society has a great deal of money from the Government for such purposes,” but that the journal had to be published simultaneously in the United States and Britain: funds would not be granted if the journal were to be published outside Britain. With this in mind, Huxley suggested to Mayr that the journal be published in Britain, with U.S. associate editors who would guarantee to send a certain number of papers per year. Huxley also proposed that the new publication be called the *Journal of Evolutionary Taxonomy* – thus stressing the newer experimental and evolutionary approaches to taxonomic practice that he was endorsing.<sup>31</sup>

Over the holidays, Mayr began to garner support as he wrote

30. The change in journal policy and the concerns it raised were made apparent in a letter that Ernst Mayr wrote to Jaques Cattell on December 28, 1945, SSE Papers.

31. This interaction was recounted by Julian Huxley to Ernst Mayr, December 19, 1945, SSE Papers; Huxley stated that British workers were feeling a comparable need for a journal, since the channels for communication to the *Proceedings of the Linnean Society* were “wholly inadequate.”

to solicit the advice of colleagues on the starting of a journal with an official society for the study of evolution.<sup>32</sup> Informing Emerson, the former secretary of the Society for the Study of Speciation, of Huxley's support in the creation of an international *Journal of Evolutionary Taxonomy*, Mayr raised a concern that Dobzhansky had made apparent regarding the choice of a British base of publication: according to Dobzhansky, who opposed the idea strongly, a British publication base would result in delays in publication, and authors would most likely receive only a small number of reprints.<sup>33</sup> An additional concern was the possibility of political difficulties in England, then undergoing postwar reconstruction and suffering from shortages of vital resources.<sup>34</sup> The choice of a British base for the journal clearly had its share of problems, but these did not appear to trouble Mayr himself too greatly.

Keeping the concerns of key participants in mind, and determined to explore available options, Mayr continued to solicit the advice of interested colleagues on the founding of a society in the United States to ensure a stable base of operations for the much-needed journal. He envisioned a formal journal-issuing society, which would still have an international orientation and would maintain close official ties to the British organization. Moving to call a meeting to discuss the possibility of a full-fledged society, Mayr wrote to Emerson for advice on the formation of an "evolutionary systematics" society at the most "logical" place, the American Association for the Advancement of Science meeting in St. Louis. He asked Emerson, "Would you be willing to support such a society and journal?"<sup>35</sup>

Emerson's reply, dated January 1, 1946, encouraged Mayr to continue with the move to form a society and echoed the need for evolutionists to start their own journal, given the "new policy of

32. Mayr solicited advice from Dobzhansky, Stebbins, Carl Hubbs, and E. R. Dunn. He did not receive replies in time for Huxley's deadline of January 9 because many of his colleagues were on holiday. In a letter to Huxley dated January 19, 1946, he stated that he had letters from G. L. Stebbins, A. E. Emerson, C. Hubbs, E. R. Dunn, R. Griggs, Ross Harrison, F. Verdoorn, and others who pledged their support, in addition to local workers like Simpson and Dobzhansky.

33. Mayr to Emerson, December 27, 1945, SSE Papers.

34. The dearth of resources in England made it difficult to secure enough paper for the publication of British journals. Huxley indicated to Mayr that the Paper Control Office had said that sufficient paper would not be available until the spring of 1947, but he added: "I am certain that if we did get a journal started they would not kill it for lack of paper" (Huxley to Mayr, March 23, 1946, SSE papers).

35. Mayr to Emerson, December 27, 1945, SSE Papers.



the American Naturalist.” Though Emerson supported the formation of a new society wholeheartedly, he strongly suggested that it be built on the foundations of the older society, which, though dormant, had a crude organizational structure already assembled.<sup>36</sup> Through Emerson, the linkage between the older society and the newer was constructed, with the Society for the Study of Speciation offering \$300.00 remaining in its treasury and the list of its members towards the formation of the newer society.<sup>37</sup> Emerson’s suggestions were well taken, with Mayr agreeing that the SSS would form the “nucleus” – with a new set of officers – of the new society.<sup>38</sup>

Informing Huxley of his “informal poll of the prevailing opinions,” Mayr transmitted the enthusiasm that his colleagues displayed but also raised the problems with the British base of publication, which would probably be “very stingy” with reprints, and the concern with the possibility of political instability in Britain. He suggested that these objections could be overcome if provisions were made to shift publication to America or some other country, if the political or economic situation in England worsened.<sup>39</sup> Through the dialogue with his British counterpart Mayr also articulated the possible function, contents, and other such details of the new journal. Arguing that the name *Journal of Evolutionary Taxonomy* was “somewhat awkward,” he suggested that the journal be called simply *Evolution*, with a possible subtitle of *International Journal of Evolutionary Taxonomy, Paleontology, Ecology, and Genetics*. He closed with this optimistic note: “It certainly would be grand if this dream of ours would become reality. The success that you had with your *New Systematics* and *Evolution* as well as I with my *Systematics and the Origin of Species* and Simpson with his new book proves what a tremendous interest there is in evolution (eighty-seven years after 1859!) and what a need for a closer cooperation of the workers in the various fields dealing with evolution.”<sup>40</sup>

36. Emerson to Mayr, January 1, 1946, SSE Papers.

37. According to the minutes of the first official meeting the amount left over from the older society was \$283.78; SSE Papers.

38. Mayr to Huxley, January 3, 1946; Mayr to Emerson, January 8, 1946, SSE papers.

39. Mayr made it clear to Huxley that American members wanted a pledge from British members, perhaps a contract with a clause that stated: “if publication in England should become too difficult for economic, political, or other reasons, the place of publication shall be shifted to America or some other country” (Mayr to Huxley, January 3, 1946, SSE Papers).

40. Mayr to Huxley, January 3, 1946, SSE Papers.

By the end of the first week in January Mayr was taking on an active leadership role in the move to organize evolution, as he made the decision that “we are all agreed that we don’t want back a strictly British journal.”<sup>41</sup> By this time, too, Mayr had begun to widen the scope of the proposed society by stressing the importance of incorporating the paleontologists, an interest that was emerging through his dialogue with Simpson and Bucher. Since the new society would be built around the “nucleus” of the now “dormant” Society for the Study of Speciation, Mayr repeated once more that it must have a wider scope than just speciation since it had to include the paleontologists, whose “sphere of interest” is “above the speciation level.”<sup>42</sup> His endorsement of the broader journal title, *Evolution*, was part of his effort to create enough space for the paleontologists and at the same time to draw on the intellectual and organizational efforts already assembled by systematists-naturalists as well as on the scientific legitimacy and experimental rigor of the geneticists.

While the need to form a society that would bring together interested researchers was one motive for organizing evolution, the more immediate problem with the *American Naturalist* and the need for a common journal of evolution was the constant, dominant motive force in organizing the society and unifying the new field. Mayr repeatedly appealed to the need for the unification of the now-emerging common field through a common journal of evolution: “Taxonomists who are interested in evolution have had no regular outlet up to now and I foresee that such a society and journal will do a great deal to vitalize and unify the field.”<sup>43</sup> Such a journal could also go a long way toward demonstrating the scientific legitimacy of the new systematics to the experimentalists, at the same time that it would facilitate the construction of communication channels between these two previously dissenting groups. Though this appeared less and less to be a clearly recognized motive to launch the journal, occasional reminders of the split between naturalists-systematists and experimental biologists crept in. Writing to E. R. Dunn on January 23, 1946, Mayr indicated: “We naturalists are beginning to be appreciated by the experimental zoologists and we should do everything in our power to assist this rapprochement rather than to impede it by concealing our results.”<sup>44</sup>

41. Mayr to Emerson, January 8, 1946, SSE Papers.

42. Ibid. It was Simpson who had most strongly advocated the emphasis on evolution rather than just speciation.

43. Mayr to Carl Hubbs, January 11, 1946, SSE Papers.

44. Mayr to E. R. Dunn, January 23, 1946, SSE Papers.

In the meantime, Mayr solicited financial support from both Huxley, to whom he wrote about sponsoring a joint society under the auspices of the Royal Society, and the NRC, the supporters of the Committee on Common Problems in Genetics, Paleontology, and Systematics. Huxley at this time was actively negotiating with the publication committee of the Association for Systematics, and exploring publication costs and grant support.<sup>45</sup> Sometime in early January Mayr also approached Cattell about the “possibility of working our plans in with the *American Naturalist*,” but he was not as sanguine about working with him since at that time Cattell’s “ideas for changing the A.N. seem to go in the opposite direction.”<sup>46</sup>

Increasingly, as Mayr gathered support and favorable replies in response to his constant queries, the tendency was to favor bringing out a new journal in the United States rather than sharing publication with Britain. Warning Huxley on January 19, Mayr relayed to him the growing American sentiment to establish an American journal, especially given the change in editorial policy of the *American Naturalist*.<sup>47</sup> Despite Mayr’s initial warning, Huxley responded that “we will definitely go ahead with a journal here – *unless* American biologists withdraw their cooperation.”<sup>48</sup> One month later Huxley suggested to Mayr a compromise solution: the formation of a joint international society with a common title. With the support of J. S. L. Gilmour, Huxley suggested forming an American branch and a British branch of the society, independent of each other but still collaborating and forming part of a single organization with a title like “Society for the Study of Evolution,” or “Association for Evolutionary Biology.”<sup>49</sup> Mayr’s reaction to Huxley’s suggestion was lukewarm, if not discouraging. Rather than supporting the new suggestion, he urged Huxley to continue to research and obtain more definite information on the probable subscription price and technical details of production in Britain; with the increasing desire to publish the journal in the United States, such information would be needed to convince Americans of the advantages of publishing an international journal.<sup>50</sup>

By this time, too, it was decided that the new society would not only be built on the nucleus of the SSS, but would also “join forces” officially with the NRC committee.<sup>51</sup> Announcing the call

45. Huxley to Mayr, January 10, 1946, SSE Papers.

46. Mayr to Hubbs, January 11, 1946, SSE Papers.

47. Mayr to Huxley, January 19, 1946, SSE Papers.

48. Huxley to Mayr, January 23, 1946, SSE Papers.

49. Huxley to Mayr, February 19, 1946, SSE Papers.

50. Mayr to Huxley, February 26, 1946, SSE Papers.

for the new meeting in *Science*, the journal that reached the widest possible audience of scientists, Emerson alerted former and potentially new members about the upcoming meeting:

*The Society for the Study of Speciation* will hold a Saturday morning session, 30 March, 8:30 A.M. in private dining room 2, Jefferson Hotel under the chairmanship of Alfred E. Emerson. The Society for the Study of Speciation is a new group first organized in 1941 for those interested in the dynamics of evolution. In preparation for a fresh start, following an inactive period during the war, this meeting is called to discuss organization and objectives and to elect officers.<sup>52</sup>

While many viewed the announcement as a call to regroup and reconstruct the formerly dormant society, one key participant pointed out that if the new society were to function properly, it should not be “tied down to the single aspect implied by the word speciation” and should be “devoted to the problem of evolution as a whole”: acting on behalf of the paleontologists, Simpson reminded Emerson, in an emphatic tone, that the new society’s name should be changed to include “the problem of biological evolution.”<sup>53</sup>

Negotiations for securing the journal accelerated just before the St. Louis meetings. On March 20 Mayr informed Emerson in “strictly confidential” terms that he, Dobzhansky, and Simpson had had a “very profitable conference” with Jaques Cattell at which they had suggested that the new society adopt the *American Naturalist* as its “official organ and elect an editor to be responsible for the scientific contents.” Cattell seemed favorable to the idea: he appeared to be convinced that his venture of changing the policy of the *American Naturalist* had not been successful, and he had made a written proposal to be considered by the members of the new society. Mayr himself was inclined to favor Cattell’s proposal, since it seemed to offer “a most opportune solution of a quandary.”<sup>54</sup>

These new arrangements created at least two problems, however, the first of which was the response of the American Society of Naturalists. According to Mayr, Cattell himself took on the task of contacting the ASN to “straighten out any possible difficulties

51. Mayr to Stebbins, January 11, 1946, SSE Papers.

52. “News and Notes,” *Science*, 103 (1946), 308.

53. Simpson to Emerson, March 11, 1946, SSE Papers.

54. Mayr to Emerson, March 20, 1946, SSE Papers.

that might arise." Mayr indicated that the ASN was actually "planning to discontinue any connection with the *American Naturalist* after it had changed its style." The second problem was that the new arrangements would leave vacant the office of secretary, because both Dobzhansky and Simpson were "anxious" to have Mayr nominated for editor, with Cattell agreeing to this choice. Finding a suitable replacement for Mayr would be difficult.<sup>55</sup>

Just one week before the scheduled meetings, the negotiations between the British and American organizers were to become even more complicated. Following Mayr's instructions, Huxley reported on the publication costs and technical details of the journal should it be published in England. He once again raised the question of a joint society with two branches, an American and a British branch, sharing a common title and a common constitution. The great advantage of this proposition would be, in Huxley's words, "that the evolutionary biologists of this country and the U.S.A. would be speaking with one voice and that the journal could then be properly regarded, since there would be equal American and British representation on the board, as a common organ of what would amount to two branches of one Society." He repeated most strongly that he did not wish to diminish the possibility of either journal's success through competition. He then added rather harshly to Mayr:

I would remind you that the whole matter, as far as we are concerned, started from your telling me of the idea of a journal, but stating that it appeared impossible to secure publication in the U.S.A. If you at that time had said that you definitely [*sic*] had the intention [*sic*] of publishing a journal in America and had adequate prospects of financial and contribution support, I am sure we would have come in on that proposals [*sic*] and not attempted to start a separate one here.<sup>56</sup>

Huxley's response indicates that he was not aware that the American branch had begun its complex negotiations with the Cattells early on in the planning for the new society. His strong response and his urging of the organization of a joint international society had little effect, however, for his letter reached Mayr just one hour

55. Mayr told Emerson that Edgar Anderson "would be quite impossible in an office that would involve the keeping of files and the regular answering of correspondence" (Mayr to Emerson, March 20, 1946, SSE Papers.)

56. Huxley to Mayr, March 23, 1946, SSE Papers.

before the founding of the society was to take place.<sup>57</sup> By then, Mayr's groundwork had been successful in launching an American society.

#### THE "FATHERS" OF CONFEDERATION: THE 1946 ST. LOUIS ORGANIZATIONAL MEETING

On Saturday, March 30, 1946, at the AAAS meetings held in St. Louis, Missouri, fifty-eight attendees – the "founding fathers" – signed a document, entering confederacy under the title of the Society for the Study of Evolution (see Fig. 1).<sup>58</sup> The minutes of the first organizational meeting and subsequent accounts indicate surprisingly little disagreement, given the diverse set of evolutionary perspectives represented by the members present; if anything, there appeared to be a significant consensus already in the new society. Mayr, though not the official chair of the meeting, appears to have played a key role in bringing the group to consensus.<sup>59</sup>

Discussing the first order of business, Emerson, the official chair of the meeting, suggested that the organization's statement of purpose should be "the promotion of the study of organic evolution and the integration of the various fields of biology." Interestingly, the sole objection raised in response to this statement came from Edgar Anderson, who objected to the definition as being too "vague." In his view, the members of the society were interested "not only in processes but results"; as presented, the statement of purpose favored a static instead of a dynamic view of evolution. Moving back to the older aims of the Society for the Study of Speciation, Anderson proposed that the statement be changed to read that the goal of the new society was the study of speciation. Anderson's proposal was countered by Mayr, who pointed out that this suggestion effectively eliminated the paleontologists, and by Simpson, who added that "the paleontologists were probably more interested in evolution than any other group repre-

57. Mayr to Huxley, April 1, 1946, SSE Papers.

58. A. E. Emerson, who presided at the meeting, used the phrase "founding fathers" in a letter to Herbert Baker dated December 29, 1965; actually, one of the signatories was a woman, Ruth Patrick. These individuals represented diverse disciplines, methods, and organismic systems. See Appendix 1 for the list of attendees. An original document with the signatures is in the possession of Warren Wagner in the Department of Botany, University of Michigan, Ann Arbor, Mich.

59. The minutes were recorded by R. P. Wagner: "The Society for the Study of Evolution, Organization Meeting, March 30, 1946, St. Louis, Missouri," SSE Papers.

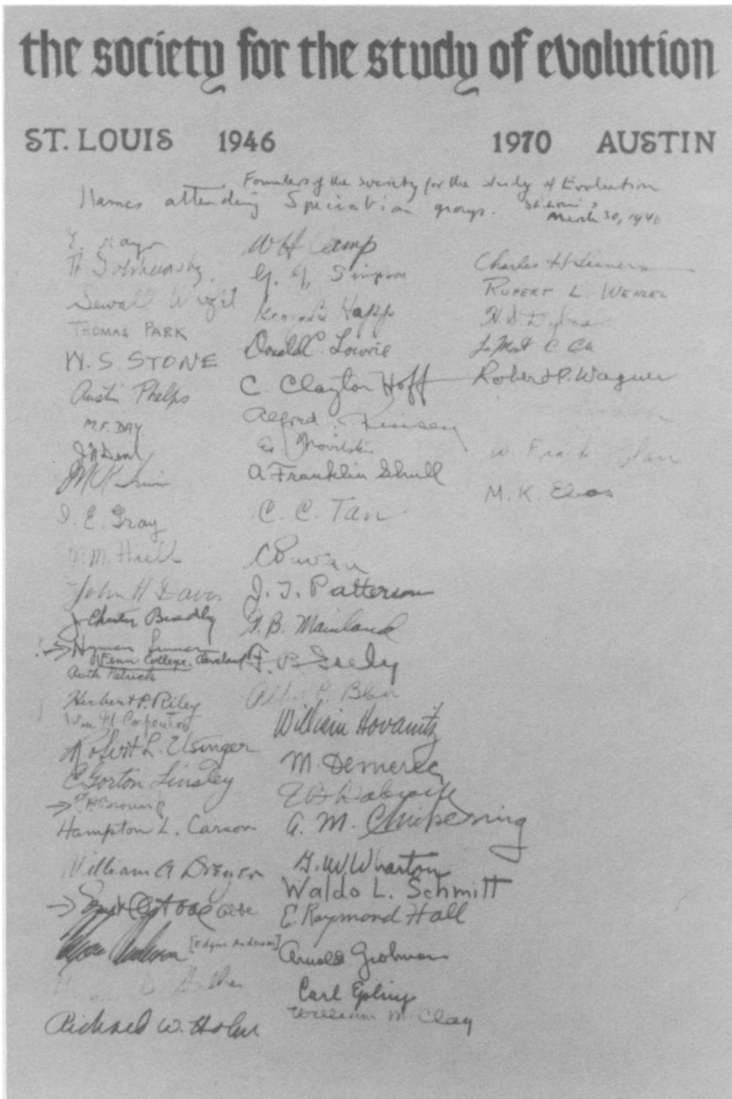


Fig. 1. Foundation document of the founders of the Society for the Study of Evolution, St. Louis, March 30, 1946. The small handwriting above the signatures is by Ernst Mayr. For a list of the signatories, see Appendix 1. The document was reproduced and distributed to members at the 1970 SSE meetings in Austin, Texas. An additional document listing founders also includes C. W. Metz. (Photograph courtesy James Crow and Donald Waller.)

sented at the meeting.” These arguments seemed to sway the members, and Mayr’s motion to approve Emerson’s statement of purpose was carried unanimously.

The election of officers for the new society for the years 1946–1947 took place without much discussion, given that the candidates had already distinguished themselves as active members of the evolutionary community. All the officers were elected unanimously: Simpson became the first president of the society;<sup>60</sup> E. B. Babcock, A. E. Emerson, and J. T. Patterson were the vice-presidents; Mayr was voted secretary; K. P. Schmidt became treasurer; and E. R. Dunn, H. J. Muller, Sewall Wright, G. L. Jepsen, Th. Dobzhansky, and R. Chaney served as council members (see Appendix 2 for a list of officers for the years 1946–1952).

While the first two orders of business did not engender much controversy or pose any great difficulty, the third brought up what was to become an increasingly difficult problem: the relationship of the new society to the older American Society of Naturalists. Many of the members of the new society had been members of or had supported the ASN and its journal, which, they felt, were now endangered. According to the minutes, Edmund Ware Sinnott from the American Society of Naturalists proposed that the new society amalgamate with the older naturalists, take over the *American Naturalist*, and appoint an editor-in-chief for the journal; this would be done by joint agreement between the societies. The minutes of the meeting indicate that many of the members, including Dobzhansky, echoed some of Sinnott’s concerns, and made a point of urging the new society to be at least sympathetic to the older society.<sup>61</sup> The motion was made by E. R. Hall that a committee be formed to consider the possibility of amalgamation. Most members appear to have approached this difficult subject with sensitivity, but at least one member, Alfred Kinsey, strongly urged that the two societies be kept apart. According to Kinsey, the ASN had only a small percentage of members actively interested in evolution, the chief interest in evolution having come from the journal, which was not even owned by the society and existed “primarily to make money for the owners.” The new group would

60. According to Emerson’s recollections of the founding of the society, Simpson was elected “somewhat to his surprise” (Emerson to Theodor Just, March 28, 1952, SSE Papers).

61. A subsequent letter from Sinnott to Dobzhansky dated April 1, 1946 (Ernst Mayr Papers, SSE Correspondence, 1946 File.) states that Sinnott himself was tied up with other business during the business meeting. It is not clear why the transcripts of the minutes of the SSE indicate that he was present. He was not one of the original signatories.



be in a stronger position if it started its own journal. Despite a lengthy discussion, little consensus appears to have been reached with respect to the American Society of Naturalists at this meeting.<sup>62</sup>

As with any democratic society, a set of rules had to be invented to ensure proper representative government. Thus, the next order of business was the creation of a constitution with the by-laws of the society. Emerson suggested that a committee consisting of the president, the secretary, and the treasurer, with help from other officers and the council, be responsible for this task. Edgar Anderson moved that W. H. Camp be added to the group, and the motion was carried. This group was thus assigned the responsibility for drafting the constitution.

Members of the new society also voted on the choice of a name that would represent most accurately the interest of all the members. After considering six names – The Society for the Study of Speciation, The Society for the Study of Evolution, The Society for the Study of Organic Evolution, The Evolution Society, The Darwinian Society, The Society for the Study of Evolution and Speciation, and The Society for the Study of Evolutionary Processes – W. L. Schmitt moved that The Society for the Study of Evolution be the chosen name, and the motion was passed by a majority vote.

Once the members of the society had decided on their name and introduced discussion of their allegiances, Emerson raised the issue of the journal of the society. Ernst Mayr, who had explored the various options, placed before the members the “concrete” possibility of taking over the *American Naturalist* and then appointing an editor, the “attractive” possibility of taking advantage of Julian Huxley’s suggestion to raise funds in England for an international journal, and finally the more “expensive” possibility of raising funds for the society to have its own journal. In his opinion, at least four to five thousand dollars would be needed to start a journal, which would most likely leave the society in debt for several years. The discussion that followed indicated some differences of opinion among the new members. Mayr himself appeared concerned with the expenses that a suitable journal relying only on members of the new society would incur; he thus favored some sort of collaboration with Huxley. Dobzhansky seemed to oppose Mayr’s wish for collaboration with Huxley, for he stated

62. This proved to be the one of the most difficult problems that the new society faced. See the discussion in the next section.

that there were more than enough workers in the United States who could contribute substantively and he added that “we would be merely sending a few papers to another journal.” The minutes of the meeting here indicate some confusion, but Dobzhansky made the motion to appoint an individual “to represent the Society for the Study of Evolution with respect to the feasibility of taking over the *American Naturalist*, and that this individual was to serve without prejudice”; the motion failed to pass, however. The policy toward the American Society of Naturalists thus appears to have been left up in the air.

The sixth order of business raised the question of available finances. The total left over from the older Society for Study of Speciation was officially listed as \$283.78. Memberships dues were set at the very affordable price of \$1.00 for the year 1946. Charter members were asked for an additional \$3.00.<sup>63</sup>

Finally, the difficult matter of the cooperation of the new society with other societies was raised. Emerson suggested that the SSE join with the AAAS and that future meetings be held in conjunction with this society. Mayr suggested that the SSE should arrange to meet in conjunction with the AAAS, but that they should postpone discussion about affiliation until a later meeting. The members voted on Emerson’s suggestion favorably, and it was decided that the society would affiliate with the AAAS. The next meeting was scheduled to take place at the next AAAS meetings in Boston. With the founding of the society completed, the meeting adjourned. Shortly thereafter, in the issue of *Nature* for May 11, 1946, there appeared the following announcement of the new Society for the Study of Evolution:

A Society for the Study of Evolution was formed on March 30, 1946, on the occasion of the meeting of the American Association for the Advancement of Science at St. Louis. The object of the Society is the promotion of the study of organic evolution and the integration of the various fields of biology, such as taxonomy, paleontology, genetics, that are interested in evolution.

Thus, with the end of the war, what had been the defunct Society for the Study of Speciation successfully joined forces with the

63. During the first year of operation just over 500 members were to join the society. Once the journal was determined, the membership dues with subscription to the journal were listed as \$5.00.

Committee on Common Problems and, under the auspices of the National Research Council, was reorganized into an official, formal organization, the Society for the Study of Evolution.

#### A JOURNAL FOR THE NEW SOCIETY

Without question, enthusiasm over the new society and the new mergers ran very high. But the problem of funding for the journal and of start-up funds for the society was to drag on for nearly eight months after the St. Louis meeting. Mayr continued his relentless search for a suitable press and for funding for the society and journal.

The day after the St. Louis meeting Mayr responded immediately to Huxley's last letter, which had urged him to secure publication for the journal. He informed Huxley of the meeting and relayed to him that the society showed very little favorable response to a joint international journal. He indicated that he was surprised at the strength of the support shown for a society-owned journal. Revealing what had been "strictly confidential" information, he softened Huxley's rejection by telling him of the arrangement with Cattell and the society's disinclination to take over the *American Naturalist*. He wrote: "Speaker after speaker stood up and emphasized his desire that science could best be served by having every cent that was earned by the journal turned right back into the enlargement of the journal." Mayr further indicated his sympathy with Huxley's suggestion but indicated that "there was a great spirit of independence about it, and I doubt whether anything could be gained from changing the title of your society in such a way as to facilitate the closer cooperation of the two societies."<sup>64</sup>

News of the possible take-over of the journal also reached Sinnott of the American Society of Naturalists, who on April 1 informed Dobzhansky that he had been authorized to name Mayr editor-in-chief of the new *American Naturalist*.<sup>65</sup> This would aid in linking the two societies officially and would prevent rivalry between the groups. Mayr responded to Sinnott's letter shortly after Dobzhansky had passed it on: while he was sympathetic to Sinnott's suggestion that the new society take over the *American Naturalist*, he indicated that the SSE did not favor this move and had voted

64. Mayr to Huxley, April 1, 1946, SSE Papers.

65. Sinnott to Dobzhansky, April 1, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

down the proposal owing to a distrust of the Cattells.<sup>66</sup> Sinnott's response a few days later showed some measure of relief at Mayr's decision, since the move to fuse the societies and take over the journal would undoubtedly have raised objections from some members of the American Society of Naturalists.<sup>67</sup>

Huxley's disappointment was evident, however, as he wrote to Mayr on April 9th:

I am glad your new Society is duly launched. On the other hand, we are all frankly disappointed that, after yourself suggesting that we should try to get the journal launched, and writing to insist that it should be international, you are now turning that idea down. I am quite sure it would be stupid to try to launch two journals, and would like your Society to consider the following suggestion, viz. that your journal should be international, in the sense that it would have on its editorial board editors from other countries than America.

Huxley made the additional suggestion that the new journal should confine itself to original articles, leaving the longer reviews to a British project in the form of an annual volume.<sup>68</sup>

Huxley's suggestion to make the journal international was well received by Mayr, who took the idea to Simpson. He indicated to Simpson that he was in favor of the plan, "provided that the basic control of the journal remains in the hands of our Society." He also added: "To have a few international editors added would no doubt help the circulation."<sup>69</sup> But Huxley's statement that it was Mayr who had first suggested the journal met a with a firm reminder from Mayr, who wrote:

Incidentally, I went over our correspondence again and found that it was you who had suggested that the journal be published in England. I had [*sic*] also written you repeatedly that there was a strong current to publish the journal in this country. I made quite an eloquent plea to endorse your proposition, but it was voted down at the foundation meeting of our Society. This is a

66. Mayr to Edmund Sinnott, April 4, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File. Mayr had written a rejection letter to Jaques Cattell promptly on April 3, 1946; SSE Papers.

67. Sinnott to Mayr, April 8, 1946, SSE Papers.

68. Huxley to Mayr, April 9, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

69. Mayr to Simpson, April 15, 1946, SSE Papers.

democracy and there isn't anything that I can do about it. It means that I now have to try to raise the money to get the journal on its feet and I am fully aware how much of a job this is. I am merely mentioning this to prevent any possible misunderstanding between us.<sup>70</sup>

While the discussion on the nature of the journal and the selection of its editorial board continued, Mayr began the laborious procedure of securing funds and promoting the new society.<sup>71</sup> Indicators were pointing to an increase in labor and printing costs, a factor that Mayr kept in mind as he began to solicit support and advice for the urgently needed journal.<sup>72</sup> On April 15 he wrote to Simpson, asking him to prepare a tentative draft of the application to be made to the Carnegie Corporation for a single donation of five thousand dollars to cover the initial expenses. Drawing on an offer made by Ross Harrison, Mayr suggested to Simpson that they enclose copies of their correspondence with the National Research Council to strengthen their case with the Carnegie Corporation. He sought further help by writing to E. B. Babcock, the representative for the Biosystematists, asking for letters of

70. Mayr to Huxley, April 30, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

71. In a letter to K. P. Schmidt dated April 26, 1946, Mayr indicated that an announcement of the formation of the society should be mimeographed and mailed to members of the former Society for the Study of Speciation, members of the American Genetics Society, and the scientific staffs of the American museums. While Mayr wished to inform these workers of the new society, he also indicated that there was "no need for any strong promotion campaign at the present time. What is more important is to get our journal going and then we will have no difficulty in getting members" (Mayr to Schmidt, April 26, 1946, SSE Papers).

72. A. G. Rehn (curator of insects at the Academy of Natural Sciences) wrote to Emerson to alert him of the rise in costs: "In the last two months all of the publications with which I am connected have been compelled to meet printers' increases of from 10% to 20%, these on top of increases of virtually the same character of not more than two years in the past. Engraving costs rose the first of March 5% on top of a 20% rise as of June 1, 1944. You will see from this that the problem of planning ahead for a journal is an exceedingly difficult one where subscriptions alone are concerned. The financing of journals today is one of the most serious angles of biological science to my mind, and is entirely due to factors beyond our control, i.e. basically steadily climbing labor costs. It is impossible for scientists to increase their subscription prices every year, or every two years, to meet these production costs without incurring a storm of protests from our colleagues who have never had any practical experiences in publication matters, except to read proof and know nothing of the dollars and cents angle of the problem" (Rehn to Emerson, March 18, 1946, SSE Papers).

support from the workers on the West Coast.<sup>73</sup> Mayr's hope was to submit the application to the Carnegie Corporation at the earliest possible opportunity so that the journal could start in January 1947. But on July 2, 1946, Mayr and Simpson received the bad news that the Carnegie Corporation application for a grant had been rejected.

This rejection, the reasons for which were not clear, only increased the urgency of finding other sources of funding for the journal. The pressure began to mount as publication charges escalated in the postwar economic destabilization. Mayr attempted to solicit support from commercial publishers like Academic Press, but they did not express much enthusiasm for publishing the journal of such a fledgling society. He also tried to at least determine the final cost of subscription for the members. Keeping in mind the fact that new members, especially the younger and more active researchers who would do the most to contribute to the future of the society, would only be enlisted by the combination of an attractive journal with a reasonable subscription price, Mayr actively sought to keep prices down and at the same time to produce a high-quality journal. At the same time, the interest of those members who had already paid their dues and declared their allegiance had to be preserved if the new society was to keep its momentum going. Writing to Emerson in early July, Mayr urged that something be supplied for the paid-up members, since the journal itself seemed a long way off. He suggested the publication of informal bulletins, and also recommended that future symposia be planned for the members; this would be one way of keeping up interest in the society.<sup>74</sup> Mayr continued to seek support for the journal, but by midsummer the more pressing problem had become the complex negotiations between the new society and the older *American Naturalist*.

#### THE AMERICAN SOCIETY OF NATURALISTS

The problem with the American Society of Naturalists was that it was *too close* to the new society. Because there were numerous

73. Mayr to Simpson, April 15, 1946, SSE Papers. Mayr provided a detailed account of what to include in the letters of support for the journal. He also added in his letter to Babcock that charter members should be defined as those who applied for membership before the convening of the first annual meeting, so that those who were unable to attend the St. Louis meetings because of the long distance would not be excluded from being charter members; Mayr to Babcock, May 7, 1946, SSE Papers.

74. Mayr to Emerson, July 3, 1946, SSE Papers.

overlapping members who had previously used the *American Naturalist* as their premier journal of publication, and because the aims of the older ASN were somewhat similar to those of the new society, a new journal-issuing society that had so much in common ran the risk of competing with the older society. As the momentum and interest in the new society grew, so too did the danger that the older *American Naturalist* and its society would be run out of business. Thus by midsummer the members of both societies raised a call of alarm.

Jaques Cattell himself had responded to the rejection of his earlier proposal to take over the journal with regret, pointing out that the seventy-year-old *American Naturalist* was "an institution in American science" and "should be perpetuated in the best way possible."<sup>75</sup> The concern with preserving the older society was mirrored by H. J. Muller, a member of both societies, who highlighted the problem of the relationship between the two societies as he accepted his election to the council of the SSE:

One critical point at present seems to be the relation of the Society, if any, to the Naturalists, and to the journal *The American Naturalist*. A considerable number among the Naturalists including perhaps the majority of the Executive Committee see little point in having it continue as a rival society and would, if there were a dignified way out, favor giving over the advantages of their old standing and prestige to the new society. As you know, they have been in the past, whether or not officially, the only society whose main interest was in evolution, and I do not think they can be changed in that respect. The membership would no doubt not be willing to abolish the society, and most of their membership is, I believe, a type which would be appropriate in the new Society. It is much better to have a society overtly dedicated to the study of evolution than one unofficially or semi-officially, and rivalry of this sort would be detrimental all around.

Should the attitude of the new Society be one of artificial aloofness as though the Naturalists did not exist, despite the greatly overlapping membership etc., allowing the Naturalists gradually to wither away, or should they, if they are willing, be accepted into some kind of association or merger?<sup>76</sup>

75. Cattell to Mayr, April 16, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

76. H. J. Muller to Mayr, June 7, 1948, Ernst Mayr Papers, SSE Correspondence, 1946 File.

Mayr's response to Muller informed him that members of the SSE had in fact expressed sensitivity to the relations between the two societies well before the new society was formed, and they still continued to negotiate with the naturalists about the take-over of the journal. Cattell, in the meantime, continued his efforts to collaborate in some manner with the new society. By June 12 he offered to sell outright the *American Naturalist* to the SSE for \$4,000.<sup>77</sup> This offer did not entice Mayr, who indicated that the purchase of the journal would not be a wise move since the society wanted an *international* journal devoted exclusively to evolution. The older journal would have to be changed so much that it would amount to the society paying \$4000 merely to keep the "undesirable name" of the *American Naturalist*.<sup>78</sup> The purchase of the *American Naturalist* and its conversion to an evolution journal would also most likely lead to a loss in membership.

Given the constitution of the American Society of Naturalists, which promoted a broader mission than the evolution society, the idea of a merger between the two societies just did not seem to Mayr to be feasible. So broad were the ASN's goals, in fact, that it served to support too heavily sciences like physiology and genetics – the very fields that Mayr felt had been undermining evolutionary studies. Because of these differences in goals, he did not think that amalgamation would be favored by members of the American Society of Naturalists, but he stressed that all efforts should be taken to prevent competition and the scattering of efforts.<sup>79</sup>

In August 1946 the tension between the two societies came to a head when Ware Cattell officially took over the journal from his brother, Jaques, and became its owner and acting editor. According to C. W. Metz, who was spending the summer at Woods Hole with Ware Cattell, Cattell was busy reformulating plans for the *American Naturalist* shortly after the take-over. Metz indicated that Cattell had said that the journal had been losing money. With the new evolution society and the prospect of a new journal that would compete with the *American Naturalist*, Cattell felt that his journal had to change its policy in order to appeal to a wider audience. Metz said that he was impressed with Cattell's plan from a practical standpoint, since "we cannot object to his trying to

77. This was the same amount the Cattells had paid to the original owners; Mayr to Muller, June 12, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

78. Ibid.

79. Mayr to C. A. Metz, June 25, 1946, SSE Papers.



making a go of it by altering the journal.” Metz indicated that the plan was to make the journal less technical and thus more popular.<sup>80</sup> On August 12, Metz circulated a memo to the members of the Executive Committee of the American Society of Naturalists informing them of the new editor and owner. He did not appear to be greatly troubled by the new developments. He stated that he was “entirely noncommittal on the matter of possible affiliation between our Society and the journal,” and added that “the change in ownership obviously relieves us of any present need of trying to select an editor for the Naturalist.”<sup>81</sup>

Mayr viewed the change in ownership as actually clarifying the situation, since the informal verbal contract between the American Society of Naturalists and Jaques Cattell was thus terminated. This would be an “opportunity to start from scratch.” Given that the members of the new society wanted an international journal expressly for evolution, Mayr suggested to Metz that the *American Naturalist* should have returned to being just a genetics journal – which would be in keeping with the fact that it had included more articles on genetics than on evolution. It would also have been one way of preventing the two journals from competing with each other. He emphasized that the two societies should continue to communicate their needs to each other to ensure cooperation rather than competition.<sup>82</sup>

Not all of the naturalists supported this position. Adopting an interventionist attitude to rescue the “oldest American biological journal” and prevent it from degenerating into “the sort of thing that Ware Cattell would put out” or disappearing altogether, Ralph Cleland made an impassioned plea to Ernst Mayr to purchase the journal. He stressed that it had had a “distinguished career” and occupied “a position second to none in the fields of genetics and evolution,” but his ultimate appeal to purchase the journal was based primarily on financial grounds: taking over the *American Naturalist* would mean that the evolution journal would not go through the difficult start-up period; and with the membership list intact, a modified version of the *American Naturalist* would find a ready-made audience. This proposal would make the journal a

80. Metz to Mayr, August 8, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

81. C. A. Metz, Memo to the Members of the Executive Committee, American Society of Naturalists, August 12, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

82. Mayr to Metz, August 14, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

self-supporting institution. Cleland urged Mayr to reconsider taking over the *American Naturalist* on yet another ground: namely, that the emotional reaction to Ware Cattell as new editor and owner should not be the basis for any decision concerning the fate of either the *American Naturalist* or the new journal.<sup>83</sup>

Mayr's reaction to Cleland was sympathetic as he indicated that there was a shared "emotional desire to preserve the venerable *Naturalist* at all costs." But he also pointed out that members of the SSE had definitely wanted the new journal to be international, and this would be hard given the *American Naturalist's* history. He also once again raised the problem of purchasing a journal that even its owners admitted had been losing money. Finally, he said that "whichever decision is reached, so much is clear, the Society will have to raise the sum of \$5,000 at the start. Under these circumstances it is quite possible that the donor of this sum will have an important voice in the final decision." He added: "Frankly, I cannot venture to guess whether a foundation would be willing to pay the money to buy a journal from a private publisher. On the other hand, there are quite a number of precedents where a foundation has financed the starting of a new journal."<sup>84</sup>

By October 7, Metz and the American Society of Naturalists had expressed to Mayr a wish to form some sort of official committee that would look after the common interests of both societies. Metz asked Mayr directly for the official decision that the new society's members had made with respect to forming a committee. Mayr repeated to Metz that the only motion that had failed to pass at the first meeting was Dobzhansky's motion that "an individual be appointed to represent the Society for the Study of Evolution with respect to the feasibility of taking over the *American Naturalist*, and that this individual was to serve without prejudice." According to Mayr's recollection of the meeting, Dobzhansky had suggested that the new society appoint an individual who would serve as editor of the *American Naturalist*. Mayr recalled that this had been opposed because it would serve to give prestige to the *American Naturalist* without a reciprocal return, and because it might actually "prejudice" members of the society against forming their own journal. While keeping the members free of such prejudice was

83. Ralph Cleland to Mayr, August 24, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

84. Mayr to Cleland, August 28, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

foremost in Mayr's mind, he did support Metz's suggestion that a joint committee be formed and he recommended suitable candidates. Mayr's own sense of the relations between the two societies continued to be that they were very different from one another.<sup>85</sup> By October 29, Emerson, A. Romer, and H. D. Stalker had been appointed members of a joint committee, the Committee to Cooperate with the American Society of Naturalists, which had been organized to look after common interests.<sup>86</sup>

Within a month the discussion over the journal and the relations between the two societies tapered off dramatically as funds to publish a new journal became available. On the advice of his American Museum colleague Robert Cushman Murphy, Mayr had applied for a grant from the American Philosophical Society. According to information supplied by Murphy, APS had been accumulating research funds over the war years, when it was unable to grant support. Murphy, who had been a fellow at APS, suggested to Mayr that he enlist the aid of astronomer Harlow Shapley at APS, who was also sympathetic to the study of evolution and would be inclined to back a proposal to sponsor the SSE. Simpson helped Mayr to carefully word the final grant application. The support of Shapley, who sat on the APS committee, was sufficient to overturn the negative vote given the proposal by the sole biologist on the committee, E. G. Conklin, who felt that evolution was a fruitless area of biological research.<sup>87</sup> On November 1, 1946, the American Philosophical Society Committee on Research voted to underwrite the Society for the Study of Evolution to the extent of \$5,000, which was to be used for the establishment of an "international quarterly journal of evolution."<sup>88</sup>

85. Mayr to Metz, October 7, 1946, SSE Papers.

86. Mayr to Metz, October 29, 1946, SSE Papers.

87. In addition to Murphy, Simpson and other members of the SSE had been fellows at APS. Mayr gave a great deal of credit to Simpson for writing the grant proposal; Mayr to E. B. Babcock, May 7, 1946, SSE Papers. In a letter to Harlow Shapley dated October 7, 1946, Mayr acknowledged Shapley's strong support in securing the grant. A letter from G. G. Simpson to Harlow Shapley dated November 6, 1946, also acknowledged Shapley's strong backing of the grant. See also the historical reflections on the founding of the society in Mayr to Emerson, October 28, 1965, SSE Papers.

88. Document dated October 20, 1947: Granted to J. J. Patterson, grant no. 2. Reserve fund for post-war expenditures. Signed L. P. Eisenhart, Executive Officer, American Philosophical Society, SSE Papers.

*“EVOLUTION: AN INTERNATIONAL JOURNAL OF ORGANIC EVOLUTION”*

Once the funds had been secured, discussion turned to the details of producing a journal for the society that would serve the needs of its members. The dialogue with Huxley had led to a consensus that the journal should be international, and that this should be reflected in the choice of the editorial board. This would not only increase membership and the quality and quantity of submissions, but would also make it apparent that the effort of U.S. scientists to understand evolution would now be equal to, if not greater than, the efforts of scientists in the nations that had traditionally led the study of evolution, especially Britain. A truly international journal of evolution that was based in the United States would indicate that the study of evolution had been transported to this country. Mayr made this point clear when he wrote: “America has lagged behind in the past in its contributions to the field of evolution, a field which was developed under the leadership of Great Britain, Germany and Russia. This has changed ruting [*sic*] the past fifteen or twenty years and I believe it can be said without exaggeration that American [*sic*] is now the unquestioned leader.”<sup>89</sup>

The international features of the journal would also help to soothe the frayed nerves of scientists whose research had been impeded under war conditions, and it would facilitate the establishment of international cooperation at a time when the drive for global unity was most intense.<sup>90</sup> Mayr had noted that the international support for the society and journal had been high, especially from workers in Brazil, Australia, China, and Europe.<sup>91</sup> an international collaboration, with the assistance of Huxley and the Systematics Association in Britain, was firmly in Mayr’s mind as he moved to start an international journal. Writing to a colleague

89. Mayr to S. Dillon Ripley, July 3, 1946, SSE Papers.

90. Huxley himself embodied the international spirit and the drive for global unity. His efforts to organize the study of evolution and to unify the biological sciences were simultaneous with his work with UNESCO. His new role in UNESCO appears in his early correspondence with Mayr on the founding of the society: his letter of February 19, 1946 (SSE Papers) mentions to Mayr that he had taken on the “arduous job” of Executive Secretary of the Preparatory Commission of UNESCO.

91. Mayr to Carl B. Hubbs, October 21, 1946, SSE Papers. Membership applications had come from Great Britain, France, Holland, Switzerland, China, Brazil, and Australia; Mayr to Metz, August 14, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File.

at the British Museum of Natural History, he reinforced the ties with his British colleagues:

I have written to Dr. Huxley that we are most anxious to collaborate with the Systematics Association, and we hope that an arrangement can be found that will be mutually satisfactory. We will welcome such an arrangement not only because it would help to bring together the many groups working on evolution that exist in various centers of learning, but also as an effective tool of international collaboration. We all realize how vitally important this is for science and, in particular, under the present world conditions.<sup>92</sup>

In addition to settling on the international character of the journal, other more detailed decisions had to be made before it could actually be published. In a mimeographed notice of November 7, 1946, Mayr asked for the opinion of the now 445-odd members on a set of issues ranging from the name of the journal to the form, the editorial policy, the constitution of the editorial board, and the publication schedule. As the numerous replies poured in, Mayr and the council members began to make the final decisions to be voted on at the first annual meetings, scheduled to take place at the end of December.

The seemingly trivial choice of name involved a complex round of negotiations between members. Various names had been proposed all along.<sup>93</sup> The shortest title, *Evolution*, was objected to by some of the older members who wanted the word "Organic" in the title to distinguish between the various types of evolution, and by those who wanted to adopt a longer title with "Evolution" somewhere in the name.<sup>94</sup> It won out, however, because of its simplicity and the fact that it corresponded to *Ecology* and *Genetics*, which were considered successful recent journals.<sup>95</sup> The broadest

92. Mayr to H. W. Parker, November 15, 1946, SSE Papers.

93. Some of the names proposed included *Journal of Evolution*, *Journal of Organic Evolution*, *Journal of Evolutionary Biology*, *Biosystematics* and *Evolution: Journal of Evolutionary Systematics*.

94. One member felt that the simple title of *Evolution* referred to too many things: "Evolution alone implies so many different fields which have no relationship to organic evolution, and that would be my main objection to the use of the single word" (J. T. Patterson to Mayr, November 19, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File).

95. Emerson told Mayr that the name *Evolution* had been used before for at least a couple of journals that had become defunct; Emerson to Mayr, November 8, 1946, Ernst Mayr Papers, SSE Correspondence, 1946 File. Mayr had to check

title would also be most likely to draw in the widest possibly community of scientists. The subtitle *An International Journal of Evolutionary Biology* was appended to further describe the journal, but it was later changed to *An International Journal of Organic Evolution* – possibly as the result of complaints raised by paleontologists who did not consider themselves biologists (or, in Simpson's terms, neobiologists).<sup>96</sup>

The editorial board of the new journal was set at twelve members of three annual classes. While the editor was to be elected by the society members, the editorial board was to be appointed by the council of the society. To ensure that the journal represented international efforts to understand evolution, it was proposed that about five members of the editorial board be foreign members. (Published papers would be accepted regardless of country of origin, but the publication language was to be English.) In Mayr's opinion, this sort of editorial board, with its reliance on the council, would be more likely to draw an international audience to the new journal and society.

After considering the design and cost of other successful journals, Mayr and the council decided that the new *Evolution* journal would be modeled after the successful *Ecology* and would adopt the two-column style.<sup>97</sup> (The fact that Emerson himself had been editor of *Ecology* very possibly influenced this decision.) It was also decided that the journal would be a quarterly, with four issues of approximately one hundred pages, published in March,

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with the U.S. Patent Office for the copyright of the name "Evolution" for the journal; Mayr to U.S. Patent Office, October 7, 1946, Ernst Mayr Papers, SSE, Journal Evolution File. The response from the library of Congress to Mayr on November 21, 1946, indicated that the "Copyright Law contains no provision for the protection of titles, as such" (document from the Library of Congress to Mayr, November 21, 1946, Ernst Mayr Papers SSE, Journal Evolution File). The stimulating effect of *Ecology* and *Genetics* on the development of their respective fields had been noted by Mayr earlier; see Mayr to Babcock, May 7, 1946, SSE Papers.

96. The minutes of the second annual meeting (1947) indicate that the initial subtitle had been crossed out and the newer title written in. I could find no reason for the title change, other than the possibility that paleontologists opposed the inclusion of the word "biology." The paleontologists carefully and consistently used the phrase "evolutionary studies" to identify the group. It is also possible that the term "organic" was introduced to keep the various evolution communities distinct; this was in keeping with J. T. Patterson's suggestion (see note 94 above).

97. According to Lancaster Press, the journal was designed to look like *Ecology*; Lancaster Press to Mayr, March 27, 1947, Ernst Mayr Papers, SSE, Journal Evolution File. Mayr had indicated that he wanted the two-column format of *Ecology*; Mayr to R. H. Rohrer, January 6, 1947, Ernst Mayr Papers, SSE, Journal Evolution File.

June, September, and December to meet the printer's needs, rather than the obvious calendar dates of January, April, September, and December. Though the number of copies for the first volume was originally scheduled by the council to be only seven to eight hundred, Mayr increased the number to fifteen hundred in anticipation of increasing membership. Unsold copies sent to potential authors would be an effective way to promote the journal and the society and to solicit more suitable manuscripts and enrol new members. Fifty free reprints of each article published would be provided to the author; these would not only serve as an enticement to publish in the journal, but would help to promote the journal when they were distributed by the author. Each member would pay a total of \$5.00 (\$4.00 of which went toward the price of the journal, the remainder to the society), and institutions and non-members would pay \$6.00 for a subscription to the journal. These figures were in line with the average figures for memberships in smaller societies and were set with the explicit goal of helping to recruit younger and less-well-paid members to the society.

By December 5, 1946, the SSE was ready to start publication of the journal. All that was needed was a suitable printer willing to produce the journal at a reasonable cost, and an editor elected by the society. During the fall of 1946 Mayr had written to six firms inviting them to submit bids for the printing of the new journal.<sup>98</sup> Because postwar conditions had led to a serious printing backlog, only two presses made acceptable bids; one of these subsequently withdrew its bid, leaving Lancaster Press as the only possibility. On January 6, 1947, Ernst Mayr granted a contract to Lancaster Press, a firm that would provide a high-quality product at a reasonable cost.<sup>99</sup> By that time, the first official SSE meeting had taken place as scheduled in Boston on December 28–31: the final decisions concerning the details of the journal had been voted on and, without surprise, Mayr had been elected the first editor.<sup>100</sup>

98. These firms were Waverly Press Inc., Baltimore, Md.; Mack Printing Co., Easton, Pa; George Banta Pub. Co.; Rudisill and Smith Co., Lancaster, Pa; E. L. Hildreth and Co., Lancaster, Pa; and Lancaster Press, Lancaster, Pa.

99. Mayr to R. H. Rohrer, January 6, 1947, Ernst Mayr Papers, SSE, Journal Evolution File.

100. At least one document points to a strong support for Mayr as first editor by the first president of the society, G. G. Simpson: "As an additional personal suggestion, which you are of course entirely free to reject or modify in any way that you please, I think special thought should be given to nomination of an editor for the proposed journal. This will involve a lot of work and will be very crucial in the life of the Society, so it is necessary to be as sure as possible beforehand that the nominee will serve and serve well. My own feeling is that Ernst Mayr is

In a special notice distributed shortly thereafter Mayr announced the new journal to the society as “a quarterly with four issues of approximately 100 pages each,” which would publish papers that dealt with “evolutionary factors and forces.” While papers with descriptive material that would shed light on evolutionary forces and factors were welcome, papers that were only descriptive were discouraged, as were papers dealing with “straight taxonomy, nomenclature, or . . . mechanics of inheritance.”<sup>101</sup> The goal of the journal, therefore, was to support and promote the view of evolution as a dynamic science.

Soon after assuming the task of editor, Mayr took on an able full-time secretarial assistant named Sophie Prywata, who was to play a key role in helping him with the burdensome duties of editorial work. For a new journal editor in the 1940s these duties included not only soliciting suitable manuscripts, but also serving as the major reviewer, the copyeditor, the intermediary with the printer, and the financial manager who paid the bills and arranged for reprints. It was also necessary to compose an annual report to the membership on the state of the journal.

With the initial arrangements completed, the most pressing problem for the editor was the dearth of high-quality manuscripts suitable for publication in the new journal – due partly to the newness of the field, and partly to the fact that many of the younger potential contributors had only just returned to their research activities after their war duties. During this time Mayr actively solicited manuscripts, using up nearly a ream of newly printed stationery in only his first six months as editor.<sup>102</sup> At least one manuscript was written especially to fill out the first issue, and two issues had to

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the ideal man for this job. I am sure that he would do it very well, and he has done more than anyone else in laying plans for the Society and for the journal and is familiar with the problems and needs involved” (Simpson to Curt Stern, June 20, 1946, SSE Papers). Mayr was elected to serve for the years 1947–49. The editorial board consisted of three classes: Class of 1947 (for one year), Epling, Huxley, Jepson, Müntzing, Westoll, and Haldane; Class of 1948 (for two years), Darlington, Fisher, Hubbs, Newell, Piveteau, and Chaney; Class of 1949 (for three years), Dobzhansky, Dubinin, Rensch, Romer, Stebbins, and Turrill. This board included six British members, four continental members, and eight American members; Minutes of the Society, First Annual Meetings, SSE Papers.

101. Announcement for *Evolution: International Journal of Evolutionary Biology*, written by Ernst Mayr, Secretary, SSE Papers. The society had arranged for “exchange advertising” with other journals. As I indicated above, the journal subtitle was subsequently changed to remove “evolutionary biology.”

102. Letter from Mayr to author, February, 27, 1989.



be combined in the first year to make the volume complete. Nevertheless, the first volume of the journal appeared on time in June 1947.<sup>103</sup>

The official function of the society and the goals of the journal were expounded to the members of the society in the following statement:

[T]he function of the society is to promote the study of organic evolution in all its aspects. The society is a common meeting ground for representatives of all fields of science concerned with organic evolution, including genetics, paleontology (vertebrate, invertebrate, plant), taxonomy (animal, plant), ecology, anthropology, and others. The journal *EVOLUTION*, is established in order to stimulate evolutionary research and to bring its results together in readily accessible form. A journal broadly devoted to the particular subject of evolution will help to counteract the previous extreme scattering of pertinent literature, which has handicapped evolutionary study by the tendency to confine results within numerous different narrowly specialized groups. The journal will not publish taxonomic monographs or other descriptive studies properly addressed to a more specialized audience, but it will encourage the expression of the evolutionary significance of such material and will make this available to the broader group of students of evolution in general. Research primarily directed towards evolutionary problems will also be encouraged.

The aims of the Society, through its journal and otherwise, reflect the conviction that the evolutionary approach will clarify many unsolved problems and will provide common goals and mutual comprehension among all the life sciences.<sup>104</sup>

Despite the difficulty of securing suitable manuscripts that represented the diverse points of view in the new field, the new journal appeared to be off to a good start very shortly after the founding of the SSE. In turn, the publication of a thick journal at a reasonable price served to increase interest in the journal-issuing society. Mayr's extra copies of the journal were distributed or sold

103. *Ibid.*

104. The statement was circulated widely. This version was written by Ernst Mayr for the history of the society; a variant appeared in the Foreword to the first volume of the journal in 1947.

out fairly quickly, and eventually the early volumes had to be reprinted.<sup>105</sup>

**FIXING THE SOCIETY: THE FIRST ANNUAL MEETINGS,  
BOSTON, DECEMBER 28–31, 1946**

By the time of its first official meeting, held in conjunction with the AAAS and other affiliated societies, the membership roster of the new society had grown to approximately 530 members,<sup>106</sup> and indications were that it was rapidly increasing in size.<sup>107</sup> Mayr's involvement in founding the journal and his active enrollment of individuals were clearly paying off. In keeping with the newness of the society, the official program for the first meetings stated that the new SSE had "abstained this year from preparing a pretentious program."<sup>108</sup> Though the SSE sponsored some of its own sessions, many of the sessions pertaining to evolution were held in conjunction with other related member societies, so that the SSE could link up with and be supported by related established organizations at the same time that they drew on the research of common members. These societies included the Botanical Society of America, the Ecological Society of America, the American Society of Naturalists, the American Society of Zoologists, and the Genetics Society of America.

The program itself may have been modest, but the meetings as a whole were extremely important in securing the new society. In addition to making arrangements for the publication of the journal of the society, and determining the editorial policy and choice of editor and editorial board, there were other critical issues brought up at the St. Louis meetings to be settled. Thirty-three members, six guests, and representatives of the Society of Vertebrate Paleontology therefore assembled in the Salle Moderne of the Statler Hotel at a business session chaired by president Simpson to make the final decisions on vital organizational matters, central among which was the fixing of the constitution and by-laws. Along

105. Just when these sold out completely is unclear; letter to author from Mayr, January 4, 1993.

106. Charter membership closed on December 31, 1946.

107. Minutes of the Society for the Study of Evolution, Business Sessions, December 29, 1946, SSE Papers.

108. Program of the First Annual Meeting of the Society for the Study of Evolution, December 28–31, 1946, Ernst Mayr Papers, Constitution and Program File. It is possible that many members of the SSE were also directing their energies to the program of the Princeton meetings, which were scheduled to take place within a week of the Boston meetings.

with W. H. Camp, Mayr, and K. P. Schmidt, Simpson, who served as chairman of the Committee for Drafting the Constitution and By-Laws, presented the articles of the constitution one by one for approval by the members present. With the exception of "Article 5," all articles were passed unanimously.<sup>109</sup> Article 5 dealt with the election of the officers of the society.

The concern over election procedures in the SSE was a common one in many organizations: how to allow for turnover, yet preserve some semblance of stability in the group. Mayr had transmitted this concern to Camp as the problem of how to choose officers with enough "experience" but at the same time to prevent "senescence" and "the capture of the Society by a clique." He recommended having "a three-year term for the business officers (secretary, treasurer, editor, and the members of the council) and . . . preventing the reelection of president and vice-presidents. There are, thus always five persons on the executive committee who have served the Society for several years. Without embodying it into the constitution, it will be advisable not to change the secretary and treasurer simultaneously so that the old treasurer can advise the new secretary and vice versa."<sup>110</sup> With concerns like Mayr's in mind, Simpson had appointed a special committee consisting of H. J. Muller, A. F. Shull, and G. L. Stebbins to present a report on the wording of Article 5 on the specifics of election procedure. After Muller presented the final wording of the article, members of the society discussed the election procedures, and voted unanimously in favor of Muller's report.<sup>111</sup>

In addition to the fixing of the constitution, and the official go-ahead for the society's journal, the other point of business that came up at the first meeting was the plans for subsequent meetings of the society and affiliations with other societies. The fact that there were so many affiliated or related societies that were willing to cosponsor the initial program reveals the extent to which the new society had deep ties to other biological societies. The problems of relations with the older American Society of Naturalists loomed large once again, as Emerson reported for the Committee to Cooperate with the American Society of Naturalists. According

109. Minutes of the Society for the Study of Evolution, Business Session, December 29, 1946, SSE Papers.

110. Mayr to W. H. Camp, May 13, 1946, SSE Papers.

111. Draft of the Constitution and By-Laws of the Society for the Study of Evolution, Adopted December 29, 1946, Boston, Revised (By-laws, Art. 2) December 29, 1947, Chicago, SSE Papers. The constitution was printed and circulated to members in 1947.

to Emerson, no official action had been taken by the ASN toward amalgamating with the newer society, and it was decided that no real report could be made on the possibility of amalgamation.

The problem of the official relationship to the American Society of Naturalists was just one of many problems that the new society would face with respect to other societies. Also raised at this meeting was the issue of whether the SSE would continue to meet with the AAAS, especially since many members of the society objected to the timing of the AAAS meetings during the Christmas holidays. One resolution passed at the end of the business meeting was the possibility of persuading the AAAS – with the help of other biological societies – to move the meetings from Christmastime to early September. This issue was brought up again at the council meeting that followed the business meeting and it was decided that the society should meet with the AAAS at the Chicago meetings, where it would consider seriously the possibility of meeting in rotation with different societies interested in evolution.<sup>112</sup> At a second council meeting, held on December 31, Mayr reported on his efforts to affiliate officially with the British group, the association for the Study of Systematics in Relation to Biology. Two other affiliations were recommended, the first with the AAAS, and the second with the National Research Council. With the move to officially incorporate the society, the first annual meetings drew to a close.

Just one week after the meetings in Boston, and shortly after the new year, Simpson, the president of the new SSE, attended the final conference organized by the Committee on Common Problems in Genetics, Paleontology, and Systematics to report on the status of the new society as chair of the committee. Using the occasion of the first meeting as evidence of the secure status of the new society, Simpson announced: “The Society held a successful First Annual Meeting at Boston in December, 1946, and its success and permanence seem to be assured.”<sup>113</sup>

#### CONVERGENCE AND CELEBRATION: THE 1947 PRINCETON MEETINGS

It was not just a society and a journal that had been fixed and sustained; simultaneously with these organizational efforts, the

112. Minutes of the Society for the Study of Evolution, Council Meeting, December 29, 1946, SSE Papers.

113. As cited by Glenn L. Jepsen in his Foreword to *Genetics, Paleontology, and Evolution* (above, n. 27), p. vii.

sense arose that evolutionists had reconfigured – without question – on a “common ground of theory.”<sup>114</sup> This much seemed certain at the final symposium of the Committee on Common Problems of Genetics, Paleontology, and Systematics.

With the end of the world war, resources were finally made available for the planning of major conferences that had to be postponed under wartime conditions. In 1946, Princeton University invited the committee to hold their final symposium in conjunction with one of the university’s Bicentennial Conferences; Princeton would provide both the location and the funds. The result of this invitation was the International Conference on Genetics, Paleontology, and Evolution, held at the Princeton Inn on January 2–4, 1947. Many of the common members from the Boston meetings reassembled at Princeton to give papers that represented their areas of research, which had now been successfully synthesized.<sup>115</sup>

The mood of the conference was optimistic and cheerful, if not ebullient. Members of the committee had good reason to rejoice as they brought in the new year: the brutal war was over,<sup>116</sup> a new journal-issuing society for the study of evolution had been established, and participants could finally agree that a convergence between their disciplines had taken place. The move to reconfigure evolutionary practice in order to bring together genetics, systematics, and paleontology appeared to have succeeded: Simpson’s 1944 announcement that a common field was emerging had given way to a sense, not only that a common field existed, but that a new and synthetic evolutionary discipline was being built.

114. H. J. Muller, “The Redintegration of the Symposium on Genetics, Paleontology, and Evolution,” in *ibid.*, p. 422.

115. See the list of the committee in *Genetics, Paleontology, and Evolution*. It included Edgar Anderson, D. I. Axelrod, Ernest B. Babcock (Chairman, Western Group), Walter Bucher (Acting Chairman to October 1944; Chairman, Eastern Group), Kenneth E. Caster, Ralph W. Chaney, Bruce L. Clark (died 1945), Edwin Colbert, G. Arthur Cooper, Kenneth W. Cooper, M. Demerec, Th. Dobzhansky (Chairman, Section on Genetics), Carl O. Dunbar, M. K. Elias, Carl Epling, Myron Gordon, Glenn L. Jepsen (Chairman, Section on Paleontology), Herbert L. Mason, Ernst Mayr (Chairman, Section on Systematics), H. J. Muller, Bryan Patterson, F. B. Phleger, Alfred Sherwood Romer, George Gaylord Simpson (Chairman), Warren P. Spencer, G. Ledyard Stebbins, Jr. (Vice-Chairman, Western Group), Curt Stern, Chester Stock, Horace E. Wood 2nd, and Sewall Wright. Included as *ex officio* members were the Chairman of the Division of Geology and Geography of the National Research Council, William W. Rubey (to 1946) and Arthur Bevan (since 1946) and the Chairman of the Division of Biology and Agriculture, Robert F. Griggs.

116. One committee member, Bruce L. Clark, had died toward the close of the war; other members, like Simpson, had served overseas; while still others, like Stebbins, undertook war-related and more applied scientific research.

Writing the summation to the edited volume of the proceedings, H. J. Muller captured the sense of agreement and consensus. Beginning his essay, entitled "The Redintegration of the Symposium on Genetics, Paleontology, and Evolution," with a section subtitled "The Convergence of Evolutionary Disciplines," Muller drew the parallel between the evolutionary convergence of types and the convergence between disciplinary types like geneticists and paleontologists. The end result of this fusion was a new and higher type through a process of synthesis: the synthetic type of evolutionist.<sup>117</sup> What had begun as a disparate set of moves to reconfigure evolutionary practice and to integrate paleontology, genetics, and systematics had led to the emergence of a synthetic evolutionary practice. In fact, the meetings were such a celebration of the convergence of disciplines that they came down in the history of evolutionary biology as simply the "Princeton meetings."<sup>118</sup>

"ONE VOICE" IN THE EVOLUTIONARY FUGUE:  
"EVOLUTION," 1947–1950

The success of the Princeton meetings drove home to the members of the new SSE that the move to reconfigure and organize evolutionary practice had been successful. The Princeton meetings and the climate of exuberance that they generated for evolutionists also served as a stimulus to enroll more researchers newly returned from war duties, and evolutionary activity thus received a shot in the arm. So too with an official society and journal, what was emerging as a discipline of knowledge, complete with textbooks and sets of common problems, was off to a good start. But given the diverse backgrounds and disciplinary affiliations of the members of the new society, learning to speak a common language while preserving a balance between the disparate points of view would prove to be a challenging and critical problem for the next three years.

This problem was evident from the outset for the journal that was to speak with "one voice." The difficulty of securing enough suitable manuscripts continued well into the second year of publication. What exactly counted as a suitable manuscript, and for

117. Muller, "Redintegration" (above, n. 114).

118. See, for instance, the brief account of the meetings and their importance to the history of evolutionary biology in Peter Grant and Henry S. Horn, eds., *Molds Molecules, and Metazoa: Growing Points in Evolutionary Biology* (Princeton: Princeton University Press, 1992).

whom, became a contentious issue for the society, since the SSE had made a strong commitment to publish manuscripts that represent the full diversity of evolutionary practices. While the first issue of *Evolution* seemed to overemphasize the research of *Drosophila* and genetics workers – a criticism that Mayr was to hear repeatedly<sup>119</sup> – subsequent issues attempted to represent as many workers, approaches, and organisms as possible. As the first editor of *Evolution*, Mayr worked hard at soliciting suitable submissions, going so far as to publish a controversial manuscript by Rainer Zangerl defending an unpopular view of typological morphology.<sup>120</sup> Despite his attempts, however, Mayr continued to receive criticism from members who felt that their respective fields were not well represented, if they were represented at all, in the new journal.

Of the recognized evolutionary fields, paleontology was least well represented, despite what could be described as aggressive measures on the part of the editor. The absence of paleontology was repeatedly pointed out by Simpson. Concerned widely with this issue, Simpson drafted a letter of distress in 1947. In addition to other complaints about the lack of consideration given to paleontologists, he pointed to the absence of paleontological articles in *Evolution* as being indicative of the general lack of support for the paleontological perspective (this despite the representation of paleontology on the editorial board). Making efforts to preserve a

119. Ralph L. Chermock wrote the following to Mayr: "Your journal arrived this morning, 112 pages on the science of evolution. On analyzing this, I noted that 62 pages alone dealt with the genetics of *Drosophila*; a total of 96 pages dealt with genetics in general; 6 pages pertained to ornithology, and 10 pages to Paleontology. If I had not looked at the cover of the journal, I would have felt that I was perusing a journal pertaining to the science of genetics alone. I, and many of my associates were disappointed. We all realize the importance of genetics in evolution, but also realize that this science has numerous outlets for their publications. Why should they completely dominate a journal which is supposedly representative of all of the biological sciences? I feel that papers on genetics should be included in the journal, but also that other fields should be represented much more strongly than they were. I feel, otherwise, the title of the journal and the society should be changed to 'Genetics and Evolution'" (Chermock to Mayr, July 15, 1947, Ernst Mayr Papers, SSE, Box A–D).

120. See, for instance, Mayr's request for botanical manuscripts in a letter he wrote to Stebbins, January 22, 1948, Ernst Mayr Papers, SSE, Box St–Z; and see Stebbins's note to Simpson, April 29, 1948, SSE Papers. Zangerl's manuscript was objectionable to Mayr for philosophical reasons: Zangerl and Mayr differed on their interpretation of "observation and experiment." See the letter from Zangerl to Mayr discussing differences in philosophy of science: Zangerl to Mayr, June 14, 1948, Ernst Mayr Papers, SSE, Box St–Z.

place for the paleontologists, Simpson requested that his letter to the secretary be read to the council at the Chicago meetings. He wrote:

This Society is in part an outgrowth and the focus of a movement to bring together geneticists, paleontologists, and systematists on the common ground of evolutionary studies. It is, of course, desirable to include not only these but all other pertinent fields, yet it remains true that what is most essentially new in this movement and in its results has arisen from the collaboration of paleontologists, on one side, with neobiologists, on the other. In spite of this background, there is already, I think, a clear tendency for the Society to become essentially neobiological, rather than to continue the synthesis of paleontology and neobiology. If this trend exists and is not checked, this will limit the effectiveness of the Society and, indeed might quite remove its *raison d'être*.

He concluded:

These remarks are not made in a spirit of criticism or in an effort to claim undue importance for the field in which I work. I am well aware that paleontology is only one of several equally important broad fields involved in the study of evolution. The Society could function and make a real contribution if paleontology were wholly excluded. It was, however, founded in order to foster a synthesis between these broad fields, and the exclusion of paleontology or any other pertinent science of comparable scope will defeat this purpose. I am intensely concerned that the Society should succeed and should serve this particular purpose, and I therefore feel it my duty to call your attention to a real danger that has perhaps not even been noticed by the non-paleontological members of the Council.<sup>121</sup>

While Simpson's letter drew a great deal of attention to the issue of representing all evolutionary points of view, especially with an eye to including paleontology in *Evolution*, paleontologists as a whole appeared not to be as actively involved in producing the new journal. Simpson actually noted that it was the paleontologists themselves who had failed to submit suitable manuscripts, despite the editor's repeated call for submissions.

121. Simpson to S. A. Cain, September 29, 1947, SSE Papers. See also the discussion on the Simpson letter of 1947 in the next section below.



This was not so for the botanists. It was, in fact, botanists like E. B. Babcock and especially G. Ledyard Stebbins who repeatedly urged the zoologist Mayr to pay more attention to botany. Writing on behalf of the botanists, Stebbins reminded Mayr of the importance of the botanical perspective: "Many of us on the plant side are beginning to feel that 'Evolution' is favoring animals too much, and our interest in the journal and society is starting to decline."<sup>122</sup> In one case, Mayr's rejection of a botanical manuscript submitted by Stebbins on the grounds that it contained "too much detail" precipitated a minor altercation between the two; protesting the unfairness of the rejection, Stebbins wrote: "It seemed to me that you were discriminating against the higher plants, except in cases like that of Verne Grant, where the information was also of great interest to zoologists."<sup>123</sup> Through Stebbins, Babcock, and others, botanists thus played an active role in producing the journal.

Gathering representative articles remained difficult for the editor of *Evolution* until well into the next decade.<sup>124</sup> The 1948 editor's report indicated that the fields of botany and anthropology were underrepresented, but that paleontological manuscripts had actually begun to increase; according to Mayr, this was an encouraging sign that could "be traced directly to the influence of the journal."<sup>125</sup> But at the council meetings of 1949 at Columbia University, Mayr still indicated that there was a "disproportionate representation" in the journal and that this was "due to his failure to receive equal quantities of acceptable manuscripts."<sup>126</sup> The 1950 editor's report continued to call for more submissions to represent fields like paleobotany and vertebrate paleontology.<sup>127</sup>

122. Stebbins to Mayr, April 21, 1949, Ernst Mayr Papers, SSE, Box St.-Z.

123. Stebbins to Mayr, October 12, 1950, Ernst Mayr Papers, SSE, Box St.-Z. The opening of Stebbins's letter complimented Mayr on his science, but cast doubts on his role as editor. According to Mayr's recollection, Stebbins's manuscript was rejected because of the unreasonable request to publish so many halftone plates that it would have driven the society to bankruptcy; the exact number is uncertain, but Mayr estimates there were close to fifty. The paper also appears to have been not as evolutionary in scope; letter from Mayr to author, January 4, 1993.

124. The minutes of subsequent meetings and the editors' reports indicate that the editors kept close tabs on the numbers and proportions of manuscripts submitted and published in the various areas of research.

125. Editor's report dated November 10, 1948, SSE Papers.

126. Minutes of Council Meeting, dated December 27, 1949, SSE Papers.

127. *Evolution*, Editor's Report for 1950, Edwin H. Colbert, Editor, SSE Papers. The editor's breakdown for the issues was plants (5), *Drosophila* (8), other insects (3), vertebrate zoology (4), vertebrate paleontology (2), reports (2), notes and comments (5); SSE Papers.

Inevitably, as some members became frustrated by the complex round of negotiations, they became pessimistic about the whole venture. Writing in October 1948 to the new secretary, Stanley Cain, Lee Dice complained that members were “losing interest” and a few had “threatened to pull out.” Hoping to make evolution itself more interesting, Dice suggested fewer original articles and more reviews, notes, and comments. As to the balance in *Evolution*, he echoed the complaints of other members: “For the other criticisms that the journal has included too much *Drosophila* and too little paleontology, I do not think the editor is to be blamed. You may quote me if you wish.”<sup>128</sup>

The selection of suitable manuscripts representing the fields of evolution thus clearly posed a critical problem. Equally important, on the other hand, was the exclusion of unsuitable manuscripts that did not conform to the goals of the society. At least one submission precipitated a minor controversy when it was rejected. M. K. Elias of the Nebraska Geological Survey had submitted his lecture read at the first annual society meeting, only to have it rejected because it was insufficiently experimental in scope. After a round of exchanges involving Wendell Camp and G. G. Simpson, a specific editorial policy was laid out that would support experimental evolution while leaving some room for nonexperimental yet dynamic approaches. In a letter to Elias, Simpson, who as a paleontologist was especially sensitive to the issue of experimentation, stated: “The policy is summed up by the title ‘Evolution,’ and I do not see how this can be considered a narrow policy or a prospectus for a journal of experimental evolution only.”<sup>129</sup> The policy therefore supported evolution as an experimental science, and at the same time served to support related evolutionary practices by linking them to experimental evolution.

128. Lee R. Dice to Cain, October 21, 1948, SSE Papers.

129. Simpson to M. K. Elias, February 10, 1947; see the file folder labeled M. K. Elias in the Ernst Mayr Papers, under the Society for the Study of Evolution, for the entire controversy. Elias eventually was the first member to resign from the society. The actual reason for his manuscript’s rejection is unclear, though it appears that Simpson’s dislike of the manuscript *and* its author were part of the reason. Simpson referred to the paper as “all nonsense, and part of it malicious nonsense.” Later on he wrote, “I am already somewhat at outs with this gentleman, who has made and continues to make large demands on my time and patience” (Simpson to Mayr, January 16, 1947). It appears that Elias was also in disfavor with Camp, who described Elias as a “sorehead” and instructed Mayr to “Read and throw in wastebasket” in a handwritten note on the top margin of the letter he had written to Elias and then sent to Mayr (Camp to Mayr, January 30, 1947).

The exchanges that took place over the inclusion and exclusion of articles in *Evolution* were so heated that they appeared to divide the society – yet these conflicts were part of the complex round of negotiations, not unlike those that had taken place between members in the earlier bulletins, that facilitated dialogue and in turn helped to construct one evolutionary voice, a common language for the members of the society. They were also part of a process that would determine what counted as evolutionary biology, the disciplinary category that would define the identities of members of the society. Smoothing out the relations between the members and attempting to build consensus and belief in a unified, legitimate science of evolution occupied much of the editor's time in the early years of the society.

#### STABILIZING COMMON GROUND: ALLEGIANCES AND AFFILIATIONS, 1947–1950

The diverse backgrounds and disciplinary ties of its members were also to prove problematic for subsequent affiliations of the society. With the proliferation of scientific societies in America following World War II, the SSE encountered inevitable points of friction as it made difficult choices such as with whom to hold joint meetings, with whom to officially affiliate, and from whom to receive sponsorship. This was part of the process of negotiating the location of the society with respect to older, closely related existing societies, and especially to newer societies representing the newer life sciences that were emerging in the postwar scientific boom.<sup>130</sup> Further, the relationship between the SSE and the closely related American Society of Naturalists continued to be a concern

130. For the most recent historical account of the development of American science in this period see Arnold Thackray, *Science After 40* (Chicago: University of Chicago Press, 1992). For the life sciences, see Toby Appel, "Organizing Biology: The American Society of Naturalists and Its 'Affiliated Societies,' 1883–1923," in *The American Development of Biology*, ed. Ronald Rainger, Jane Maienschein, and Keith Benson (Philadelphia: University of Pennsylvania Press, 1988); Robert Kohler, *From Medical Chemistry to Biochemistry* (Cambridge: Cambridge University Press, 1982); Lily Kay, "Selling Pure Science in Wartime: The Biochemical Genetics of G. W. Beadle," *J. Hist. Biol.*, 22 (1989), 73–101; and idem, *The Molecular Vision of Life* (Cambridge: Cambridge Press, 1992). See also the discussion of organizational efforts in Keith Benson, Jane Maienschein, and Ronald Rainger, *The American Expansion of Biology* (New Brunswick, N.J.: Rutgers University Press, 1991). Some of these organizational efforts followed in the wake of Vannevar Bush's call to increase support for basic research in his epoch-making *Science: The Endless Frontier*.

among members, as did the collaboration with the British group of systematists organized by Huxley. Though arrangements were often agreeable, conflicts of interest between the members and previous allegiances to other societies threatened to divide the members of the new society. But patient and indeed laborious negotiations served to stabilize the society in its early difficult years.

The difficulty of maintaining a balance of representative points of view and negotiating allegiances with related societies was made especially apparent by Simpson's 1947 letter of distress. In addition to saying that not enough paleontological manuscripts were being published in *Evolution*, Simpson complained of other problems. The actual catalyst for the letter was a conflict in the meeting times of the society. Unable to attend the second annual meeting of the SSE in Chicago (on December 29–31, 1948) because of the conflicts with three other related societies, he took effective measures to point out to the secretary of the SSE, Stanley Cain, and to the council that all was not well. For Simpson, there were clear indications that the new society was overly neobiological, to the exclusion of the paleontological perspective. In addition to the problem with the journal, there was a problem with the nomination of the new set of officers for the society, none of whom were paleontologists, and with the fact that meetings of the SSE conflicted with both the Paleontological Society and the Society of Vertebrate Paleontology. Simpson was careful to suggest that these were not conscious moves made by any member of the society, but he indicated that the efforts "to cause *diverse* interests to *converge*" were not working.<sup>131</sup>

Cain quickly duplicated and distributed Simpson's letter to the officers of the society to be considered at the Chicago meetings. Rather than viewing the letter as a divisive manoeuvre, Mayr quickly responded favorably and applauded Simpson's efforts to keep the society together by the inclusion of the paleontologists. Responding to the three issues raised by Simpson, Mayr echoed his concern by stating that he deplored the lack of paleontologists nominated for officers. He also supported Simpson's concern over the lack of paleontological articles in *Evolution*; however, as an editor who had done his best to present articles on evolution from all pertinent fields, he added that the paleontologists were simply not contributing enough manuscripts to the journal. Turning to Simpson's concern with the timing of the meetings, Mayr made it clear that he considered this the most important of the issues

131. Simpson to Cain, September 29, 1947, SSE Papers.

raised: "Unless there are joint meetings with the paleontologists, it will be very difficult to have a unified society." The question, for Mayr, was put simply: "Shall the neobiologists meet with the geologists or the paleontologists with the biologists"? Since members who were geneticists or zoologists had little to do with geology and would therefore not be interested in joint meetings, he made it apparent that he thought that paleontologists, who were students of extinct organisms, had at least as much in common with the biological sciences as with geology. His solution to the problem of joint meetings was to hold every third meeting jointly with geneticists and neobiologists, and to make sure that some meetings were with the Geological Society. He pointed out that any solution would have to involve compromise, and he ended his official response to Simpson by suggesting that the annual meetings be moved from Christmastime to a date before Labor Day and the first day of college.<sup>132</sup>

Mayr's support of Simpson's letter and the concerns it raised appears to have been echoed by the council of the society, which formally discussed the letter at the December meetings. The consensus of the council was that "the paleontologists have the good will of the group and that every reasonable effort be made to cooperate with them by avoidance of conflicts in meeting dates, etc." It was also decided that the council would pass on to the incoming president (voted at the December meeting to be G. Ledyard Stebbins) that appointments to committees should represent all the fields of interest of the SSE.<sup>133</sup>

Dealing with these issues was just part of the work facing the SSE at this time. The possibility of affiliation with the NRC, the AAAS, the British group of systematists, the older American Society of Naturalists, and the newly formed American Society of Professional Biologists was being discussed, as well as the question of the potential advantages of full incorporation.<sup>134</sup> The relationship of the SSE to the ASN, formerly a pressing concern, had begun to diminish in importance: according to Emerson and

132. Mayr to Simpson, October 24, 1947, SSE Papers.

133. Minutes of the Council Meeting of the Society for the Study of Evolution, December 30, 1947, Chicago, SSE Papers.

134. The initial decision to incorporate the society was deferred by Secretary Cain for at least three years. I could find no documents that accounted for the reluctance displayed by the society, or when the society actually made the final decision to incorporate. It was eventually incorporated at the location of Bloomfield Hills in Oakland County, Mich.; the post office address of the registered office was Cranbrook Institute of Science, Bloomfield Hills, Mich.

the Committee to Cooperate with the American Society of Naturalists, the "formation and activities of the SSE had failed materially to affect adversely the American Society of Naturalists," and therefore Emerson and the committee did not feel the immediate need for amalgamation or for taking any further steps.<sup>135</sup> The American Society of Naturalists themselves had begun to create space for the newer evolution society, and also to serve as a forum for the increasing numbers of biologists. They made this public in 1946 when they shortened their statement of purpose to read "A Semi-Monthly Journal Devoted to the Advancement of the Biological Sciences Relations," by removing the subclause "with Special Reference to the Factors of Evolution." In so doing, the older society had redefined its identity. Relations with the British group also continued to be cordial: common members served on the editorial board of the journal, and there was continued exchange between members of the two groups.

Affiliation with the AAAS had been a relatively easy decision to make, given that so many members of the SSE were also members of the AAAS, and that the SSE had sprung out of the annual meetings of the AAAS. Mayr had initially written to Howard A. Meyerhoff at the AAAS requesting information about affiliation, just after the St. Louis Meetings, and had received an encouraging response.<sup>136</sup> On October 21, 1947, Stanley Cain made an official application for affiliation to the AAAS. On November 1, 1947, the executive committee of the AAAS met to decide on the applications, and on December 2 the administrative secretary of the AAAS, F. R. Moulton, informed Cain that through a unanimous vote of the executive committee of the council of the AAAS the SSE's application was accepted. There were in fact so many AAAS Fellows in the SSE that the SSE was entitled to have *two* representatives on the council of the AAAS. By April 1, 1948, Stebbins, the new president of the SSE, had appointed Carl Epling and J. Brookes Knight as the two representatives.<sup>137</sup>

Unlike the encouragement the SSE received from the AAAS,

135. One incident at the same meetings indicates that the American Society of Naturalists actually benefited from the new society. At the Chicago meetings the ASN faced a deficit due to a "mismanagement of arrangements" in their organization of the Biologists' Smoker. The SSE agreed to help pay for the Smoker with an amount that was not to exceed \$15.00. The request for assistance from the secretary of the ASN was made to the SSE and was considered favorably at the Council meeting; Minutes of the 1947 Meeting, SSE Papers.

136. Howard A. Meyerhoff to Ernst Mayr, June 17, 1946, SSE Papers.

137. Stebbins to Cain, April 1, 1948, SSE Papers.

an initial inquiry about affiliation with the National Research Council had met with a mildly discouraging response. This was something of a surprise, given the NRC's strong sponsorship of the Committee on Common Problems in Genetics, Paleontology, and Systematics. Responding to Simpson's inquiry, Robert F. Griggs, chair of the Division of Biology and Agriculture, stated that it was too early to file for membership. Griggs's initial resistance appears not to have been due to doubts as to the aims or suitability of the SSE, but rather to the fact that the Division felt that it had too many member societies: with some twenty-seven such societies, Griggs indicated that the Division was somewhat "overgrown."<sup>138</sup> Affiliation with the NRC was discussed once again at the 1947 meetings, and the affiliation was granted subsequently.<sup>139</sup>

The decision to affiliate with the newly organized American Institute of Biological Sciences met initially with some resistance, in part because AIBS was so new and still developing, and in part because it had a high representation of diverse groups of nonevolutionary experimental biologists who had initially denigrated evolutionary practice. At the rather steep sum of \$100 per annum, moreover, official affiliation involved a serious investment of financial resources, especially for a new society. Some caution had to be exercised before the members made a final decision. With due consideration, influenced strongly by Mayr's feeling that there was an advantage to having "one strong central business organization," especially since the AAAS had "fallen down on the job," the SSE moved to request affiliation with AIBS.<sup>140</sup> On October 24, 1950, Frank P. Cullinan, chairman of the governing board of the American Institute of Biological Sciences, wrote to Theodor Just to inform him that the governing board had voted on, and passed, the request for membership.<sup>141</sup>

While the decisions to affiliate with societies like AAAS, NRC, and even AIBS were favorable, the decision in regard to other societies, especially other biological societies, was not as favorable. On December 12, 1947, President J. T. Patterson received an invitation from Norman C. Laffer, president of the American Society of Professional Biologists, to form a committee of SSE members who would work with the ASPB "in problems of mutual interest."

138. Robert F. Griggs to G. G. Simpson, July 10, 1946, SSE Papers.

139. The NRC's *Handbook of Scientific and Technical Societies of the United States and Canada* lists the vital statistics of the SSE in the fifth edition, published in 1947.

140. Mayr to Theodor Just, May 26, 1950, Ernst Mayr Papers.

141. Frank P. Cullinan to Just, October 24, 1950, SSE Papers.

The ASPB had been formed to look out for the socioeconomic interests of the "professional biologist."<sup>142</sup> The response of Patterson and the council members was lukewarm, if not discouraging. After the letter was read to the council members at the 1947 council meetings it was decided to take no action on the forming of a committee, or on the initiation of official cooperation between the two societies. The minutes of the meeting stated that "the opinion was apparently unanimous that any member of the SSE was free to work with the Professional Biologists, but that the SSE as a society would not formally cooperate."<sup>143</sup>

The proposal of affiliation with another society, the National Society for Medical Research, met with an exceptionally hostile reaction. Two officers of the NSMR, President A. J. Carlson, Secretary-Treasurer A. C. Ivy, wrote to SSE president Patterson in 1948, pointing out that the SSE was one of the few scientific groups that was not officially associated with the National Society for Medical Research in "its cooperative program to build public understanding of medical research."<sup>144</sup> The NSMR had been organized under the sponsorship of the Association of American Medical Colleges in 1946. The response of the SSE to the proposal to associate with the NSMR was *strongly* negative. According to Cain, council members considered the proposal at the 1949 council meetings of the SSE: three council members voted for approval; and five said no. One member suggested a \$25 appropriation. One strong response was especially lucid in explaining the nature of

142. The description of the ASPB was given as follows:

During the past decade, there has been considerable interest, among the younger biologists in particular, in the professional and economic problems of biologists. Many of the scientific biological societies were not organized in a manner to deal with such problems, and the major activity of such societies is the promotion of science. Consequently, the AMERICAN SOCIETY OF PROFESSIONAL BIOLOGISTS has been formed. It is the purpose of this Society to deal with those professional and economic problems that have not been aggressively faced by the older scientific organizations.

One of the first undertakings of the ASPB will be the definition of the "professional biologist," and a committee is now working on that project. Secondly, the problems of "classification" and "certification" are active ones in many societies at the present time, and they will be considered soon. (Laffer to Patterson, December 12, 1947, SSE Papers.)

143. The membership lists were shared with the ASPB; Minutes of 1947 meeting, SSE Papers.

144. A. J. Carlson and A. C. Ivy to J. T. Patterson, December 2, 1948, SSE Papers.



the objection to affiliation with the NSMR: "I am emphatically opposed. . . . The medical researchers are exceedingly well organized and have managed to corral nearly all the research money that is available in the field of biology. They have not done a thing for the naturalist and never will. . . . On the contrary, we must point out again that ecological and evolutionary research is not properly endowed and, least of all, taken care of by groups interested in medical and physiological research."<sup>145</sup>

The hostile reaction to the NSMR and the somewhat lukewarm response to cooperation with the American Society of Professional Biologists reveals just how the SSE viewed itself and its relations to other proliferating postwar scientific societies. From the intensity of their responses, the members evidently felt themselves underprivileged with respect to the securing of resources in comparison to medical researchers and other areas of applied research, which were getting the lion's share of available funds – a situation only exacerbated by the development of other newer sciences such as molecular biology and biochemistry, which were heavily supported by agencies like the Rockefeller Foundation. Yet SSE members were not so threatened that their interests had to be secured through affiliation with an organization like the ASPB, whose purpose was stated as being concerned solely with the socioeconomic features of the practice of professional biology. For the most part, members of the SSE indicated that their interests in their newly established organization continued to be not socioeconomic but scientific: the society was valued for facilitating communication among interested members through its journal and its conferences, as well as for promoting the scientific practice of evolution as a unified biological field.

The SSE's involvement in AIBS surged in the 1950s and early 1960s as key members of the SSE (Mayr, Simpson, Stebbins, Dobzhansky, Muller, and others) took on visible positions at AIBS; simultaneously, members of the SSE became representatives, spokespersons, and promoters of not only evolution but also biological science as they contributed to textbook series like the Biological Sciences Curriculum Study and wrote other textbooks of biology. AIBS and BSCS documents revealing the SSE's extensive involvement in the wider biological sciences were scattered between the SSE papers transmitted by successive secretaries.<sup>146</sup> At the same time, SSE members also began to redefine their disci-

145. Cain to A. J. Carlson, January 18, 1949 SSE Papers.

146. This material is scattered through approximately five cardboard boxes.

plinary appellation as evolutionary biologists. In an era that was witnessing the maturation of biological science, evolutionary biology as a discipline of knowledge would function as the unifying central science for the various biological sciences.<sup>147</sup>

### THE SSE AS THE CENTRAL “ORGAN” OF EVOLUTIONARY BIOLOGY

Membership growth rates continued to grow steadily into the 1960s (see Fig. 2). Though members were drawn from an international audience, most were primarily U.S.-based. A 1949 breakdown of their geographic distribution (see Appendix 3) reveals that they came from nearly all fifty states, with by far the greatest number coming from California, New York, and Illinois (the next three states were Michigan, Massachusetts, and Pennsylvania). This geographic breakdown is consistent with the initiatives to organize evolution that had come from scientists in the Chicago area, the New York City area, and the San Francisco Bay area: key early organizers and officers of the society – like Bucher, Dobzhansky, Dunn, Emerson, Mayr, Simpson, and Stebbins – resided in one of these three locales. Subscription numbers, which included subscriptions from major university and research libraries undergoing a period of rapid growth in the late 1950s and 1960s,

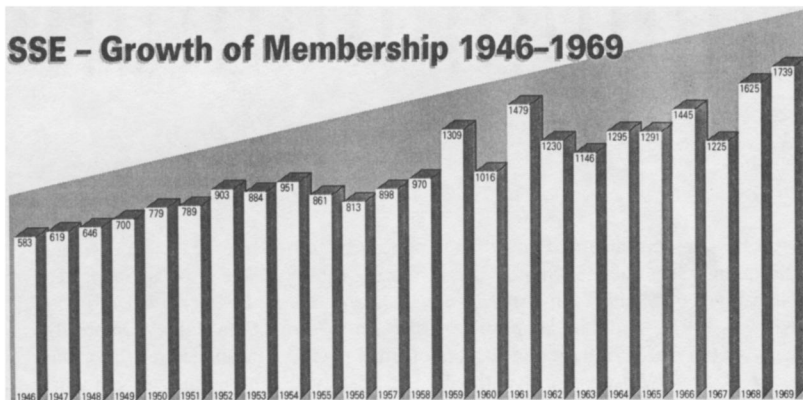


Fig. 2. Growth of membership in the Society for the Study of Evolution, 1946–1969.

147. For the discussion of evolutionary biology as a unifying central science see Smocovitis, “Unifying Biology” (above, n. 3).

increased at a slightly higher rate, in keeping with the institutional growth (see Fig. 3). In 1949 the first membership booklet was finally published and distributed to the members, as was a printed version of the Constitution and By-Laws of the society.

On November 21, 1959 – exactly one hundred years after the publication of Darwin’s *Origin* – the SSE sponsored one of its earliest official ritual celebrations when it held its fourteenth annual meeting in conjunction with the Darwin Centennial Celebration in honor of Darwin, the “founding father” of the new synthesis of evolution.<sup>148</sup> Cosponsored by the University of Chicago, an institution founded exactly ten years after Darwin’s death, the celebration lasted five days.<sup>149</sup> With Sol Tax as initiator and head organizer of the festivities, the University of Chicago was high-

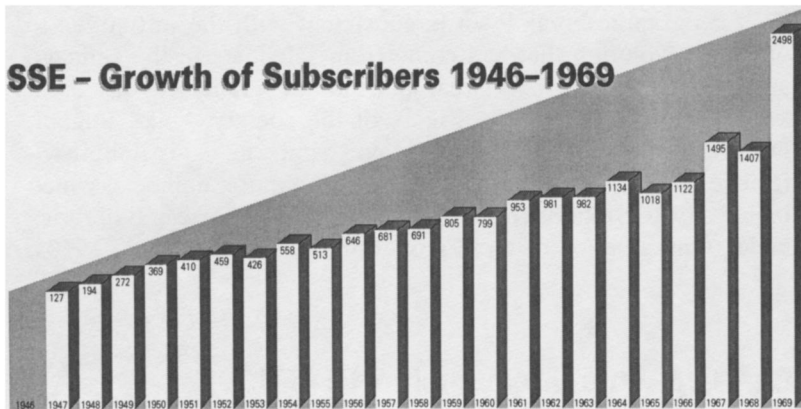


Fig. 3. Growth of subscribers to *Evolution*, 1946–1969.

148. For an account of how “founding father” stories emerge from and sustain disciplinary identities see Smocovitis, “Unifying Biology.” See also Jan Sapp, “The Nine Lives of Mendel,” in *Experimental Inquiries*, ed. H. E. Legrand (Dordrecht: Kluwer 1991); and Jan Sapp, *Where the Truth Lies* (Cambridge: Cambridge University Press, 1990). For an account of ritual practices in science see the special issue of *Social Epistemology* entitled “The Historical Ethnography of Scientific Rituals”: *Social Epistemology*, 6 (1992), especially the contribution by Pnina Abir-Am, “A Historical Ethnography of a Scientific Anniversary in Molecular Biology: The First Protein X-ray Monograph (1984, 1934),” pp. 323–354.

149. The program had numerous multiple sessions; program entitled Society for the Study of Evolution, Fourteenth Annual Meeting in Conjunction with The University of Chicago Darwin Centennial Celebration, SSE Papers. For an account of the proceedings see Sol Tax and Charles Callender, eds., *Evolution after Darwin*, vol. III (Chicago: University of Chicago Press, 1960).

lighted as a center of activity in evolutionary biology, and so too was Tax's own discipline of anthropology, which had previously not played a critical role in the evolutionary synthesis.<sup>150</sup>

The celebration called to the University of Chicago internationally distinguished evolutionists who participated in panel discussions, special addresses, and even televised talk-show programs that discussed the state of the art in evolution. As part of the celebration, a special convocation ceremony was held with participants dressed in full academic regalia: Julian Huxley gave the convocation address, and Simpson, among numerous others, received an honorary degree (Mayr, who had held a museum position, was not eligible to receive the degree). Evolutionary entertainment came in the form of an evening at the theater with the evolutionary play *Time Will Tell* (an original musical written especially for the Celebration) performed before a large, enthusiastic audience. Along with research scientists, the celebration also attracted the National Conference for High School Biology Teachers, who held an Institute for High School Biology Teachers funded by the National Science Foundation with the intent of "widening the influence of the Centennial Celebration."<sup>151</sup> The celebration served to solidify the links between the disciplines of evolutionary knowledge by focusing on common problems among the disciplines: one televised discussion entitled "At Random" brought together Sol Tax, Julian Huxley, Harlow Shapley, Sir Charles Darwin (grandson of Charles Darwin), and Adlai Stevenson. For Tax, himself, the celebration was successful in bringing "Darwin and evolution back into anthropology."<sup>152</sup>

Unsurprisingly, membership enrollments peaked dramatically in 1959, following the Darwin Centennial Celebration and the rush of related publications, including the three-volume *Evolution after Darwin* edited by Sol Tax and Charles Callender and numerous biographies of Darwin. The planning process for the celebrations also generated discussion of a Darwin Fellowship program that would serve to initiate practitioners into the science of evolution and would also help to promote the teaching of evolution in American high schools.<sup>153</sup>

150. These motives as well as his personal recollection of the celebration are discussed in Sol Tax, "The Celebration: A Personal View," in Tax and Callender, *Evolution after Darwin*, pp. 271–282.

151. See *ibid.*, photographs and program inclusions, pp. 278–279.

152. *Ibid.*, p. 282.

153. Minutes of 1957 Meetings, recorded by Harlan Lewis, Secretary, SSE Papers.

In the next decades, the SSE continued to hold its annual meetings, to sponsor special conferences, and to facilitate communication between evolutionists, other scientists, and the wider culture dealing with critical political and ethical issues such as the evolution/creation controversies. It also continued to sponsor the journal *Evolution*. The 1993 membership list stands at 3,111 individuals (with a healthy annual increase of approximately 100) plus 1,477 institutional subscribers; the total number of volumes printed is 5,000.<sup>154</sup> Regular membership dues are currently \$50.00, an average for scientific societies. The present object of the society is stated in the first issue of 1993: "The object of the Society for the Study of Evolution, which was founded in March 30, 1946, is the promotion of the study of organic evolution and the integration of the various fields of science concerned with evolution. The Society endeavors to accomplish this through the publication of the journal and through meetings and working committees."

The American Society of Naturalists, the most closely related society, lists the significantly smaller number of 993 members in 1993, with 1,600 institutional subscribers, and a total of 3,800 issues printed.<sup>155</sup> Its own present objective has altered appreciably to return to an evolutionary slant: "to advance and diffuse the knowledge of organic evolution and other broad biological principles so as to enhance the conceptual unification of the biological sciences." This return to a strong evolutionary emphasis was concomitant with the emphasis on supporting theoretical and mathematical approaches to evolution: hence, the goal of "conceptual unification" stands clearly as the objective of the society. The shift was especially pronounced under the editorial direction of Marcus Feldman at Stanford University.<sup>156</sup> The two societies continue to collaborate on joint meetings and to share common members.

The SSE itself may be understood to function as the central site for negotiation and communication transfer, facilitating the construction of the discourse of the discipline through its sponsored activities. Among its key nodal participants is Douglas J. Futuyma,

154. Data obtained from Donald Waller, Executive Vice-President, SSE.

155. Data obtained from Peter Chabora, Secretary, American Society of Naturalists.

156. A detailed history of the American Society of Naturalists describing its shifts in editorial policies, especially within changing American organizational contexts, remains to be written. Up until 1989, the ASN appears to have functioned as a smaller and possibly more elite society of members. In 1989 the membership increased dramatically from 599 to 1107, due to the fact that the University of Chicago Press took over publication of the journal and also to the "liberalization" of the society, which made subscribers into members.

author of the very widely read textbook *Evolutionary Biology* and president-elect of the American Society of Naturalists.

#### CLOSING THOUGHTS ON THE FOUNDING OF THE SOCIETY

This paper has offered a detailed historical reconstruction of the complex round of negotiations leading to the organization and institutionalization of the study of evolution by the founding of the Society for the Study of Evolution in the period recognized as the evolutionary synthesis. These efforts followed from movements to experimentalize and quantify the study of evolution in order to lend it legitimacy within the positivist theory of knowledge that then held sway. The initial push to organize a society came from systematists-naturalists in both Britain and the United States who were in the process of reconfiguring disciplines like ecology and genetics, with a view of reforming taxonomic practice into an experimental science. One motive was the need for a suitable forum for the publication of research articles on evolutionary topics. With support from paleontologists, the initial efforts to organize around newer developments in speciation through the Society for the Study of Speciation were extended into an official NRC-backed committee that would attempt to integrate the discipline of genetics (which had experimental legitimacy) with paleontology (which provided direct evidence of evolution), and then with systematics (which would link the two). These earlier institutions fed eventually into the Society for the Study of Evolution, which was founded during the postwar boom in American science that profited from newly available material, personal, and financial resources. The new society would function as the disciplining site for evolutionary biology.

Ernst Mayr played an especially critical role in the founding of the SSE. Facilitating communication transfer between members through his correspondence networks, securing resources to fund the society and journal through grant proposals, promoting the cause of evolution by disseminating information (which in turn attracted new members), Mayr became a central disciplining force of the society. In editing the journal *Evolution*, moreover, he directed the process of reconfiguring evolutionary studies into a rigorous science. The publication of a journal that successfully represented the diversity of members' voices within a unified voice of evolution was due to the editor's active involvement in the selection/exclusion and solicitation of suitable manuscripts and in the dialogue that took place between members.

Mayr's centrality in this historical reconstruction is not merely an artifact of the set of documents deposited at APS by the successive secretaries of the society, moreover.<sup>157</sup> His role as the "effective leader" in the early period of the SSE and his contributions to the "onerous" task of editor of the new journal were given credit by Simpson in his autobiography, *Concession to the Improbable*, in which he stated that he himself had been more active in promoting the Society for Vertebrate Paleontology, though he did take credit for arranging the APS grant that would fund the SSE.<sup>158</sup> Emerson also gave credit to Mayr in founding the society, and to Simpson as well. In an amusing series of exchanges between Mayr and Emerson in 1946, Mayr had emerged as the "founding father" of the society, and Emerson as the "midwife."<sup>159</sup> In 1952 Emerson reflected on the history of the society of writing: "I feel a certain paternal pride in the Society for the Study of Evolution. Ernst Mayr said that I was not the father, however, but only the midwife. I resent the implication concerning my masculinity."<sup>160</sup> This quotation sheds interesting light on Mayr's "leadership" role in the society: if we view Mayr as a "leader" of the SSE, it was because he effectively *served* the society. His role was especially critical in that he served as the central conduit of a network of workers, all of whom played their own roles in the process of disciplining evolutionary biology.

With the concomitant establishment of meetings, conferences, rituals of celebration, textbooks of instruction, agreed-upon problems (and their solutions), and semipopular and popular books that extended the linkages to a wider audience, evolutionary biology emerged as a discipline of knowledge. The organ of this new discipline, the SSE, in turn went through a process of negotiating its identity and location among other closely related societies. Only through prolonged negotiations did the most closely related society, the American Society of Naturalists, give way to the newer society; in the process, the ASN redefined its own identity as a biological

157. In 1965 Mayr declined an invitation from Emerson to write the history of the society because he had been too closely involved in its founding: "I was the one who carried the ball both in the founding of the society and in the establishment of the journal. I wouldn't like to write an account in which the word 'I' occurs too often" (Mayr to Emerson, October 28, 1965, SSE Papers).

158. G. G. Simpson, *Concession to the Improbable: An Unconventional Biography* (New Haven: Yale University Press, 1978), p. 129.

159. Emerson had responded to Mayr's designation of him as midwife by saying "Thanks for the official title! I think I prefer 'obstetrician at the time of delivery'" (Emerson to Mayr, July 5, 1946, SSE Papers).

160. Emerson to Theodor Just, March 28, 1952, SSE Papers.

society that no longer placed special emphasis on evolution. The SSE received its greatest support from the scientists of many disciplines who were members of the AAAS, who effectively served to “pull” the society as it was forming and to mediate conflict with closely related biological societies whose members looked on evolution as an illegitimate science or who felt themselves endangered by the emergence of the new society. The initial reluctance to join AIBS, the new “umbrella society,” was in some measure due to the fact that the new organization that would cover all biologists included experimentalists who had denigrated non-experimental evolutionary practice – yet with time, the key players in the SSE would be supported heavily by, and in turn receive support from, the same general audience of biologists. Evolutionary science itself became more experimental, but through linkages with traditional natural history practices it also preserved more traditional areas of evolutionary research at the same time that it became a legitimate science.

Critical to this process of disciplining evolutionary biology was the construction of a disciplinary discourse that would bind together the heterogeneous practices of evolutionary biology. The emergence of this discourse was facilitated, and in fact was accelerated, by the availability of communications technology: from the initial mimeographed questionnaires that gauged the interest in and support for a new organization and probed the direction it should take, to bulletins that introduced members to each other and that problematized the new field, to correspondence networks between key members and the remaining members of the society, to the founding of an international journal that brought the study of evolution to postwar America, and then to the promotion of evolution on television and radio broadcasts, the growth of a self-aware community of workers with a common discourse relied heavily on communications technology. This was especially important in a postwar society that had to connect workers across the continent.

Examining communications practices, the German philologist Friedrich Kittler has drawn on the work of Walter Benjamin and Marshall McLuhan to demonstrate how “discourse networks” emerged from the technological practices of recording in the period 1800–1900.<sup>161</sup> The study of the growth of such discourse networks has also formed the program of research for Charles Bazerman and others, who have explored written scientific texts and their roles

161. Friedrich Kittler, *Discourse Networks* (Stanford: Stanford University Press, 1985, 1987).



in discipline formation.<sup>162</sup> In a similar manner, the emergence of the *sensus communis* of the SSE – with its own common language that identified members of the group (recall, for instance, the negotiation over the names of the society and journal), and with the linkages between disciplinary discourses that took place through the detailed “minutes” of the society, through the correspondence, and, more interestingly, through the use of questionnaires and published letters to and from the members – relied heavily on the practices of recording (recall the *detailed* minutes of the societies). Historians of scientific societies of the seventeenth century have examined similar communications patterns in the establishment of correspondence networks and have revealed the key role of nodal participants like Nicholas-Claude Fabri de Peiresc in serving as conduits for information transfer between members during the emergence of a scientific community.<sup>163</sup> The pattern of formation in these earliest scientific organizations is analogous to the pattern seen in the organization of evolution in the twentieth century. From the initial queries, to “letters” of exchange that gave way to a “journal” (with the “daily” temporal notion built into its meaning), to the institutionalization of a disciplining society, the formation of other such societies is of great interest to historians of modern science. What makes the study of the emergence of twentieth-century scientific disciplines especially interesting is the forms of the technologies employed and the acceleration of communication.

Another interesting aspect of the formation and growth of the SSE is the role played by specific disciplines. Anthropology was not centrally located within the society, or in the evolutionary synthesis as whole – a fact borne out by Mayr’s complaints that not enough manuscripts on the subject were being submitted to the journal. In this regard, Tax’s initiating and organizational role in bringing anthropology into the synthesis through his involvement in the Darwin Celebration of 1959 deserves special note. Paleontology proved equally difficult to incorporate into the SSE.

162. The most recent book is Charles Bazerman and James Paradis, eds. *Textual Dynamics of the Professions* (Madison: University of Wisconsin Press, 1991); and see Bazerman’s pioneering *Shaping Written Knowledge* (Madison: University of Wisconsin Press, 1988). For an application of rhetorical theory to biology see Greg Myers, *Writing Biology* (Madison: University of Wisconsin Press, 1991).

163. See the classic study, Harcourt Brown, *Scientific Organizations in Seventeenth-Century France* (Baltimore: Williams and Wilkins, 1934). For a recent article on Peiresc see Lisa T. Sarasohn, “Nicolas-Claude Fabri de Peiresc and the Patronage of the New Science in the Seventeenth Century,” *Isis*, 84 (1993), 70–80.

Very possibly paleontologists could not identify comfortably with the appellation of “evolutionary biology” – the category of knowledge that was being negotiated – for the primary allegiance of most of them was to geology, not biology. Given this split identity, paleontologists like Simpson referred to the “others” as “neobiologists.” Our understanding of the turmoil and debates surrounding paleontologists in the late 1970s and early 1980s – and the subsequent reidentification of these scientists as “paleobiologists” – may become clearer with further historical work on paleontology, the SSE, and the evolutionary synthesis.<sup>164</sup>

One final point deserves special emphasis: the organization of evolution, complete with the establishment of a journal-issuing society, officers to manage the society, and meetings and ritual practices, took place within the positivist theory of knowledge. If the actors were engaged in an act of legitimation, it was by constructing a science of evolution that had legitimacy within an epistemic framework. While we may view the historical actors in this story as tacticians, strategists, “mere” rhetoricians, or even scientist-entrepreneurs trying to have their way in the seizing of resources at a time of “threat”, acting in the interest of their careers, and excluding others, no amount of interest, personal or social, is sufficient to account for the emergence of a legitimate category of research and a *science* of evolutionary biology. In this historiographic perspective the organizers of the SSE, and the organization itself, were part of a process of unification whose *script* had been written within the positivist theory of knowledge as it had emerged from Enlightenment thought. The most faithful rendering of the historical account is best given by the voices of the actors who saw themselves engaged in a project to organize knowledge within a unified theory of knowledge: the organization of evolution had been a “dream.”

To close with a historiographic consideration, the conscious and deliberate preservation of the documents concerning the founding of the society, and the subsequent attempt in the mid-1960s to write a history of the society, is also worthy of future consideration by historiographers of science. If the selective writing and rewriting of the history of science serves to construct and reconstruct the memory and to redefine the collective identity of the

164. See Rainger, *Agenda for Antiquity* (above, n. 24), on twentieth-century paleontology. See also Léo F. Laporte, “George G. Simpson, Paleontology, and the Expansion of Biology,” in Benson, Maienschein, and Rainger, *American Expansion of Biology* (above, n. 130), pp. 80–106; and Joe Cain, “Building a Temporal Biology: Simpson’s Program for Paleontology during an American Expansion of Biology,” *Earth Sciences History*, 11 (1992), 30–36.

discipline (in so doing, disciplining its boundaries), then this historical account reconstructs the memory of the SSE as the organizational “organ” – the instrument or tool – for the unification of the discipline of evolutionary biology.

### *Acknowledgments*

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## APPENDIX 1: Attendees present in St. Louis (see Figure 1)

E. Mayr	W. H. Camp	Charles H. Seevers
Th. Dobzhansky	G. G. Simpson	Rupert L. Wenzel
Sewall Wright	George B. Happ	H. S. Dybas
Thomas Park	Donald C. Lowrie	LaMont C. Cole
W. S. Stone	C. Clayton Hoff	Robert P. Wagner
Austin Phelps	Alfred Kinsey	Alfred Emerson
M. F. Day	E. Novitski	W. Frank Blair
J. N. Dent	A. Franklin Shull	M. K. Elias
M. R. Irwin	C. C. Tan	
I. E. Gray	C. Pavan	
F. M. Hull	J. T. Patterson	
John H. Davis	G. B. Mainland	
J. Chester Bradley	F. B. Isely	
Hyman Linner	Albert P. Blair	
Ruth Patrick	William Hovanitz	
Herbert P. Riley	M. Demerec	
John M. Carpenter	E. B. Babcock	
Robert L. Usinger	A. M. Chickering	
E. Gorton Linsley	G. W. Wharton	
F. J. Broun	Waldo L. Schmitt	
Hampton L. Carson	E. Raymond Hall	
William A. Dreyer	Arnold Grobman	
Ernst C. Abbe	Carl Epling	
Edgar Anderson	William M. Clay	
Harrison D. Stalker		
Richard W. Holm		

APPENDIX 2: Officers of the Society for the Study of Evolution, 1946–1952<sup>165</sup>

<i>Year</i>	<i>President</i>	<i>Vice-presidents</i>	<i>Secretary</i>	<i>Treasurer</i>	<i>Editor</i>
1946	G. G. Simpson	A. S. Emerson J. T. Patterson E. B. Babcock	E. Mayr	K. P. Schmidt	
1947	J. T. Patterson	L. R. Dice A. S. Romer G. L. Stebbins	S. A. Cain	K. P. Schmidt	E. Mayr
1948	G. L. Stebbins	A. E. Emerson J. Huxley S. Wright	S. A. Cain	K. P. Schmidt	E. Mayr
1949	N. D. Newell	E. Anderson Th. Dobzhansky A. Müntzing	S. A. Cain	K. P. Schmidt	E. Mayr
1950	E. Mayr	A. S. Romer D. Lack S. A. Cain	T. Just	K. P. Schmidt	E. H. Colbert
1951	Th. Dobzhansky	J. C. Clausen D. D. Davis E. B. Ford	T. Just	K. P. Schmidt	E. H. Colbert
1952	E. B. Babcock	K. P. Schmidt H. J. Muller A. Gustafsson	T. Just	C. M. Bogert	E. H. Colbert

*Council Members*

Class of 1946:	E. R. Dunn H. J. Muller	Class of 1951:	G. L. Stebbins R. A. Stirton
Class of 1947:	G. L. Jepsen S. Wright	Class of 1952:	C. L. Hubbs C. Epling
Class of 1948:	Th. Dobzhansky R. Chaney	Class of 1953:	A. E. Emerson E. Mayr
Class of 1949:	W. M. Hiesey G. G. Simpson	Class of 1954:	S. Wright A. H. Miller
Class of 1950:	L. R. Dice W. H. Camp		

165. Reproduced from a document in the SSE Papers.

APPENDIX 3: Geographic distribution of members – 1949<sup>166</sup>

Alabama	1	New Hampshire	4
Arizona	2	New Jersey	10
Arkansas	1	New Mexico	–
California	100	New York	91
Colorado	6	North Carolina	6
Connecticut	15	North Dakota	1
Delaware	–	Ohio	20
Florida	9	Oklahoma	5
Georgia	4	Oregon	3
Idaho	3	Pennsylvania	31
Illinois	60	Rhode Island	2
Indiana	16	South Carolina	–
Iowa	4	South Dakota	2
Kansas	15	Tennessee	7
Kentucky	4	Texas	15
Louisiana	2	Utah	2
Maine	2	Vermont	3
Maryland	11	Virginia	9
Massachusetts	31	Washington	7
Michigan	33	West Virginia	1
Minnesota	9	Wisconsin	17
Mississippi	1	Wyoming	–
Missouri	11	District of Columbia	10
Montana	1	Hawaii	4
Nebraska	4	Puerto Rico	1

166. From a document appended to the Minutes of the 1949 Meeting of the SSE, SSE Papers.